The Direct and Intergenerational Effects of Criminal History-Based Safety Net Bans in the U.S.

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Abstract

We study the lifetime banning, as introduced by United States Public Law 104-193, of individuals convicted of felony drug offenses after August 22, 1996 from ever receiving future SNAP benefits. Using a regression discontinuity design that leverages CJARS criminal history records with federal administrative and survey data, we estimate the causal impact of safety net assistance bans, finding significant reductions in SNAP benefit take-up, which creates unintentional spillovers to spouses and children and persist long after ban revocations occurred. While we observe limited changes to other adult outcomes, children's short- and long-run outcomes worsen, especially those impacted at young ages.

Keywords: Food Stamps; Labor Supply; Criminal Behavior; Childhood Development
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1 Introduction

The social safety net in the United States is a key policy lever for reducing poverty and improving household well-being, providing valuable assistance for households in economic distress. Nearly one in eight individuals received benefits through the Supplemental Nutritional Assistance Program in 2021 alone (Hall and Nchako, 2022). However, criminal histories often preclude individual participation in cash assistance, housing assistance, or employment opportunities, undermining the economic well-being of this increasingly large, vulnerable segment of the population.

In this paper, we examine the impact of criminal history-based bans from public assistance programs on individuals and their families, combining a wealth of administrative data on criminal histories, labor market outcomes, sociodemographic characteristics, and survey-based measures of public benefit receipt and well-being. Using a series of regression discontinuity designs across eight states,[1] we leverage sharp changes in assistance eligibility as a result of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), which prohibited individuals with felony drug convictions for offenses committed after August 22, 1996 from receiving benefits through either the Supplemental Nutrition Assistance Program (SNAP) or Temporary Assistance for Needy Families (TANF) programs.[2] We interpret these regression discontinuity estimates as causal impacts given our evidence of balance in both the caseload density and pre-existing characteristics across the implementation threshold.

We quantify the degree to which these safety net bans actually translate into lower take-up of SNAP benefits.[3] These novel estimates are critical for interpreting the reduced form impacts on future outcomes, which have been the main focus of prior work in this literature (Yang 2017; Tuttle 2019). We find that felony drug convicts who became ineligible for SNAP benefits when PRWORA was implemented in 1996 are 13.9 percentage points ($\downarrow$ 38%) less likely to report receiving benefits on an annual basis between 1997 and 2019.[4] Such findings though rely on repeated cross-section data contained in the Current Population Survey and the American Community Survey. If lagged SNAP receipt impacts future outcomes, such first stage estimates may be too small given high churn rates year-to-year in benefit receipt.

[1] Our sample includes Arizona, Florida, Georgia, New Jersey, North Carolina, North Dakota, Oregon, and Texas.

[2] SNAP was formerly known as the Food Stamps Program until 2008.

[3] While TANF eligibility was also affected by PRWORA, we focus on SNAP eligibility and participation given very low rates of TANF participation in our sample of mostly male defendants with drug felony convictions.

[4] Applying these estimates to the duration of the follow-up period implies 3.2 fewer years of SNAP benefits on average in this population over a 23 year follow-up period.

To address this, we develop an aggregation procedure leveraging population-specific SNAP churn statistics combined with year-specific first stage estimates. Together, our approach yields a cumulative first-stage estimate of 32 percentage points that accounts for both realized shocks to contemporaneous and prior benefit receipt in our repeated cross-section data. If SNAP benefits have dynamic impacts on individuals and their families, the simple contemporaneous receipt approach to the first stage will severely understate the true size of the marginal population (43%) and overstate the implied treatment effects (230%). These first-stage estimates (both contemporaneous and cumulative) are novel and to the best of our knowledge, we are the first to document changes in benefit receipt as a result of this disqualifying criterion.[5]

From a legal perspective, defendants' romantic partners and children's SNAP eligibility should not be jeopardized by the PRWORA bans. Expanding the set of survey responses, however, to include both the focal justice-involved individual as well as their romantic partners/co-parents and children still shows strong evidence that households altogether were less likely to receive any SNAP benefits as a result of the bans. This pattern suggests an unintended outcome of the policy: that SNAP-eligible romantic partners and dependent children had lower benefit receipt, whether due to being incorrectly removed from the program or because of being discouraged from applying for benefits in the first place.

In the intervening years since PRWORA, many states have modified their criminal history-based bans to affect narrower segments of the justice-involved population (e.g., drug distribution felonies only) or have repealed them altogether. Despite the goals of these policy changes, when we limit our follow-up period to just jurisdictions and times when bans had been scaled back or removed, we strikingly find no change in impacts to take-up. The continued presence of a sharp discontinuity in benefit receipt during these post-ban periods suggests that imperfect information, path dependence, or other take-up frictions continue to play a significant role in determining household benefit usage, despite the disqualification criteria being eliminated.

In spite of a strong and persistent first stage relationship, we fail to find evidence of meaningful changes to measurable adult outcomes in our sample observed during the 20+ years following the ban. Using a variety of outcomes, we do not observe differences in future justice involvement across the discontinuity. These results align with findings from Luallen, Edgerton, and Rabideau (2018) and findings for TANF/cash assistance in Sugie and Newark (2023), but contrast with Yang (2017) and Tuttle (2019) who find increases in re-

[5] While changes in SNAP take-up are our preferred measure of the first-stage, the ban may also influence outcomes through individuals experiencing the insurance value of knowing a safety net exists (e.g., Deshpande and Lockwood 2023), even if they never take-up benefits. Such a response could generate a violation of the exclusion restriction, and so we present both reduced form and instrumental variables estimates throughout.

incarceration among prisoners and drug traffickers, respectively, disqualified from assistance as a result of the PRWORA restrictions. Sugie and Newark (2023) find quicker arrests among the individuals ineligible for food stamps due to a drug felony conviction in California.[6] Given that our study population has high recidivism risk with more than 60 percent of defendants experiencing a new criminal charge over the follow-up period, these null results may be unsurprising, yet intensive margin estimates also yield a similar conclusion.

We similarly find null effects on employment rates, measured using employer-reported W-2 information returns on annual earnings. While economic theory would predict increases in household labor supply should compensate for the lost transfer income, we build on a growing body of empirical evidence that fails to observe such an employment response (Hoynes and Schanzenbach 2012; East et al. Forthcoming; Gray et al. 2023; Cook and East 2023 ; Cook and East 2024). Cook and East (2023) only find changes in labor supply among the minority of SNAP participants who work prior to applying for benefits. Recall that the universe of our study population holds felony conviction records, which research has shown to generate labor market scarring (e.g., Pager 2003; Mueller-Smith and Schnepel 2021). Consequently, it may be even less likely that this population is able to adjust their labor supply to compensate for lost benefits. Consistent with this mechanism and with public benefits supporting household labor supply, we find that social safety net bans actually lead to declines in earnings for those with little attachment to formal labor markets.

While structural factors like criminal records and weak labor market attachment might limit the capacity for adults in our sample to respond to safety-net bans, a contraction in SNAP receipt may impact children who still remain innocent of such scarring effects and are in the midst of a critical phase of human capital development. A growing economic literature documents a causal link between child outcomes and parental access to social assistance (Hoynes, Schanzenbach, and Almond 2016; Bronchetti, Christensen, and Hoynes 2019; East 2020; Barr and Smith 2023; Bailey et al. 2024). Bailey et al. (2024) find that access to food stamps before the age of five improves long-term education, health and economic outcomes. In our context, we evaluate impacts on children in households that already experience substantial disadvantage due to a caretaker carrying a felony conviction. This is a large and extremely vulnerable population within the United States (Finlay, Mueller-Smith, and Street, 2023). Using the 2008-2019 waves of the American Community Survey, we find declines in measures of family stability and childhood well-being, including lower rates of

[6] One complication with interpreting the results from Luallen, Edgerton, and Rabideau (2018) is that their running variable is based on conviction date, rather than the offense date and there may be a significant lag between these two dates. Yang (2017) and Tuttle (2019) both examine return to prison as an outcome, which may not capture all possible forms of criminal justice contact. We build on both of these findings by examining recidivism across several jurisdictions and multiple definitions of criminal justice contact.

growing up in two-parent households and of completing high school.[7] These deleterious impacts on childhood outcomes also extend into their transition to adulthood as well, with affected children having lower earnings, living in higher poverty neighborhoods, and being more likely to be on SNAP themselves as young adults. Consistent with Bailey et al. (2024) and a large literature emphasizing the importance of the early-life environment (see Heckman 2007 and reviews by Almond and Currie 2011; Currie and Almond 2011; and Almond, Currie, and Duque 2018), these negative impacts are concentrated among children who are under five when SNAP eligibility is removed for a member of the household.

This paper offers several key contributions to a large literature evaluating the economic and social impacts of safety net programs. First, we are the first to quantify the long-term impacts of the PRWORA drug felony disqualifications on program participation both at the individual and household level and to document persistence in lower take-up after disqualifications are repealed. Second, we are able to observe spousal and intergenerational impacts while previous research generally focuses on the direct impacts of access to the social safety net on individual behavior in the context of SNAP and TANF (Yang 2017; Luallen, Edgerton, and Rabideau 2018; Tuttle 2019 ; Sugie and Newark 2023), SSI (Deshpande and Mueller-Smith, 2022), and Medicaid (Arenberg, Neller, and Stripling Forthcoming; Jácome 2020). Finally, we add a novel source of identifying variation to the literature on the causal impact of safety-net program access and takeup on childhood development, adding to growing evidence that the social safety net protects vulnerable children and improves health and long-term outcomes (Hoynes, Schanzenbach, and Almond 2016; East 2020; Barr and Smith 2023; Hawkins et al. 2023; East et al. Forthcoming; Bailey et al. 2024).

2 Institutional Setting and Data Infrastructure

2.1 The Personal Responsibility and Work Opportunity Reconciliation Act of 1996

The mid-1990s marked a period of dramatic changes in social welfare policy in the United States, culminating in the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996. The reform implemented more stringent work requirements and time limitations for assistance programs and replaced the traditional cash welfare program, Aid to Families with Dependent Children (AFDC), with Temporary Assistance to Needy Families (TANF) block grants.

PRWORA excluded several population groups from participating in assistance programs

[7] Recent work from Kearney (2023) highlights the socioeconomic advantages of two parent families and how these advantages accrue to and improve the long-run outcomes of children in the household.

altogether, including individuals with convictions for felony drug offenses, as part of the "War on Drugs" (Paresky, 2017). Specifically, Section 115 of PRWORA permanently banned individuals who committed a felony drug offense after August 22, 1996 from receiving SNAP or TANF benefits, regardless of whether the conviction was for a use, possession, or distribution charge. The key concern for legislators was that public assistance benefits were being used to purchase illegal substances.

The reform also provided states greater discretion in how they used federal funding to deliver program benefits, including both eligibility rules and benefit levels. States were able to modify or opt out of the bans imposed by Section 115 of PRWORA for convicted drug offenders. Over the past several decades, nearly every state has either modified these bans or opted out entirely. Typical modifications include imposing restrictions only on the most serious types of drug charges (e.g., trafficking/distribution); requiring drug testing among applicants with drug convictions; requiring participation in a drug treatment program; or imposing only temporary disqualification periods following a drug felony conviction.

Among the eight states included in our analysis, two had completely opted out of the SNAP ban (New Jersey and Oregon) and six had modified (or later opted out of) the ban (Arizona, Florida, Georgia, North Carolina, North Dakota, and Texas) by 2020.[8] Several of these modifications occurred during our 2005-2019 ACS and 1997-2019 CPS analysis window allowing us to evaluate whether there are persistent differences in participation even after the bans are lifted or modified.

2.2 Data

We use detailed criminal history information from the Criminal Justice Administrative Records System (CJARS) and link these records to a broad set of socioeconomic outcomes accessed through the Federal Statistical Research Data Center (FSRDC) system.

The Criminal Justice Administrative Records System compiles criminal histories from jurisdictions across the United States and currently covers roughly eighty-four percent of the U.S. population (Finlay and Mueller-Smith n.d.; Finlay, Mueller-Smith, and Papp 2022).

[8] Oregon fully opted out of the ban in 1997, but this was complicated by two factors. First, the Portland Metropolitan Statistical Area has significant overlap with the state of Washington, which did not lift its ban until 2004, potentially leading to misinformation among potential beneficiaries. Second, Oregon later allowed for parole/probation officer discretion in recommending that benefits be denied for individuals convicted of distribution offenses. New Jersey opted out of the SNAP ban in 2000, but left in place a lifetime ban from their general assistance program for those convicted of drug distribution charges until 2022. Arizona lifted the ban on SNAP for individuals convicted of use or possession offenses in 2017, Florida modified the ban to only apply to drug trafficking offenses in 1997, and North Carolina restricted the ban to individuals convicted of certain classes of felonies, primarily distribution offenses, in 1997. North Dakota first partially removed the ban in 2013 and then fully repealed in 2017. Texas and Georgia lifted the ban on SNAP in 2015 and 2016, respectively.

Individuals are linked across jurisdictions and stages of the criminal justice system using a probabilistic matching algorithm (Gross and Mueller-Smith, 2021) and are also assigned Protected Identification Keys (PIKs) using the Census Bureau's Person Identification Validation System (PVS), permitting linkage to other survey and administrative records within the internal Census Bureau data infrastructure.[9] In this paper, we use CJARS records to define our estimation sample of interest and to construct future and prior measures of criminal justice involvement, classifying offenses using the procedure from Choi et al. (2023).

We link a wealth of demographic and socioeconomic outcomes to these criminal histories. We first construct individual demographics using the Census Bureau's Numident and Best Race and Ethnicity files.[10] To measure non-criminal justice outcomes, we link individuals to IRS W-2 tax records, the 2005-2019 American Community Surveys (ACS), and the 1997-2019 Current Population Survey Annual Social and Economic Supplement (CPS ASEC). In particular, the ACS and CPS survey responses on public assistance usage allow us to quantify the impact of the PRWORA ban on benefit receipt.

One feature of the survey-based responses of SNAP receipt is that they measure benefit receipt at the household-level, rather than the individual-level. These surveys are an imperfect measure of benefit receipt since the ban should only affect disqualified individuals, rather than the entire household. Moreover, while we also have administrative records on individual-level SNAP benefit receipt for a subset of years in Arizona, North Dakota, and Oregon, these data are insufficient to fully characterize the first-stage response across the entire sample, both geographically and temporally.[11] Instead, we use these administrative data to characterize both churn rates and mean benefit duration in our sample population, and later combine them with our survey-based measures to estimate the fraction of the sample population who are ever affected by the ban. We discuss this exercise in greater detail in Section 4.

Recent research highlights the important issue of measurement error in safety net benefit receipt survey responses (Meyer, Mittag, and Goerge 2022). To address this concern, we use administrative SNAP data from Michigan and Maryland, in conjunction with the previously mentioned states, linked to contemporaneous survey responses for felony drug defendants to quantify the degree of measurement error in our target study population.[12] This exercise

[9] Our sample implicitly contains only individuals who are U.S. citizens or legal immigrants as an individual can only be assigned a PIK if they have a valid social security number or individual taxpayer identification number. For more on the PVS process, see Wagner and Lane (2014).

[10] We code race/ethnicity as a singular measure using information from the Census Bureau Best Race and Ethnicity files.

[11] Our data covers 2005-2019 in North Dakota and 2009-2019 in Arizona and Oregon.

[12] Michigan and Maryland are excluded from our main analysis samples as both states issued repeals of the bans soon after PRWORA. Furthermore, Maryland also deployed proactive outreach to potentially impacted

and how we use these estimates in our analysis are described in greater detail in Section 3.

To construct our estimation sample, we first identify CJARS jurisdictions with criminal court data coverage dating back to at least 1994 which limits our analysis to individuals in eight states: Arizona, Florida, Georgia, New Jersey, North Carolina, North Dakota, Oregon, and Texas. Among justice-involved individuals in these jurisdictions, we track outcomes for individuals whose first disqualifying felony drug conviction occurred within 330 days of the August 22, 1996 cutoff date.[13] Including individuals only once in the estimation sample ensures short-run recidivism outcomes are not biased by individuals endogenously appearing on both the left- and right-hand sides of the discontinuity.

2.3 Identifying Romantic Partners/Co-Parents and Children of Justice-Involved Individuals

To quantify spillover effects of the bans, we use detailed records on household composition from Finlay, Mueller-Smith, and Street (2023) to identify romantic partners/co-parents and children of the justice-involved individuals in our estimation sample. These data provide person-level relationship and residency links on an annual basis, quantifying how individuals are related (e.g., parent-child) and the exact address where each of them are living. In addition, we leverage these crosswalks to examine the dynamics of household structure and the fraction of childhood that children spend in two-parent households.

3 Empirical Strategy and Identifying Assumptions

We estimate the effect of being banned from public assistance programs using a pooled regression discontinuity design (RD). Our identifying variation is based on the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, which prohibited individuals convicted of felony drug offenses from receiving public assistance through SNAP or TANF if their offense was committed after August 22, 1996. Formally, we

populations to ensure they were aware of their benefit eligibility in spite of the federal legislation.

[13] The exact disqualifying conviction varies depending on the jurisdiction. Following the relevant statutes, we include only individuals convicted of drug trafficking felonies in Florida and distribution offenses in North Carolina. We also focus on drug distribution felonies in New Jersey due to the offense's disqualifying interaction with general assistance programs in the state. Other states in our sample include use, possession, and distribution offenses. See Appendix Table 1 for additional information on our sample construction and relevant repeal legislation.

estimate the reduced form impact of the ban using linear regressions of the following form:

$$\begin{aligned} Y_{it,s} = \alpha + &\beta \text{After PRWORA}_i + \gamma_s(\text{Offense Date}_i) \\ &+ \delta_s(\text{After PRWORA}_i \times \text{Offense Date}_i) + X'_{it}\phi + \varepsilon_{it,s} \end{aligned} \tag{1}$$

where $Y_{i,s}$ is a measure of contact with the criminal justice system, labor market outcome, or survey response for individual or household i from state s in survey year or follow-up period t.[14] After PRWORA$_i$ is an indicator that is equal to one if the offense occurred after PRWORA was enacted and is zero otherwise. Offense Date$_i$ is the running or forcing variable in our regression discontinuity, which we normalize to zero at the cutoff.[15] We also allow the relationship between the running variable and the outcome to flexibly vary by state s on either side of the discontinuity. X_{it} is a vector of controls, including race-by-sex indicators, age, number of prior misdemeanor convictions, an indicator for whether the conviction occurred in an urban county, and Commuting Zone fixed effects. The coefficient of interest, β, captures the reduced-form effect of being banned from SNAP and TANF on future outcomes.

The key identifying assumption in our research design is that outcomes would have continued to evolve smoothly across the cutoff in the absence of the policy reform. We also require that individuals did not strategically commit their disqualifying offenses before the cutoff date in order to avoid the public assistance ban. We provide empirical support for both of these identifying assumptions in Figure 1. First, we plot the average daily caseload density in bins for a bandwidth of 330 days on either side of the discontinuity. Consistent with our identifying assumption, we find no evidence of systematic date manipulation.

We next test whether individuals on either side of the discontinuity are observably similar. As a summary measure of future criminal activity, we predict the probability the justice-involved individual receives a future criminal charge in the following ten years using all two-way interactions of the above listed covariates (Panel B). We find no consistent evidence that individuals on either side of the discontinuity are observably different, either when using this summary measure or when testing covariates individually in Table 1.[16]

Finally, we test whether individuals who are banned from SNAP and TANF are differentially linked to romantic partners and/or children. We find no effect on matching to a

[14] Other papers have used similar research designs to study diversion in the criminal justice system (Mueller-Smith and Schnepel, 2021), the impact of financial sanctions in the criminal justice system (Finlay et al., 2023), and the impact of SSI on criminal behavior (Deshpande and Mueller-Smith, 2022).

[15] In practice, we use either the offense or filing date to account for variation in data availability across jurisdictions. These two date measures should be highly correlated (Mueller-Smith and Schnepel, 2021).

[16] Appendix Figure 1 also provides a graphical depiction of the magnitude of the estimated change in covariate across the discontinuity relative to the sample mean.

romantic partner/co-parent or being matching to a dependent child in Panels E and F of Figure 1. Such estimates are inclusive of potentially endogenously formed children during the follow-up period, providing not only evidence of caseload balance but also some initial evidence that adult behavior does not respond to the safety net bans.[17]

To help interpret the magnitude of the impact of the PRWORA bans, we also present and discuss instrumental variable (IV) estimates where the reduced form discontinuity estimates for our outcomes are scaled by the discontinuous change in SNAP receipt. The IV coefficients can be interpreted as an estimate of the effect of contemporaneous or prior SNAP receipt on a particular outcome with the exogenous variation in SNAP receipt coming from the discontinuous jump in eligibility as a result of the ban. These IV estimates are particularly useful when comparing implied effects across groups with differential responses to the ban in terms of SNAP receipt. However, they should be interpreted with caution for a few reasons. First, losing eligibility for SNAP/TANF could impact outcomes even if there is no change in participation since there may be insurance value to program eligibility that itself may influence behavior and decisions (e.g., Deshpande and Lockwood 2023). Second, we observe SNAP receipt in the ACS and CPS surveys and use this for our first-stage but outcomes could also be impacted through discontinuous changes in TANF eligibility/receipt. Both of these are violations of the exclusion restriction assumption needed to attach a causal interpretation to the IV estimate.

Measurement Error in Reporting of Benefit Receipt: A common concern when using survey-based data to quantify benefit receipt is that benefits are measured with error (Bound, Brown, and Mathiowetz 2001). These sources of error can include recall bias, survey complexity, or social stigma. Such measurement error if left unaddressed could lead to underestimates of our first stage relationship and consequently overestimates of our IV estimates.

Prior work has illustrated that the degree of SNAP benefit misreporting in the ACS is potentially large (Meyer, Mittag, and Goerge 2022). However, the populations used in previous work to estimate the degree of misreporting may not be directly comparable to our sample since individuals with felony drug convictions may fear additional stigma or be skeptical of engaging with government surveys.

We quantify the degree of measurement error using a sample of individuals with felony drug convictions, the ACS, and administrative records on benefit receipt from five states (Arizona, Maryland, Michigan, North Dakota, and Oregon). We use this expanded sample, relative to the subset of our RD analysis sample that is linkable with administrative SNAP records, to improve the geographic representation of this exercise while still capturing the

[17] For completeness, we also show that the number of children, as well as the observable characteristics of romantic partners/co-parents and children are also smooth across the discontinuity in Table 1.

justice-involved specific misreporting rates.[18]

Appendix Table 2 compares different measures of benefit receipt for these households based on ACS responses versus administrative benefit records. Overall, we find a concordance rate of 83.4% between the ACS and administrative records. The vast majority of the off-diagonal records are false negatives (26.9%), compared to false positives (3.8%), consistent with prior research finding underreporting in surveys (Meyer, Mittag, and Goerge 2022). Nonetheless, while the overall concordance rate is relatively high, the measurement error would still attenuate our estimates of the first-stage and subsequently inflate our IV estimates. Throughout the remainder of the text, we adjust our estimates of the first-stage for this measurement error by dividing by the concordance rate.[19]

4 Quantifying the First-Stage Impact of the PRWORA Ban on Benefit Receipt

To quantify the first-stage impact of the PRWORA ban on benefit receipt, we leverage survey responses from the 2005-2019 American Community Survey (ACS) and 1997-2019 Current Population Survey Annual Social and Economic Supplement (CPS).

In Figure 2, we first document the "direct" effects on benefit receipt for justice-involved individuals in Panel A. Over the 1997-2019 follow-up period, justice-involved individuals just to the right of the discontinuity are 13.9 percentage points less likely to receive SNAP benefits on average in a given year, which is a decline of over 30% relative to the mean participation rate among those with drug convictions prior to the cutoff date.[20] Recall that ACS and CPS questions about SNAP receipt are asked at the household-level, and so we should not expect banned individuals to have zero amounts of benefit receipt since they may coreside with eligible individuals receiving benefits. Additional factors that might lead to non-zero take-up among those to the right of the cutoff include: survey responses from periods after the bans have been modified or lifted, measurement error in survey responses, or imperfect enforcement of the ban by case workers.

In Panel B, we find a large and significant decline in whether anyone in the justice-involved individual's household received food stamps. Focusing on individuals matched with families

[18]Specifically, we identify households with a member who has had a felony drug conviction in the years preceding the focal ACS survey response and use this subsample in our analysis.

[19]Appendix B provides additional details and a methodological underpinning for this adjustment procedure. Additionally, we abstract from any additional estimation error in our adjustment process and treat the diagonal concordance as a known scalar, given the precision associated with the estimate (s.e. = 0.002).

[20]Point estimates of the average annual change in SNAP receipt are presented both in the panels of each figure and in Appendix Table 3. Throughout, we report measurement error-adjusted estimates of the discontinuity, but leave the means and RD bins unadjusted.

in the follow-up period (Panel C) yields a similarly large reduction in the contemporaneous probability of receiving SNAP benefits. Given the structure of the program, we would expect a smaller point estimate, if anything, since only the justice-involved individual should be prevented from receiving benefits, rather than the entire household. Instead, we find consistent evidence that our estimates are not just driven by single-individual households, but that households with families are also consistently less likely to receive SNAP benefits as a result of the disqualifying criteria.

In recent years, many states have partially or entirely repealed the PRWORA ban on SNAP and TANF receipt. In Panel D of Figure 2, we test whether these repeals succeeded in eliminating the benefit receipt discontinuity, estimating equation (1) in mutually exclusive subsamples based on whether a ban was in place or not. Perhaps strikingly, we continue to find strong evidence of a discontinuity in the post-repeal subsample, suggesting that simply repealing the ban without additional outreach to the affected population is unlikely to fully eliminate lower take-up of benefits.[21]

One drawback to our cross-sectional RD estimates is that they capture the change in the average annual probability that an individual received any benefits in a given year or not. From an IV perspective, this imposes that SNAP benefits only impact adult and child outcomes through contemporaneous receipt, an assumption that we are uncomfortable making given ample evidence on the long-term effects of safety net assistance in the literature (e.g., Bailey et al. 2024; Hawkins et al. 2023). Alternatively, to reflect the total fraction of the caseload ever affected by the ban, the average annual marginal share must equal the cumulative marginal share. Given the high degree of short duration spells in Panel E, this seems unlikely to be the case. Differently stated, if average benefit take-up only lasts a few years at a time (perhaps in response to economic shocks rather than continual dependence), then marginal compliers from early in our follow-up window are different from marginal compliers late in our follow-up window. Consequently, ignoring these marginal intensive-margin compliers could severely understate the true size of the first-stage impact of the policy, and thereby overstate the magnitude of the local average treatment effect.

With these high churn rates in mind, we develop a strategy to temporally aggregate cross-sectional RD estimates into a quantity that more fully characterizes the proportion of the caseload that was marginal over the follow-up period. Using administrative caseload data from three states, we first compute a series of weights which measure the fraction of the caseload that was marginal in any given year, normalizing the 2005 (and prior years

[21] We view the persistence of the discontinuity, even after the disqualification is repealed as reflecting path dependence in benefit receipt, imperfect information about the restored eligibility, or incorrect behavior by caseworkers.

with more limited data) estimate to have a weight of one.[22] We then estimate year-by-year first-stage impacts of the ban, combining information from nearby years using a triangular kernel.[23] The weighted sum of these estimates is our estimate of the fraction of the caseload whose SNAP benefit receipt was impacted by the PRWORA ban. Formally, we compute the total first-stage response as:

$$\beta^{\text{Total}} = \sum_{y=2005}^{2019} \omega_y \beta^y \tag{2}$$

$$\text{where } \omega_y = \frac{N_y - N_{y-1} \times (1 - \text{Exit rate}_{y-1}) - N_y \times (\text{Re-entry rate}_y)}{N_y}$$

Panel F of Figure 2 depicts both the year-by-year estimates as well as the weights we use to construct this estimate. Together, our estimates suggest a total first-stage response of 32.4 percentage points, an estimate significantly larger than the simple cross-sectional approach, which also has important implications for the magnitude of our IV estimates, as an improperly scaled reduced form estimate would cause us to overstate the ban's impacts on subsequent socioeconomic outcomes.

The temporal pattern of the year-by-year estimates also reveals dynamics over the follow-up period that are masked by the single estimate in Panel A. The ban's impact on SNAP receipt appears largest in the earliest part of the follow-up window before shrinking in magnitude. The shrinking of the discontinuity during the Great Recession years is consistent with two possible mechanisms. The first is generally increased leniency among caseworkers and the safety net system, allowing previously disqualified individuals to take-up benefits. The second, and more likely mechanism is that other individuals in the household, such as romantic partners, are the marginal individuals who are receiving benefits as a result of expanded SNAP generosity. Either of these channels are consistent with a reduced discontinuity, holding fixed the behavior of the control group.

5 The Impact of Safety Net Assistance on Individuals and their Families

In this section, we present our reduced form estimates of criminal history-based safety net bans in the United States. We first examine effects on criminal justice involvement and the formal labor market for the affected justice-involved individuals before examining spillover

[22]We compute these weights using observations in our control group. Computing weights based on statewide data may decrease the variance of the weights but also requires that the churn rates observed in the broader population are representative of the churn rates in our focal sample.

[23]Specifically, we use the full set of survey responses from justice-involved individuals from Panel A of Figure 2.

responses on romantic partners and children in the households. We note that while our first-stage analysis is narrowed to the subsample of individuals who we could match to the ACS or CPS, our reduced form analysis of criminal justice and labor market outcomes have no such restriction, as we leverage the full series of administrative records from CJARS and IRS W-2 tax records to measure outcomes across our entire study population.[24]

5.1 Effects of the PRWORA Ban on Justice-Involved Individuals

Figure 3 presents graphical reduced form evidence of the PRWORA ban on outcomes for justice-involved individuals.[25] In Panel A we examine whether individuals who are prohibited from receiving SNAP and TANF are more likely to engage with the criminal justice system through receiving new criminal charges.[26] We find limited evidence of any increases in criminal justice involvement on this margin. We also do not find strong effects on specific types of re-offending (e.g., charge vs conviction, income- vs non-income generating charge, drug charge) as reported in Table 2.[27]

We next examine changes in formal labor market outcomes in Panels B and C. We find little evidence of any changes in annual employment rates (measured by any positive W-2 earnings in a year) or in median annual earnings (measured by the median of annual earnings from 2005-2019).[28] These results are contrary to a standard theoretical prediction that households increase labor supply following declines in transfer income, but this typical response is likely muted among a population with limited formal labor market opportunities because of a prior felony conviction.[29]

In the face of declining income and benefit receipt, one might expect higher degrees of

[24]Our ACS and labor market outcomes also do not have any geographic restrictions since we observe the full population of survey responses and tax filings. The exception in our analysis is that we are only able to reliably track criminal justice outcomes for individuals in the state in which they were convicted. We view this limitation as mild as it is implicitly present in any study of criminal justice outcomes without population-level coverage.

[25]Point estimates are also reproduced in Table 2.

[26]Across all panels in this figure, we define outcomes using information from the justice-involved individual. Criminal history-based outcomes cover the period after the focal justice-involved event through 2019 or the end of data coverage and W-2 outcomes cover the period 2005-2019. This follow-up period allows us to capture non-contemporaneous lagged effects of the ban on future outcomes, since inaccess to the social safety net may change household outcome trajectories even after the ban is lifted.

[27]In Appendix Figure 2, we additionally explore temporal heterogeneity over the follow-up period using both number of charges and incarceration as outcomes. Consistent with our previous results, we continue to find little evidence of recidivism responses along these margins.

[28]We find a decline in the probability of earning more than $5,000 per year associated with the ban for the justice-involved individuals reported in Table 2, suggesting SNAP benefits may support labor supply in the left tail of the earnings distribution, but this estimate is small in magnitude (3% relative to the mean) and only marginally significant.

[29]Consistent with this finding, Cook and East (2023) also find no evidence that SNAP receipt changes labor supply among the majority of working-age applicants.

stress in the household. Surprisingly, we do not find an impact on our ACS survey measure of cognitive difficulty/stress ("have difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition") among the affected justice-involved individuals who have disqualifying convictions. We hypothesize two potential reasons that could explain this pattern of evidence, explored in Section 5.3: (1) that these measures of adult well-being are not sensitive to drops in consumption implied by the contraction in the household budget constraint, or (2) that adults preserve their own consumption through sacrificing intergenerational investment in their children (i.e., the children are the residual claimants to the household budget). We return to evaluating these hypotheses after presenting the child impacts later in this section.

We also create a summary index of outcomes for justice-involved individuals to assess an overall impact of the ban. The creation of a summary index can both improve power with a range of outcomes as well as limit risks associated with the testing of multiple hypotheses (Viviano, Wüthrich, and Niehaus 2024; Kling, Liebman, and Katz 2007). To calculate a summary index outcome measure, we orient each outcome such that an increase represents an improvement in well-being, standardize each outcome, and then take an unweighted average of the standardized components.[30] Formally, we calculate the index $\tilde{Y}$ as

$$\tilde{Y} = \frac{1}{M}\sum_{m=1}^{M} \frac{y_m - \overline{y_m}}{\sigma_{y_m}}. \tag{3}$$

Given the results discussed so far, we unsurprisingly find no significant impact of the bans on the outcome index. Moreover, the 95 percent confidence interval rules out any impacts to the index greater than -0.023.

5.2 Spillover Effects of the PRWORA Ban on Romantic Partners

We use detailed records on household composition from Finlay, Mueller-Smith, and Street (2023) to identify romantic partners who are observed with the justice-involved individual in our estimation sample during the post-PRWORA follow-up period.

Figure 4 presents graphical reduced form evidence of the PRWORA ban on outcomes for the romantic partners of justice-involved individuals. In Panel A, we examine whether the partners of individuals who are prohibited from receiving SNAP and TANF are more likely to engage with the criminal justice system through receiving new criminal charges. We find

[30] For adults, the index includes the four outcomes in Panels A-D in Figure 3 and the probability of having any income-generating criminal charge. We classify income-generating offenses following the definition from Deshpande and Mueller-Smith (2022). Examples of income-generating offenses include burglary, larceny, forgery/fraud, commercialized vice, and other similar offenses.

suggestive evidence that the bans decrease the probability of criminal justice involvement for romantic partners. This result could reflect a specific deterrence effect among partners for SNAP disqualifying drug crimes or other changes in the social environment for romantic partners. Table 2 suggests that these effects are most prominent for the income-generating crime category, suggesting specific deterrence is not the primary mechanism for this reduction. In contrast, our findings regarding greater family fragility among banned individuals (discussed in the next subsection) suggest peer influence or the lack thereof might contribute to the lower observed crime rates for romantic partners (Billings, Deming, and Ross 2019; Billings and Schnepel 2022).

We do not find evidence of any labor market response among partners of the justice-involved individuals. Panels B and C of Figure 4 report little change in either employment rates or median earnings. We also do not find an impact on our ACS survey measure of cognitive difficulty/stress for our sample of romantic partners of banned individuals. Our summary index further confirms these null findings.

Overall, while there is suggestive evidence that the ban may have decreased crime for a romantic partner, other outcomes and a summary index outcome imply a lack of any large negative or positive spillover impacts on romantic partners.

5.3 Spillover Effects of the PRWORA Ban on Children

The children of our defendant sample reflect perhaps the most innocent group and deserving of government support considered in this analysis. Such minors could have no culpability for the illicit actions of their parents and grow up in an environment with many barriers to their long-term success (Finlay, Mueller-Smith, and Street, 2023). Yet, as observed in Section 4, the PRWORA safety net bans did reduce benefit receipt in this population, and without a corresponding increase in adult formal labor supply, household resources likely contracted barring some unobserved change in informal earnings. This raises the fundamental question of how this unanticipated consequence impacted the children's well-being and development.

A large literature emphasizes importance of the early-life environment (see Heckman 2007 and reviews by Almond and Currie 2011; Currie and Almond 2011; and Almond, Currie, and Duque 2018). Recent work by Bailey et al. (2024) documents long-term benefits for children in households with access to food stamps but finds that early exposure matters—increased resources for mothers during pregnancy and in their children's first five years of life improve human capital, health, and productivity; while household access to food stamps for kids six and older is not linked with improved adult outcomes. Given this evidence, we separately evaluate outcomes for children exposed to food stamp bans starting below and above the age of five.[31]

To evaluate the impacts of exposure to the bans on children, we start with measuring outcomes and household circumstances during childhood. We focus on three measures of well-being in childhood: the number of parents within the household, high school dropout status, and mental/emotional well-being. We then evaluate measures of young adult well-being from administrative datasets for these children as they transition into adulthood (ages 19-22), including earnings, criminal charges and the type of neighborhoods the grown children are living in.[32] Finally, we calculate an analogous summary index of well-being in early adulthood following our approach for adults.[33]

Our main evidence on child impacts are presented in Figure 5 (RD scatterplots and trend lines), Table 3 (reduced form and IV estimates for an extended set of outcomes), and Figure 6 (standardized effect size coefficient plots for all outcomes). Starting with the childhood environment, we find evidence that children of individuals banned from food stamp access spend greater fractions of their youth in single-parent households. This effect is evident for both younger and older children perhaps reflecting latent strain in their parents' relationship caused by the bans or the adults responding to the income and benefit measure incentives of the SNAP program itself since banned individuals' income counts against benefit eligibility. We do observe that banned child exhibit meaningfully higher rates of cognitive difficulty and/or stress, which would be consistent with the former hypothesized mechanism. It is not the case though that children of banned individuals exit the household entirely (e.g., foster care placement following a child welfare intervention) as we find small and insignificant effects on the probability of living with neither parent.

These childhood environment changes appear to be crystallized in ways that likely will affect these kids for the rest rest of their lives. For instance, we find striking evidence that young children in banned households are also less likely to complete high school.[34] Many of these children are too young to follow beyond early adulthood in available data, making our study of the intergenerational labor market impacts partially obscured by ongoing educational attainment, but it does appear that this decline in educational attainment for young children also translates to lower earnings overall, albeit with some degree of impre-

[31]Age five is also when children commonly enter the public school system and may receive access to free and reduced-price lunch. Appendix Figure 3 reports the distribution of child birth cohorts.

[32]For earnings, we focus on median rather than mean earnings in order to minimize the influence of potential censoring from differences in high school enrollment for children from non-banned households.

[33]Specifically, we include the following measures in our summary index for children: high school completion, cognitive difficulty, mean employment, median earnings, any criminal charge, income-generating criminal charge, neighborhood poverty rates and future SNAP receipt as a young adult.

[34]We define high school completion as having obtained a high school degree or more. We consider a GED as not graduating as this is most often obtained among individuals who dropped out of high school.

cision. When we restrict to earnings in high skill industries, we find more precise earnings penalties for children from banned households (Brough, Phillips, and Turner, Forthcoming). In contrast, we find little change in earnings in lower-skilled industries, where the marginal value of a high school diploma is less.[35]

Surprisingly, there is no observed differential effect on the probability of avoiding contact with the criminal justice system. This is true overall and for income-generating criminal charges as well. Justice-involvement though has declined significantly among recent birth cohorts (Shen et al. 2020; Neil and Sampson 2021), which may explain why we observe weaker effects here relative to prior research.

We do, however, find evidence that young children from banned households live in higher poverty and lower employment neighborhoods as they establish independent households as young adults, a location pattern that further compounds rates of disadvantage among this subpopulation (Chetty, Hendren, and Katz 2016).[36] Ironically, children from banned households have higher rates of SNAP receipt themselves as young adults, a pattern inconsistent with some evidence of intergenerational welfare dependence (Dahl, Kostøl, and Mostad, 2014), although there are substantial contextual differences between prior work and our setting. Instead, our results suggest that depriving children of SNAP in economically vulnerable households increases their use of the same program in adulthood.

Turning to the composite index for children's outcomes, we find that younger children from banned households exhibit significantly lower composite index scores while the bans appear less consequential for older children, a pattern that is consistent with Bailey et al. (2024)'s findings. Taken together, these negative impacts to children are substantial and build on a long literature documenting the connection between household resources, the social safety net, and child development (e.g., Currie and Cole 2016; Hoynes, Schanzenbach, and Almond 2016; Bailey et al. 2024; East 2020; Aizer, Hoynes, and Lleras-Muney 2022).

Potential Mechanisms: Two potential mechanisms could help explain the discordance between effects across adults and children. First, children could be residual claimants to household resources such that a decline in support from SNAP or TANF could have a negative impact on resources devoted towards children without altering adult consumption. Stated

[35]We define lower-skilled industries as industries with at least a 15% share of workers with less than a high school diploma using the 2010-2019 ACS.

[36]In Appendix Table 4, we explore the temporal pattern of neighborhood residency and job quality to answer whether better neighborhoods lead to better jobs or vice versa. We find no evidence that children in banned households are differentially less likely to first move to a low-poverty neighborhood and then obtain a job in a higher-skill industry, but in fact find the opposite pattern. This temporal pattern is inconsistent with a Moving to Opportunity type of mechanism where high quality neighborhoods permit young adults to obtain higher-paying jobs (Chetty, Hendren, and Katz 2016), but instead consistent with the idea that better jobs lead individuals to move out of high poverty neighborhoods.

more simply, parents may "rob" their children of potential investment in order to smooth their own consumption. Since the capacity for adults to maintain their own consumption levels at the expense of other family members is only available to individuals with children, we should expect to see heterogeneous treatment effects for adults with and without children. When we test this hypothesis (see Section 5.4) however, we find that adult responses are similar for individuals regardless of the presence of children, suggesting that this theoretical channel is not consequential in practice.

On the other hand, it could also be the case that the outcomes of children are more sensitive to variation in household resources than adult outcomes. In this scenario, each household member suffers a similar shock to consumption, but this drop is more consequential for the outcomes of children in particular. This second hypothesis is supported by a large literature suggesting that there are sensitive and critical periods in childhood development (e.g., Heckman 2007). In particular, resource variation during the early life environment has been shown to have long-term outcomes for affected children.

Our results appear to corroborate this second mechanism. While both younger and older children experience reductions in household resources and increased household instability, the lasting deleterious effects of these disruptions are more concentrated among younger children. This pattern is consistent with models of child development emphasizing critical periods where access to household resources is a key determinant of long-run outcomes and is not consistent with children being residual claimants to household resources unless it is also the case that parents cut spending more for younger children compared with older children.

Program Participation Spillovers: For completeness, we also examine spillovers onto participation in other programs for children in Appendix Table 5. We find little changes in yearly Medicaid or HUD participation rates for younger (<5 in 1996) or older (5+) kids, (Columns 1-4), although older children in banned households have marginally higher HUD participation rates. We find some evidence that banned households who are ever observed with young children are more likely to participate in other state and local cash assistance programs, as measured by the ACS (Column 5). However, due to data limitations in the way the survey question was framed, we are unable to directly attribute which specific program may be driving these results. Instead, we interpret these results as suggestive evidence that banned households seek out other programs in the social safety net to compensate for lost SNAP benefits, but these resources are insufficient to mitigate the deleterious effects to child development.

5.4 Heterogeneity in Effects Across Observable Characteristics

Demographic Characteristics: We explore heterogeneous impacts across a variety of subgroups defined by race/ethnicity (White, Black, and Hispanic), gender and the presence of kids.[37] Appendix Table 6 reports the impacts on receipt, reduced form and IV estimates for each summary index outcome and subgroup. Appendix Figures 4 and 5 plot corresponding estimates in graphical form.

Across racial and ethnic subgroups, we find that the drug felony SNAP/TANF restriction has larger impacts on receipt among Black individuals, as well as those with kids, though this estimate is not statistically significant for Black individuals. For these groups, participation declines by around 30 percentage points. Results by subgroup for our summary indices consistently point to negative impacts for children, though some of the estimates are less precise due to smaller samples. We find suggestive evidence that both young boys and girls in households affected by the ban are negatively impacted.

Type of Disqualifying Offense: We also explore whether individuals convicted of drug use or possession felonies experience different changes in outcomes compared to individuals convicted of drug distribution or trafficking offenses. Appendix Figure 6 reports first-stage and reduced form results for our outcome summary indices among these two subsamples. Individuals on use/possession offenses experience a slightly greater reduction in SNAP receipt, perhaps because control individuals are more closely attached to the social safety net in this subsample, although the difference in first-stage SNAP receipt across offense types is not statistically different. For all other adult and child outcomes we find similar results across both offense groups.[38]

State-Specific Results: Our primary specification pools information across eight different states, each with their own unique institutional details which may also influence the socioeconomic consequences of removal from the social safety net. Our goal in this approach is to maximize sample size given the constraints imposed by sampled survey data.[39] In Appendix Figure 7 we reproduce our main reduced form estimates at the state-specific level after first verifying the experimental validity remains intact in each subsample. We focus on the reduced form as applying our first-stage aggregation strategy would require estimating state-by-year discontinuities, which may lead to unstable estimates given sparsity in the underlying survey data.

[37] These characteristics are based on the justice-involved individual.

[38] We interpret these results with some care given that our method of classifying offenses is based on a probabilistic algorithm (Choi et al. 2023) and jurisdictions have varying degrees of data quality with respect to offense reporting.

[39] The ACS targets sampling 1% of the U.S. population each year it is conducted. The CPS collects information from around 75,000 households each wave.

Across all states, we find similarly-sized responses with respect to the first-stage - there is a consistent decline in contemporaneous SNAP receipt in all of our states, although smaller samples mean some of the estimates are not statistically significant at conventional levels. We also estimate similarly consistent results across summary index outcomes for justice-involved individuals and romantic partners in Panel D.[40]

Finally, we estimate impacts for our summary index measure of outcomes for children at the state-level. We typically continue to find a negative impact for young kids across most states, although some of the estimates are imprecise due to the smaller samples. These patterns suggest that our full sample estimates are not driven by a single state where young children are uniquely adversely affected by the ban and instead are indicative of the broader socioeconomic consequences of removing vulnerable children from the social safety net as a result of their parent's criminal justice involvement.

ABAWD Waivers: By federal law, able-bodied adults without dependent children (ABAWD) are ineligible to receive SNAP benefits for more than three months in any three-year time span unless they additionally meet certain work requirements. State agencies are able to petition the USDA to grant waivers which relax these conditions for areas with poor labor markets. Intuitively, waiving the ABAWD restrictions should allow the control group of individuals easier access to SNAP benefits. Correspondingly, we would anticipate a larger first-stage discontinuity and greater reduction in labor supply in areas with ABAWD waivers.

Using historical petitions from state agencies to the USDA, we classify states into high and low ABAWD waiver groups based on the mean prevalence of waivers over the period 1998-2008.[41] Specifically, we compute the average waiver prevalence rate across years at the county level and then take the statewide mean of these prevalence rates to calculate the average waiver rate across counties in a given state. We define states as high waiver prevalence if this rate is at least 60 percent.[42] Appendix Figure 8 reports the results of this exercise. Consistent with ABAWD waivers increasing ease of access to SNAP benefits for the non-banned individuals, we find a greater reduction in contemporaneous SNAP receipt and labor supply in states with high ABAWD waiver prevalence.

[40] In additional exercises, we also test whether there is an increase in the three-year felony conviction rate, an outcome which more closely approximates return to prison used in other settings. While we generally find similarly small and null effects using this measure of justice system re-contact, we do find a positive, albeit imprecise increase in Florida, which qualitatively aligns with the results in Tuttle (2019).

[41] We exclude 2001-2003 due to national waivers that were in place due to the recession.

[42] High prevalence states in our sample are Arizona, New Jersey, and Oregon.

5.5 Robustness of Results to Alternative Specifications

Finally, we conduct a series of robustness checks to assess whether our results are sensitive to our specification choices. We estimate a number of alternative models and present results in Appendix Table 7 and Appendix Figure 9. The alternative specifications include models which: do not include our vector of baseline controls (Column 2); modify the baseline 330 day bandwidth used for the focal drug conviction to 270 days (Column 3) and 450 days (Column 4); restrict slopes on either side of the discontinuity to be uniform across states (Column 5); use triangular weights instead of the baseline uniform weights (Column 6); and include a local quadratic rather than the baseline local linear approach (Column 7). We present both reduced form and IV estimates for the summary index outcomes for JII individuals, their romantic partners and children.

Overall, the magnitude and precision of our estimates are similar across each specification. We consistently find differences in contemporaneous SNAP receipt as a result of the ban; limited evidence of changes in outcomes for the affected justice-involved individuals; modest, but imprecise, improvements for romantic partners of banned individuals; and consistently negative impacts for children under five in affected households.

We also conduct a series of placebo tests to rule out that our estimates are simply driven by seasonal factors or other contemporaneous shocks to the caseload. In Appendix Table 8 we reproduce our main first-stage and reduced form estimates in Column 1 before estimating the same model on a set of individuals with non-drug felony convictions around the August 23, 1996 cutoff date in Column 2. In sharp contrast to the null response of the placebo sample of non-felony drug convictions, our focal sample of individuals convicted of disqualifying drug offenses shows a sizeable and precise reduction in SNAP benefit receipt and no changes in outcomes for younger children. This result is indicative of an effect not simply driven by seasonal factors around the cutoff date. In Columns 3-8 we also generate placebo cutoffs using our focal sample and re-estimate the first-stage or reduced form model for the same set of outcomes. In general, we continue to find small and imprecise changes in SNAP receipt and with our summary index outcome. While we do see some idiosyncratic changes in outcomes, as would be the case by chance, the estimated effects often have different signs as our main results. Together, we interpret this set of estimates as providing additional validity that our main specification is capturing real changes in outcomes as a result of the PRWORA ban that are not simply a result of seasonal factors or idiosyncratic shocks to the caseload.

6 Conclusion

Disqualifications based on criminal histories affect many aspects of society, including participation in the social safety net, occupational licensing, and public housing. Given that the burden of interactions with the criminal justice system predominantly falls on disadvantaged communities, criminal history-based disqualifications effectively remove social support for the populations most in need of its assistance. In this paper, we examine the effect of criminal history-based bans from the Supplemental Nutrition Assistance Program on a host of outcomes including benefit receipt, future criminal justice involvement, labor market participation, and survey-based measures of well-being.

We find a strong first stage relationship of the criminal history-based ban on future SNAP receipt that is remarkably persistent over time and across household structure. While there is limited evidence of increases in long-run participation in the criminal justice system to accompany this first-stage response, we find suggestive evidence that economic circumstances worsen for those in the bottom part of the earnings distribution. This pattern suggests that individuals most in need of public assistance are also those most affected by the inability to receive it.

We find negative impacts for young children linked to affected households. While both younger and older children experience increases in household instability, younger children experience sharply worse outcomes over their early life cycle. Children who are under five when a justice-involved parent is banned from SNAP and TANF support are less likely to complete high school, earn less, live in lower quality neighborhoods, and more likely to take up SNAP themselves in early adulthood. These large impacts for younger children are consistent with a large body of evidence suggesting that access to household resources during critical periods of development is a key determinant long-run outcomes (e.g., Heckman 2007), including recent work evaluating the long-term impacts of access to food stamps in the United States (Bailey et al. 2024). These results suggest that criminal history-based bans can negatively affect the short and longer term well-being of the most vulnerable members of a household, even if there are limited detectable negative impacts on parents.

While most states have modified or repealed the restrictions based on drug felony convictions, we find that take-up of assistance among those whose eligibility is restored by the modifications remains low. This suggests that other efforts may be necessary to get SNAP assistance to households with formerly disqualified members. Such efforts may also be necessary to ensure expansions to SNAP eligibility requirements or to those for other social safety net programs affect take-up for those targeted by the expansions.

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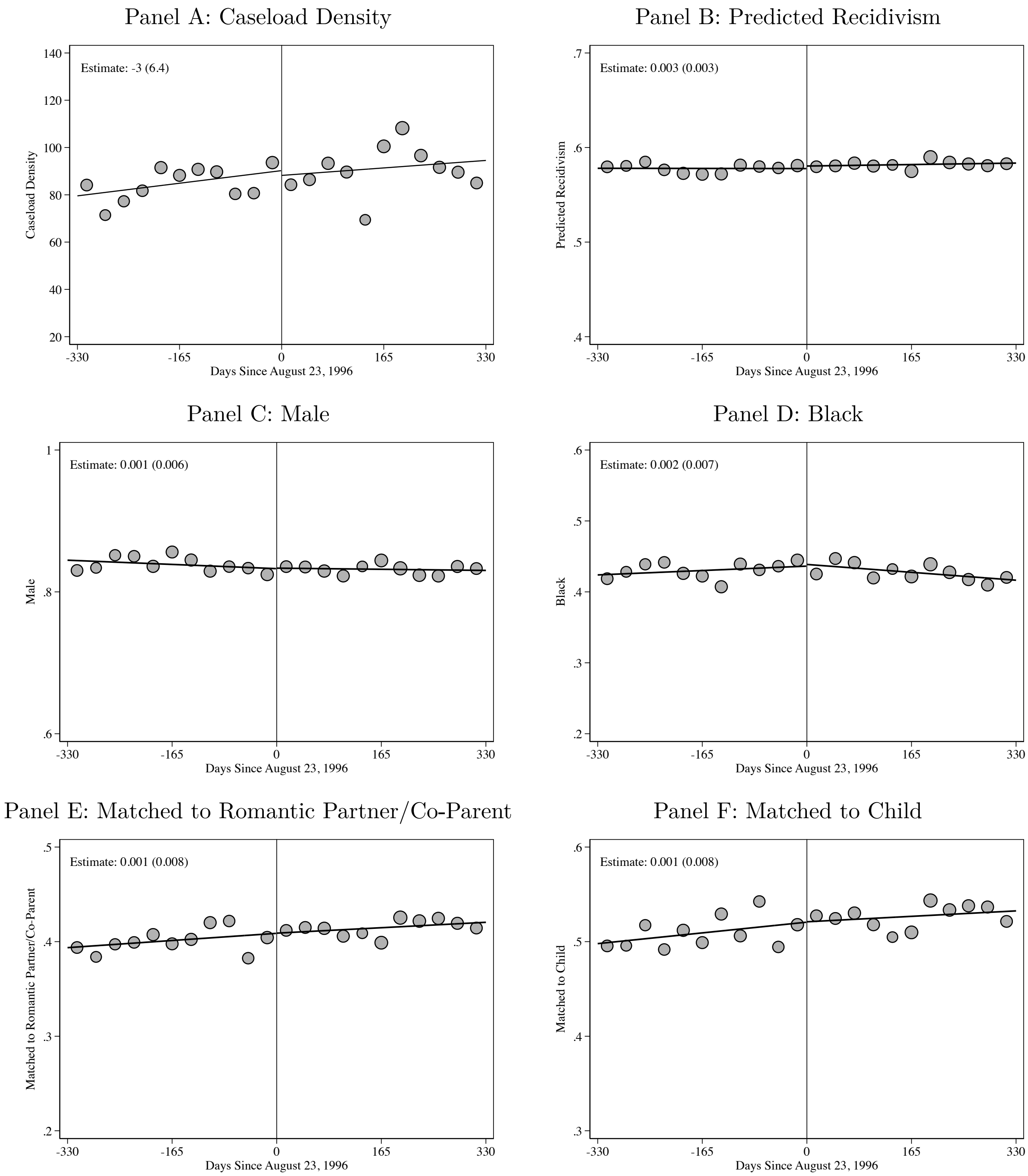
Figure 1: Evaluating the Validity of the Natural Experiment



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots graphical reduced form evidence of the validity of the natural experiment. The outcome is listed in each panel title and the corresponding point estimate at the discontinuity is displayed in each figure, with robust standard errors reported in parentheses. Each point represents the midpoint of a 30-day bin of the running variable and plots the mean of the outcome within that bin, residualized on Commuting Zone fixed effects. Each line represents a linear regression estimated separately on either side of the discontinuity. Both points and lines of best fit are weighted using caseload size. Robust standard errors in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
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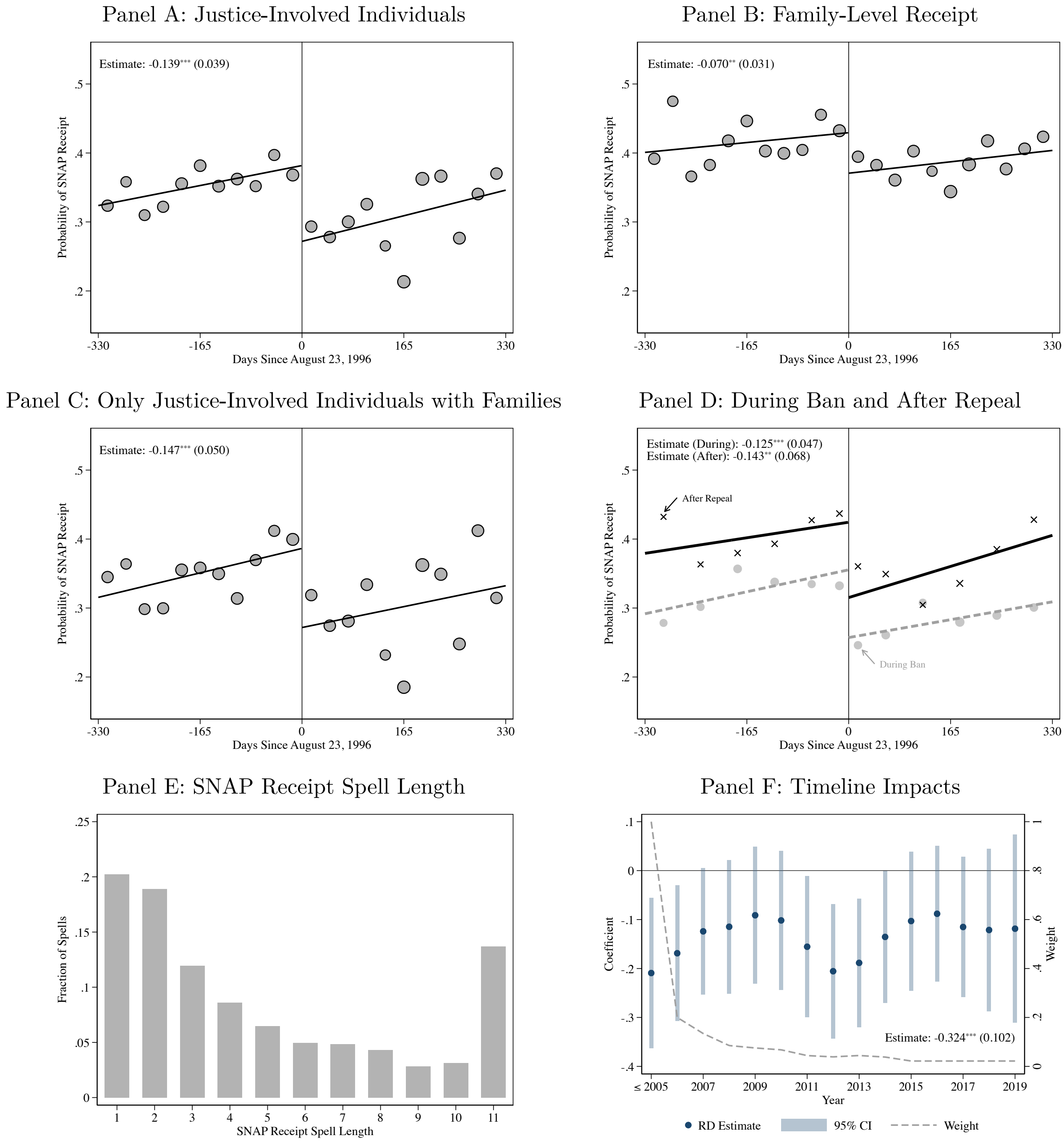
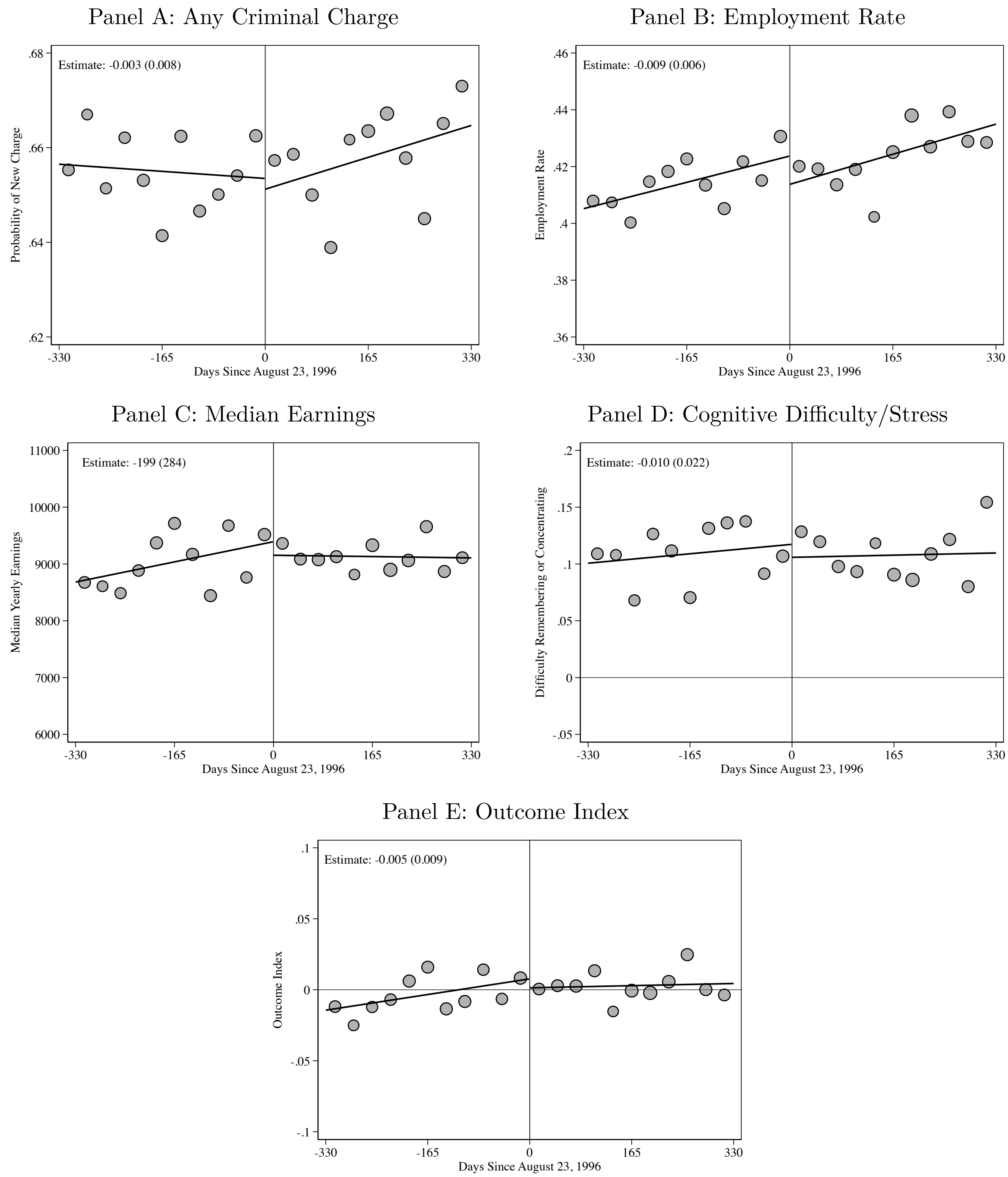
Figure 2: First Stage Estimates of PRWORA Ban on SNAP Receipt



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of the subset which are matched to the 2005-2019 American Community Survey and 1997-2019 Current Population Survey Annual Social and Economic Supplement. This figure plots graphical evidence of the first-stage relationship between SNAP receipt and the PRWORA ban. Panel A includes the subsample of justice-involved individuals matched to the ACS, Panel B combines justice-involved individuals with survey responses from romantic partners/co-parents and children, identified using crosswalks from Finlay, Mueller-Smith, and Street (2023), Panel C restricts the sample in Panel A to only be justice-involved individuals who we observe with families, Panel D splits the sample in Panel A into mutually exclusive and exhaustive subsamples based on whether the PRWORA ban was repealed or not, Panel E shows the SNAP spell length for control observations in Arizona, North Dakota, or Oregon with any SNAP participation, and Panel F splits the sample in Panel A into follow-up year bins, combining information from nearby years using a triangular kernel. Each point in Panels A-D represents the midpoint of a 30-day bin and the within-bin mean, residualized on Commuting Zone and year fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls, weighted using ACS and CPS sampling weights. Panel D groups observations into 60 day bins and points nearest the discontinuity are 30-day bins. Points and lines of best fit are weighted using caseload density. Listed point estimates and points are estimated using equation (1) in Panels A-D and equation (2) in Panel F, weighted using ACS and CPS sampling weights. Point estimates are measurement-error adjusted. Shaded bars represent 95 percent confidence intervals, with standard errors clustered at the household level in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-020, #CBDRB-FY23-CES014-051, #CBDRB-FY24-CES014-004, #CBDRB-FY24-CES014-011, #CBDRB-FY24-0344, & #CBDRB-FY24-0450.

Figure 3: Reduced Form Estimates of PRWORA Ban on Justice-Involved Individuals



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2008-2019 American Community Survey, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots graphical evidence of the reduced form relationship between being banned from SNAP as a result of PRWORA and the outcome listed in the panel title. Outcomes are measured for justice-involved individuals. Each point represents the midpoint of a 30-day bin and the within-bin mean, residualized on Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Points and lines of best fit are weighted using caseload density. Cognitive Difficulty/Stress measured using the ACS question about individuals having "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition." Listed reduced form point estimates are estimated using equation (1), with robust standard errors reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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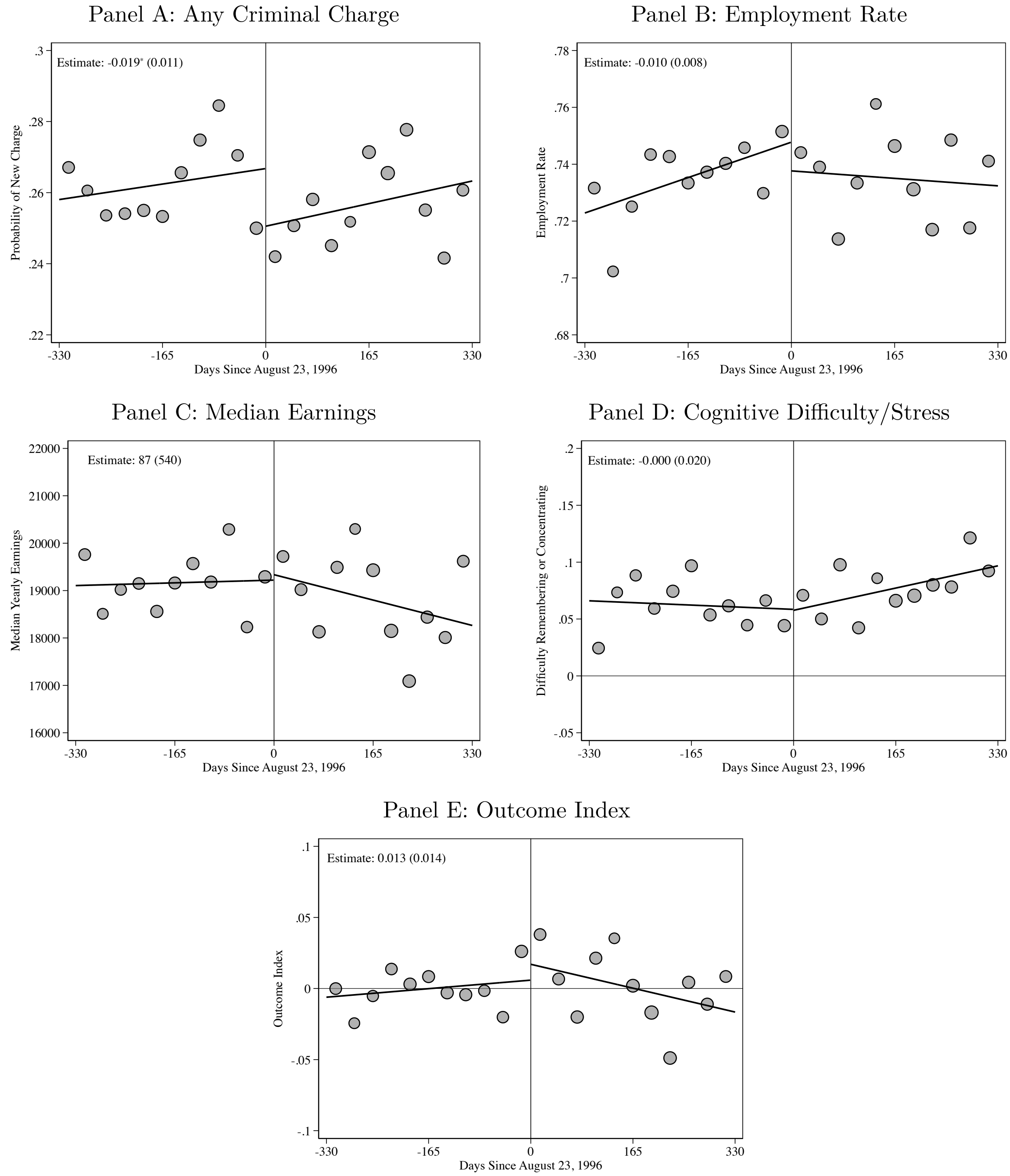
Figure 4: Reduced Form Estimates of PRWORA Ban on Romantic Partners



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, crosswalks from Finlay, Mueller-Smith, and Street (2023), 2008-2019 American Community Survey, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots graphical evidence of the reduced form relationship between being banned from SNAP as a result of PRWORA and the outcome listed in the panel title. Outcomes are measured for romantic partners/co-parents of justice-involved individuals. Each point represents the midpoint of a 30-day bin and the within-bin mean, residualized on Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Points and lines of best fit are weighted using caseload density. Cognitive Difficulty/Stress measured using the ACS question about individuals having "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition." Listed reduced form point estimates are estimated using equation (1), with robust standard errors reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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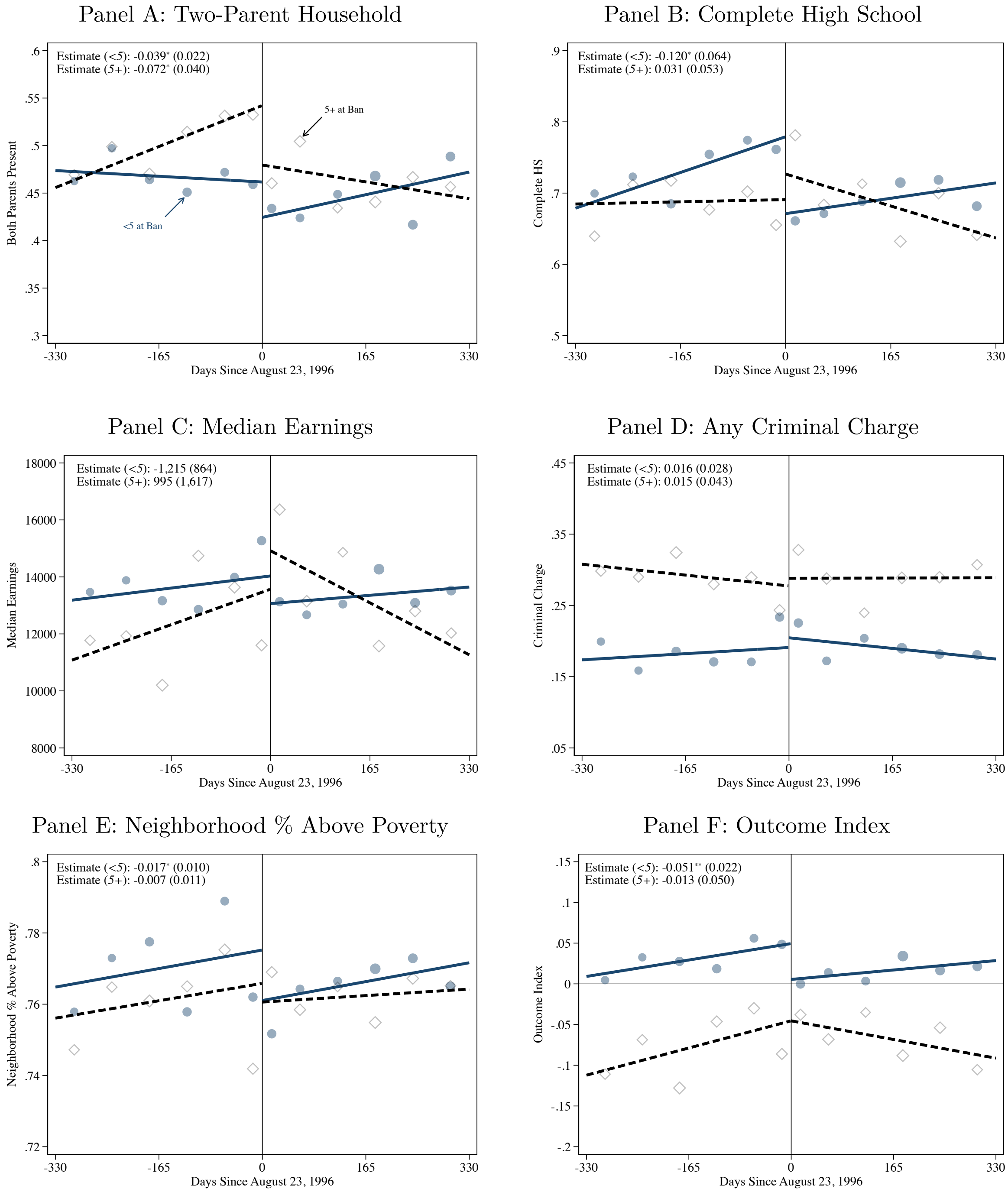
Figure 5: Reduced Form Estimates of PRWORA Ban on Child Outcomes



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2008-2019 American Community Surveys, crosswalks from Finlay, Mueller-Smith, and Street (2023), IRS W-2s, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of children matched to justice-involved individuals and were matched to the 2008-2019 American Community Surveys. This figure plots graphical evidence of the reduced form relationship between being banned from SNAP as a result of PRWORA and the outcome listed in the panel title for children who are observed with the justice-involved individual. Blue dots and lines correspond to estimates for children under 5 in 1996 and gray diamonds and black dashed lines correspond to estimates for children above 5 in 1996. Each point represents the midpoint of a 60-day bin (30-day bin for points nearest discontinuity) and the within-bin mean, residualized on Commuting Zone and year fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, indicators for any missing controls, and sex-by-age fixed effects of the children. ACS outcomes weighted using sampling weights and estimated at the survey-response level. Points and lines of best fit are weighted using the number of kids in each bin. Standard errors clustered at the household level in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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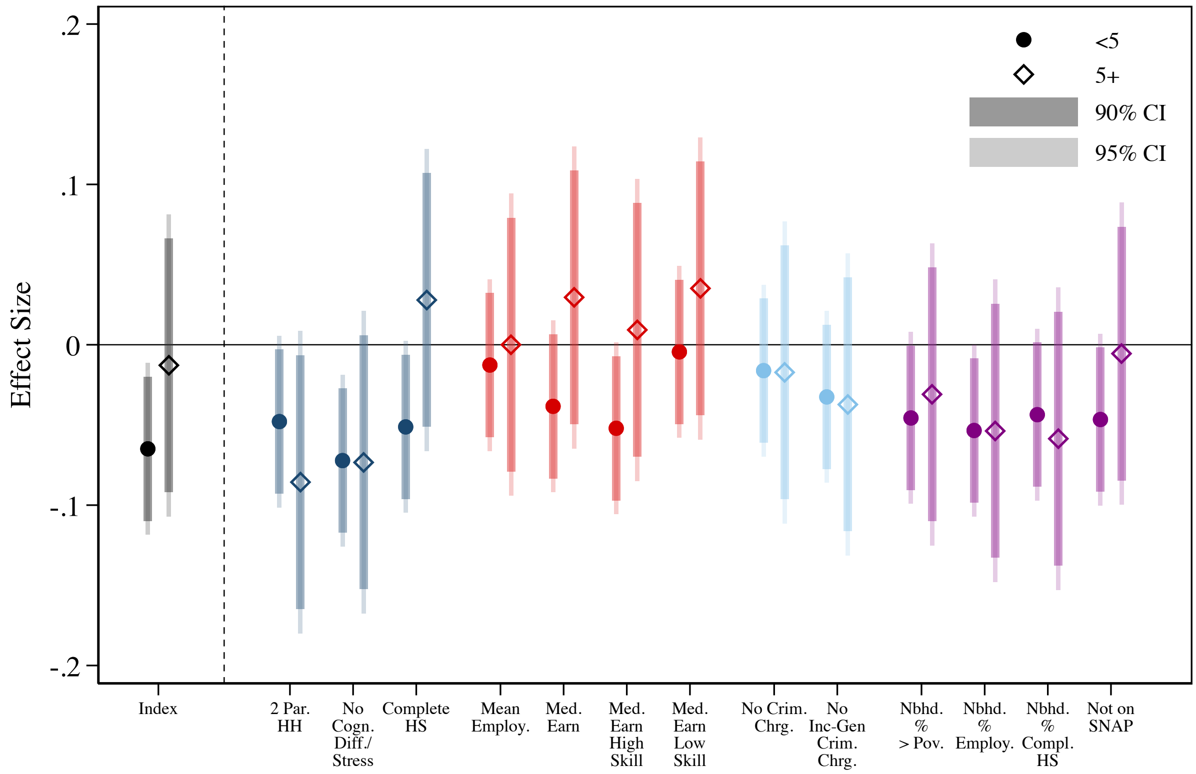
Figure 6: Reduced Form Effect Sizes of PRWORA Ban on Child Outcomes



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2008-2019 American Community Surveys, crosswalks from Finlay, Mueller-Smith, and Street (2023), IRS W-2s, and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The sample consists of children matched to justice-involved individuals and were matched to the 2008-2019 American Community Surveys. Reduced form point estimates have been converted to effect sizes. Solid dots correspond to effect sizes for children under 5 in 1996 and hollow diamonds correspond to estimates for children above 5 in 1996. Black estimates represent the index, navy estimates represent childhood outcomes, red estimates represent labor market outcomes, light blue estimates represent criminal justice outcomes, and purple estimates represent early adulthood household circumstance outcomes. The outcome is listed on the x-axis. ACS outcomes weighted using sampling weights and estimated at the survey-response level. 95 and 90 percent confidence intervals are indicated by vertical shaded regions.
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Table 1: Descriptive Statistics and Experimental Validity

	Sample Mean (1)	RD Estimate (2)		Sample Mean (3)	RD Estimate (4)
Panel A: Caseload Statistics					
Caseload Density	87.460	−3.209	Number of Children	1.518	−0.012
		(6.435)			(0.033)
Predicted Recidivism	0.580	0.003	Children <5 in 1996	1.191	−0.018
		(0.003)			(0.028)
			Children 5+ in 1996	0.327	0.006
Panel B: JII Characteristics					(0.014)
Male	0.835	0.001			
		(0.006)	*Panel C: Romantic Partner Characteristics*		
Age	29.650	−0.244	Female	0.834	0.003
		(0.155)			(0.011)
White	0.413	−0.006	Age	32.010	0.091
		(0.008)			(0.236)
Black	0.429	0.002	White	0.492	−0.001
		(0.007)			(0.012)
Hispanic	0.122	0.002	Black	0.349	−0.000
		(0.005)			(0.012)
Urban County of Conviction	0.845	−0.001	Hispanic	0.106	0.004
		(0.005)			(0.008)
Prior Misdemeanor Convictions	0.447	0.012	Any Criminal Charge Before Relationship	0.235	−0.008
		(0.018)			(0.010)
Sentenced Incarceration Length (Months)	37.130	0.622			
		(1.317)	*Panel D: Child Characteristics*		
Use/Possession Offense	0.418	−0.000	Male (<5 in 1996)	0.512	0.009
		(0.006)			(0.008)
Match to Partner/Co-Parent	0.408	0.001	Male (5+ in 1996)	0.515	−0.003
		(0.008)			(0.015)
Match to Child	0.518	0.001	Child (<5 in 1996) Age in 2019	17.780	−0.043
		(0.008)			(0.115)
Child <5 in 1996	0.462	−0.005	Child (5+ in 1996) Age in 2019	31.660	0.003
		(0.008)			(0.115)
Child 5+ in 1996	0.192	0.003			
		(0.007)			
Number of Felony Drug Convicts			58,000		

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. Columns 1 and 3 report sample means for the listed covariate in each row. Columns 2 and 4 reports the point estimate from a simple regression discontinuity design, testing whether the listed covariate changes discontinuously at the threshold. The regression for caseload density is estimated at the day level. Predicted recidivism is generated using all two-way interactions of race, sex, age, number of prior misdemeanor convictions, Commuting Zone fixed effects, urban convicting county, and indicators for any missing controls. Sentence length includes only observations for which we have non-missing sentencing information. Panels C and D report corresponding means and point estimates for the sample of romantic partners/co-parents and children who are observed with the focal justice-involved individual. Robust standard errors or standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
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Table 2: Reduced Form and IV Estimates of PRWORA Ban on JII and Partner Outcomes

	JII			Romantic Partner		
	Control Mean	RF Estimate	IV Estimate	Control Mean	RF Estimate	IV Estimate
Panel A: Outcome Index	(1)	(2)	(3)	(4)	(5)	(6)
Outcome Index	0.002	-0.005	0.015	0.010	0.013	-0.040
		(0.009)	(0.029)		(0.014)	(0.046)
Panel B: Crime Outcomes						
Any Criminal Charge	0.656	-0.003	0.010	0.261	-0.019*	0.058
		(0.008)	(0.024)		(0.011)	(0.039)
Income-Generating Charge	0.436	-0.004	0.013	0.142	-0.015	0.046
(e.g., Larceny, Forgery/Fraud, Drug Dist., Comm. Vice)		(0.008)	(0.025)		(0.009)	(0.031)
Forgery/Fraud Charge	0.102	0.005	-0.017	0.047	-0.004	0.011
		(0.005)	(0.017)		(0.006)	(0.018)
Non-Income-Generating Charge	0.591	-0.004	0.013	0.220	-0.010	0.029
		(0.008)	(0.024)		(0.011)	(0.034)
Drug Charge	0.428	0.006	-0.019	0.104	-0.006	0.020
		(0.008)	(0.025)		(0.008)	(0.025)
Conviction	0.601	-0.008	0.025	0.203	-0.021**	0.065*
		(0.008)	(0.025)		(0.010)	(0.037)
Felony Conviction	0.497	-0.002	0.006	0.109	-0.007	0.022
		(0.008)	(0.025)		(0.008)	(0.026)
Income-Generating Conviction	0.376	-0.008	0.024	0.103	-0.014*	0.045
		(0.008)	(0.025)		(0.008)	(0.028)
Non-Income-Generating Conviction	0.523	-0.012	0.036	0.164	-0.014	0.044
		(0.008)	(0.027)		(0.009)	(0.032)
Felony Conviction through Year 3	0.222	0.002	-0.005	–	–	–
		(0.007)	(0.021)		(–)	(–)
Panel C: Labor Market Outcomes						
Mean Employment Rate	0.422	-0.009	0.029	0.745	-0.010	0.030
		(0.006)	(0.020)		(0.008)	(0.028)
Above $5k in Earnings	0.330	-0.010*	0.031	0.642	-0.005	0.015
		(0.006)	(0.020)		(0.010)	(0.030)
Median Annual Earnings	9,171	-199	616	19,190	87	-270
		(284)	(910)		(540)	(1,693)
Mean Annual Earnings	10,570	-150	462	19,780	253	-781
		(267)	(849)		(507)	(1,600)
Panel D: Cognitive Difficulty/Stress						
Cognitive Difficulty/Stress	0.095	-0.010	0.031	0.045	-0.000	0.000
		(0.022)	(0.068)		(0.020)	(0.063)

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. The JII sample contains 58,000 justice-involved individuals and the romantic partner sample contains 23,500 romantic partners and co-parents. This table reports reduced form (Columns 2, 5) and instrumental variables (Columns 3, 6) estimates of being banned from or receiving SNAP on various outcomes for justice-involved individuals (Columns 1-3) and romantic partners (Columns 4-6). The first-stage is measurement error-adjusted and estimated using the weighted sum described in Section 4. Regressions control for Commuting Zone fixed effects, year fixed effects in IV specifications, and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. The outcome index is constructed following the method described in the main text. Cognitive difficulty estimates weighted using ACS sampling weights. Cognitive Difficulty/Stress measured using the ACS question about individuals having "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition." Control means in Columns 1 and 3 include observations within 75 days to the left of the discontinuity. Standard errors clustered at the household level or robust standard errors are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Table 3: Reduced Form and IV Estimates of PRWORA Ban on Child Outcomes

	<5 at Time of Ban			5+ at Time of Ban		
	Control Mean	RF Estimate	IV Estimate	Control Mean	RF Estimate	IV Estimate
Panel A: Outcome Index	(1)	(2)	(3)	(4)	(5)	(6)
Outcome Index	0.047	-0.051**	0.131*	-0.070	-0.013	0.034
		(0.022)	(0.073)		(0.050)	(0.131)
Panel B: Childhood Outcomes						
Two-Parent Household	0.468	-0.039*	0.100	0.555	-0.072*	0.183
		(0.022)	(0.069)		(0.040)	(0.123)
Not Living with Either Parent	0.081	0.006	-0.016	0.044	0.016	-0.040
		(0.010)	(0.028)		(0.016)	(0.043)
Complete High School	0.750	-0.120*	0.307	0.734	0.031	-0.078
		(0.064)	(0.197)		(0.053)	(0.138)
Cognitive Difficulty/Stress	0.045	0.046***	-0.118*	0.021	0.050	-0.128
		(0.017)	(0.062)		(0.033)	(0.094)
Panel C: Labor Market Outcomes (Ages 19-22)						
Mean Employment Rate	0.786	-0.010	0.026	0.703	0.000	-0.000
		(0.022)	(0.057)		(0.038)	(0.099)
Median Earnings	14,820	-1,215	3,103	12,860	995	-2,541
		(864)	(2,530)		(1,617)	(4,286)
Median Earnings in Higher Skill Industries	5,293	-1,219*	3,112	4,569	222	-567
		(639)	(2,005)		(1,151)	(2,991)
Median Earnings in Lower Skill Industries	8,507	-126	322	7,380	1,029	-2,629
		(776)	(2,043)		(1,407)	(3,768)
Panel D: Crime Outcomes (through Age 22)						
Criminal Charge	0.209	0.016	-0.042	0.287	0.015	-0.040
		(0.028)	(0.074)		(0.043)	(0.113)
Income-Generating Criminal Charge	0.106	0.027	-0.068	0.179	0.029	-0.073
		(0.022)	(0.063)		(0.037)	(0.100)
Panel E: Household Circumstances (Ages 19-22)						
Neighborhood Share Above Poverty	0.780	-0.017*	0.043	0.758	-0.007	0.019
		(0.010)	(0.031)		(0.011)	(0.030)
Neighborhood Employment Rate	0.886	-0.009*	0.022	0.879	-0.006	0.015
		(0.004)	(0.014)		(0.005)	(0.014)
Neighborhood Share Complete HS	0.800	-0.016	0.042	0.787	-0.014	0.035
		(0.010)	(0.031)		(0.011)	(0.031)
Own SNAP Receipt	0.292	0.162*	-0.415	0.383	0.017	-0.044
		(0.095)	(0.287)		(0.152)	(0.367)

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), Business Register, administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports reduced form (Columns 2, 5) and instrumental variables (Columns 3, 6) estimates of being banned from or receiving SNAP on various outcomes for children under 5 in 1996 (Columns 1-3) or over 5 in 1996 (Columns 4-6). The sample contains children of justice-involved individuals linked to the 2008-2019 ACS. The first-stage is measurement error-adjusted and estimated using the weighted sum described in Section 4. Regressions control for Commuting Zone fixed effects, year fixed effects in IV specifications, and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Survey outcomes weighted using ACS sampling weights. Cognitive Difficulty/Stress measured using the ACS question about individuals having "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition." Control means in Columns 1 and 3 include observations within 75 days to the left of the discontinuity. Standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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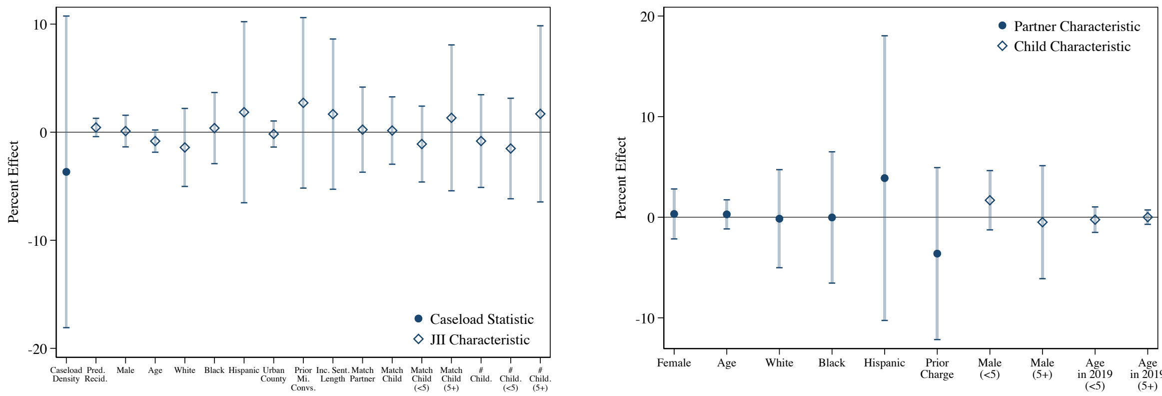
Appendix A: Supplementary Results

Appendix Figure 1: Summarizing Experimental Validity

Panel A: Caseload and JII Characteristics

Panel B: Partner/Kid Characteristics



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots the point estimate from a regression discontinuity design, testing whether the listed covariate changes discontinuously at the threshold. Coefficients are converted to percent effects to standardize magnitudes. Panel A reports caseload and justice-involved individual estimates and Panel B reports estimates for romantic partners and children. The regression for caseload density is estimated at the day level. Predicted recidivism is generated using all two-way interactions of race, sex, age, number of prior misdemeanor convictions, Commuting Zone fixed effects, urban convicting county, and indicators for any missing controls. Sentence length includes only observations for which we have non-missing sentencing information. 95 percent confidence intervals are based on robust standard errors. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region. See notes to Table 1 for additional details.

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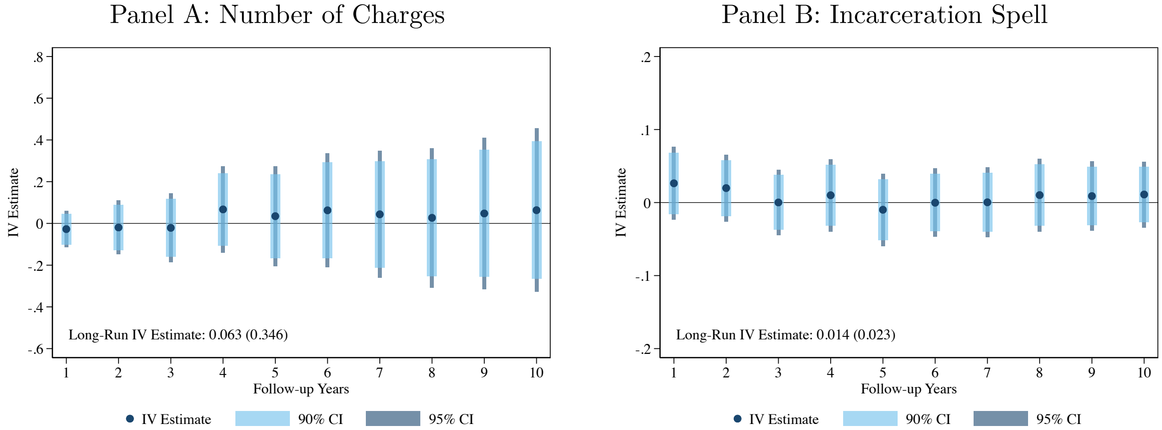
Appendix Figure 2: IV Estimates of Justice-Involved Individual Recidivism After Focal Event



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure reports instrumental variables estimates of receiving SNAP benefits on number of criminal charges (Panel A) and the probability of an incarceration spell (Panel B) for justice-involved individuals over varying time horizons. The first-stage is estimated using the weighted sum described in Section 4 and adjusted for measurement-error. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Confidence intervals based on standard errors clustered at the household level are reported in parentheses.

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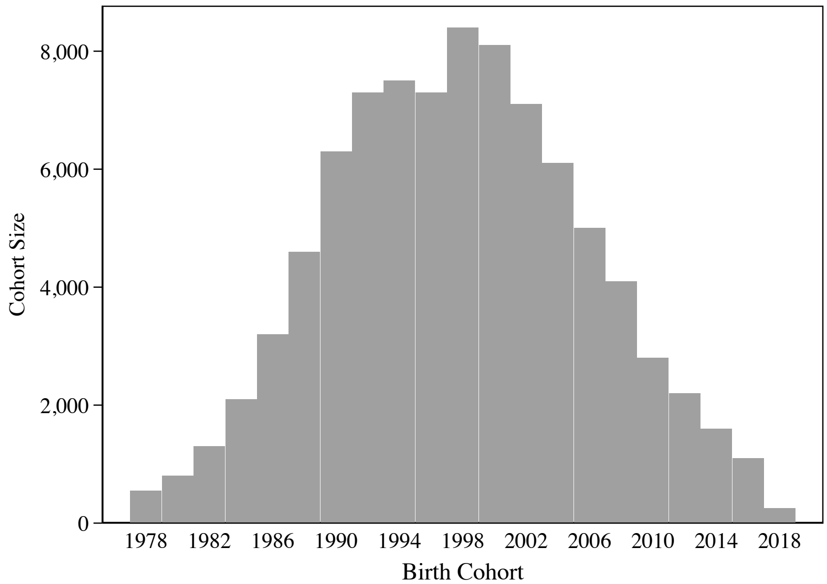
Appendix Figure 3: Distribution of Child Birth Cohorts



Source: Authors' calculations from the 2022Q2 CJARS vintage and crosswalks from Finlay, Mueller-Smith, and Street (2023).
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure reports the distribution of birth cohorts for children observed with justice-involved individuals in the analysis sample. Each bar represents and aggregated birth cohort bin of two years, except for 2018.
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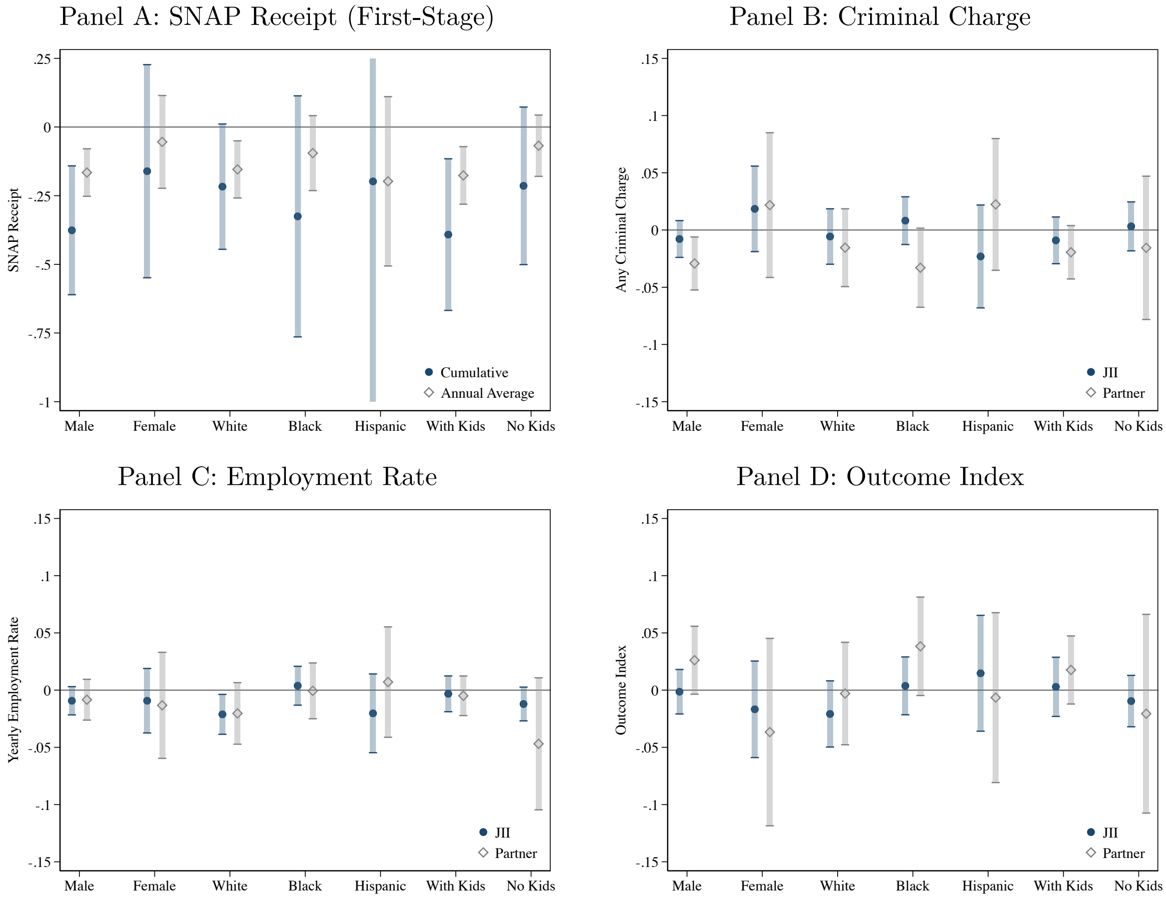
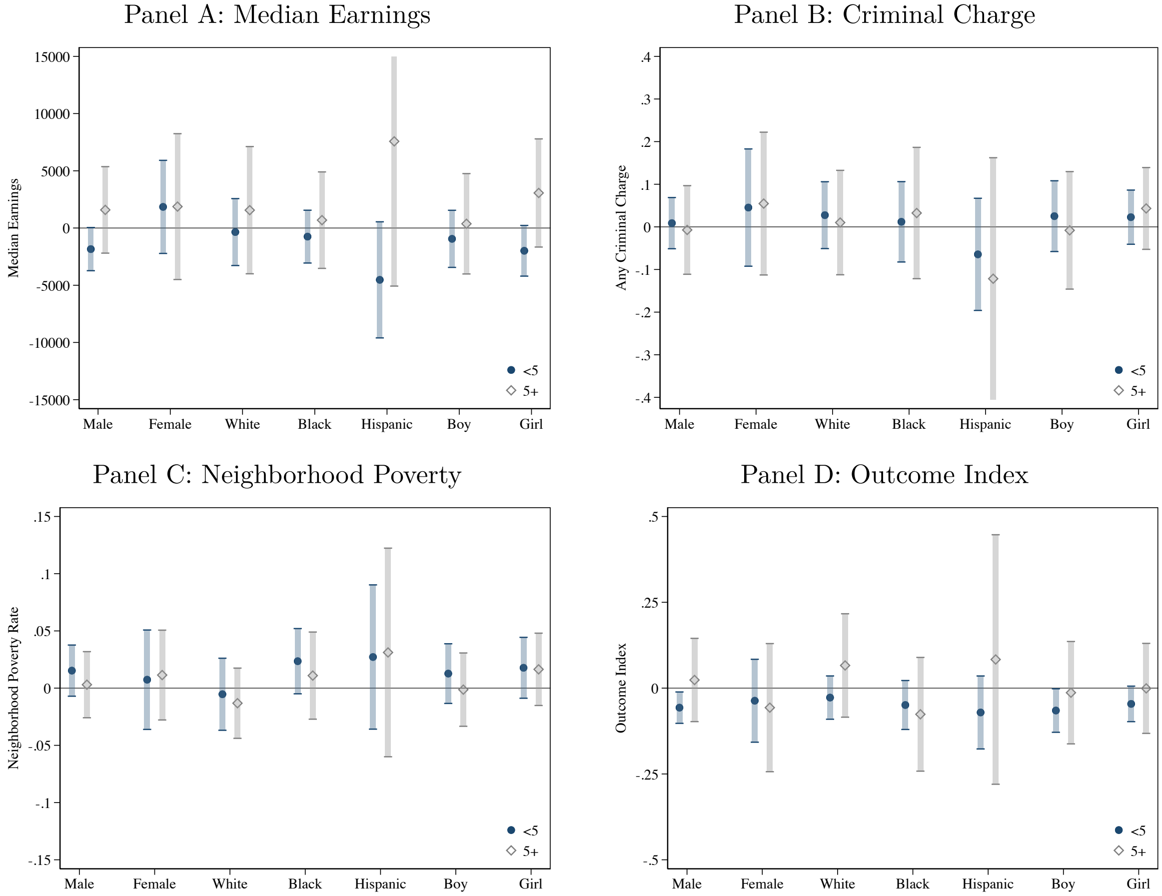
Appendix Figure 4: Heterogeneity of Estimated Adult Impacts



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots point estimates for subsamples indicated on the horizontal axis for each outcome listed in the panel title. Panel A reports contemporaneous and cumulative first-stage estimates, adjusted for measurement error. Panels B-D report reduced form estimates for justice-involved individuals and romantic partners. The outcome index is constructed following the method described in the main text. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. SNAP Receipt regressions additionally control for year fixed effects. 95 percent confidence intervals based on robust standard errors indicated by vertical shaded regions and clustered at the household level in Panel A. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY23-CES014-020, #CBDRB-FY23-CES014-051, #CBDRB-FY24-CES014-004, #CBDRB-FY24-CES014-011, #CBDRB-FY24-0344, & #CBDRB-FY24-0450.

Appendix Figure 5: Heterogeneity of Estimated Kid Impacts



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots reduced form point estimates for subsamples indicated on the horizontal axis for each outcome listed in the panel title. Each panel title lists the outcome. Solid dots report estimates for children under 5 in 1996 and hollow diamonds report estimates for children over 5 in 1996. The outcome index is described following the method described in the main text. Standard errors clustered at the household level are reported in parentheses. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, indicators for any missing controls, and child age-by-sex fixed effects. 95 percent confidence intervals based on standard errors clustered at the household levelindicated by vertical shaded regions. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
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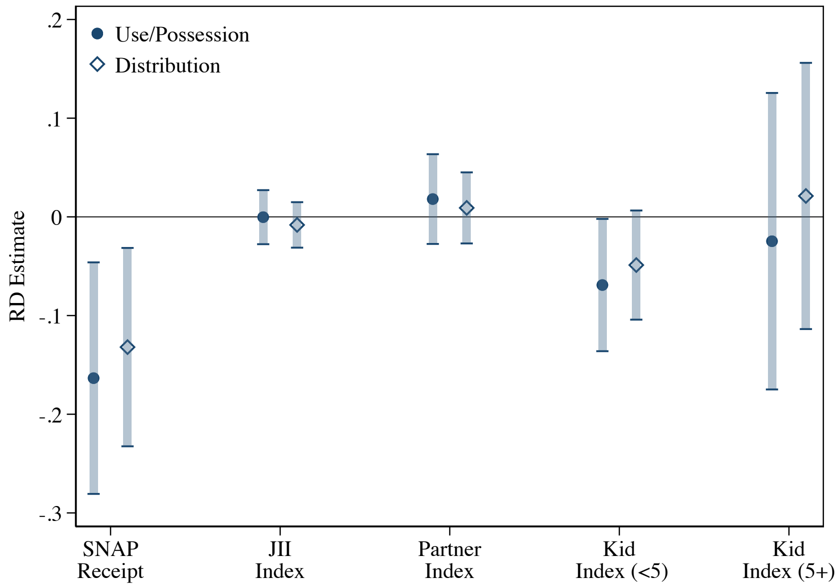
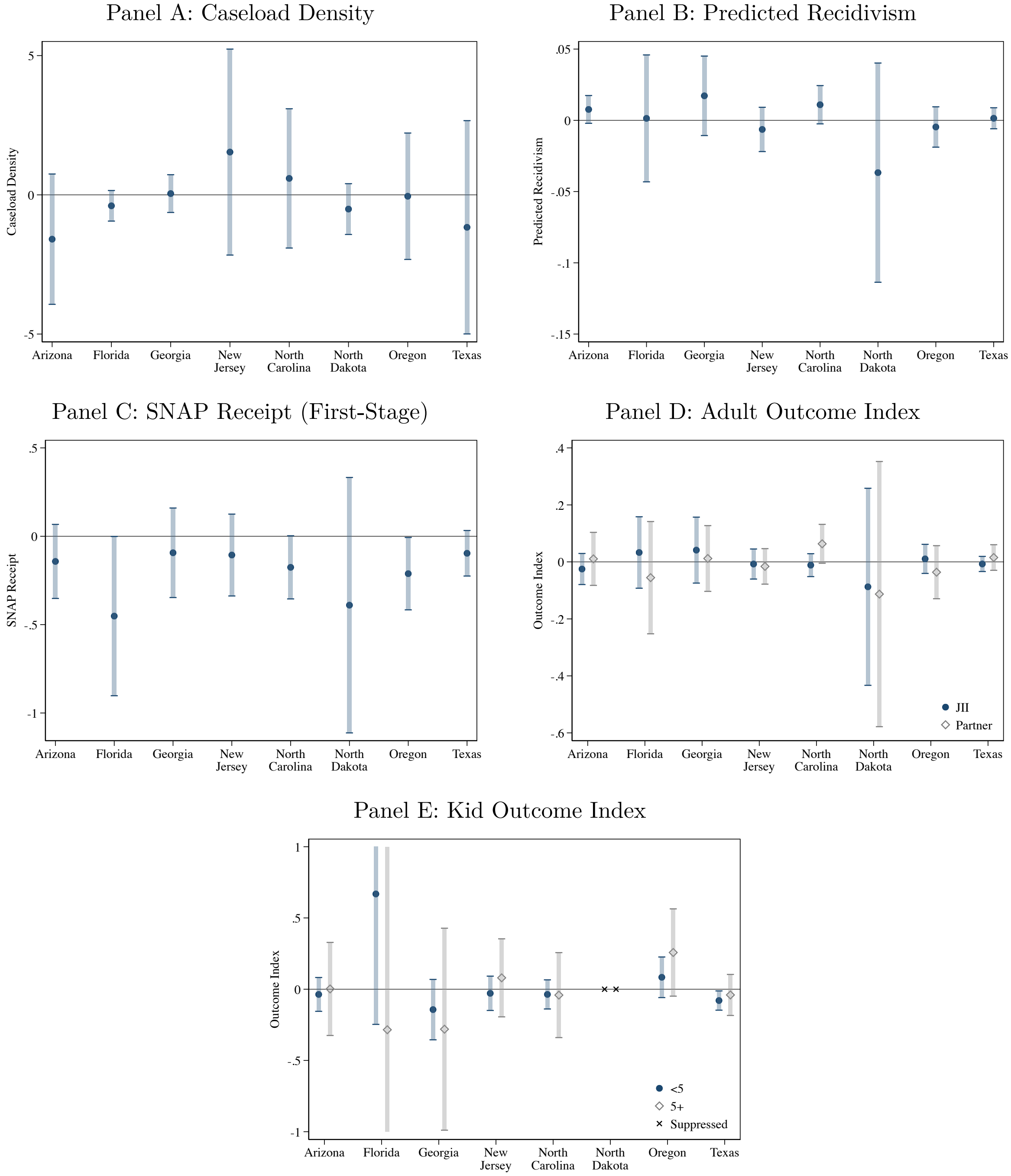
Appendix Figure 6: Heterogeneity of Reduced Form Effects by Disqualifying Conviction Type



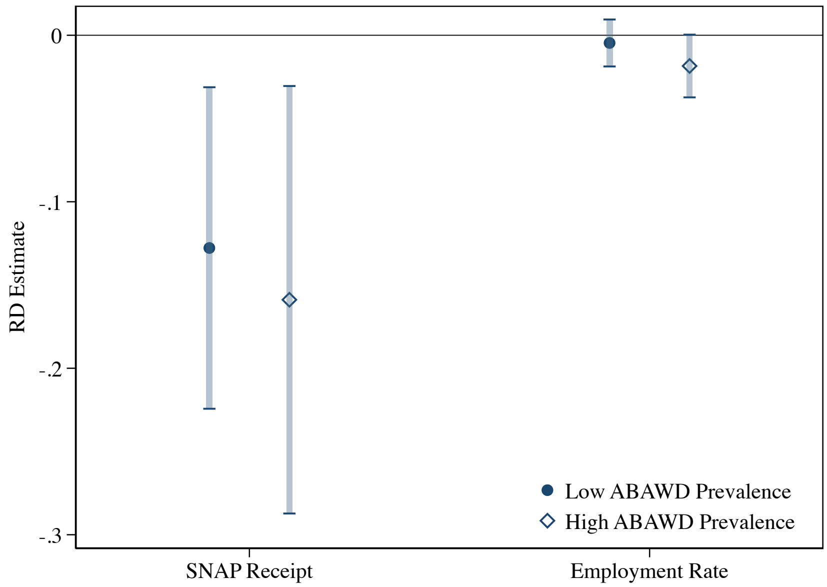
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots contemporaneous first-stage and reduced form point estimates across different outcomes among subsamples defined by individuals with use/possession or distribution disqualifying offenses. Solid circles represent use/possession estimates and hollow diamonds represent distribution subsample estimates. The outcome is listed on the x-axis. The outcome indices are constructed following the method described in the main text. Estimates of SNAP receipt are measurement error-adjusted. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. SNAP Receipt regressions additionally control for year fixed effects. Child outcomes additionally control for child age-by-sex fixed effects. 95 percent confidence intervals based on standard errors clustered at the household level (SNAP Receipt, Kid outcomes) or robust standard errors (adult outcomes). Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY23-CES014-020, #CBDRB-FY23-CES014-051, #CBDRB-FY24-CES014-004, #CBDRB-FY24-CES014-011, #CBDRB-FY24-0344, & #CBDRB-FY24-0450.

Appendix Figure 7: Reduced Form Estimates of PRWORA Ban by State



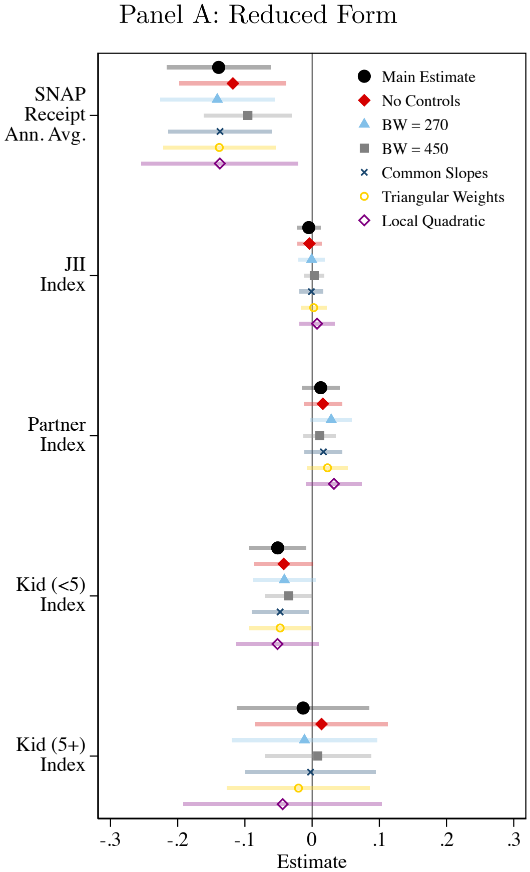
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots contemporaneous first-stage and reduced form point estimates across different outcomes among subsamples stratified by state. Each point represents a separate RD point estimate. First-stage estimates are measurement error-adjusted. X markers indicate estimates suppressed due to small sample sizes. Outcome indices are constructed following the method described in the main text. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. SNAP Receipt outcomes additionally control for year fixed effects. Child outcomes additionally control for child age-by-sex fixed effects. 95 percent confidence intervals are based on robust standard errors or clustered at the household level. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY23-CES014-020, #CBDRB-FY23-CES014-051, #CBDRB-FY24-CES014-004, #CBDRB-FY24-CES014-011, #CBDRB-FY24-0344, & #CBDRB-FY24-0450.

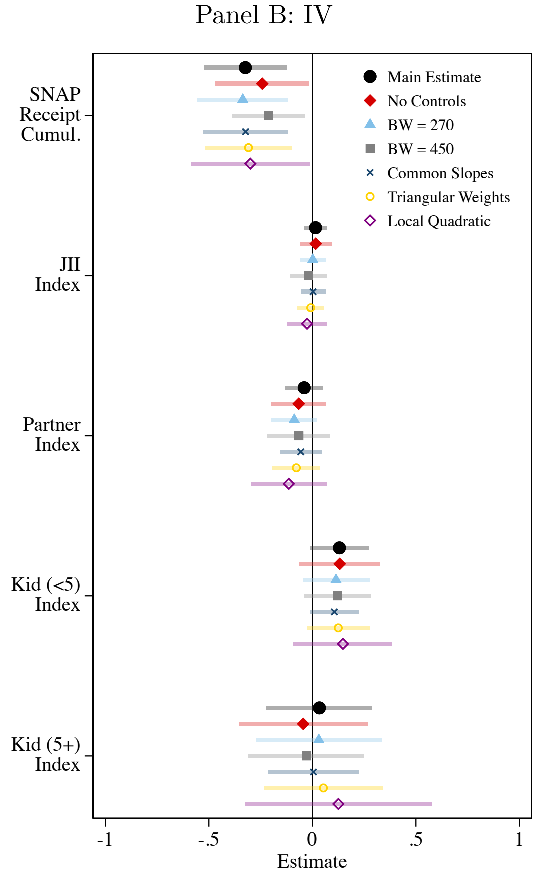
Appendix Figure 8: Reduced Form Estimates of PRWORA Ban on SNAP Receipt and JII Employment by Prevalence of ABAWD Waivers



Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots contemporaneous and reduced form point estimates across subsamples defined by state prevalence of ABAWD waivers from 1998-2008. We classify states as high waiver prevalence if their mean county yearly prevalence rate is at least sixty percent. High ABAWD waiver prevalence states include Arizona, New Jersey, and Oregon. Solid circles indicate low ABAWD prevalence states and hollow diamonds indicate high ABAWD prevalence states. The outcome is listed on the x-axis. First-stage estimates are measurement error-adjusted. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. SNAP Receipt regressions additionally control for year fixed effects. 95 percent confidence intervals based on robust standard errors or standard errors clustered at the household level. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY23-CES014-020, #CBDRB-FY23-CES014-051, #CBDRB-FY24-CES014-004, #CBDRB-FY24-CES014-011, #CBDRB-FY24-0344, & #CBDRB-FY24-0450.

Appendix Figure 9: Robustness of Estimated Impacts to Specification Choices





Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This figure plots the point estimates for each key outcome across six different specification choices as indicated by labels on the horizontal axis. In each panel, the first estimate is from our baseline specification in equation (1). Moving to the right, we display estimates for specifications that: do not include baseline controls; modify the bandwidth used to 270 and 450 days on each side of the discontinuity, respectively; restrict the slope on each side of the discontinuity to be the same across states; use a triangular weight in the estimation instead of the baseline uniform weights; and allow for a quadratic fit on each side of the discontinuity instead of imposing a linear relationship. Outcome indices are constructed following the method described in the main text. Estimates of SNAP receipt are measurement error-adjusted. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. SNAP Receipt regressions additionally control for year fixed effects and are weighted using sampling weights. Child outcomes additionally control for child age-by-sex fixed effects. 95 percent confidence intervals based on robust standard errors or standard errors clustered at the household level indicated by shaded regions.
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Appendix Table 1: Summary of Estimation Sample and Repeal Legislation

State	Estimation Sample	Repeal Year	Repeal Population
Arizona	Use/Possession and Distribution	2017	Use/Possession
Florida	Trafficking	None	
Georgia	Use/Possession and Distribution	2016	Use/Possession and Distribution
New Jersey	Distribution	1997	Use/Possession
North Carolina	Distribution	None	
North Dakota	Use/Possession and Distribution	2013	Use/Possession and Distribution
Oregon	Use/Possession and Distribution	1997	Use/Possession and Distribution
Texas	Use/Possession and Distribution	2015	Use/Possession and Distribution

Notes: This table summarizes the sample population and how we consider legislation repealing the bans over the follow-up period, along with the repeal year and the population the repeal affects. In general, we list the first relevant repeal legislation in the event there are multiple repeals with different conditions (e.g., North Dakota). New Jersey also had a second repeal in 2000 which removed the restriction for individuals with disqualifying distribution offenses.

Appendix Table 2: Comparing Measures of Benefit Receipt from Survey and Administrative Records Among Households with Felony Drug Convictions

	Administrative Records		
	No Receipt	Receipt	
Survey Records	(1)	(2)	(3)
No Receipt	0.428	0.150	
Receipt	0.017	0.406	
	False Positive	False Negative	Concordance
	0.038	0.269	0.834
			(0.002)

Source: Authors' calculations from the 2022Q2 CJARS vintage, 2005-2019 American Community Surveys, and administrative SNAP benefit records from AZ, MD, MI, ND, and OR.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports the degree of concordance between survey-based and administrative measures of SNAP receipt. The sample includes households from the 2005-2019 American Community Surveys who have a member with a felony drug conviction for an offense that occurred before the survey year. The sample is further restricted to Arizona, Maryland, Michigan, North Dakota, and Oregon. Each cell reports the fraction of records with that combination of survey and administrative SNAP receipt. Column 3 lists the concordance rate, adding up the diagonal of the concordance matrix. Standard error reported in parentheses.
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Appendix Table 3: First-Stage Estimates of PRWORA Ban on Contemporaneous SNAP Receipt

	Main JII	Add Partners/Kids	Conditional on Partners/Kids	During Ban	After Repeal
	(1)	(2)	(3)	(4)	(5)
SNAP Receipt	−0.139***	−0.070**	−0.147***	−0.125***	−0.143**
	(0.039)	(0.031)	(0.050)	(0.047)	(0.068)
Control Mean	0.365	0.432	0.384	0.326	0.445
Number of Observations	5,300	9,500	3,400	3,450	1,850

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, and crosswalks from Finlay, Mueller-Smith, and Street (2023).

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports first-stage estimates of the PRWORA ban on contemporaneous SNAP Receipt. Estimates and means correspond to estimates in Figure 2. Regressions control for Commuting Zone fixed effects, year fixed effects, and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Estimates weighted using ACS and CPS sampling weights. Control means include observations within 75 days to the left of the cutoff. Estimates are measurement error-adjusted. Standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Appendix Table 4: IV and Reduced Form Estimates of Timing of Neighborhood Location and Employment in Higher-Skill Job for Children

	<5 at Time of Ban			5+ at Time of Ban		
	Control Mean	RF Estimate	IV Estimate	Control Mean	RF Estimate	IV Estimate
	(1)	(2)	(3)	(4)	(5)	(6)
Move First, Higher-Skill Job Second	0.149	−0.010	0.026	0.143	0.014	−0.036
		(0.033)	(0.086)		(0.036)	(0.094)
Higher-Skill Job First, Move Second	0.193	−0.077**	0.195*	0.189	−0.058	0.148
		(0.035)	(0.115)		(0.041)	(0.119)

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), Business Register, administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports reduced form (Columns 2, 5) and instrumental variables (Columns 3, 6) estimates of being banned from or receiving SNAP on various outcomes for children under 5 in 1996 (Columns 1-3) or over 5 in 1996 (Columns 4-6). The first-stage is measurement error adjusted and estimated using the weighted sum described in Section 4. The outcome is listed in each row. Outcomes are defined over the ages 19-22. Move is an indicator for being in a low-poverty (less than 20 percent) neighborhood and higher-skill job is defined as an indicator for being employed in an industry with less than 15% prevalence of high school dropouts. Control means in Columns 1 and 3 include observations within 75 days to the left of the discontinuity. Standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Appendix Table 5: Reduced Form Estimates of Additional Measures of Child and Household Program Participation

	Medicaid		HUD		Other Assistance	
	Kids <5	Kids 5+	Kids <5	Kids 5+	HHs w/ Kids <5	HHs w/ Kids 5+
	(1)	(2)	(3)	(4)	(5)	(6)
Banned from SNAP	0.001	0.009	−0.008	0.019*	0.032**	−0.009
	(0.007)	(0.015)	(0.006)	(0.010)	(0.015)	(0.017)
Control Mean	0.712	0.626	0.187	0.161	0.037	0.036
Number of Observations	69,000	17,000	69,000	17,000	5,200	2,100

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2005-2019 American Community Surveys, 2000-2019 CMS enrollment files, 1997-2019 HUD program files, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports reduced form point estimates of the PRWORA ban on additional child program participation and household benefit usage. The outcome and sample are listed in the column titles. Medicaid and HUD measure the yearly participation rate. Other assistance calculates the probability of a household using other state and local assistance programs using information from the American Community Surveys. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Columns 1-4 additionally control for sex-by-age fixed effects. Columns 5-6 additionally control for year fixed effects. Control means include observations within 75 days to the left of the cutoff. Standard errors clustered at the household are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-020, #CBDRB-FY23-CES014-051, #CBDRB-FY24-CES014-004, #CBDRB-FY24-CES014-011, #CBDRB-FY24-0344, & #CBDRB-FY24-0450.

Appendix Table 6: Heterogeneous Effects of Main IV and Reduced Form Estimates

	JII Characteristics								Kid Characteristics	
	Base Estimate	Male	Female	White	Black	Hispanic	With Kids	No Kids	Boy	Girl
Panel A: First-Stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SNAP Receipt	−0.324***	−0.376***	−0.161	−0.217*	−0.325	−0.198	−0.392***	−0.214	–	–
	(0.102)	(0.120)	(0.198)	(0.116)	(0.224)	(0.643)	(0.141)	(0.146)	(–)	(–)
SNAP Receipt - Annual Average	−0.139***	−0.166***	−0.054	−0.154***	−0.095	−0.198	−0.176***	−0.068	–	–
	(0.039)	(0.044)	(0.086)	(0.053)	(0.070)	(0.157)	(0.053)	(0.057)	(–)	(–)
Reduced Form Estimates										
Panel B: JII										
JII Index	−0.005	−0.001	−0.017	−0.021	0.004	0.015	0.003	−0.010	–	–
	(0.009)	(0.010)	(0.022)	(0.015)	(0.013)	(0.026)	(0.013)	(0.011)	(–)	(–)
Panel C: Romantic Partners										
Partner Index	0.013	0.026*	−0.037	−0.003	0.038*	−0.007	0.018	−0.021	–	–
	(0.014)	(0.015)	(0.042)	(0.023)	(0.022)	(0.038)	(0.015)	(0.044)	(–)	(–)
Panel D: Children										
Kid Index (<5)	−0.051**	−0.057**	−0.037	−0.028	−0.049	−0.071	–	–	−0.065**	−0.046*
	(0.022)	(0.023)	(0.062)	(0.032)	(0.036)	(0.054)	(–)	(–)	(0.032)	(0.026)
Kid Index (5+)	−0.013	0.024	−0.057	0.066	−0.076	0.083	–	–	−0.013	−0.001
	(0.050)	(0.062)	(0.095)	(0.077)	(0.084)	(0.185)	(–)	(–)	(0.076)	(0.067)
IV Estimates										
Panel E: JII										
JII Index	0.015	0.004	0.104	0.096	−0.012	−0.074	−0.007	0.045	–	–
	(0.029)	(0.027)	(0.187)	(0.087)	(0.041)	(0.238)	(0.034)	(0.062)	(–)	(–)
Panel F: Romantic Partners										
Partner Index	−0.040	−0.070	0.228	0.014	−0.118	0.033	−0.045	0.096	–	–
	(0.046)	(0.046)	(0.395)	(0.108)	(0.104)	(0.228)	(0.042)	(0.225)	(–)	(–)
Panel G: Children										
Kid Index (<5)	0.131*	0.155	0.175	0.103	0.164	0.217	–	–	0.172	0.120
	(0.073)	(0.095)	(0.426)	(0.147)	(0.182)	(0.606)	(–)	(–)	(0.111)	(0.089)
Kid Index (5+)	0.034	−0.065	0.271	−0.248	0.254	−0.256	–	–	0.035	0.002
	(0.131)	(0.173)	(0.663)	(0.340)	(0.356)	(0.996)	(–)	(–)	(0.203)	(0.175)

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports heterogeneous effects of the first-stage, instrumental variable, and reduced form estimates. The main estimate is listed in Column 1. Each subsequent column uses subsamples based on characteristics of the focal justice-involved individual. Blank cells contain no estimates. The first-stage in row one is constructed using the weighted sum described in Section 4 and is adjusted for measurement error. The sample of individuals is listed in the panel title. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Child regressions additionally control for sex-by-age fixed effects. First-stage estimates weighted using ACS and CPS sampling weights. Indices constructed following the method described in the main text. Robust standard errors or standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Appendix Table 7: Robustness Checks of Main IV and Reduced Form Estimates

	Base Estimate	No Controls	BW = 270 Days	BW = 450 Days	Common Slopes	Triangular Weights	Local Quadratic
Panel A: First-Stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SNAP Receipt	−0.324***	−0.242**	−0.336***	−0.211**	−0.323***	−0.309***	−0.300**
	(0.102)	(0.116)	(0.112)	(0.089)	(0.104)	(0.107)	(0.147)
SNAP Receipt - Annual Average	−0.139***	−0.118***	−0.141***	−0.095***	−0.137***	−0.138***	−0.137**
	(0.039)	(0.041)	(0.043)	(0.033)	(0.039)	(0.043)	(0.060)
Reduced Form Estimates							
Panel B: JII							
JII Index	−0.005	−0.004	−0.001	0.003	−0.001	0.002	0.007
	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)	(0.010)	(0.013)
Panel C: Romantic Partners							
Partner Index	0.013	0.016	0.028*	0.011	0.017	0.023	0.033
	(0.014)	(0.015)	(0.016)	(0.012)	(0.014)	(0.016)	(0.021)
Panel D: Children							
Kid Index (<5)	−0.051**	−0.042*	−0.041*	−0.035**	−0.048**	−0.048**	−0.051*
	(0.022)	(0.022)	(0.024)	(0.018)	(0.021)	(0.023)	(0.031)
Kid Index (5+)	−0.013	0.014	−0.011	0.009	−0.002	−0.020	−0.044
	(0.050)	(0.050)	(0.055)	(0.040)	(0.049)	(0.054)	(0.075)
IV Estimates							
Panel E: JII							
JII Index	0.015	0.016	0.002	−0.018	0.003	−0.008	−0.026
	(0.029)	(0.039)	(0.031)	(0.045)	(0.030)	(0.034)	(0.049)
Panel F: Romantic Partners							
Partner Index	−0.040	−0.066	−0.088	−0.065	−0.056	−0.077	−0.114
	(0.046)	(0.067)	(0.057)	(0.077)	(0.052)	(0.059)	(0.093)
Panel G: Children							
Kid Index (<5)	0.131*	0.132	0.114	0.122	0.106*	0.125	0.148
	(0.073)	(0.100)	(0.082)	(0.082)	(0.060)	(0.078)	(0.122)
Kid Index (5+)	0.034	−0.044	0.031	−0.030	0.005	0.053	0.126
	(0.131)	(0.159)	(0.156)	(0.142)	(0.111)	(0.147)	(0.231)

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports robustness checks of the first-stage, instrumental variable, and reduced form estimates. The main estimate is listed in Column 1. Each subsequent column imposes the listed specification permutation. The first-stage in row one is constructed using the weighted sum described in Section 4. The sample of individuals is listed in the panel title. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Child regressions additionally control for sex-by-age fixed effects. First-stage estimates weighted using ACS and CPS sampling weights and are measurement error-adjusted. Indices are constructed using the method described in the main text. Robust standard errors or standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Appendix Table 8: Placebo Checks of Reduced Form Estimates Using Alternative Samples and Cutoff Dates

	Main Estimate	Non-Drug Felonies	Alternative Cutoff Dates: August 23, 1995	December 23, 1995	April 23, 1996	December 23, 1996	April 23, 1997	August 23, 1997
Panel A: First-Stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SNAP Receipt - Annual Average	-0.139^{***}	0.020	-0.005	0.047	0.018	0.015	0.011	-0.065^{*}
	(0.039)	(0.023)	(0.041)	(0.041)	(0.041)	(0.038)	(0.037)	(0.037)
Panel B: JII								
JII Index	-0.005	0.004	-0.017^{*}	0.022^{**}	-0.003	-0.004	0.004	-0.014
	(0.009)	(0.005)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Panel C: Romantic Partners								
Partner Index	0.013	0.012	0.022	0.005	-0.004	-0.016	0.023^{*}	-0.017
	(0.014)	(0.009)	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)
Panel D: Children								
Kid Index (<5)	-0.051^{**}	-0.008	0.014	0.038^{*}	-0.017	0.036^{*}	0.004	-0.008
	(0.022)	(0.012)	(0.023)	(0.023)	(0.021)	(0.021)	(0.020)	(0.020)
Kid Index (5+)	-0.013	-0.027	-0.008	-0.020	0.071	-0.037	-0.017	0.057
	(0.050)	(0.028)	(0.049)	(0.050)	(0.049)	(0.048)	(0.048)	(0.047)

Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, Mueller-Smith, and Street (2023), administrative SNAP records from AZ, ND, and OR, and the 2022Q2 CJARS vintage.

Notes: Estimates and sample sizes have been rounded to preserve confidentiality. This table reports falsification tests using different samples and placebo cutoffs. The outcome is listed in each row. Column 1 reproduces the main estimate from the focal sample. Column 2 uses a sample of non-drug felony convictions around the August 23, 1996 cutoff date. Columns 3-8 use the focal sample and redefine the cutoff date as listed in the column title. Regressions control for Commuting Zone fixed effects and controls for race-by-sex, age, number of prior misdemeanor convictions, whether the convicting county was urban, and indicators for any missing controls. Child regressions additionally control for sex-by-age fixed effects. First-stage estimates weighted using ACS and CPS sampling weights and are measurement error-adjusted. Indices constructed following the method described in the main text. Robust standard errors or standard errors clustered at the household level are reported in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Appendix B: Adjusting for Measurement Error in Survey-Based Measures of Benefit Receipt

In this appendix, we provide additional details and an underlying econometric framework for our procedure to adjust the survey-based measures of benefit receipt for measurement error.

Our goal is to estimate the impact of SNAP receipt on future outcomes. Ideally, we would have a population-level measure of benefit receipt using administrative data so that we could estimate the following model, abstracting from auxiliary covariates, for simplicity:

$$Y_i = A + dD_i^{Admin} + \nu_i \tag{B1}$$

Unfortunately, data limitations preclude us from estimating such a model. As a result, we rely on survey-based measures of benefit receipt from the American Community Survey and the Current Population Survey. However, survey data are well-known to contain measurement error, leading to underreporting of benefit receipt. Formally, let the population relationship between benefit receipt in administrative records and the survey responses be the following:

$$D_i^{admin} = \alpha + \beta D_i^{Survey} + \varepsilon_i \tag{B2}$$

In the absence of measurement error, β will be one. However, our confusion matrix in Appendix Table 2 confirms that the correspondence between the survey and administrative records is not one-to-one. As a result, our first-stage estimates will be the following:

$$\hat{D}_i^{admin} = \hat{\beta} D_i^{Survey} = a + \delta Z_i + e_i \tag{B3}$$

where the coefficient δ will be biased downward since $\hat{\beta} < 1$. Dividing by the degree of measurement error $\hat{\beta}$ yields a measurement error-corrected estimate of the first-stage $\frac{\hat{\delta}}{\hat{\beta}}$. This requires an assumption that the degree of measurement error is stable across the discontinuity. We view this assumption as reasonable given the dearth of discontinuous jumps in observable covariates, many of which predict underreporting (Meyer, Mittag, and Goerge 2022). Thus, we divide all first-stage estimates by our estimate of $\hat{\beta}$ which we get from the confusion matrix as C which is the degree of concordance along the diagonal. This adjustment has the consequence of marginally inflating the first-stage magnitude and subsequently reduces the size of the IV estimates. Given the high degree of certainty in the diagonal concordance estimate, we abstract from estimation error in our adjustment process and treat C as a known scalar.