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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 cognitive and educational outcomes worsen, especially those impacted at young ages.

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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 11.6 percentage points (↓ 32%) less likely to report receiving benefits on an annual basis between 1997 and 2019.[4] Such findings rely on repeated cross-section data contained in the Current Population Survey and the American Community Survey, which, given high churn rates year-to-year in benefit receipt, have the drawback of constraining our ability to quantify the first stage's true extensive

[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 2.7 fewer years of SNAP benefits on average in this population over a 23 year follow-up period.

margin. To address this, we develop an aggregation procedure leveraging administrative caseload statistics on SNAP benefit receipt in a subset of our analysis states, which yields a cumulative first-stage estimate of 27 percentage points. 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 and overstate the implied treatment effects. 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 negatively impacted 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 childrenstill shows strong evidence that households 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 children were incorrectly removed from the program or 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 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 individual 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 adult outcomes in our sample. Using a variety of measures, we do not observe differences in future justice involvement across the discontinuity. These results align with findings from Luallen, Edgerton, and Rabideau (2018), but contrast with Yang (2017) and Tuttle (2019) who find increases in recidivism among individuals disqualified from assistance as a result of the PRWORA restrictions. Given that our study population has very high recidivism risk with more than 60 percent of defendants experiencing a new

[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.

criminal charge over the follow-up period, these null results may be unsurprising.[6]

We similarly find null effects on employment rates, measured using employer-reported W-2 tax forms. 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 an employment response (Hoynes and Schanzenbach 2012; East et al. Forthcoming; Gray et al. 2023; Cook and East 2023). 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 impact of bans on adult outcomes, 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; Bailey et al. 2020; Barr and Smith 2023). 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 large and significant increases in the probability that children in these households experience "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition," a measure that we interpret as simultaneously capturing stress and cognitive decline. Accordingly, we find suggestive evidence that the safety net restrictions increase the probability of high school dropout for exposed children. Both of these effects are concentrated among children who are younger when SNAP eligibility is removed for a member of the household, consistent with 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 results suggest that disqualifying individuals from the social safety net may harm child mental health, cognitive

[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.

development, and human capital accumulation.

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 household-level impacts while previous research primarily 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), SSI (Deshpande and Mueller-Smith, 2022), and Medicaid (Arenberg, Neller, and Stripling Forthcoming; Jácome 2020). Finally, we add novel estimates of the impact of the PRWORA drug felony disqualifications 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; Bailey et al. 2020; Barr and Smith 2023; Hawkins et al. 2023; East et al. Forthcoming).

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 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.[7] 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). 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.[8] 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.[9] To measure non-criminal justice outcomes, we link individuals to

[7] Oregon and New Jersey fully opted out of the ban in 1997 and 2000, respectively, although Oregon later allowed for parole/probation officer discretion in recommending that benefits be denied for individuals convicted of distribution offenses. 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.

[8] 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).

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

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.[10] 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.

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.[11] 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. We first focus on identifying romantic partners/co-parents and children who are observed with the justice-

Ethnicity files.

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

[11] 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.

involved individual during the post-PRWORA follow-up period when evaluating the impact of the ban on household benefit take-up, as this population are also most likely directly affected by the bans. Below, we also examine the outcomes of older children who may be more weakly attached to the household.

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 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.[12] 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.[13] 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

[12] 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).

[13] 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).

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.[14]

Finally, we test whether individuals who are banned from SNAP and TANF are differentially more or less likely to form families. In the structure of public assistance programs, earnings from disqualified individuals continue to count towards the income threshold, even though benefit levels are determined for the remaining non-banned individuals. Consequently, the program design disincentivizes household formation for banned individuals as they effectively penalize total benefits for their (non-banned) family members. However, despite these disincentives, we find no effect on household formation in Panels E and F of Figure 1. We interpret this null result as evidence that our household and spillover estimates are unlikely to be contaminated by differential selection into household formation across the discontinuity.[15]

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 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 survey and use this for our first-stage but outcomes could also be impacted through discontinuous changes in TANF eligibility/receipt. Both of these are a violation of the exclusion restriction assumption needed to attach a causal interpretation to the IV estimate. Finally, when performing

[14]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.

[15]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.

subgroup analysis, the IV estimate is sometimes based on a first-stage discontinuity that is less precise than our estimate for the full sample.

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

In this section, we quantify the first-stage impact of the PRWORA ban on benefit receipt, leveraging 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 11.6 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.[16] 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. 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 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

[16] Point estimates of the average annual change in SNAP receipt are presented both in the panels of each figure and in Appendix Table 2.

repealing the ban without additional outreach to the affected population is unlikely to fully eliminate lower take-up of benefits.[17]

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. 2020; 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 with more limited data) estimate to have a weight of one.[18] We then estimate year-by-year first-stage impacts of the ban, combining information from nearby years using a triangular kernel.[19] 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:

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

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

[17]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.

[18]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.

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

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 27.0 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 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.[20]

[20] 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.

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.[21] In Panel A we examine whether individuals that are prohibited from receiving SNAP and TANF are more likely to engage with the criminal justice system through receiving new criminal charges.[22] 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 Appendix Table 4.[23]

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). However, this mean result masks some heterogeneity across the earnings distribution, as we find small declines in the probability of earning more than $5,000 per year.[24] This result is 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.[25] Taken together, our results instead suggest that SNAP benefit receipt plays a supporting role in stabilizing labor supply, particularly in the lower part of the earnings distribution, or that programmatic work requirements increase intensive margin labor supply.

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

[21]Point estimates are also reproduced in Appendix Table 3.

[22]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.

[23]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.

[24]Combining W-2 tax returns with crosswalks from Finlay, Mueller-Smith, and Street (2023), we also estimate that banned households are 1.1 percentage points more likely to have earnings under half the poverty line.

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

sacrificing intergenerational investment in their children (i.e., the children are the residual claimants to the household budget).

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 that are prohibited from receiving SNAP and TANF are more likely to engage with the criminal justice system through receiving new criminal charges. We find suggestive evidence that the bans decrease criminal justice involvement of a romantic partner. This could reflect a deterrence effect among partners for SNAP disqualifying drug crimes. However, Appendix Table 4 suggests that these effects are most prominent for the income-generating crime category.[26]

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 the fraction with earnings above $5,000. 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.

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 labor supply, household resources must have contracted. This raises the fundamental question of how this unanticipated consequence impacted the children's well-being and development.

The ACS is unfortunately limited in its coverage of outcomes for young children. We currently focus on two available measures, high school dropout and mental/emotional well-being, but acknowledge that these are only a small window into the lives of these children

[26]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.

and further research is warranted.[27,28]

We present graphical evidence for these two measures of child well-being in Figure 5. In Panel A, we find striking evidence that children in banned households are more likely to experience cognitive difficulty/stress, suggesting that the household instability induced from the lost transfer income affects child stress or cognitive development, in contrast to the null impacts on stress for adults. In Panel B, we examine whether these negative effects on cognitive development also translate into worse realized long-run outcomes, finding suggestive evidence that children in banned households are more likely to drop out of high school.[29] These negative impacts to children are substantial and consistent with 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. 2020; East 2020; Aizer, Hoynes, and Lleras-Muney 2022).

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 more simply, parents may "rob" their children of potential investment in order to sustain 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, evidence in support of this mechanism would be heterogeneous treatment effects in the presence of children. When we test this hypothesis in Appendix Table 5 however, we find that adult responses are similar for households both with and without children, suggesting that this channel is not a primary driver of child responses. 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 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.

[27] These exercises include all children connected to the focal justice-involved individual in order to examine heterogeneous effects across the age at treatment distribution. Match rates and child characteristics remain smooth through the discontinuity in this sample.

[28] Unfortunately, we are limited in our ability to look at outcomes for children who were older at the time of the ban, given the limited density of the birth cohort distribution for these older cohorts (see Appendix Figure 3). Moreover, many of the children are too young to reliably appear in our other outcome data during life-cycle stages where either earnings have stabilized or cohorts have passed through the peak of the age-crime profile. Future research should continue to follow these cohorts over time.

[29] We define drop out as having not obtained a high school degree or having completed a GED degree as this is most often obtained among individuals who dropped out of high school.

To further investigate this second hypothesis, Panels C and D of Figure 5 plot estimated IV impacts for these outcomes by the age of the child at the time of the SNAP ban. In both cases, we find that effects are concentrated among individuals who were young or not-yet born when the ban was implemented. We find the largest well-being and educational attainment declines associated with SNAP receipt for children under 6 when the ban was implemented in 1996.[30] 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 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 6. We find little changes in yearly Medicaid or HUD participation rates for all kids or young kids, defined as kids in the 1996 and later birth cohorts (Columns 1-4), although younger children in banned households have marginally lower HUD participation rates. However, we find some evidence that 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. However, due to data limitations, we are unable to directly attribute which program may be driving these results. Instead, we interpret these results as suggestive evidence that banned households may turn to 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.[31] Appendix Table 5 reports the impacts on receipt, reduced form and IV estimates for each outcome and subgroup and Appendix Figure 4 plots the IV estimates for comparison but should be interpreted with caution given the weak first-stage estimate among certain subgroups.[32]

Across racial and ethnic subgroups, we find that the drug felony SNAP/TANF restriction has the largest impact on receipt among Black individuals, as well as those with kids. For these groups, participation declines by around 30 percentage points. These groups also drive the reduced form decrease in criminal justice involvement for romantic partners of the

[30] We apply an Epanechnikov kernel across the estimates to reduce idiosyncratic variability in the point estimates between birth cohorts due to smaller samples.

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

[32] For child outcomes we use the subgroup-specific first-stage estimate among households with children.

banned individuals. Further, Black children are the most impacted in terms of the cognitive difficulty.

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 5 reports first-stage and reduced form results among these two subsamples. Individuals on use/possession offenses experience a marginally 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 strikingly similar results across both offense groups.[33]

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.[34] In Appendix Figure 6 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 using survey data, which may lead to unstable estimates.

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 criminal activity and labor supply for both justice-involved individuals and romantic partners. For JIIs, we also include an outcome that measures felony convictions within the first three years after the end of the focal justice-event to more closely compare our results to those in the literature. We view this measure as a middle ground outcome that balances CJARS adjudication data coverage with more commonly available measures in the literature such as return to prison, representing more serious types of justice involvement in a short-run follow-up window that captures near-term dynamics that our broader long-run estimates may mask. While we largely find consistent null effects using this measure, we see a small but imprecise increase in Florida, which qualitatively aligns with the results in Tuttle (2019).

Finally, we estimate our measure of child cognitive difficulty/stress at the state-level.

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

[34]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.

Strikingly, we continue to find near-universal increases in child cognitive difficulty, although some of the estimates are imprecise due to the smaller samples available in the survey records. These patterns suggest that our full sample estimates are not driven by a single state where child development is 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.[35] 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.[36] Appendix Figure 7 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.

5.5 Robustness of Results to Alternative Specifications

Finally, we conduct a series of robustness checks to assess whether our results are specific to our specification choices, we estimate a number of alternative models and present results in Appendix Table 7 and Appendix Figure 8. 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); do not allow for different slopes on each side of the discontinuity (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).

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 (our

[35]We exclude 2001-2003 due to national wavers that were in place due to the recession.
[36]High prevalence states in our sample are Arizona, New Jersey, and Oregon.

first-stage); limited evidence of changes in criminal justice involvement or employment rates for the affected justice-involved individuals; modest declines in criminal justice involvement for romantic partners of banned individuals; and increases in cognitive difficulty for the children 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 sharp increases in child cognitive difficulty, 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 child cognitive difficulty. While we do see some idiosyncratic changes in adult 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 children linked to affected households. Children of justice-involved individuals banned from SNAP and TANF support are more likely to dropout of high school and also more likely to exhibit difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition. These impacts are largest for children under the age of six when resources are affected which is 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). These results suggest that criminal history-based bans can negatively affect the well-being of the most vulnerable members of a household, even if there are no 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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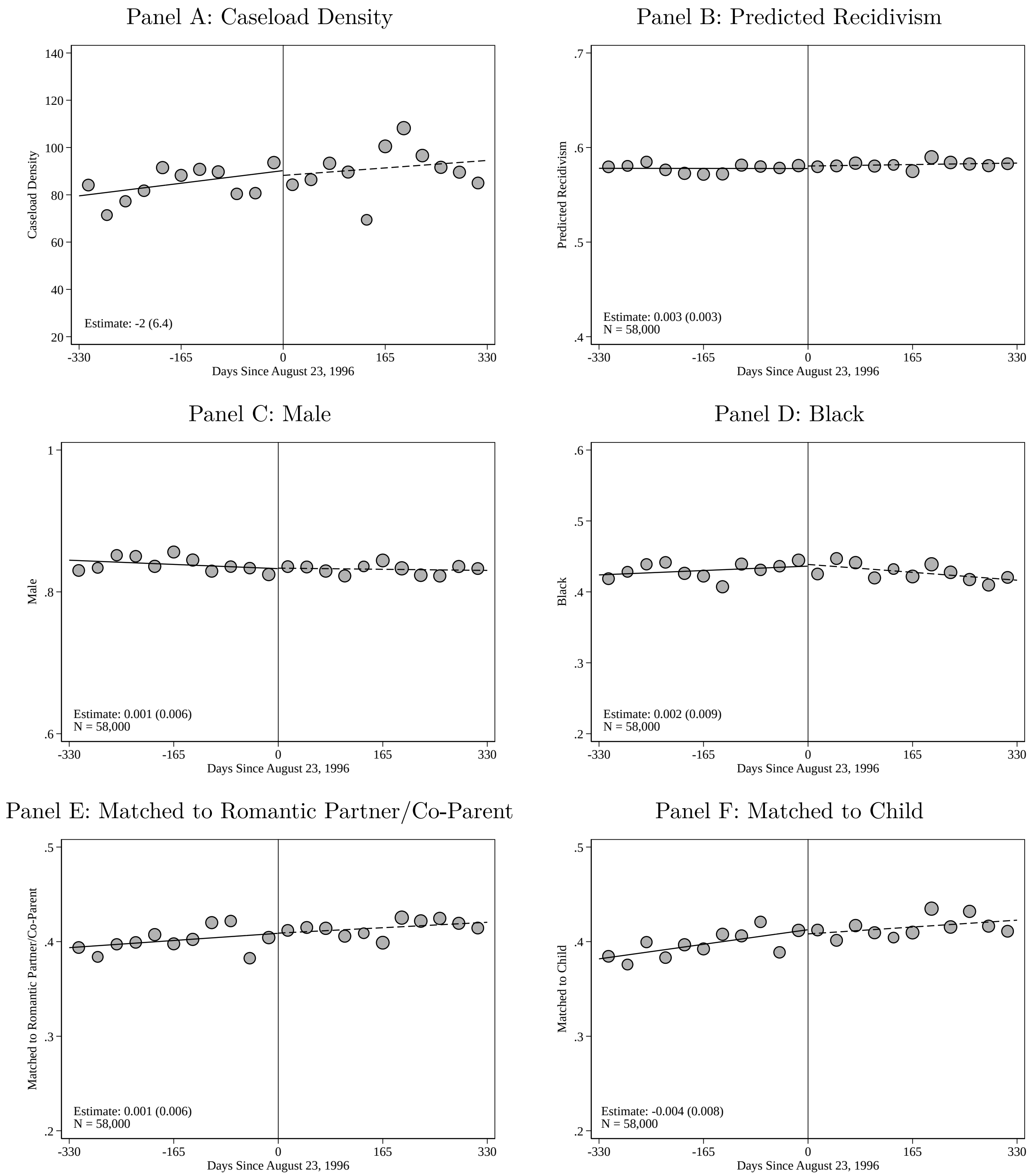
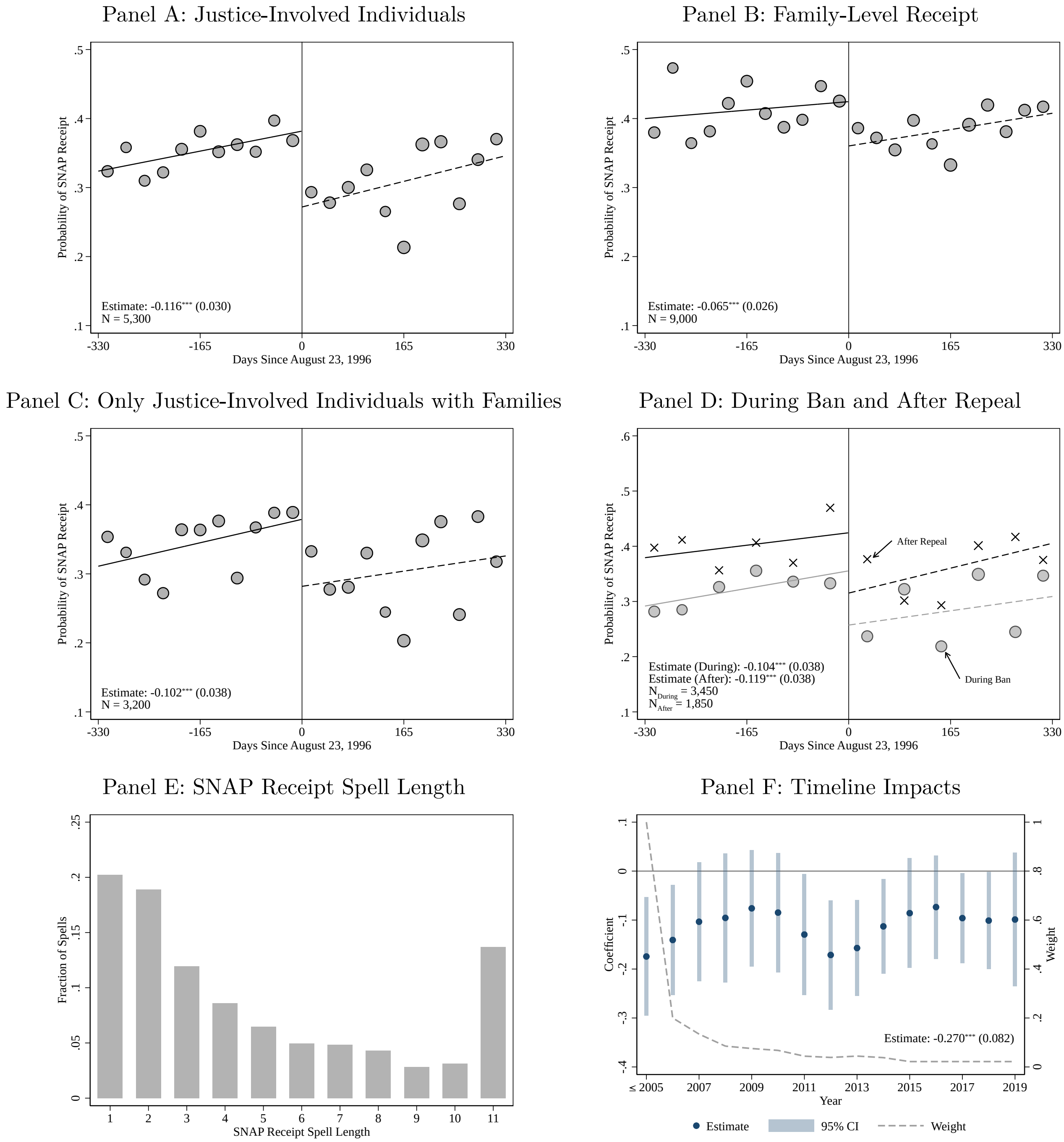
Figure 1: Evaluating the Validity of the Natural Experiment



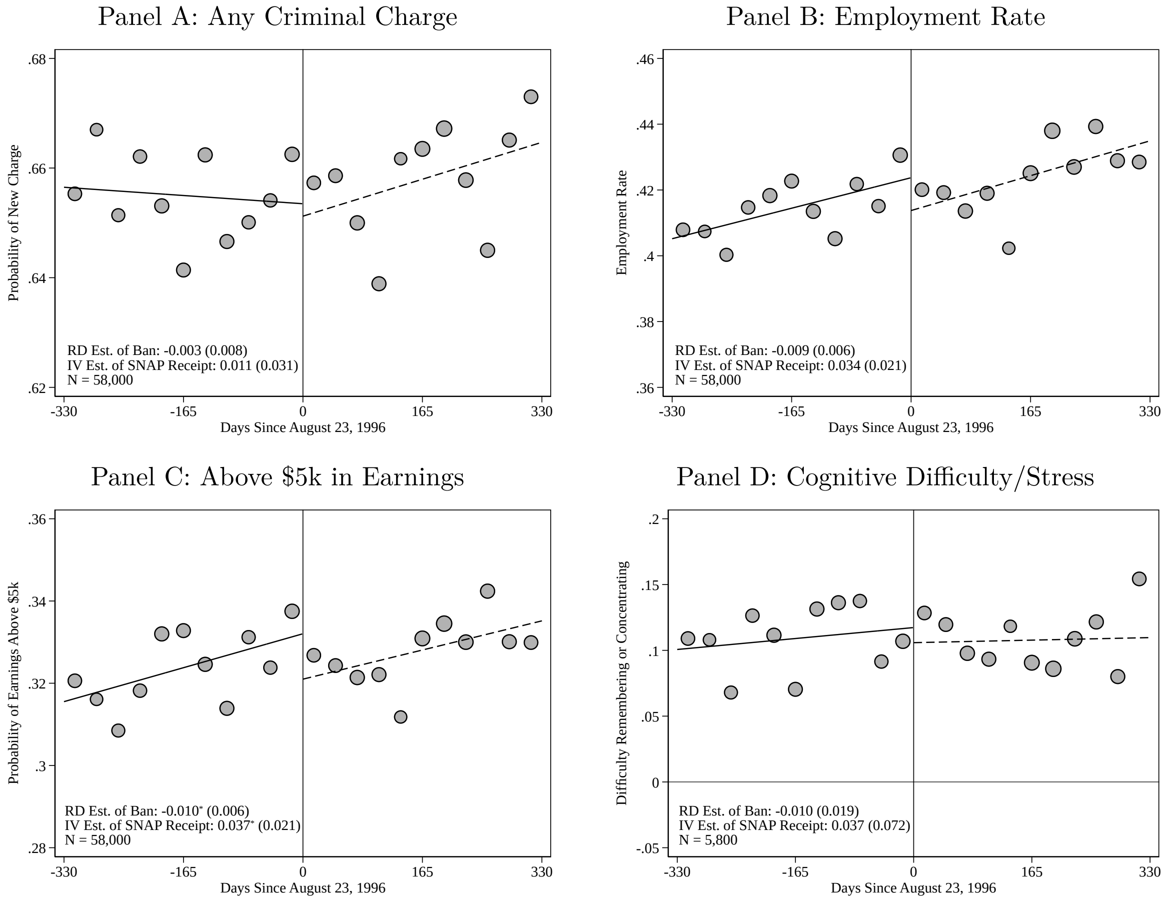
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded 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 in parentheses in Panel A and robust standard errors clustered at the Commuting Zone level in all other panels. 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. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-020.

Figure 2: First Stage Estimates of PRWORA Ban on SNAP Receipt



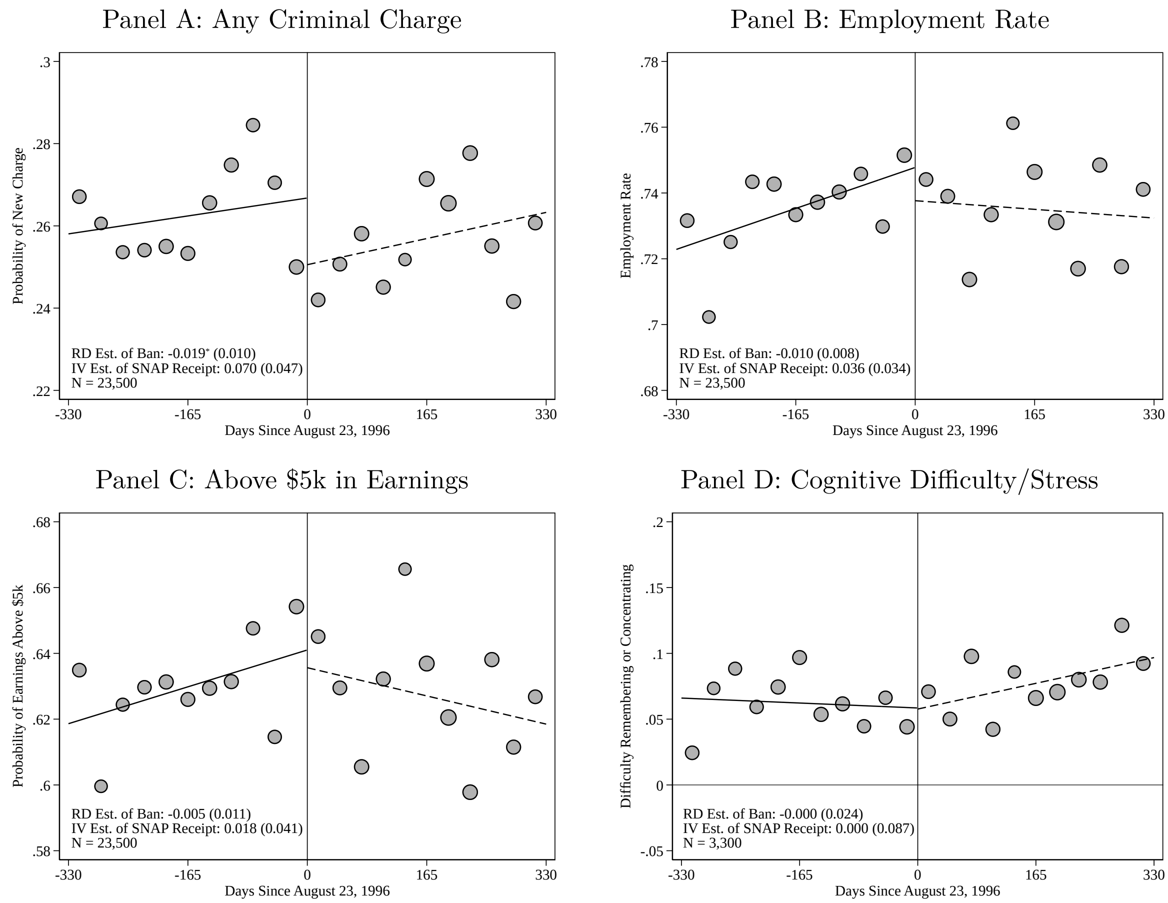
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from Finlay, 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 end points on the left and right boundaries of the support 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. Shaded bars represent 95 percent confidence intervals, with standard errors two-way clustered at the Commuting Zone and household levels in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-051 & #CBDRB-FY24-CES014-011.

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



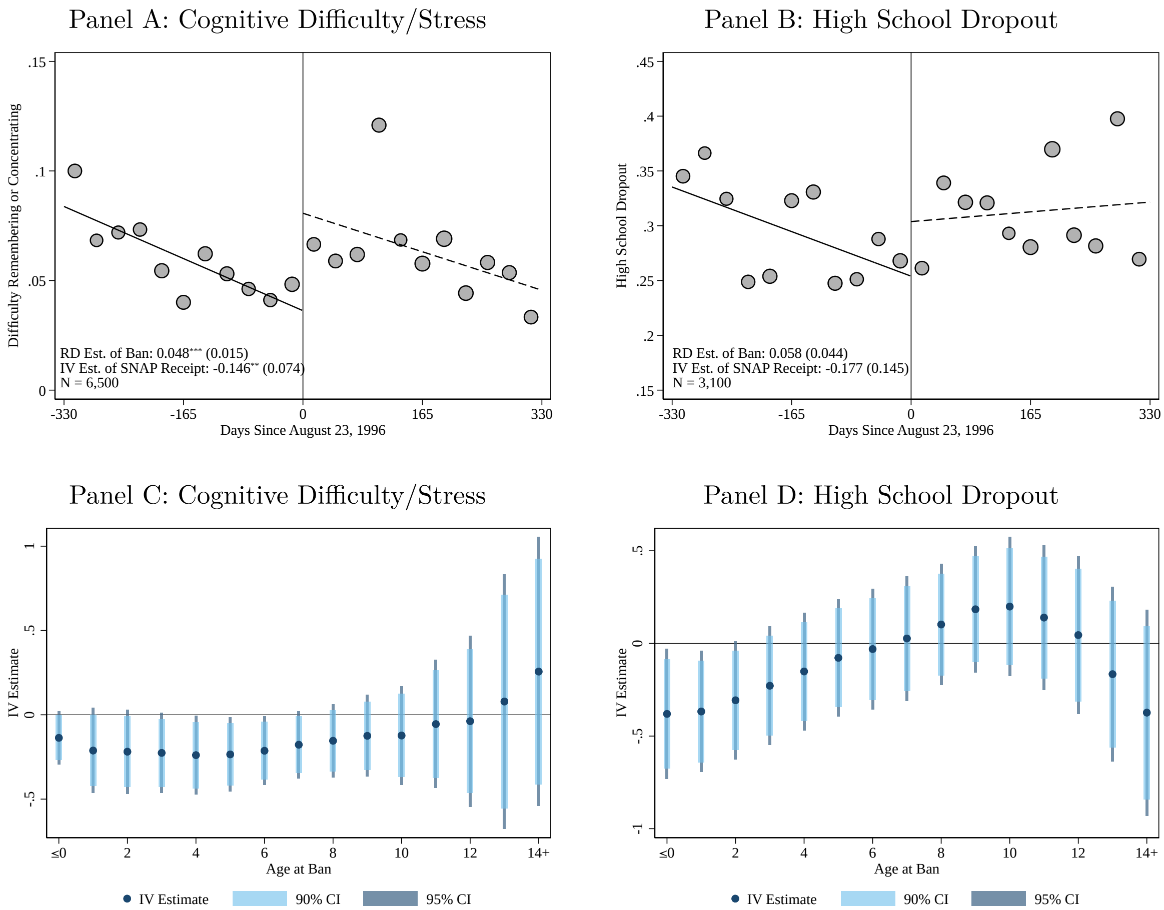
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2008-2019 American Community Survey, and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve 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 standard errors clustered at the Commuting Zone level in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-051.

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 standard errors clustered at the Commuting Zone level in parentheses. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-051.

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), 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 and who were between the ages of 5 and 25 in the year of the survey (Panels A and C) or at least 19 in the year of the survey (Panels B and D). 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. Each point 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, indicators for any missing controls, and sex-by-age fixed effects of the children, weighted using ACS sampling weights. 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." Panels C and D report estimates by birth cohort, incorporating information from nearby bins with an Epanechnikov kernel. Listed point estimates are estimated at the survey-response level, weighted using ACS sampling weights, with standard errors two-way clustered at the Commuting Zone and household levels in parentheses. Confidence intervals based on these standard errors are reported in Panels C and D. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.

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Table 1: Descriptive Statistics and Experimental Validity

	Sample Mean	RD Estimate
Panel A: Caseload Statistics	(1)	(2)
Caseload Density	87.460	−2.108
		(6.369)
Predicted Recidivism	0.580	0.003
		(0.003)
Panel B: JII Characteristics		
Male	0.835	0.001
		(0.006)
Age	29.650	−0.244*
		(0.144)
White	0.413	−0.006
		(0.011)
Black	0.429	0.002
		(0.009)
Hispanic	0.122	0.002
		(0.006)
Urban County of Conviction	0.845	−0.001
		(0.006)
Prior Misdemeanor Convictions	0.447	0.012
		(0.013)
Sentenced Incarceration Length (Months)	37.130	0.622
		(1.780)
Use/Possession Offense	0.418	−0.000
		(0.009)
Match to Partner/Co-Parent	0.408	0.001
		(0.006)
Match to Child	0.407	−0.004
		(0.008)
Number of Children	0.936	−0.012
		(0.023)
Panel C: Romantic Partner/Co-Parent Characteristics		
Female	0.834	0.003
		(0.012)
Age	32.010	0.091
		(0.214)
White	0.492	−0.001
		(0.016)
Black	0.349	−0.000
		(0.014)
Hispanic	0.106	0.004
		(0.007)
Any Criminal Charge Before Observed Relationship	0.235	−0.008
		(0.008)
Panel D: Child Characteristics		
Male	0.512	−0.007
		(0.011)
Age (in 2019)	12.620	−0.062
		(0.089)
Number of Households		58,000

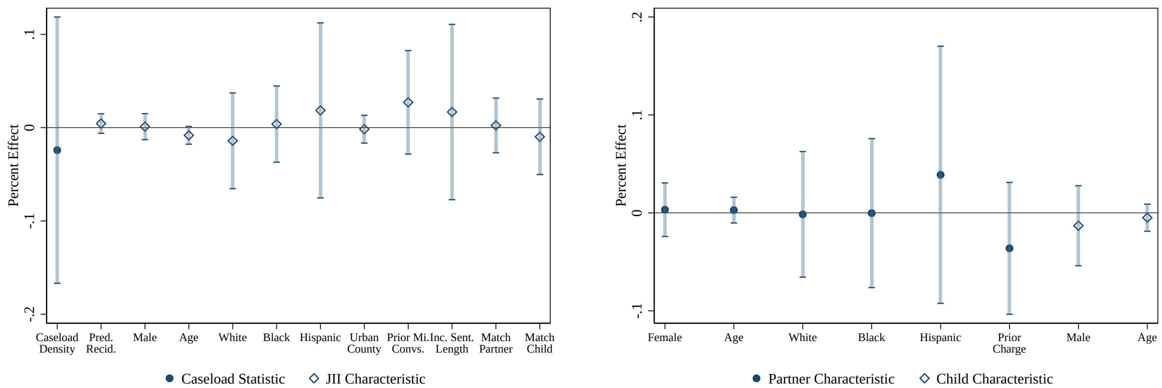
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, crosswalks from Finlay, Mueller-Smith, and Street (2023), and the 2022Q2 CJARS vintage.
Notes: Estimates and sample sizes have been rounded to preserve confidentiality. Column 1 reports sample means for the listed covariate in each row. Column 2 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. Robust standard errors are reported in parentheses for caseload density and are clustered at the Commuting Zone level otherwise. 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. * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
Approved under #CBDRB-FY23-CES014-020.

Appendix: 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 simple regression discontinuity design, testing whether the listed covariate changes discontinuously at the threshold. Coefficients are divided by the sample mean 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 (caseload density) or clustered at the Commuting Zone level. 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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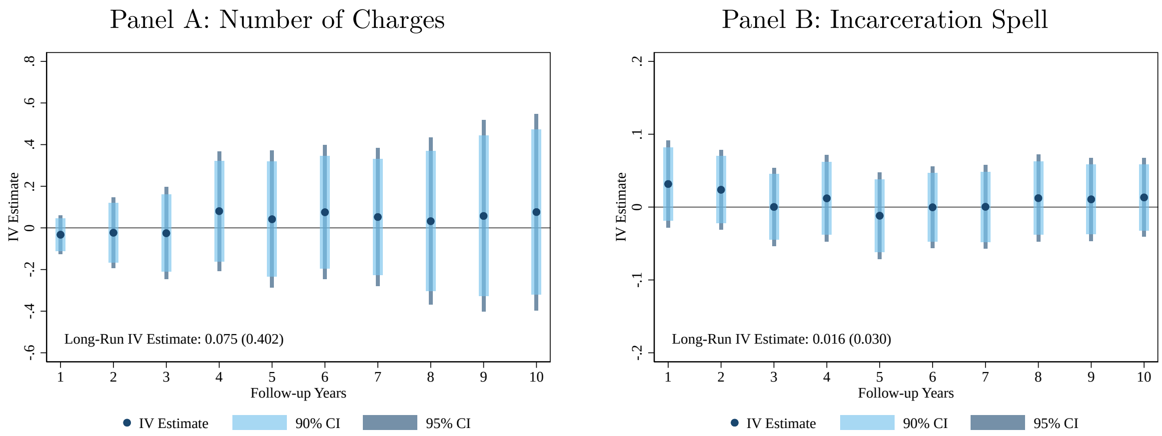
Appendix Figure 2: IV Estimates of Justice-Involved Individual Recidivism After Focal Event



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.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. 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 Commuting Zone level.

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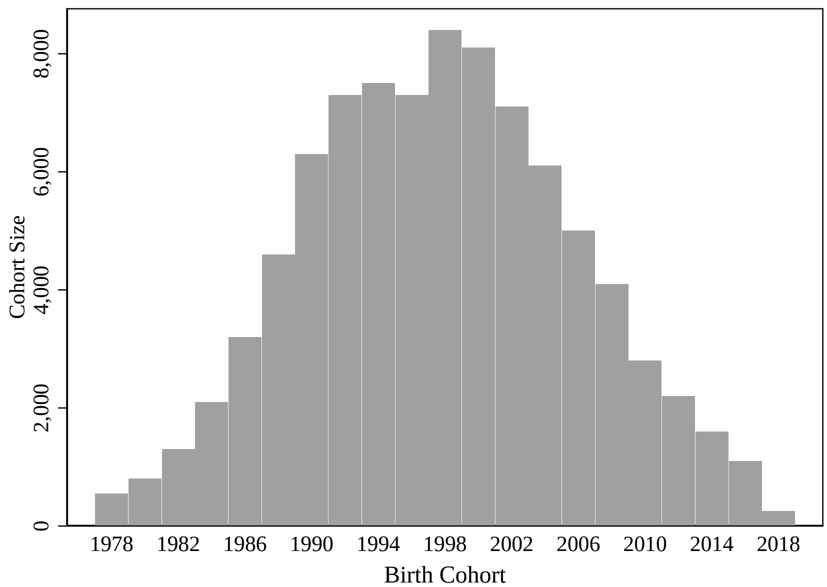
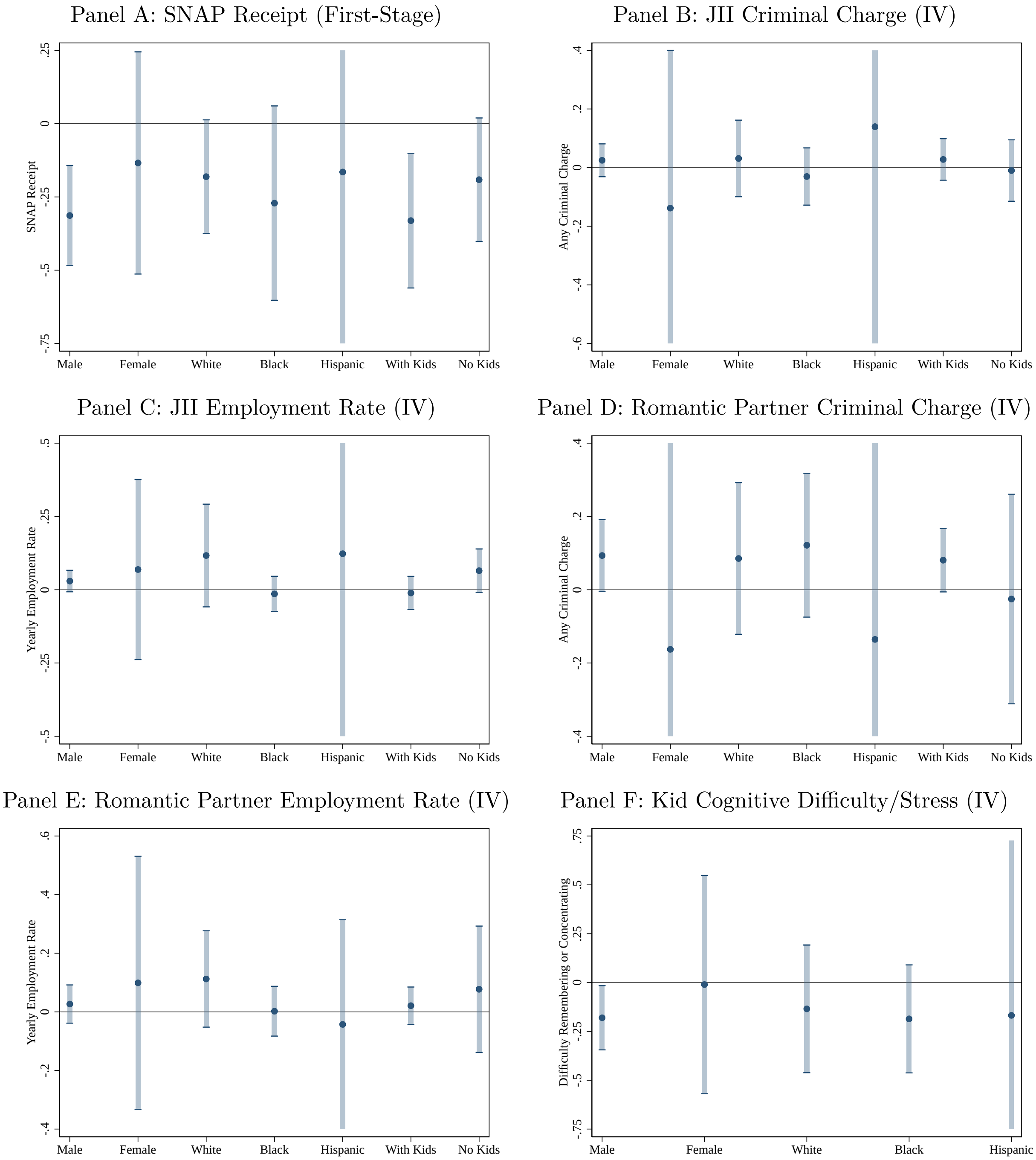
Appendix Figure 3: Distribution of Child Birth Cohorts



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.
Approved under #CBDRB-FY24-CES014-004.

Appendix Figure 4: Heterogeneity of Estimated 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. Panels B through F present fuzzy regression discontinuity estimates which scale the change in outcomes from (1) by the first-stage estimate specific to each subsample. The first-stage is estimated using the method described in Section 4. 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." 95 percent confidence intervals based on errors clustered at the Commuting Zone level for all specifications except those in Panels A and F which are two-way clustered at the Commuting Zone and household levels. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.

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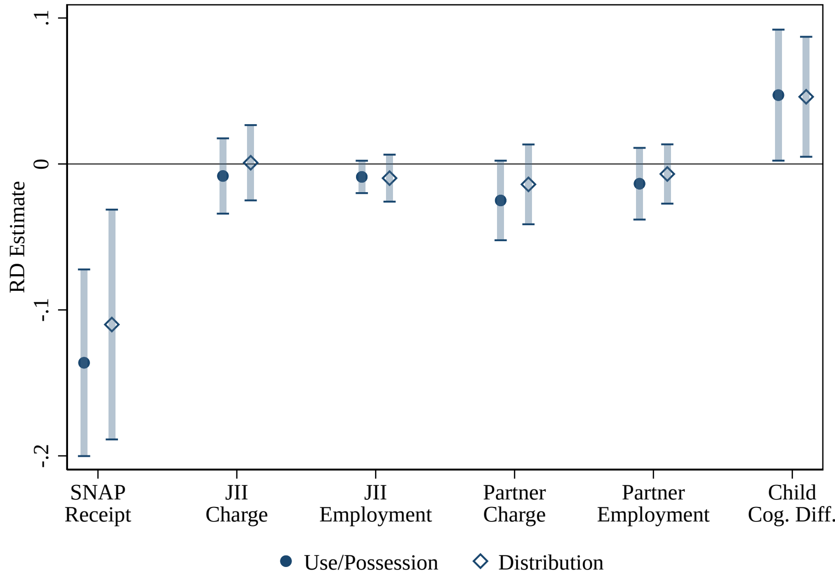
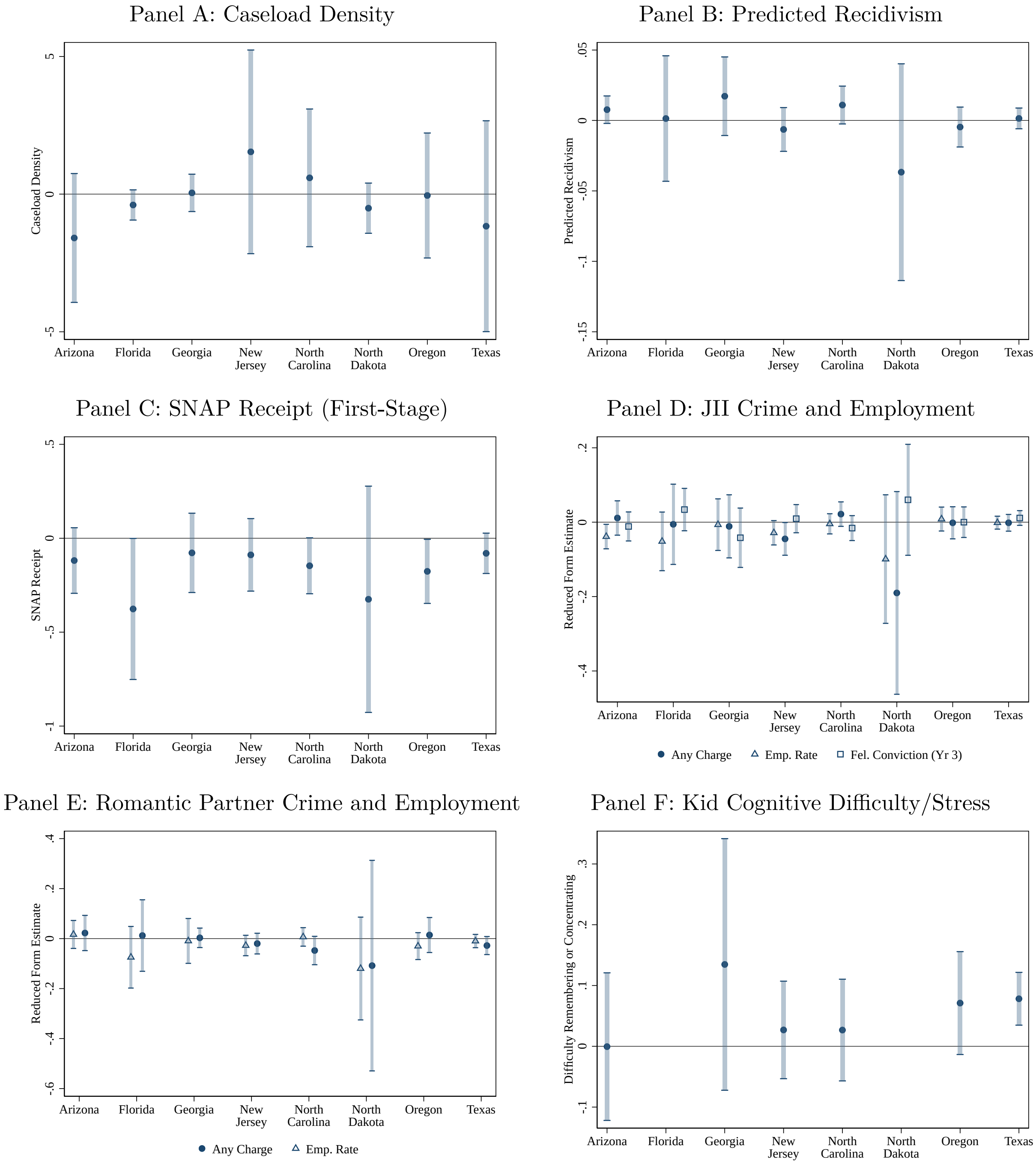
Appendix Figure 5: Heterogeneity of Reduced Form Effects by Disqualifying Conviction Type



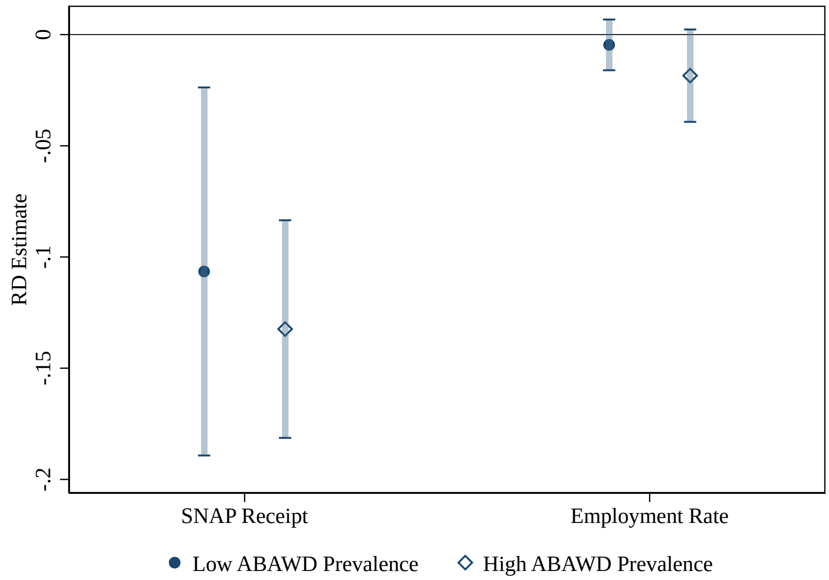
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from 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. Child cognitive difficulty is measured using the ACS question about individuals having "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition." 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. Survey outcomes 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 Commuting Zone level or two-way clustered at the Commuting Zone and household levels (SNAP Receipt and Child Cognitive Difficulty). Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY24-CES014-011.

Appendix Figure 6: Reduced Form Estimates of PRWORA Ban by State



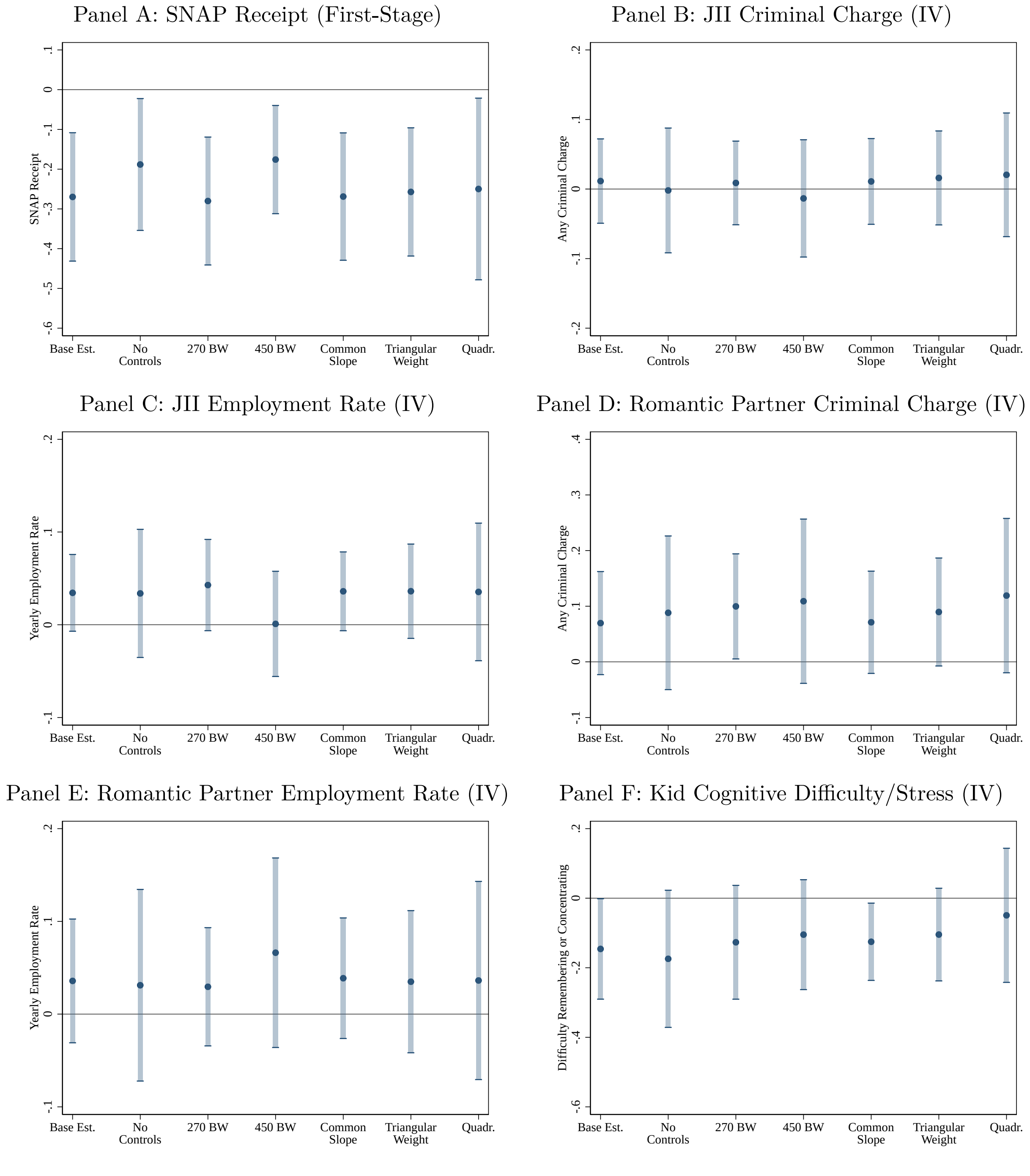
Source: Authors' calculations from the 2022Q2 CJARS vintage. Outcomes and demographic information come from the Census Numident, Census Best Race and Ethnicity file, IRS W-2s, 2005-2019 American Community Surveys, 1997-2019 Current Population Survey Annual Social and Economic Supplement, crosswalks from 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. Child cognitive difficulty is measured using the ACS question about individuals having "difficulty concentrating, remembering, or making decisions as a result of a physical, mental, or emotional condition." Panel F omits estimates for Florida and North Dakota due to small sample sizes. 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. Survey 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. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY24-CES014-011.

Appendix Figure 7: 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. 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. Survey outcomes additionally control for year fixed effects. 95 percent confidence intervals based on standard errors clustered at the Commuting Zone level or two-way clustered at the Commuting Zone and household levels (SNAP Receipt). Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.
Approved under #CBDRB-FY24-CES014-011.

Appendix Figure 8: 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 (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. 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." 95 percent confidence intervals based on errors clustered at the Commuting Zone level for all specifications except those in Panels A and F which are two-way clustered at the Commuting Zone and household levels. Bars bracketing confidence intervals indicate confidence intervals that are fully contained within the plot region.

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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: 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	-0.116^{***}	-0.065^{***}	-0.102^{***}	-0.104^{***}	-0.119^{***}
	(0.030)	(0.026)	(0.038)	(0.038)	(0.038)
Control Mean	0.365	0.423	0.369	0.326	0.445
Number of Observations	5,300	9,000	3,200	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. Standard errors two-way clustered at the Commuting Zone and 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 3: IV and Reduced Form Estimates of PRWORA Ban on JII and Partner Outcomes

	JII		Romantic Partner	
	Control Mean	Estimate	Control Mean	Estimate
Panel A: IV Estimates	(1)	(2)	(3)	(4)
Any Criminal Charge	0.656	0.011	0.261	0.070
		(0.031)		(0.047)
Employment Rate	0.422	0.034	0.745	0.036
		(0.021)		(0.034)
Above $5k in Earnings	0.330	0.037*	0.642	0.018
		(0.021)		(0.041)
Cognitive Difficulty/Stress	0.095	0.037	0.045	0.000
		(0.072)		(0.087)
Panel B: Reduced Form Estimates				
Any Criminal Charge	0.656	-0.003	0.261	-0.019*
		(0.008)		(0.010)
Employment Rate	0.422	-0.009	0.745	-0.010
		(0.006)		(0.008)
Above $5k in Earnings	0.330	-0.010*	0.642	-0.005
		(0.006)		(0.011)
Cognitive Difficulty/Stress	0.095	-0.010	0.045	-0.000
		(0.019)		(0.024)

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 instrumental variables estimates (Panel A) of receiving SNAP benefits on various outcomes for justice-involved individuals (Column 2) and romantic partners (Column 4). Corresponding reduced form estimates are presented in Panel B. The first-stage is estimated using the weighted sum described in Section 4. 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. 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 Commuting Zone level are reported in parentheses and two-way clustered at Commuting Zone and household-level for cognitive difficulty outcome. * = 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 PRWORA Ban on Additional JII and Partner Outcomes

	JII		Romantic Partner	
	Control Mean	Estimate	Control Mean	Estimate
Panel A: IV Estimates	(1)	(2)	(3)	(4)
Income-Generating Charge *(e.g., Larceny, Forgery/Fraud, Drug Dist., Comm. Vice)*	0.436	0.016	0.142	0.055
		(0.033)		(0.039)
Forgery/Fraud Charge, specifically	0.102	-0.020	0.047	0.013
		(0.021)		(0.020)
Non-Income-Generating Charge	0.591	0.015	0.220	0.035
		(0.028)		(0.036)
Drug Charge	0.428	-0.023	0.104	0.024
		(0.028)		(0.027)
Conviction	0.601	0.031	0.203	0.078*
		(0.030)		(0.044)
Felony Conviction	0.497	0.008	0.109	0.026
		(0.029)		(0.029)
Felony Conviction through Year 3	0.222	-0.006	–	–
		(0.025)		(–)
Income-Generating Conviction	0.376	0.029	0.103	0.053*
		(0.030)		(0.030)
Non-Income-Generating Conviction	0.523	0.044	0.164	0.053*
		(0.030)		(0.032)
Yearly W2 Earnings	10,570	554	19,780	-937
		(1,104)		(2,435)
Panel B: Reduced Form Estimates				
Income-Generating Charge *(e.g., Larceny, Forgery/Fraud, Drug Dist., Comm. Vice)*	0.436	-0.004	0.142	-0.015*
		(0.009)		(0.009)
Forgery/Fraud Charge	0.102	0.005	0.047	-0.004
		(0.005)		(0.005)
Non-Income-Generating Charge	0.591	-0.004	0.220	-0.010
		(0.007)		(0.009)
Drug Charge	0.428	0.006	0.104	-0.006
		(0.007)		(0.007)
Conviction	0.601	-0.008	0.203	-0.021**
		(0.008)		(0.009)
Felony Conviction	0.497	-0.002	0.109	-0.007
		(0.008)		(0.008)
Felony Conviction through Year 3	0.222	0.002	–	–
		(0.007)		(–)
Income-Generating Conviction	0.376	-0.008	0.103	-0.014**
		(0.008)		(0.006)
Non-Income-Generating Conviction	0.523	-0.012	0.164	-0.014*
		(0.007)		(0.008)
Yearly W2 Earnings	10,570	-150	19,780	253
		(305)		(657)

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 instrumental variables estimates (Panel A) of receiving SNAP benefits on various outcomes for justice-involved individuals (Column 2) and romantic partners (Column 4). Corresponding reduced form estimates are presented in Panel B. The first-stage is estimated using the weighted sum described in Section 4. 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. Control means in Columns 1 and 3 include observations within 75 days to the left of the discontinuity. Standard errors clustered at the Commuting Zone 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: Heterogeneous Effects of Main IV and Reduced Form Estimates

	Base Estimate	Male	Female	White	Black	Hispanic	With Kids	No Kids
Panel A: First-Stage	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SNAP Receipt	−0.270***	−0.313***	−0.134	−0.181*	−0.271	−0.165	−0.331***	−0.191*
	(0.082)	(0.087)	(0.193)	(0.099)	(0.169)	(0.703)	(0.117)	(0.107)
SNAP Receipt - Annual Average	−0.116***	−0.138***	−0.045	−0.129***	−0.079	−0.165	−0.108***	−0.112***
	(0.030)	(0.038)	(0.081)	(0.043)	(0.050)	(0.209)	(0.043)	(0.043)
IV Estimates								
Panel B: JII								
Any Criminal Charge	0.011	0.025	−0.138	0.031	−0.030	0.140	0.028	−0.010
	(0.031)	(0.029)	(0.234)	(0.067)	(0.050)	(0.510)	(0.036)	(0.053)
Employment Rate	0.034	0.030	0.069	0.117	−0.014	0.123	−0.011	0.065*
	(0.021)	(0.019)	(0.157)	(0.089)	(0.031)	(0.510)	(0.029)	(0.038)
Panel C: Romantic Partners								
Any Criminal Charge	0.070	0.093*	−0.163	0.085	0.121	−0.136	0.081*	−0.025
	(0.047)	(0.050)	(0.362)	(0.106)	(0.100)	(0.536)	(0.044)	(0.146)
Employment Rate	0.036	0.027	0.099	0.112	0.002	−0.043	0.021	0.077
	(0.034)	(0.033)	(0.220)	(0.084)	(0.043)	(0.182)	(0.033)	(0.110)
Panel D: Children								
Cognitive Difficulty/Stress	−0.146**	−0.180**	−0.010	−0.135	−0.186	−0.168	–	–
	(0.074)	(0.084)	(0.285)	(0.167)	(0.141)	(0.457)	(–)	(–)
Reduced Form Estimates								
Panel E: JII								
Any Criminal Charge	−0.003	−0.008	0.018	−0.006	0.008	−0.023	−0.009	0.002
	(0.008)	(0.008)	(0.017)	(0.011)	(0.012)	(0.021)	(0.011)	(0.010)
Employment Rate	−0.009	−0.009	−0.009	−0.021**	0.004	−0.020	0.004	−0.012*
	(0.006)	(0.006)	(0.013)	(0.010)	(0.009)	(0.016)	(0.009)	(0.007)
Panel F: Romantic Partners								
Any Criminal Charge	−0.019*	−0.029***	0.022	−0.015	−0.033**	0.022	−0.027***	0.005
	(0.010)	(0.011)	(0.039)	(0.015)	(0.015)	(0.028)	(0.011)	(0.028)
Employment Rate	−0.010	−0.008	−0.013	−0.020*	−0.001	0.007	−0.007	−0.015
	(0.008)	(0.010)	(0.020)	(0.011)	(0.012)	(0.028)	(0.009)	(0.020)
Panel G: Children								
Cognitive Difficulty/Stress	0.048***	0.055***	0.002	0.030	0.047*	0.046	–	–
	(0.015)	(0.014)	(0.047)	(0.028)	(0.025)	(0.031)	(–)	(–)

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 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. Child regressions 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." Standard errors clustered at the Commuting Zone level or two-way clustered at the Commuting Zone and household level (Panels A, D, and G) 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 6: Reduced Form Estimates of Additional Measures of Child and Household Program Participation

	Medicaid		HUD		Other Assistance	
	All Kids	Young Kids	All Kids	Young Kids	HHs w/ Kids	HHs w/ Young Kids
	(1)	(2)	(3)	(4)	(5)	(6)
Banned from SNAP	0.001	−0.001	−0.004	−0.011*	0.022	0.035**
	(0.005)	(0.007)	(0.004)	(0.006)	(0.016)	(0.017)
Control Mean	0.625	0.706	0.164	0.179	0.038	0.038
Number of Observations	87,500	54,000	87,500	54,000	5,700	4,700

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: 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. Young kids include the 1996 and later birth cohorts. 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 two-way clustered at the Commuting Zone and household levels 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	−0.270***	−0.188**	−0.280***	−0.176***	−0.269***	−0.257***	−0.250**
	(0.082)	(0.085)	(0.082)	(0.069)	(0.082)	(0.082)	(0.117)
SNAP Receipt - Annual Average	−0.116***	−0.099***	−0.118***	−0.080***	−0.114***	−0.115***	−0.114***
	(0.030)	(0.030)	(0.033)	(0.030)	(0.030)	(0.032)	(0.044)
IV Estimates							
Panel B: JII							
Any Criminal Charge	0.011	−0.002	0.009	−0.013	0.011	0.016	0.020
	(0.031)	(0.046)	(0.031)	(0.043)	(0.031)	(0.034)	(0.045)
Employment Rate	0.034	0.034	0.043*	0.001	0.036*	0.036	0.035
	(0.021)	(0.035)	(0.025)	(0.029)	(0.022)	(0.026)	(0.038)
Panel C: Romantic Partners							
Any Criminal Charge	0.070	0.088	0.100**	0.109	0.071	0.090*	0.119*
	(0.047)	(0.070)	(0.048)	(0.075)	(0.047)	(0.049)	(0.071)
Employment Rate	0.036	0.031	0.029	0.066	0.039	0.035	0.036
	(0.034)	(0.053)	(0.033)	(0.052)	(0.033)	(0.039)	(0.055)
Panel D: Children							
Cognitive Difficulty/Stress	−0.146**	−0.174*	−0.127	−0.105	−0.125**	−0.105	−0.049
	(0.074)	(0.101)	(0.083)	(0.081)	(0.057)	(0.068)	(0.098)
Reduced Form Estimates							
Panel E: JII							
Any Criminal Charge	−0.003	0.000	−0.002	0.002	−0.003	−0.004	−0.005
	(0.008)	(0.009)	(0.008)	(0.007)	(0.008)	(0.009)	(0.011)
Employment Rate	−0.009	−0.006	−0.012*	−0.000	−0.010*	−0.009	−0.009
	(0.006)	(0.006)	(0.007)	(0.005)	(0.006)	(0.007)	(0.009)
Panel F: Romantic Partners							
Any Criminal Charge	−0.019*	−0.017*	−0.028***	−0.019**	−0.019*	−0.023**	−0.030*
	(0.010)	(0.010)	(0.010)	(0.009)	(0.010)	(0.011)	(0.015)
Employment Rate	−0.010	−0.006	−0.008	−0.012*	−0.010	−0.009	−0.009
	(0.008)	(0.009)	(0.008)	(0.006)	(0.008)	(0.009)	(0.013)
Panel G: Children							
Cognitive Difficulty/Stress	0.048***	0.046***	0.038**	0.025*	0.047***	0.033*	0.014
	(0.015)	(0.015)	(0.019)	(0.014)	(0.015)	(0.018)	(0.027)

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 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. Child regressions 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." Standard errors clustered at the Commuting Zone level or two-way clustered at the Commuting Zone and household level (Panels A, D, and G) 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

			Alternative Cutoff Dates					
	Main Estimate	Non-Drug Felonies	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	-0.116^{***}	0.016	-0.004	0.039	0.015	0.012	0.009	-0.054^{*}
	(0.030)	(0.017)	(0.030)	(0.036)	(0.033)	(0.027)	(0.027)	(0.029)
Panel B: JII Outcomes								
Any Charge	-0.003	-0.000	0.002	-0.011	0.003	0.011	-0.009	0.007
	(0.008)	(0.004)	(0.007)	(0.007)	(0.010)	(0.010)	(0.010)	(0.007)
Employment Rate	-0.009	-0.000	-0.014^{***}	0.016^{***}	-0.006	0.009^{*}	-0.004	-0.008
	(0.006)	(0.003)	(0.005)	(0.007)	(0.007)	(0.005)	(0.005)	(0.006)
Panel C: Partner Outcomes								
Any Charge	-0.019^{*}	-0.004	-0.002	0.000	0.013	0.011	-0.015	0.023^{*}
	(0.010)	(0.006)	(0.009)	(0.011)	(0.012)	(0.011)	(0.010)	(0.013)
Employment Rate	-0.010	0.003	0.013	0.019^{***}	-0.002	0.006	0.004	0.004
	(0.008)	(0.005)	(0.009)	(0.008)	(0.009)	(0.007)	(0.009)	(0.006)
Panel D: Child Outcomes								
Cognitive Difficulty/Stress	0.048^{***}	0.013	0.017	-0.027^{*}	-0.019	-0.020	-0.014	0.017
	(0.015)	(0.010)	(0.017)	(0.015)	(0.015)	(0.015)	(0.014)	(0.018)

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. Child regressions 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." Standard errors clustered at the Commuting Zone level or two-way clustered at the Commuting Zone and household level (Panels A and D) are reported in parentheses * = significant at 10 percent level, ** = significant at 5 percent level, *** = significant at 1 percent level.
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