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Safety Net Access and Reintegration: Evidence from California's Felony Drug Ban Repeal

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Abstract

Following incarceration, individuals face substantial systemic barriers to successful reintegration, including limited access to the social safety net. This study evaluates California's 2015 repeal of lifetime bans on SNAP and TANF eligibility for people with felony drug convictions. Using linked administrative dataset from the California Department of Corrections and Rehabilitation and the California Department of Social Services (2010–2020), we use a difference-in-differences design comparing outcomes for individuals convicted of drug felonies (newly eligible) to those convicted of non-drug felonies (always eligible) asking two questions: did the ban repeal increase benefit take-up, and did increased access subsequently influence future contact with the criminal justice system? We find the repeal substantially increased benefit take-up, raising SNAP enrollment within one year of release from 17% to 51% for people with drug felonies, which closed nearly all of the gap with the comparison group. Net of a smaller rise in the comparison group, the repeal added 22.5 percentage points. TANF enrollment rose by 1.9 percentage points, a small increase that partly reflects earlier trends. We find no detectable effect of the policy on overall returns to prison, whether we compare the groups directly or use the policy as an instrument for benefit receipt. The direct estimate rules out a decline larger than about 3 percent. Returns to prison for serious and violent felonies are very rare for both groups. They fell for both groups after the repeal, but fell less for people with drug felonies, in part because their rate was already so low. This gap is statistically significant and robust to multiple estimation approaches, but the counts are very small and its size varies across models. Together, the findings show that lifting the ban substantially expanded safety net access for a vulnerable population but did not produce a broad reduction in recidivism – suggesting that removing eligibility barriers is necessary but not sufficient to reduce reincarceration risk.

Introduction

Following incarceration, individuals face systemic barriers to successful reintegration into society (Chesney-Lind & Mauer, 2003; Kirk & Wakefield, 2018). While securing employment and housing are foundational to the conditions of parole and longer-term stability, formerly incarcerated individuals are less likely to be employed or have stable housing than the general population. This study evaluates a policy change designed to strengthen social safety net

support available to individuals with felony records, thereby facilitating their transition back into the community.

Specifically, we evaluate the impact of California's 2015 legislative repeal of lifetime bans on two important social welfare programs—SNAP (CalFresh, or food stamps) and TANF (CalWORKs) – for people with felony drug convictions. These programs provide essential assistance for low-income Californians; however California was previously one of several states that imposed a full ban on cash assistance and a partial ban on food assistance for individuals convicted of felony drug offenses. The consequences of this policy change remain under-examined. California is one of at least 18 states that have lifted or modified the ban on eligibility for public assistance since 2015, but research on the impact of these changes is limited. Preliminary evidence suggests that eligibility for welfare (TANF) or full eligibility for food stamps (SNAP) can reduce recidivism by about 10 to 13 percent within one year (Yang 2017a). While the detrimental effects of benefits bans are well-documented (Morgan et al., 2022), there is a pressing need for empirical analysis in states that have opted for restoration. This study estimates the effects of California's policy change on individual reintegration by analyzing whether the repeal increased benefit take-up and subsequently influenced future contact with the criminal justice system. Earlier work finds that losing benefits raises recidivism; we also ask whether regaining them does the reverse. Our findings assess whether barriers to benefit take-up persist in California, despite the repeal of the ban, and offer broader implications for national discourse on public assistance bans.

Literature review

Successful reintegration into society following a period of incarceration is influenced by several factors including connections to family, stable housing, employment opportunities or

access to financial support while looking for employment, and good physical and mental health (Nahra, Knight & Western 2025). Social ties to family provide emotional and financial support during the reentry process (Harding et al. 2019; Western 2018) and can generate connections to potential employers (Sullivan 1989) and housing (Sirois 2019).

Housing following release can be very unstable (Geller and Curtis 2011): rates of shelter use are highest in the first month following prison release (Metraux and Culhane 2004) and newly released people often move between multiple addresses within the first year (Sirois 2019). As a result, formerly incarcerated people are more likely to experience homelessness than the general population (Metraux and Culhane 2004).

Formerly incarcerated individuals are more likely to have chronic health conditions, substance use disorders, and serious mental illness (Binswanger et al. 2009, Favril et al. 2024, Massoglia & Remster 2019), and these conditions can lead to high rates of hospitalization and mortality upon release (Wang et al. 2013). Poor physical and mental health can inhibit employment (Mallik-Kane and Visher 2008) and make finding stable housing more challenging. Most acutely, death rates and overdose mortality rates are highest for the formerly incarcerated particularly in the first weeks after release (Binswanger et al. 2007, Brinkley-Rubinstein et al. 2019).

Employment is one of the most powerful predictors of recidivism, in addition to age and education level (Nally et al. 2014). Specifically, in Ohio, newly released individuals with stable employment have much lower recidivism rates compared to people who were marginally employed (Kolbeck 2023). Connections to the workforce following incarceration are generally weak due to low educational attainment, health challenges, and interruptions in work history and personal networks (Western et al. 2001, Harding et al. 2019, Smith 2007). Further, the range of

employment options may be narrower due to legal limitations on the types of work people can do and the stigma associated with a criminal record (Pager 2003, Harding et al. 2019). For those who find employment, higher wages for low-skilled workers, and openings in better-paying sectors such as construction and manufacturing, are associated with lower recidivism (Yang 2017b; Schnepel 2018), with many reentering individuals having difficulty retaining employment (Western 2018). Without stable and sufficient employment, reentering individuals are more likely to experience poverty (Western 2018).

The Role of Safety Net Programs in Reentry

Given the factors that promote successful reentry and the challenges faced by formerly incarcerated individuals in securing them, government programs play an important role in the reentry process. Research has documented the benefits of public welfare programs on recidivism. Here we summarize findings on Medicaid, SSI, welfare, and SNAP.

Several studies leverage the expansion of Medicaid through the ACA to estimate effects on crime and recidivism. In Wisconsin, Medicaid expansion and the launch of a new pre-release enrollment program resulted in a 60 percentage point increase in Medicaid coverage among newly released people (Burns & Dague 2023). Conversely, the unexpected loss of Medicaid coverage in Tennessee in 2005 resulted in an increase in total crime rates and overdose rates in counties that were more affected relative to those that were less affected (Deza et al. 2025). Further, terminating Medicaid eligibility for those aged 19 increases the likelihood of subsequent incarceration in the following two years, with particularly strong findings for those with prior mental health diagnoses (Jacome 2022). Effects of Medicaid access on recidivism may be driven in part by expanded access to treatment for substance use disorder (SUD) among a population with common SUD diagnoses (He & Barkowski, 2020; Vogler, 2020; Wen et al. 2017).

Recent research suggests that the initial passage of legal welfare restrictions, generating a loss of cash assistance from SSI, welfare (or TANF), and food stamps (or SNAP), is causally linked to increased recidivism (Tuttle, 2019; Yang, 2017a). Young people who lost SSI benefits following welfare reform in 1996 experienced a 20% increase in criminal involvement over the next 20 years, primarily driven by an increase in income-generating crimes (Deshpande & Mueller-Smith 2022). The loss of eligibility for SNAP after the 1996 welfare reform reveals more nuanced effects on recidivism. A study leveraging state-level variation in implementation found no aggregate effect of the initial passage of the drug felony ban on food stamps on recidivism among those convicted of drug offenses (Luallen, Edgerton, and Rabideau 2018). Another found that strict SNAP bans for those convicted of drug trafficking increased the probability of a return to prison by 9.5 percentage points, an effect driven entirely by financially motivated crimes such as theft and drug distribution (Tuttle 2019). Using California administrative arrest records from the state's 1996–1998 implementation of the same felony drug ban studied here, a third analysis found no measurable effect of the TANF ban on rearrest, but found that the SNAP ban hastened time to arrest, with effects concentrated in counties with more accessible welfare policies and more generous benefits (Sugie and Newark 2023). However, none of these studies measured individual benefit receipt, leaving the question of whether bans actually reduced benefits. While Mueller-Smith et al. (2023) confirmed that the 1996 reform reduced benefit receipt by 38%, they found limited evidence of a direct recidivism response for the individuals themselves, though the loss of support created significant economic hardships for their families.

However, there appears to be an asymmetry in the effects of benefit loss compared to benefit restoration. Yang (2017a) found that full eligibility for food stamps—without

restrictions—significantly reduced one-year recidivism for drug offenders (a 13.1% decrease), using variation from both the 1996 ban and later state opt-outs, so her estimate does not isolate restoration. However this analysis does not evaluate the effect of changes in eligibility on actual participation in SNAP. This asymmetry may be due to the fact that benefit losses often occur abruptly and have the largest effects on people with few economic options (Morash et al., 2017; Burns & Dague, 2023; Tuttle, 2019). In contrast, benefit gains can improve stability and reduce recidivism. Medicaid expansion, for example, lowered returns among people with repeat violent or public-order offenses but not property or drug offenses (Aslim et al., 2022). Effects may also be spread over time and partly offset by bureaucratic hurdles to enrollment and limited program generosity (Golembeski et al., 2020; Bryan, 2023).

Formerly incarcerated people have great need for public assistance during reentry, but face significant structural and geographic barriers to access (Harding et al., 2014). The Boston Reentry Study (Western et al., 2015), which tracked 122 individuals during their first year post-release, found that two-thirds of the sample relied on public assistance programs like SNAP for basic subsistence, underscoring the vital role of the safety net in preventing immediate destitution. Because many formerly incarcerated individuals have interrupted or limited work histories, they often qualify for more support through means-tested programs like SSI and SNAP than through social insurance programs like Unemployment Insurance (Bryan, 2023). However, the effectiveness of these benefits is often dictated by physical accessibility; research in California indicates that the spatial proximity of social service providers is a critical factor in success. Specifically, when agencies are located within a short distance (e.g., two miles), the likelihood of recidivism is substantially reduced—an effect that is particularly pronounced for

minority formerly incarcerated individuals who may face greater transportation and neighborhood-level resource hurdles (Hipp, Petersilia, & Turner, 2010).

Policy context

In 1996, Congress passed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA)—often referred to as the "federal welfare reform bill." One component of this federal law was the prohibition of individuals who were convicted of certain felony drug offenses from receiving TANF and SNAP benefits. States were allowed to opt out partially or entirely from the provisions of the automatic aid disqualification through the enactment of state legislation. As a result, in 2012, at least 20,000 individuals were ineligible for SNAP (CalFresh) due to having a drug-related conviction. California partly opted out of the SNAP ban in 2005, so before 2015 people convicted of possession or use could receive CalFresh if they showed they were in or had finished drug treatment, while people convicted of selling, transporting, possessing for sale, or manufacturing drugs were excluded (Shah et al., 2025). California had not opted out of the TANF ban, which covered all drug felonies (California Assembly Bill 1468, 2014).

In California, state criminal justice policy was shifting following AB 109 (Realignment) in 2011, which moved most people convicted of non-violent, non-serious, and non-sexual offenses from state prison to county jails and probation departments to serve their sentences. In the ensuing years, it became clear to legislators that more needed to be done to ensure that people could successfully reenter the community. AB 1468, the "Public Safety Omnibus Bill," achieved several criminal justice reforms with reentry at its core, but the most significant change was the repeal of the lifetime ban on public assistance for people convicted of drug felonies.

After the bill's passage, beginning in April 2015, Californians who were convicted of felony drug possession, use, or distribution were no longer disqualified from receiving TANF and SNAP benefits. AB 1468 ended the TANF ban and the remaining SNAP exclusions, and dropped the SNAP treatment-proof rule. Eligibility still depends on following the terms of probation or parole. Yet little is known about whether the ban removal changed benefit application or take-up among this population, and if so, whether benefit receipt improved desistance from future criminal activity. This project examines the impact of reversing this safety net ban on those affected (individuals incarcerated for felony drug convictions), using California as a natural experiment. AB 1468 creates a sharp change in the eligibility of re-entering individuals for basic needs support. This study examines whether the affected population responded to this legal change by enrolling in TANF and/or SNAP, and whether any changes in uptake for these social programs are tied to reductions in future interactions with the criminal justice system.

The removal of California's public assistance exclusion applied to only a subset of the incarcerated population — the subset of individuals incarcerated for felony drug offenses and who were either released from California correctional facilities after April 2015 or already in the community at the time of the policy change. Because the focus of this project is on the impact of AB 1468 on prisoner reentry, we focus on program uptake and recidivism within a year after release, a crucial period during which re-offense risk is acute. The treatment group includes individuals incarcerated for drug felonies, who become newly eligible for public assistance after April 1, 2015. The comparison group consists of formerly incarcerated individuals who were convicted of felony offenses that were never subject to the ban.

Data and sample

This study utilizes individual-level data provided by two state-level administrative agencies. First, the California Department of Corrections and Rehabilitation (CDCR) holds information on all individuals incarcerated in state prison in California between 2000-2020, and includes details on conviction charges, institutional experiences, and subsequent incarcerations. The data from CDCR includes event-level criminal history with individual identifiers (hashed to protect individual privacy). These data allow for the examination of subsequent reentry into the state prison system. Although returns to prison provide a rigorous metric of re-incarceration, they represent a conservative measure of recidivism. This indicator fails to capture broader criminal justice contact, such as arrests or convictions that do not culminate in a state prison commitment. These lower level contacts with the criminal justice system are more frequent and may be more likely to be affected by the policy change, but unfortunately they are not observable in the current study. Another limitation of the recidivism measure used here is the one year follow up window. Most cases that result in a return to prison take a long time to resolve, therefore the one-year return to prison measure may not capture the bulk of recidivism if it takes longer than one year following release to commit a new crime and have that new case work through the court system. While these limitations apply equally to our treatment and comparison groups, they limit our ability to make broader statements about the impact of the policy change on criminal justice contact more broadly.

The CDCR data are merged to individual-level safety net program enrollment data collected by the California Department of Social Services (CDSS). CDSS maintains monthly enrollment information on state safety net programs including SNAP and TANF, allowing observation of participation in both programs over time. We observe CDSS records from January

2010 through February 2020. Along with enrollment indicators, these panel data contain basic demographic information about each recipient, including their date of birth, race, language, and sex.

The resulting linked administrative dataset captures demographic, criminal history, incarceration, and public assistance outcomes for a substantial subset of the American population — all individuals with a prison commitment to state prison in California between 2010 and 2020. The pre-period is defined by records between 2010-March 2015, prior to the repeal of California's ban, and the post-period from April 2015-2020 allows formerly incarcerated individuals to enroll in benefits following the reinstatement.

While the policy change affected all individuals with felony drug convictions, this study focuses specifically on those reentering society following prison terms. The reasons are twofold: first, the period of reintegration from incarceration is a critical window of vulnerability when any additional support may help individuals find the stability they need to comply with parole requirements and avoid return to custody. As a result, we might expect the marginal impact of SNAP or TANF to be greater for this population. Second, in the linked dataset we only observe individuals who serve state prison terms. At a minimum, this allows us to estimate the total number of Californians affected by the policy change and determine the proportion of the population represented within the CDCR data.

Trends in prison releases in California over the past 10 years are an important context for this study. Mirroring admissions trends, since 2010 the number of people released from California prisons has declined by 41%, from 64,694 people released in 2010, to 38,378 people released in 2019 (Figure 1). This decline is largely driven by AB 109 (Public Safety Realignment), which went into effect in October 2011 and reduced the number of people

returned to state prison for parole violations to nearly zero. As the prison population decreased, the share of people serving time for serious or violent felonies nearly doubled, from 24% in 2010 to 47% in 2019 (Figure 1).

Between 2010 and 2020, 438,610 individuals were released from CDCR after serving sentences for felony convictions (Table 1). People are released at an average age of 36 years old. Original sentences were 2.7 years long, on average, and people had an average of 2 prior prison admissions. Nearly 40% were serving time for a serious or violent felony, per the California penal code. The overall release population is 42% Hispanic or Latino, 27% White, 25% Black, 1.4% Asian or Pacific Islander, and 1% American Indian or Alaskan Native. The treatment and comparison groups had fairly similar distributions of time served (Figure 2) — with the majority of released individuals in both groups serving less than two years in prison.

The composition of the treatment group — people convicted of drug felonies — is fairly consistent in the pre- and post-policy periods, though it reflects the overall trends in prison releases discussed above. Specifically, 38,771 people with felony drug convictions were released in the pre-period, and only 16,595 people were released in the post period (Table 1). These cohorts differ in the share serving time for serious and violent felonies - 9.3% had serious and violent convictions in the pre-period, compared to 19.2% in the post-period. These compositional changes to the prison population are reflected in the comparison group as well.

We also provide insight into pre-incarceration benefits receipt rates. Our benefits records begin partway through the study period, so we can see pre-prison SNAP receipt only for people whose records cover their time before prison. That is 29% of treatment group releases in the pre-period, rising to 90% in the post-period (42% and 88% for the comparison group). Among those people, 19.5% of the treatment group and 32.6% of the comparison group received SNAP

before prison in the pre-period, and 29.3% and 38.2% in the post-period. Rates are lower for the treatment group in both periods. These descriptive statistics identify a large number of previously incarcerated individuals who are former or current benefits participants.

Research design

This project assesses the role of access to safety net benefits in supporting successful reintegration. To do so, we answer two research questions. First, did the restoration of public assistance eligibility through California AB 1468 increase the use of SNAP and TANF by individuals convicted of felony drug crimes? To answer this question, we observe SNAP and TANF enrollment rates among the formerly incarcerated population before and after the policy change. If the policy change resulted in an increase in uptake of benefits among individuals who were previously ineligible, we will leverage that change to answer the second research question: Does access to the social safety net among individuals with felony drug convictions reduce rates of future crime?

To estimate the impact of restoring public assistance benefits access on formerly incarcerated individuals, we use a difference-in-differences (DiD) research design. The approach controls for unobserved coincident shocks that may also have impacted public assistance take-up or recidivism, but were not the result of the policy change. We proceed in two main parts that correspond to our main research questions: (1) testing whether California AB 1468 increased the use of SNAP and TANF by formerly incarcerated individuals, and (2) to the extent that it did, was the law effective in reducing future incarceration.

To estimate the impact of AB 1468 — which restored public assistance benefits for individuals who were formerly incarcerated for drug felonies — we compare public assistance uptake and recidivism outcomes between our treated (drug felony) and untreated (non-drug

felony) before and after AB 1468 comes into effect. We employ a difference-in-differences (DiD) statistical method that is common in the literature examining the effects of state-level policies on crime and public assistance access (see, e.g., Vogler 2020). This approach allows the comparison of outcomes between individuals convicted of drug felonies and those convicted of non-drug felonies (the first difference), before and after the law takes effect in April 2015 (the second difference). Two groups of formerly incarcerated individuals will comprise the main analysis dataset: the "treatment group" will include those individuals convicted of drug felonies, incarcerated in state prison and who were released after April 2015. The "comparison group" will consist of individuals convicted of felony offenses not subject to the ban. The DiD approach controls for unobserved coincident shocks that may also have impacted public assistance take-up or recidivism, but were not the result of California's policy change.

Difference-in-differences models

First, we assess whether reentering individuals are able to enroll in social safety net resources in California after the full repeal of the public assistance ban via AB 1468. Specifically, we estimate a relatively simple linear probability model specification:

$$PA_{ict} = \beta_0 + \beta_1 Drug_i + \beta_2 Post_t + \beta_3 (Drug_i \times Post_t) + \gamma X_{ict} + \theta_c + \delta_{y(t)} + \lambda_{m(t)} + \varepsilon_{ict} \quad (1)$$

In this model, our primary outcome PA is an indicator for whether a formerly incarcerated individual became enrolled in the SNAP and/or TANF programs up to one year after release, as we are focused on how access to safety net resources affects the likelihood of successful reentry. $Drug$ indicates whether a person had been convicted of a drug felony that would lead to a ban from public assistance prior to 2015. As described above, this design leveraged two sources of comparison to understand the effect of California's policy change that

was designed to affect reentry through public assistance. In addition to comparing benefit use to individuals serving incarceration sentences for drug felonies and released prior to 2015, the approach leverages an additional source of comparison: incarcerated individuals convicted of non-drug felonies (who were thus always eligible for benefits upon release). The model adjusts for differences in the demographic and offense characteristics of the booked population (X_{ict}), and includes release county, year, and month fixed effects to adjust for seasonal and geographic differences. Models also include a control for individuals who are partially exposed to the policy change during the year following their release (as shown in the shaded band of Figure 4). Standard errors are clustered by release county. A necessary assumption for β_3 to identify the causal effect of AB 1468 is that in absence of this statute, trends in public assistance usage would be the same for individuals who were formerly incarcerated for either assistance-disqualifying drug felonies or other felonies. We examine the validity of the assumption by estimating the difference in treatment and control groups year-by-year (i.e., an event study design).

In the second part of the analysis, we assess our main question, which is whether this access to the social safety net eases the process of reintegration using the same empirical framework. The primary outcomes are subsequent returns to prison and the types of offenses associated with subsequent incarceration. Admittedly, the usefulness of testing this hypothesis depends in part on the extent to which reintegrated individuals successfully take up public assistance; we thus treat Equation (1) as a "first stage" regression. Conditional on the first stage effect on welfare take-up being met, we test whether SNAP or TANF access affect future criminal behavior. The second part of the project takes the following form:

$$Y_{ict} = \beta_0 + \beta_1 Drug_i + \beta_2 Post_t + \beta_3 (Drug_i \times Post_t) + \gamma X_{ict} + \theta_c + \delta_{y(t)} + \lambda_{m(t)} + \varepsilon_{ict} \quad (2)$$

In this regression, the outcome Y_{it} is subsequent returns to prison for any new conviction and for a new serious and violent conviction. In addition to those included in equation 1, these models also control for time served. Standard errors are clustered by release county.

We observe data on nearly all individuals incarcerated for felony offenses and benefit enrollment in California between 2000 and 2020. This allows for examination of public assistance enrollment and recidivism outcomes for individuals incarcerated for both non-drug offenses (always eligible for public assistance) and felony drug offenses (only eligible after AB 1468) for several years prior to and after the law was passed. We rely on a pre-period of January 2010-March 2015, a period that prevents confounding effects of AB1468 with other policy changes or changes in economic conditions. We use April 2015-February 2020 as the post period, which allows for "treated" incarcerated individuals to enroll in public assistance programs upon reentry. For models estimating returns to prison, we subset the sample to people who are released at least one year prior to February 2020, when our CDCR data end.

Instrumental Variable/LATE

Pending the identification of a first stage effect of the policy change on benefit take-up, we leverage new eligibility for the benefits (or the interaction term between *Drug* and *Post*) as an instrument for benefit receipt in a two-stage-least squares estimator. As noted above, Equation (1) serves as the first stage. We retain its predicted values of *PublicAssistance* for the second stage (equation 3):

$$Y_{ict} = \alpha_0 + \alpha_1 Drug_i + \alpha_2 Post_t + \alpha_3 \widehat{PA_{ict}} + \gamma X_{ict} + \theta_c + \delta_{y(t)} + \lambda_{m(t)} + \mu_{ict} \quad (3)$$

All models include controls for case and demographic characteristics, time served, partial exposure flag, and fixed effects for release county, year, and month.

Poisson Models

Returns to prison for new serious and violent offenses are rare. Rates for these offenses are very small in absolute terms (Figure 6) and sit near an absolute floor, so small changes in levels translate into large percentage changes. A linear model is not the right functional form for an outcome that is bounded at zero and skewed. We therefore also estimate Poisson models, which respect the lower bound and model the outcome in proportional terms, for both the reduced form and the instrumental variable analyses.

The Poisson difference-in-differences model has the same structure as Equation (2), but specifies the expected value of the outcome as an exponential function of the same covariates and fixed effects (equation 4), where i indexes individuals, c is the release county, t is the month of release, and y(t) and m(t) are its year and month:

$$Y_{ict} = exp(\beta_0 + \beta_1 Drug_i + \beta_2 Post_t + \beta_3 (Drug_i \times Post_t) + \gamma X_{ict} + \theta_c + \delta_{y(t)} + \lambda_{m(t)}) + \varepsilon_{ict} \quad (4)$$

All models include controls for case and demographic characteristics, time served, partial exposure flag, and fixed effects for release county, year, and month. The coefficient β_3 is the proportional change in the expected outcome associated with the policy change, so $\exp(\beta_3) - 1$ is the percent change. To report effects in percentage points, we multiply that percent change by the comparison group's mean outcome in the pre-period.

To estimate the effect of benefit enrollment on returns to prison for serious and violent offenses in the Poisson model, we use a control function approach instead of two-stage least squares. The two approaches are equivalent in linear models but diverge in nonlinear models

(Wooldridge, 1997). Equation (1) serves as the first stage. We then include the first-stage residual, $\hat{v}_i$, as an additional regressor in the Poisson second stage (equation 5):

$$Y_{ict} = exp(\beta_0 + \beta_1 Drug_i + \beta_2 Post_t + \beta_3 PA_{ict} + \rho\hat{\nu}_{ict} + \theta_c + \delta_{y(t)} + \lambda_{m(t)}) + \varepsilon_{ict} \quad (5)$$

A statistically significant coefficient on the residual, ρ, indicates that benefit enrollment is endogenous, which supports the control function correction. Standard errors from the second stage alone do not account for the first stage. We instead resample release counties 200 times and repeat both stages in each draw.

Finally, we re-estimate the models on a propensity score matched (PSM) sample. Each drug felony observation is matched to a comparison group observation with similar observed characteristics using nearest neighbor matching, and the models are estimated on the matched sample (columns labeled PSM in the results tables). Standard errors are clustered by release county and do not account for the uncertainty in estimating the propensity score. The Appendix describes the matching procedure and reports covariate balance (Table A1).

Heterogeneity

We also look at how the effects vary across groups, because an average effect can hide differences between them. First, we estimate the difference-in-differences effect on SNAP enrollment separately for people who did and did not receive SNAP before prison, who may already know the program and the application process. Second, we estimate it separately by type of prison release. Local post-release community supervision (PRCS) programs may do a better job of connecting people to SNAP than state parole offices. Third, we estimate the effect on SNAP enrollment by county. SNAP and TANF enrollment goes through county offices, and

some counties may enroll people faster than others. We also describe the types of SNAP cases people open after release, to see whether people mostly return to an existing case or open a new one.

For returns to prison, we estimate the effect separately by type of drug conviction and by county. Other studies suggest that people convicted of financially motivated crimes, such as possession for sale, may respond more to financial support like SNAP, so we compare offense types. Counties may also differ in the criminal justice context people return to. These analyses are exploratory, and we do not adjust for multiple comparisons, so we read individual estimates cautiously and focus on overall patterns. Results are in the Appendix (Table A2 and Figures A3 through A7).

Robustness Tests

We employ a series of robustness and placebo checks to confirm the validity of results. First, we test the sensitivity of the main results to two primary sample restrictions. The first drops from the sample individuals who were released prior to the policy change in April 2015, but whose one year follow up period extends beyond the policy change. These people are technically able to enroll in benefits despite being in the "pre-period" treatment group. Second, we drop people who received resentencing due to Proposition 47, which went into effect in November 2014. These people were admitted on felony drug convictions, but had their convictions downgraded to misdemeanors prior to the April 2015 ban removal, making them eligible for benefit receipt during the pre-period.

Second, we explore the dynamic effects of treatment by examining whether the magnitude of the effect of expanded public assistance eligibility varies over time relative to the

date of release. We do this by independently examining post-treatment windows of 3 months, 6 months, 12 months, and 18 months to examine whether the effect builds or fades over time.

Third, we examine public assistance uptake after "placebo" alternate eligibility cutoff dates — such as two years before April 1, 2015. Individuals leaving prison in April of 2013 would have remained ineligible for public assistance benefits, and thus their behavior should be unaffected by earlier "placebo" cutoff dates.

Fourth, we use placebo crime categories to rule out universal changes in outcomes or other omitted time-variant confounders. Specifically, we conduct the analyses comparing outcomes for formerly incarcerated individuals convicted of property offenses to people convicted of all other non-drug felonies, before and after the policy change.

Results

To set the stage for the reduced form difference-in-difference analysis, we first examine pre-period trends for each of the outcome measures (Figure 3). The event study results for each outcome measure are presented in separate figures. We observe parallel trends in SNAP enrollment rates within one year of release (panel A), returns to CDCR within 1 year (panel C), and returns to CDCR for serious or violent felonies (panel D). The trends for TANF enrollment within 1 year are noisier due to the small number of people released from prison who enroll in the program (panel B).

Descriptively, we observe large increases in monthly SNAP enrollment rates for the treatment group relative to the comparison group following the policy change (Figure 4), providing initial evidence of a strong first stage relationship between the lifting of the ban, and benefits enrollment. Following the policy change, SNAP enrollment rates for the treatment group jumped more than 30 percentage points to be nearly on par with enrollment rates of the

comparison group. Again, trends for TANF enrollment rates are much noisier, but the treatment group does experience an increase following the lifting of the ban. However these changes should be understood in the context of declining TANF enrollment rates for the comparison group over the full time period. This may be, in part, due to the declining share of women in the prison population over this period, and women are more likely to be recipients of TANF benefits.

Benefit enrollment rates, particularly for SNAP, appear to increase prior to the policy change implementation date (Figure 4). This occurs for two potential reasons. First, individuals released in the year prior to policy change (those released in the shaded area of the graphs) were able to apply for and enroll in benefits after the policy change but still during the 1 year follow-up period. Second, Prop 47 made some individuals initially convicted of drug felonies eligible for benefits prior to the lifting of the ban, if their felony convictions were downgraded to misdemeanors after November 2014. In the robustness tests below, we examine whether omitting individuals affected by both of these timing issues affect our main results.

Cumulative benefit enrollment rates also clearly increase following the lifting of the ban (Figure 5). Shown here as the share of total individuals released in each group that is enrolled in benefits by month of enrollment, the SNAP and TANF enrollment rates for the treatment group mirror the comparison group rates following the policy change.

Primary models

The first stage models estimate large effects of lifting the ban on SNAP enrollment rates (Table 2). The coefficient on the interaction term is a 22.5 percentage point increase in SNAP enrollment, a 53.6 percent increase from the comparison group pre-period mean of 42.0 percent. The raw rise was larger, from 16.6 to 50.7 percent (34 points; Table 1), because SNAP enrollment also rose by about 11 points in the comparison group (from 42.0 to 52.7 percent),

which the estimate nets out. Put another way, the policy closed about 90 percent of the pre-policy gap between the groups (25 points before, 2 points after). The effect on TANF enrollment is also statistically significant, but smaller in magnitude — 1.9 percentage points — but represents a 74.0 percent increase from the very low comparison group pre-period mean of 2.6 percent. In raw terms, TANF enrollment in the treatment group rose from 0.9 to 1.9 percent, while the comparison group's fell from 2.6 to 1.8 percent, so about half of the estimate comes from the comparison group's decline. The placebo tests below suggest part of it also reflects earlier trends. The results for the "any public assistance" models are driven by the SNAP results, and are similar in sign and magnitude throughout.

Given the large and significant first stage results, we proceed to estimate the reduced form effect on returns to prison. Overall, the lifting of the ban yielded no statistically significant effect on the general rate of returns to prison. The 95 percent interval runs from −0.3 to +0.7 percentage points, so we can rule out a decline larger than about 3 percent of the comparison group's pre-period rate (8.0%). Yang (2017a) finds that full food stamp eligibility lowers one-year returns to prison by 2.2 percentage points, or 13.1 percent of her sample's 16.8 percent baseline. Like our direct estimate, that study's result is an effect of eligibility, not enrollment. Our interval rules out a decline that large, in both absolute terms and as a share of our baseline. However, returns to prison for new serious or violent offenses are very rare for both groups throughout the study period (Figure 6). Before the policy, the rate was about 1.2% for the comparison group and 0.6% for the treatment group. It fell for both groups afterward, to about 0.6% and 0.5%. The comparison group's rate fell more, in part because the treatment group's rate was already so low that it had less room to fall. Using OLS, this smaller decline appears as a statistically significant 0.38 percentage point difference between the groups, though as we

discuss below, this functional form is likely inappropriate for this outcome. As described in the Research design section, OLS is not the right functional form for rates this close to zero, so we also estimate a Poisson model. The Poisson DiD results, presented in the Poisson columns of Table 3, compare proportional changes. They also show a smaller decline for the treatment group: relative to the comparison group, its rate was about 34 percent higher after the policy than before, equal to 0.42 percentage points at the comparison group's pre-period rate of 1.23%. The gap is statistically significant but very small in absolute terms. These intent-to-treat estimates capture the overall effect of the policy change, regardless of actual benefit enrollment rates.

To understand the effects of the policy change on recidivism for people whose benefit enrollment actually changed, we estimate two-stage-least-squares models using the policy change interacted with the treatment group as an instrument for benefit enrollment. The results of the linear IV models are consistent with the reduced form results (Table 4). As noted previously, the first stage relationship between the interaction term and benefits enrollment is strong, with F-stats of nearly 2,000. In the second stage, there is no detectable effect of benefits enrollment induced by the policy change on overall returns to prison (columns 1 to 3), but these estimates are imprecise: their intervals are wide enough that they cannot rule out sizable effects in either direction. For serious or violent offenses, the linear IV models show the same pattern as the reduced form: people brought into benefits by the policy had a return rate 1.7 percentage points higher than the comparison group's trend would predict. This assumes the policy affects returns only through benefits, which we cannot test, and it rests on very few events. Once again, we estimate the control function Poisson model described in the Research design section.[1] The estimate remains positive and is statistically significant at the 5 percent level when we use

[1] The coefficient on the first-stage residual is statistically significant in the IV Poisson model (Table 4), which is evidence that benefit enrollment is endogenous and supports the control function correction.

bootstrap standard errors (1.25 in logs, or a 247 percent increase; the 95 percent interval runs from 1 to 702 percent).

We also re-estimate the main models on the matched sample (columns labeled PSM in Tables 3 and 4). The benefit results hold up. The policy raised SNAP enrollment by 23.0 percentage points and TANF enrollment by 1.8 percentage points, close to the 22.5 and 1.9 points in Table 2 (matched estimates are in the note to Table 2). Returns to prison show no effect in the matched sample (−0.4 percentage points, not statistically significant). The increase in serious and violent returns is smaller in the matched sample: 0.22 percentage points, down from 0.38, and statistically significant at the 5 percent level. The matched 2SLS estimate for these returns is 0.95 percentage points, down from 1.7, and also significant at the 5 percent level. For any return to prison, the matched 2SLS estimate is −1.7 percentage points (standard error 1.4) and not statistically significant. Matching balances the groups on the traits in Table A1, but not on the starting level of the outcomes, so these estimates still rely on the assumption that the two groups would have followed similar trends without the policy.

Robustness tests

The primary DiD results are robust to multiple different tests, summarized here. First, we subset the sample in two different ways to account for ways in which the treatment group might have had access to benefits around the time of the policy change (Table 5). Panel A drops individuals released less than one year before the policy change, from both the treatment and comparison groups, because they would have been able to enroll in benefits during the follow up period, despite being released in the pre-period. The results are consistent in both sign and magnitude with the primary results. Next, we drop individuals who were affected by Proposition 47 resentencing, and again find results almost identical to the primary results.

Next, we examine variation in the impact estimates by enrollment periods (Table 6). The impact of lifting the ban on both SNAP and TANF enrollment grows over time from 3 months following release through 18 months. Notably, the effect on SNAP enrollment is immediate: within 3 months of release SNAP enrollment increases by 19 percentage points, and then incrementally increases to 21 percentage points by 6 months, and 23.0 percentage points by 18 months. Similarly, TANF enrollment increases by 1.2 percentage points in the first 3 months following release, and eventually grows to 2.2 percentage points at 18 months.

The SNAP and returns-to-prison results hold up under two placebo tests (Table 7), but the TANF results do not. First we estimate the effect of the policy change using people with property crime convictions (which were not subject to the ban) as the treated group, and other (non-drug) felonies as the comparison group. We observe a marginally significant increase in SNAP enrollment, and a decrease in TANF enrollment for this group - both effects are much smaller in magnitude than the main effects from the primary models. There is no effect on returns to prison.

Finally we shift the implementation date of the policy change to be two years prior to the actual change. We find no effect of the placebo policy date on SNAP enrollment rates, but a statistically significant 0.9 percentage point increase in TANF enrollment, about half the main estimate of 1.9 percentage points. Because TANF enrollment is so low in this population, this suggests that part of the main TANF estimate may reflect trends that started before the policy. Subtracting the placebo estimate leaves roughly 1 percentage point, though this is only a rough adjustment because the two estimates come from different samples. There is no effect on returns to prison. Taken together, the SNAP and returns-to-prison results hold up under these additional tests, but the TANF result does not.

Variation across groups

The appendix explores two questions. The first is what might have helped people reentering society from prison enroll in SNAP. We looked at three possibilities. First, people who received SNAP before prison have familiarity with the program and the application process. Among people who enrolled after the policy change, 48% of those with drug felony convictions rejoined their own pre-prison case, compared with 55% of the comparison group, and 27% joined someone else's case, compared with 22% (Table A2). This fits the treatment group's lower use of SNAP before prison (29% versus 38% among people whose pre-prison records we can observe). People who had received SNAP before prison also gained more from the policy (27 versus 18 percentage points; Figure A3). Second, people released to post-release community supervision may have better links to local services than people released to state parole. The SNAP gain was nearly the same for both groups, 21.9 and 21.2 percentage points (Figure A4). Third, counties may differ in how quickly their benefits offices enroll people. The estimated gains vary widely across counties, from about 13 to about 42 percentage points, and are positive in every county (Figure A5). The second question is where the effects on returns to prison are concentrated. We discuss the results by type of drug conviction (Figure A6) above, alongside Tuttle (2019). By county (Figure A7), three of 24 intervals exclude zero, and they differ in sign, which is not unusual with this many comparisons. County estimates are imprecise, so they cannot rule out real differences, but we see no consistent geographic pattern. All of these estimates are exploratory, and we do not adjust for multiple comparisons.

Conclusion and policy implications

Many individuals re-entering society from prison or functioning in society with a criminal record face challenges in gaining employment and stable housing, among other

elements of well-being. This study estimates that lifting the ban on benefits receipt for people convicted of drug felonies increased benefits participation substantially. Specifically, following the policy change there was a 22.5 percentage point increase in SNAP enrollment, which nearly closed the gap with the comparison group, and a 74% relative increase in TANF enrollment for the treatment group. Given the low rates of TANF enrollment for this population, however, this large percent change is a small increase in absolute terms (1.9 percentage points), and the placebo tests suggest about half of it may reflect earlier trends, leaving roughly 1 percentage point. This strong first stage effect confirms that the policy was successful in expanding access to the safety net for a vulnerable population. Furthermore, about 83 percent of the 18-month SNAP effect was in place within 3 months of release, a moment of even greater vulnerability.

However, we detect no effect of the policy change on overall returns to prison. This lack of an effect on future criminal activity may have several explanations, and must be contextualized by the limitations of the measure used. As noted above, returns to prison is a limited measure of recidivism, as it does not capture subsequent, lower-level contact with the criminal justice system that are more frequent and may be more likely to be affected by the policy change. Additionally, the one-year follow-up window may not capture the bulk of returns to prison, as cases that result in a return to prison often take longer than one year to resolve. Our results also do not mirror the earlier finding that losing benefits raises recidivism: we rule out a decline in overall returns larger than about 3 percent, and we see no decline among people convicted of sale or transport.

Still, the finding is consistent with an asymmetry between the effects of losing and gaining benefit eligibility. Evidence on the ban's initial imposition is itself mixed: Yang (2017a) and Tuttle (2019) find that losing eligibility increased recidivism, while Luallen, Edgerton, and

Rabideau (2018), Mueller-Smith et al. (2023), and Sugie and Newark (2023, for TANF specifically) find no effect of the ban's imposition on future justice involvement — though Mueller-Smith et al. (2023) confirm a 38% reduction in benefit receipt and document real harms to affected children's well-being and family stability, and Sugie and Newark (2023) find faster rearrest under the food stamps ban specifically, a program-specific split that echoes our own decision to estimate SNAP and TANF effects separately. Comparable evidence on restoration is more limited. The closest study we are aware of, Yang (2017a), finds that full SNAP eligibility reduced one-year recidivism by 13.1%, but her estimate pools the 1996 ban and later state opt-outs, so it does not isolate restoration. Our estimates do not replicate it: our confidence interval for overall returns rules out a decline of that size. This divergence may reflect differences in outcome measures, follow-up windows, populations, or the mechanics of eligibility restoration, and is worth further investigation. Taken together with Mueller-Smith and colleagues' (2023) finding of no recidivism effect alongside real economic and family harms, our results suggest that removing the legal barrier of ineligibility may be a necessary but insufficient condition to produce large-scale reductions in reincarceration. The financial support provided by benefits, while critical for basic subsistence (two-thirds of a sample of reentering individuals relied on public assistance for basic needs in one study) may not be robust enough to overcome the structural barriers related to employment and housing that drive recidivism.

Tuttle (2019) found that Florida's 1996 ban raised recidivism among drug traffickers and that the increase came from financially motivated crimes such as property crime and drug sales. If the same financial motivations were at play in California, we would expect a decrease in recidivism among individuals incarcerated for similar, financially motivated crimes. Here, we treat possession for sale and sale or transport as the closest match to Tuttle's drug trafficker

category, and we see no decline in recidivism for either group. Returns are higher for possession for sale (+0.7 percentage points) and statistically unchanged for sale or transport (−0.5 percentage points, with a confidence interval that extends to −1.4) (Figure A6). SNAP enrollment rose by about 24 percentage points for possession for sale and 23 for sale or transport, close to the 22.5 overall (Figure A6). Before 2015, California's SNAP ban covered these two groups fully, while people convicted of possession or use could already enroll if they showed they were in or had finished drug treatment, so the overall SNAP increase pools full and conditional bans and these two groups give the cleaner test of full restoration (California Assembly Bill 1468, 2014). These are exploratory subgroup estimates, so we do not read much into the positive result. Two factors may explain the difference in results between our studies. First, losing income may push people toward crime faster than regaining it pulls them away. Second, our outcome is a return to prison within one year, which misses arrests and short jail stays that are likely more responsive recidivism measures.

While we observe no effect on overall returns to prison within one year of release, serious and violent returns fell less for people with drug felonies than for the comparison group. These returns are very rare for both groups throughout the study period (about 1.2% for the comparison group and 0.6% for the treatment group before the policy), and the treatment group recidivism rate had less room to fall, so this finding should be interpreted cautiously. The estimate has the same direction across the linear, Poisson, matched, and IV models, but these models use the same data, so they are not independent checks. As shown in Figure 7, the number of serious and violent returns to prison among the treatment group is very low (fewer than 30 per year) and falling in the post-period, and fewer than 10 in 2018. We cannot say why the gap exists. It may

reflect changes that affected the comparison group more than the treatment group, or chance in very small counts.

The results inform future policy action to facilitate connections between individuals preparing for release and existing safety net programs. The SNAP effect was already 19 points at 3 months and rose to 23 points by 18 months (Table 6), so most take-up happened quickly, but some people enrolled later. Our data cannot tell us why, or what happened in the first weeks after release. Federal rules generally bar people in prisons and jails from enrolling in SNAP, so California sought and received a federal waiver to enroll people before release. This type of pre-release enrollment already occurs in at least one California county (Orange). However, statewide implementation of in-reach benefits enrollment services has been delayed because of H.R. 1. Pre-release enrollment would make benefits available immediately upon release; whether that would change returns to prison is untested.

Tables and Figures

Table 1. Descriptive characteristics of the treatment and comparison groups

Characteristic	Overall	Treatment		Comparison	
		Pre	*Post*	*Pre*	*Post*
Observations	438,610	38,771	16,595	215,744	167,500
Female	8.5	9.5	6.6	9.5	7.2
Age at release (average years)	36	38	40	36	36
American Indian/Alaskan Native	1.0	0.6	0.8	1.0	1.2
Asian	1.1	0.8	1.3	1.0	1.2
Black	24.8	31.7	23.4	24.1	24.4
Hispanic	42.3	41.0	42.8	41.3	43.9
Other	3.5	3.2	3.4	3.7	3.3
Pacific Islander	0.3	0.1	0.3	0.2	0.3
White	27.0	22.6	27.9	28.7	25.7
Baseline information					
Average sentence length (years)	2.7	2.3	2.7	2.4	3.1
Prior prison admissions (n)	2.0	1.9	2.6	2.0	1.9
Serious violent felony	38.1	9.3	19.2	35.9	49.5
Pre-incarceration SNAP	35.0	19.5	29.3	32.6	38.2
Pre-incarceration TANF	2.1	1.0	2.1	1.9	2.6
Partial follow-up period	9.5	10.3	0.0	17.4	0.0
Outcomes					
Post-release SNAP in 1 yr	44.1	16.6	50.7	42.0	52.7
Post-release TANF in 1 yr	2.1	0.9	1.9	2.6	1.8
Any public assistance in 1 yr	44.2	16.8	50.8	42.1	52.7
Return to CDCR 1yr	7.0	5.6	5.0	8.0	6.4
Return to CDCR SVF 1yr	0.9	0.6	0.4	1.2	0.5

Notes. Pre = January 2010 to March 2015. Post = April 2015 to February 2020. Sample based on release date. Pre-incarceration SNAP is shown only for people whose pre-prison records are in the data; the overall rate is a weighted average.

Table 2. Difference-in-Difference results: Benefits Enrollment

Variable	SNAP enrollment	TANF enrollment	Public Assistance enrollment
Drug Felony	-0.2476***	-0.0162***	-0.2465***
	(0.0183)	(0.0021)	(0.0182)
Post April 2015	0.0085	0.0034	0.0091
	(0.0089)	(0.0030)	(0.0090)
Drug Felony x Post April 2015	0.2248***	0.0191***	0.2242***
	(0.0211)	(0.0025)	(0.0209)
Comparison pre-period mean	0.4196	0.0258	0.4205
Observations	438,610	438,610	438,610
R2	0.07372	0.02023	0.07340

Notes. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Standard errors are clustered by release county. All models include covariates: race, ethnicity, sex, age at release, sentence length, prior CDCR admissions, felony class of controlling offense, flag for partial follow-up period, and release county, year, and month fixed effects. Matched-sample (PSM) estimates for these outcomes are very similar, so we do not show them: SNAP 0.2298 (0.0221), TANF 0.0180 (0.0031), and any public assistance 0.2289 (0.0221), all $p < 0.001$, with 110,724 observations in each model.

Table 3. Difference-in-Difference results: Recidivism

	Return to Prison			Return to prison for serious & violent offense		
Variable	DiD	Poisson	PSM	DiD	Poisson	PSM
Drug Felony	-0.0217***	-0.2941***	-0.0217***	-0.0052***	-0.5542***	-0.0040***
	(0.0018)	(0.0193)	(0.0019)	(0.0005)	(0.0764)	(0.0006)
Post April 2015	-0.0051	-0.0770	-0.0047	-0.0010	-0.0645	0.0020
	(0.0055)	(0.0740)	(0.0079)	(0.0012)	(0.1100)	(0.0021)
Drug Felony x Post April 2015	0.0020	0.0320	-0.0038	0.0038***	0.2919* *	0.0022*
	(0.0024)	(0.0404)	(0.0033)	(0.0005)	(0.0978)	(0.0011)
Comparison pre-period mean	0.0797		0.0761	0.0123		0.0104
Observations	400,403	400,403	104,998	400,403	399,711	104,998
R2	0.02876		0.02710	0.00639		0.00633

Notes. *** p < 0.001, ** p < 0.01, * p < 0.05. Standard errors are clustered by release county. All models include covariates: race, ethnicity, sex, age at release, sentence length, prior CDCR admissions, felony class of controlling offense, flag for partial follow-up period, and release county, year, and month fixed effects. The recidivism models include controls for time served in prison.

Table 4. Two-stage least squares results

Outcome measure:	**Return to Prison**			**Return to prison for serious & violent offense**		
Endogenous variable: Benefits enrollment	Linear IV	IV Poisson	IV PSM (linear)	Linear IV	IV Poisson	IV PSM (linear)
Drug Felony	-0.0194*** (0.0025)	-0.2600*** (0.0423)		-0.0009 (0.0005)	-0.2271 (0.1281)	
Post April 2015	-0.0051 (0.0054)	-0.0741 (0.0766)		-0.0013 (0.0011)	-0.0733 (0.1113)	
Benefits enrollment[a]	0.0090 (0.0111)	0.1129 (0.1960)	-0.0167 (0.0142)	0.0169*** (0.0030)	1.246* (0.5142)	0.0095* (0.0045)
First stage residual term	N/A	-0.4336* (0.1820)	N/A	N/A	-1.645*** (0.4319)	N/A
Comparison pre-period mean	0.0797			0.0123		
Observations	400,403	400,403	104,998	400,403	399,711	104,998
R2 (pseudo R2 for Poisson)	0.02697	0.05964	0.02820	-0.00380	0.07219	0.00282
F-test (1st stage)	1,986.9	1,986.9	1,300.8	1,986.9	1,986.6	1,300.8

Notes. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Standard errors are clustered by release county. All models include covariates: race, ethnicity, sex, age at release, sentence length, prior CDCR admissions, felony class of controlling offense, time served, flag for partial follow-up period, and release county, year, and month fixed effects.

[a] Benefits enrollment in the IV Poisson model uses SNAP enrollment, while the PSM IV model uses any public assistance. Benefits enrollment is predicted in linear IV models and observed in IV Poisson. In the IV Poisson columns, the standard error on benefits enrollment comes from the county bootstrap described in the Research design section; other standard errors are clustered by release county.

Table 5. Robust tests: Sample restrictions

	Outcome measure			
Variables	**SNAP enrollment**	**TANF enrollment**	**Return to prison**	**Return to prison for serious & violent offense**
A. Drop partial follow-up				
Drug Felony	-0.2595***	-0.0179***	-0.0222***	-0.0050***
	(0.0186)	(0.0025)	(0.0020)	(0.0006)
Drug Felony x Post 2015	0.2378***	0.0209***	0.0020	0.0035***
	(0.0214)	(0.0029)	(0.0025)	(0.0006)
Observations	397,009	397,009	358,802	358,802
R2	0.07691	0.02034	0.02893	0.00621
B. Drop Prop 47 affected cases				
Drug Felony	-0.2478***	-0.0163***	-0.0218***	-0.0052***
	(0.0184)	(0.0021)	(0.0014)	(0.0005)
Post	0.0174*	0.0031	-0.0044	-0.0026
	(0.0079)	(0.0030)	(0.0049)	(0.0020)
Drug Felony x Post 2015	0.2255***	0.0192***	0.0021	0.0038***
	(0.0215)	(0.0026)	(0.0025)	(0.0008)
Observations	434,670	434,670	391,681	391,681
R2	0.07291	0.02014	0.02850	0.00624

Notes. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Standard errors are clustered by release county. All models include covariates: race, ethnicity, sex, age at release, sentence length, prior CDCR admissions, felony class of controlling offense, time served, flag for partial follow-up period, and release county, year, and month fixed effects.

Table 6. Robustness tests: Follow up periods

	Time since release			
Variables	**3 months**	**6 months**	**12 months**	**18 months**
A. SNAP enrollment				
Drug Felony x Post 2015	0.1905***	0.2083***	0.2251***	0.2301***
	(0.0180)	(0.0204)	(0.0213)	(0.0231)
Observations	438,610	438,610	438,610	438,610
R2	0.06706	0.07031	0.07251	0.07336
B. TANF enrollment				
Drug Felony x Post 2015	0.0124***	0.0150***	0.0191***	0.0220***
	(0.0020)	(0.0019)	(0.0025)	(0.0030)
Observations	438,610	438,610	438,610	438,610
R2	0.01130	0.01452	0.02021	0.02496

Notes. *** p < 0.001, ** p < 0.01, * p < 0.05. Standard errors are clustered by release county. All models include covariates: race, ethnicity, sex, age at release, sentence length, prior CDCR admissions, felony class of controlling offense, time served, flag for partial follow-up period, and release county, year, and month fixed effects.

Table 7. Placebo tests:

Variables	SNAP enrollment	TANF enrollment	Return to prison
A. Property convictions			
Placebo treatment: property convictions	0.0317***	0.0042***	0.0258***
	(0.0036)	(0.0008)	(0.0020)
Property*Post 2015	0.0090	-0.0057***	0.0019
	(0.0045)	(0.0016)	(0.0026)
Observations	383,244	383,244	347,905
R2	0.04677	0.02133	0.03057
B. Policy placebo date 2013			
Drug felony	-0.2617***	-0.0188***	-0.0229***
	(0.0177)	(0.0026)	(0.0030)
Drug Felony x Post 2013	0.0118	0.0093***	0.0004
	(0.0180)	(0.0014)	(0.0038)
Observations	212,914	212,914	212,914
R2	0.07813	0.02202	0.03141

Notes. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$. Standard errors are clustered by release county. All models include covariates: race, ethnicity, sex, age at release, sentence length, prior CDCR admissions, felony class of controlling offense, time served, flag for partial follow-up period, and release county, year, and month fixed effects.

Figure 1. Prison release trends and share serious & violent releases (2010-2019)

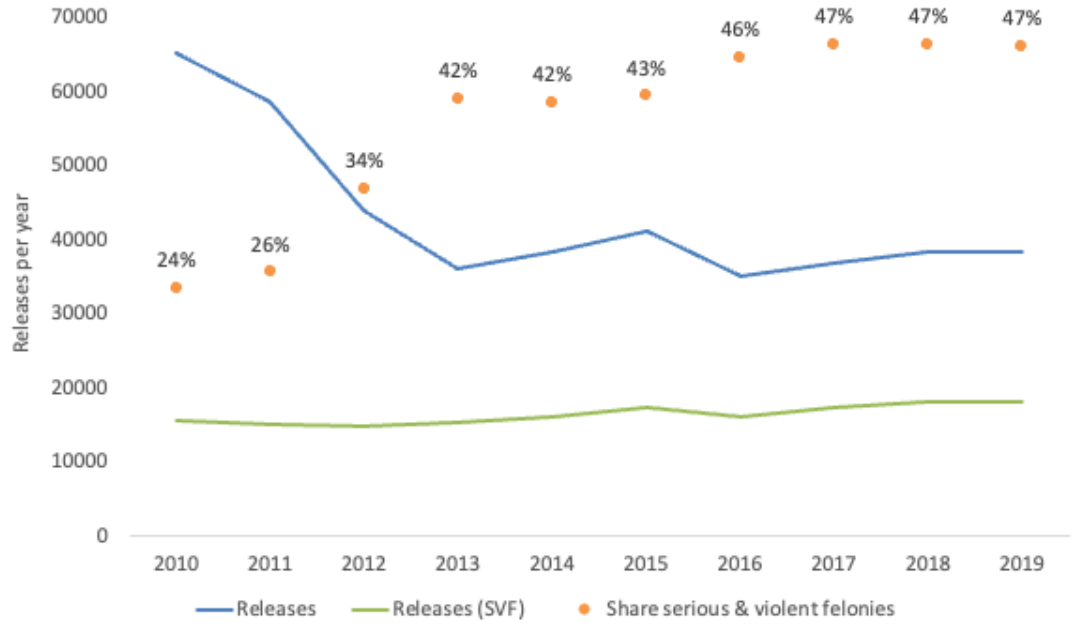


Figure 2. Distribution of time served at release, Tx and Comparison groups

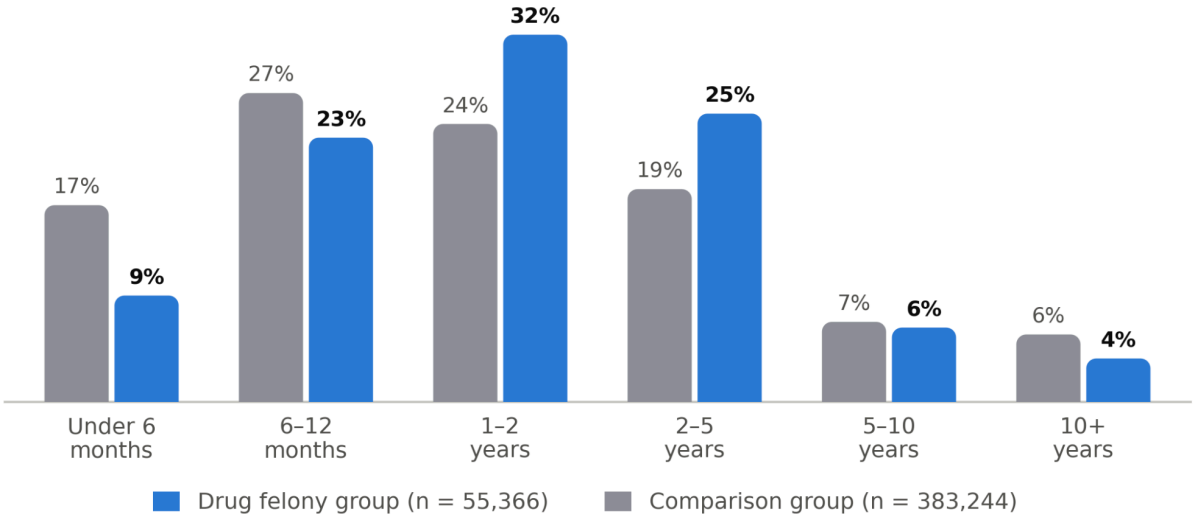
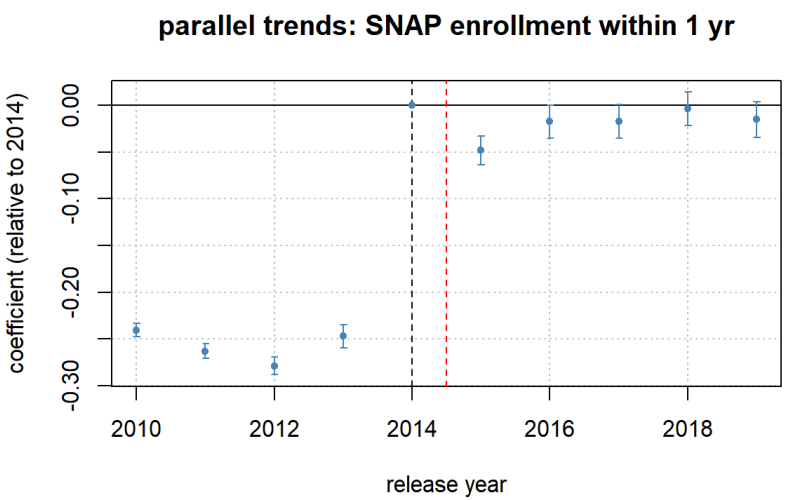
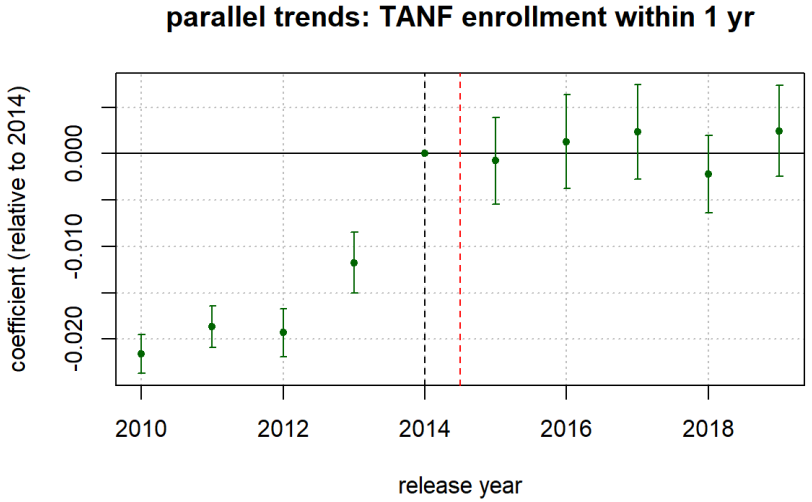
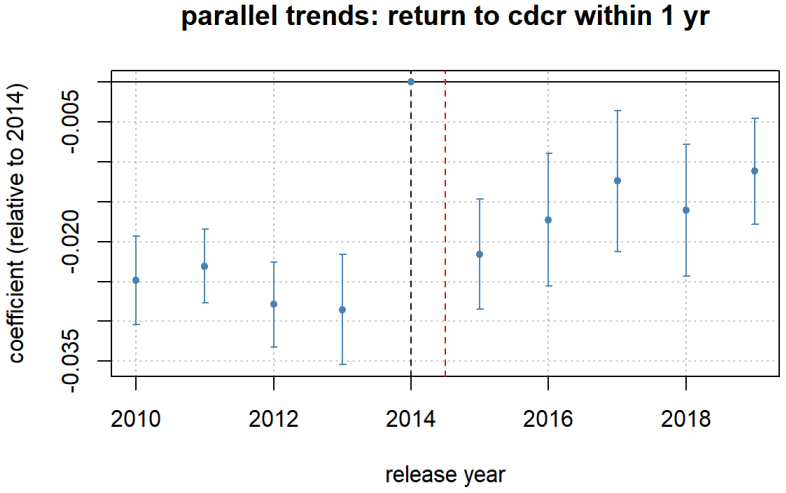


Figure 3. Parallel trends in outcome measures







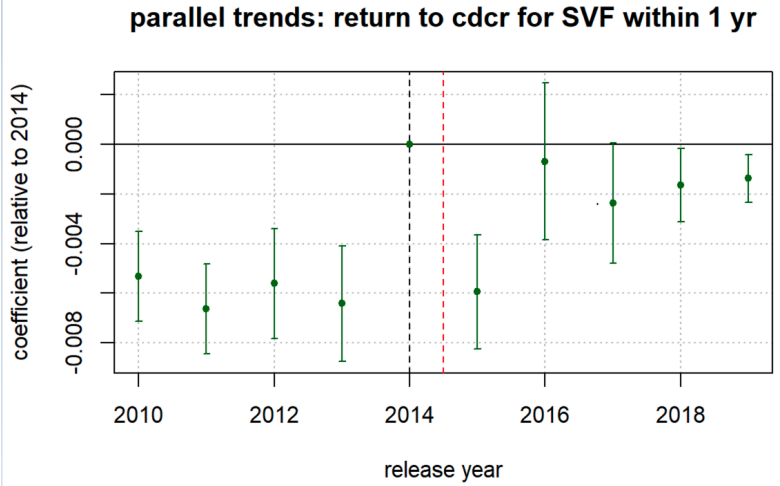


Figure 4. Monthly benefit enrollment rates (by month of release)

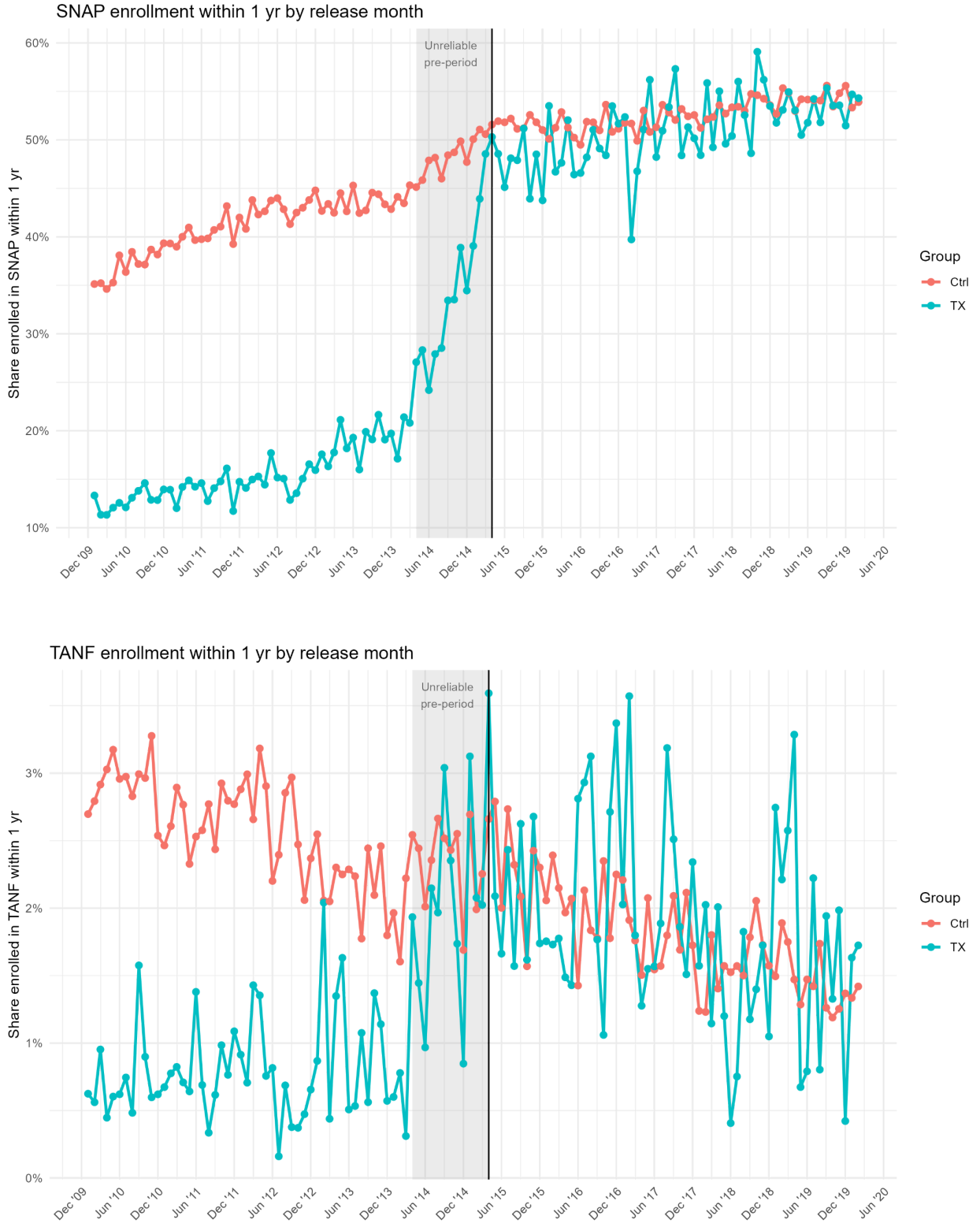
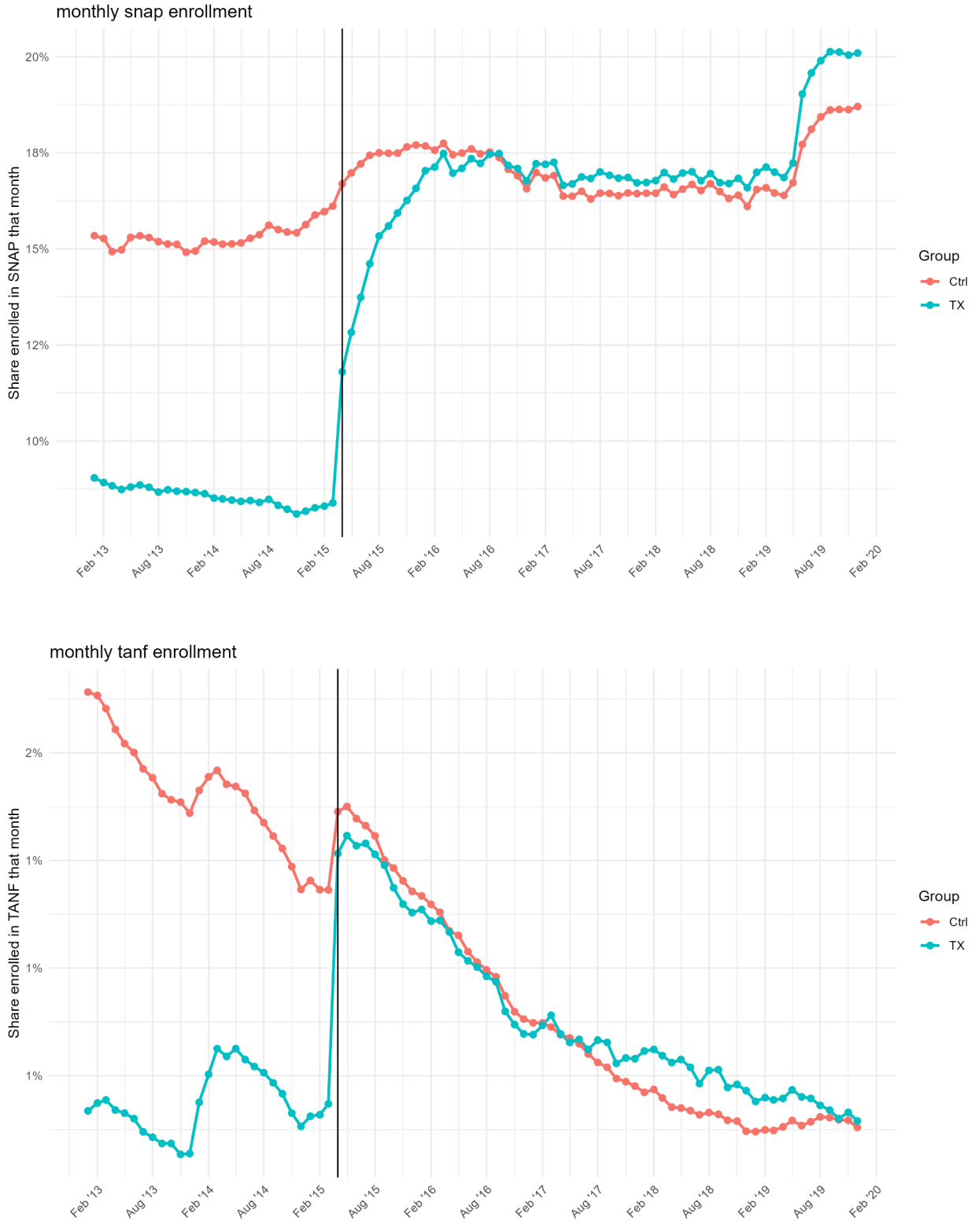


Figure 5. Cumulative benefit enrollment rates (by month of enrollment)



Notes. Graphs reflect the cumulative enrollment rate for individuals who enroll in the benefit program at some point.

Figure 6. Rate of returns to prison for serious and violent felony offenses by year of release (rate)

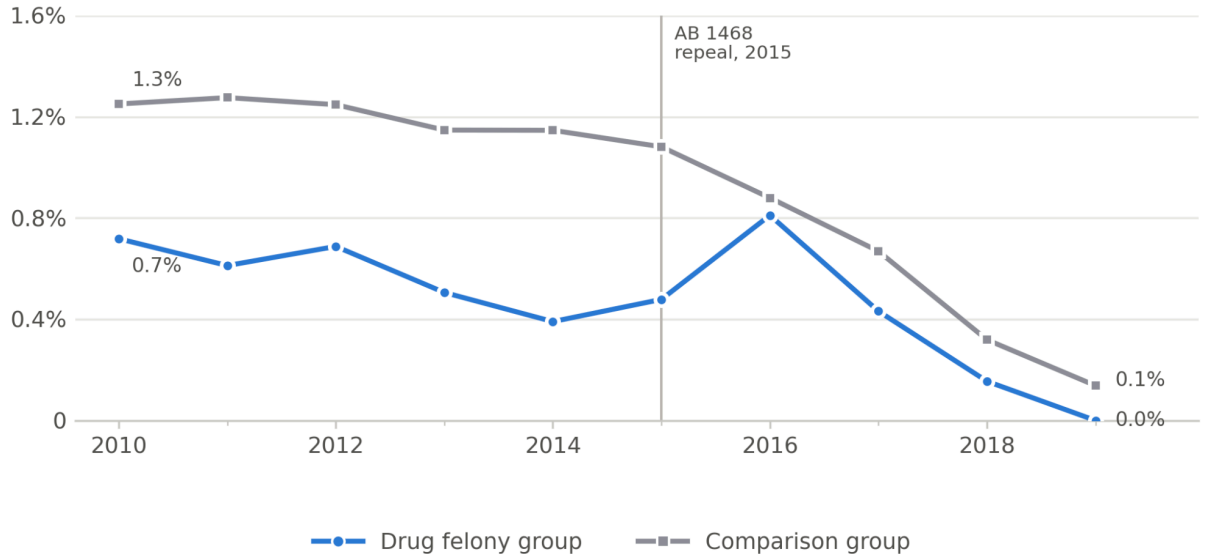
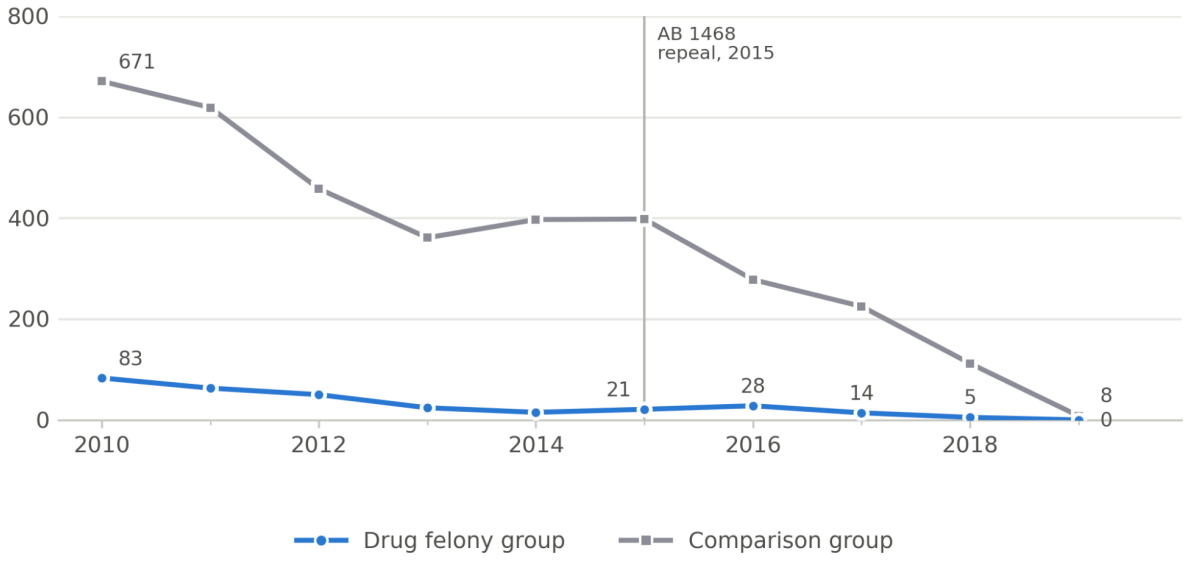


Figure 7. Returns to prison for serious and violent felony offenses by year of release (count)



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Appendix

This appendix reports additional analyses that support the main results. The first section (Table A1) shows how well propensity score matching balanced the treatment and comparison groups. The second section (Table A2 and Figures A3 to A5) looks at what may drive benefit take-up after release, using the type of SNAP case people open and how the effect of AB 1468 on SNAP enrollment differs by prior SNAP receipt, by release type and by county. The third section (Figures A6 and A7) looks at whether the effect on returns to prison differs by type of drug conviction and by county.

Propensity score matching

We match each treatment group observation to a comparison group observation using nearest neighbor matching on the propensity score without replacement, which we estimate from the characteristics listed in Table A1 and caliper of 0.2 standard deviations. Nearly all treatment observations were matched (55,362 of 55,366 drug felony releases). The matched sample has 110,724 observations in the benefits models and 104,998 in the recidivism models. Table A1 compares the groups before and after matching.

Table A1. Covariate balance before and after matching

Variable	Difference (unadjusted)	Difference (adjusted)	M.Threshold
Distance	1.11123	0.00150	<0.1
White	-0.03209	0.02549	<0.1
American Indian/Alaskan Native	-0.00420	0.00049	<0.1
Asian	-0.00139	0.00083	<0.1
Black	0.05024	-0.04449	<0.1
Hispanic/Latino	-0.00867	0.01425	<0.1
Other	-0.00275	0.00325	<0.1
Pacific Islander	-0.00110	0.00016	<0.1
Unknown race/ethnicity	-0.00005	0.00002	<0.1
Male	-0.00090	-0.00246	<0.1
Age at release	0.22474	0.01280	<0.1
Sentence length	-0.08307	-0.00300	<0.1
Prior CDCR admissions	0.04316	0.10613	*>0.1
Felony admission	0.30284	0.01465	<0.1
Serious Felony admission	-0.13217	-0.00172	<0.1
Violent Felony admission	-0.17067	-0.01293	<0.1
Release year	-0.36183	0.00000	<0.1

Notes. Differences are standardized differences between treatment and comparison observations, before and after nearest neighbor matching. A value below 0.1 indicates adequate balance. Prior CDCR admissions (marked with an asterisk) is the only variable above 0.1 after matching (0.106, up from 0.043 before matching).

Benefits Enrollment Mechanisms

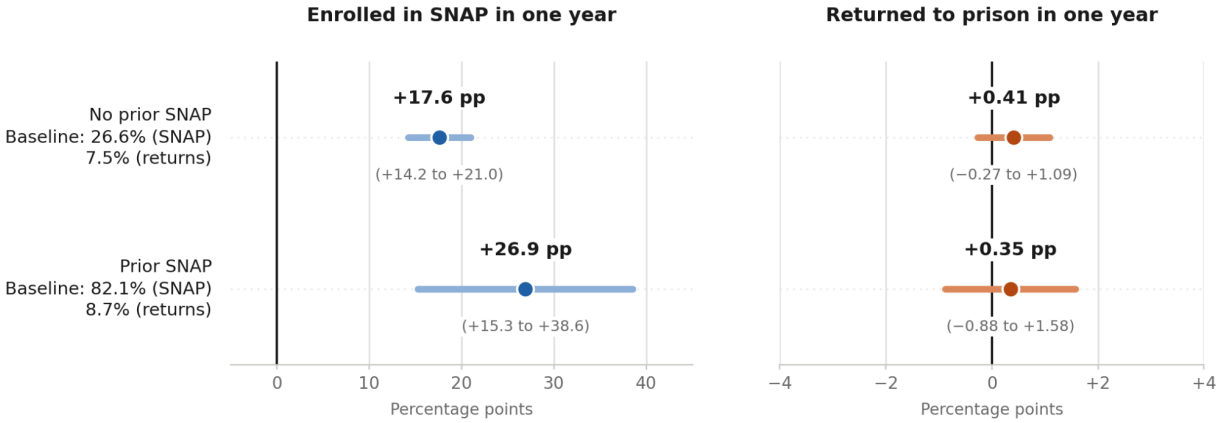
We explore three factors that might help people enroll in SNAP after release. First, people who received SNAP before prison already know the program and how to apply (Table A2 and Figure A3). Second, people released to post-release community supervision may have better links to local services than people released to state parole (Figure A4). Third, counties may differ in how quickly their benefits offices enroll people (Figure A5).

Table A2. Distribution of SNAP case types by treatment status

	Drug felony conviction (Treatment)		**Non-drug felony conviction (Comparison)**	
Case type	**N**	**%**	**N**	**%**
Rejoined own pre-prison case	4,048	48.1%	48,223	54.7%
Joined existing case	2,275	27.1%	18,937	21.5%
New case, 1 person	2,009	23.9%	20,379	23.1%
New case, 2+ people	78	0.9%	651	0.7%
Total	8,410	100.0%	88,190	100.0%

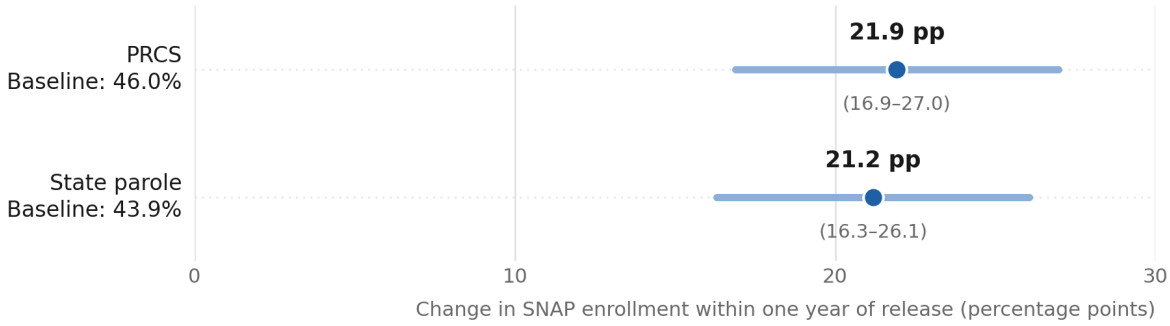
Notes. Percentages are shares of each group's total, so each column sums to 100 percent. The sample is people released after April 2015 who enrolled in SNAP. Rejoined own pre-prison case means the person reopened a SNAP case held before prison; joined existing case means the person was added to another household's case; new case means the person opened a new case alone (1 person) or with others (2+ people).

Figure A3. Variation in DID estimates by prior SNAP enrollment



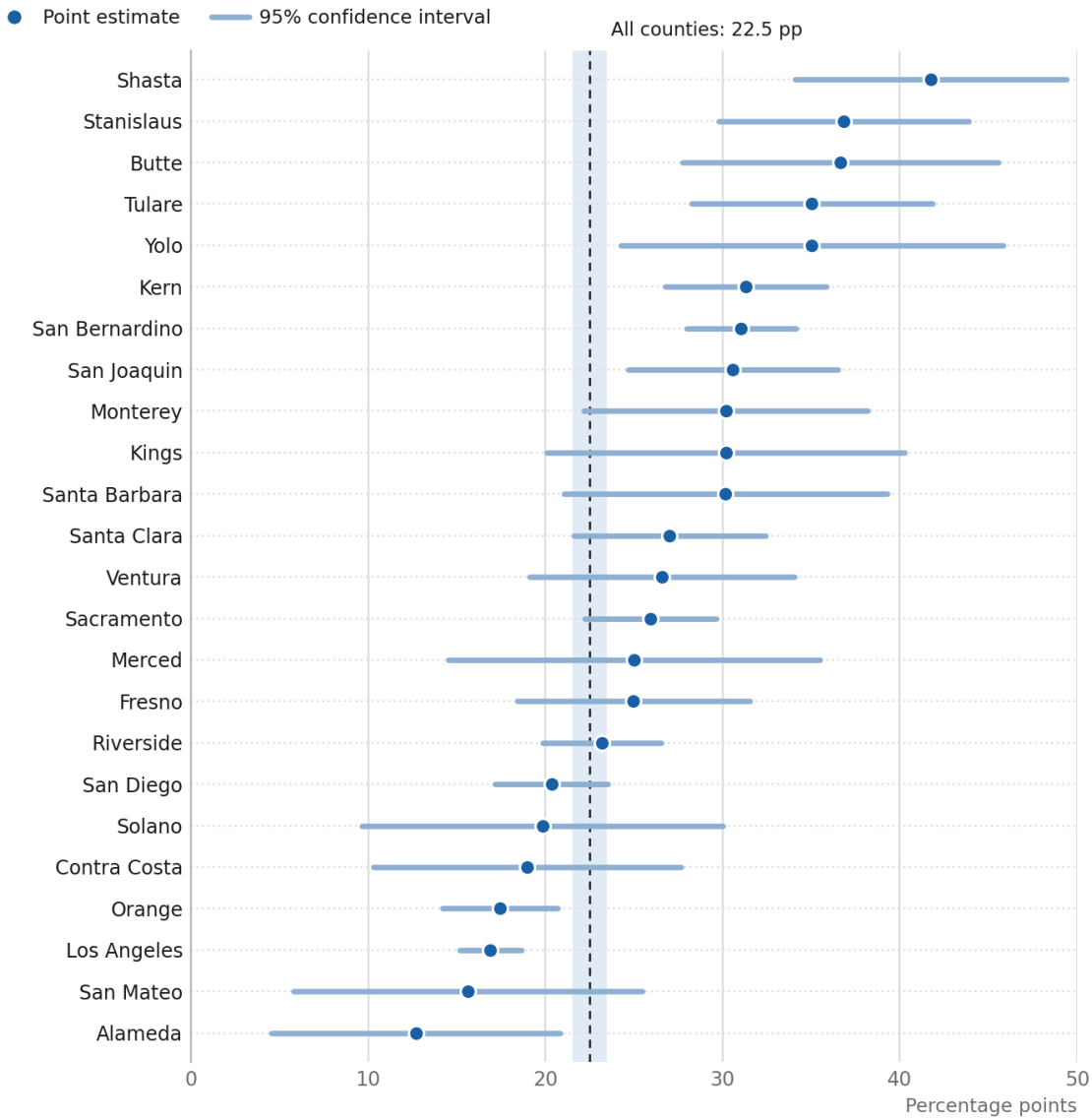
Notes. Each estimate is the difference-in-differences effect of AB 1468 on SNAP enrollment (left panel) and on returning to prison (right panel) within one year of release, estimated separately for people who did and did not receive SNAP before prison. The sample is limited to people whose pre-prison benefits records are in the data. Lines show 95 percent confidence intervals. Baseline is the comparison group's average before April 2015 (26.6 percent for SNAP among people with no prior SNAP, and 82.1 percent among people with prior SNAP). Estimates are in percentage points. Standard errors are clustered by release county. The SNAP gain is larger for people with prior SNAP, but the intervals overlap, and these people start from a much higher baseline. Returns to prison show no detectable change in either group. These are exploratory subgroup estimates.

Figure A4. Variation in DID estimates by type of prison release



Notes. Each estimate is the DiD effect of AB 1468 on SNAP enrollment within one year of release, estimated separately for people released to post-release community supervision (PRCS) and to state parole. Lines show 95 percent confidence intervals. Baseline is the comparison group's average before April 2015. Estimates are in percentage points. Standard errors are clustered by release county. The sample covers releases from October 2011, when post-release community supervision began, and excludes the 110,000 releases before then, nearly all to parole, and a small number of other release types.

Figure A5. Variation in DID estimates of SNAP enrollment by county of conviction

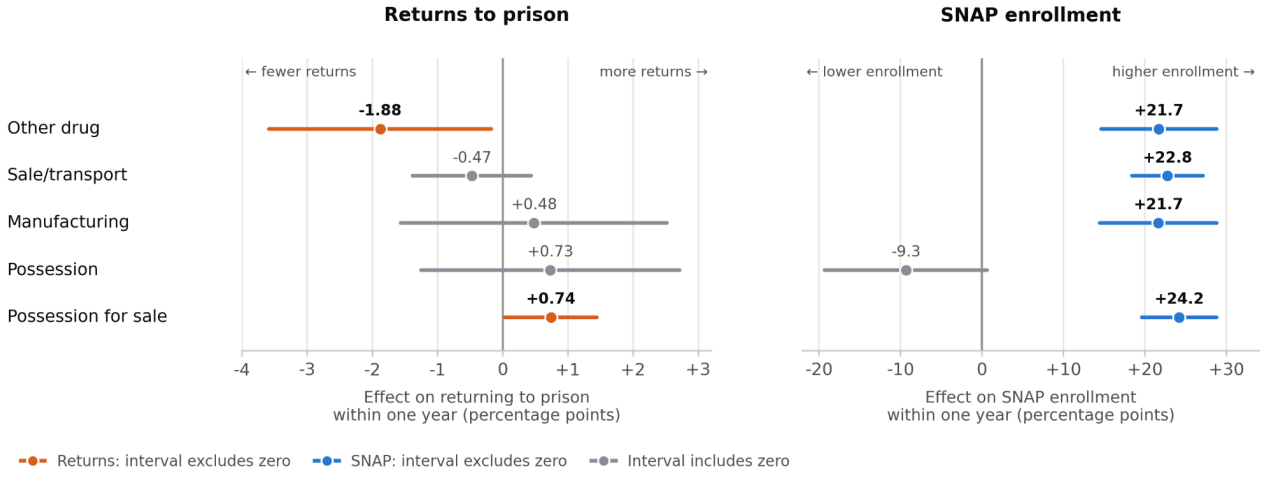


Notes. Each estimate is the difference-in-differences effect of AB 1468 on SNAP enrollment within one year of release for one county. Lines show 95 percent confidence intervals. The shaded band is the 95 percent confidence interval for all counties combined, and the dashed line is the all-county estimate (22.5 percentage points). Counties are sorted by point estimate. Counties not shown are pooled in an "Other counties" group. Estimates are in percentage points.

Variation in recidivism outcomes

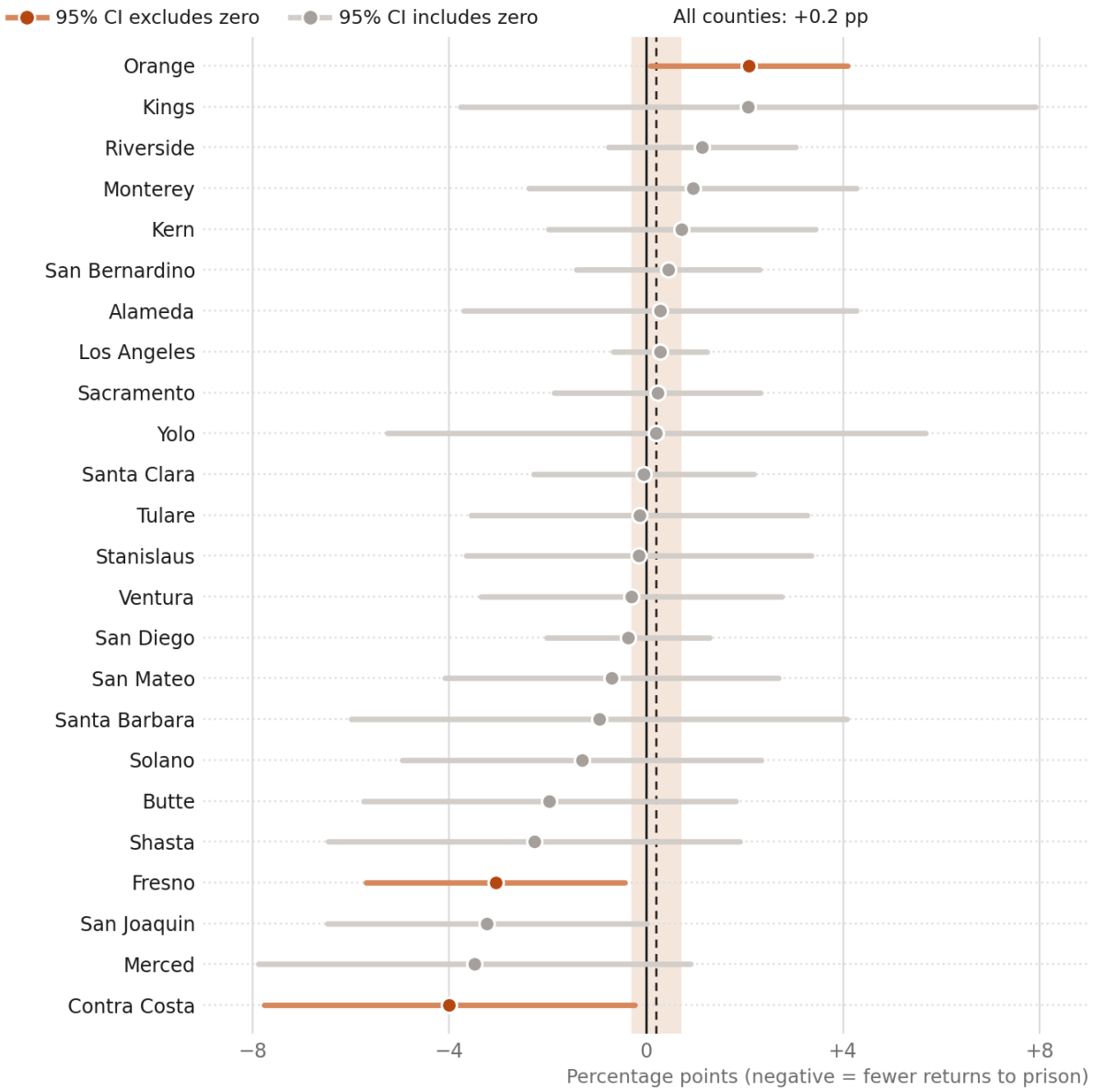
We look at whether the effect on returns to prison is concentrated in certain groups or places. Figure A6 compares types of drug conviction, because people convicted of financially motivated crimes may respond more to income support. It also shows the SNAP effect for each type, so we can see whether the benefit gain was similar. Figure A7 compares counties. The results are exploratory, because the large number of subgroups makes it likely that a few point estimates will exclude zero by chance.

Figure A6. Type of Drug Conviction



Notes. Each estimate is the difference-in-differences effect of AB 1468 on returning to prison (left panel) and on SNAP enrollment (right panel) within one year of release, by type of drug conviction. The five categories (main drug offense) are mutually exclusive. Lines show 95 percent confidence intervals. Orange marks the two intervals that exclude zero (other drug, −1.9 percentage points; possession for sale, +0.7 percentage points); blue marks SNAP intervals that exclude zero; gray marks intervals that include zero. These are exploratory subgroup estimates that share one comparison group, and the two significant results point in opposite directions, so we do not read either as a finding. Possession has no increase in SNAP enrollment (−9.3 percentage points, 317 people after the policy), so its returns estimate is not informative about benefits. A negative estimate means fewer returns to prison. Estimates are in percentage points.

Figure A7. Variation in DID estimates of returns to prison by county of conviction



Notes. Each estimate is the difference-in-differences effect of AB 1468 on returning to prison within one year of release for one county. Lines show 95 percent confidence intervals. Orange marks the three counties whose intervals exclude zero (Contra Costa, Fresno and Orange); gray marks the other 21. The shaded band is the 95 percent confidence interval for all counties combined (+0.2 percentage points), and the dashed line is the all-county estimate. With 24 counties, about one interval that excludes zero would be expected by chance. Counties are sorted by point estimate. A negative estimate means fewer returns to prison. Counties not shown are pooled in an "Other counties" group. Estimates are in percentage points.