The Effect of Drug Screening Laws on Access to Public Assistance

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November 17, 2026

Abstract

This paper studies the effect of drug testing policies rolled out through the 2000s targeting access to welfare benefits, specifically the Temporary Assistance for Needy Families (TANF) program. We study the rollout of these state-level policies on TANF caseloads and spillovers to SNAP caseloads using a stacked difference-in-differences design with annual state-level data on enrollment information. We find a 21.9% decrease in families enrolled in TANF per 100,000 households. Despite the laws applying only to adults, we find a meaningful spillover to children, with a 21.1% decrease in enrollment among children.

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1 Introduction

The United States social safety net comprises a variety of programs providing income assistance, in-kind support, and retirement benefits. However, many eligible households do not participate in public assistance, in part due to barriers from limited information, complicated applications, and re-enrollment requirements (Finkelstein and Notowidigdo, 2019; Homonoff and Somerville, 2021; Gray, 2019; Heinrich et al., 2022). For example, in FY 2020, only 20% of eligible households participated in Temporary Assistance for Needy Families (TANF), compared to approximately 80% in the Supplemental Nutrition Assistance Program (SNAP) (Cunnyngham, 2023; Dehry and Knowles, 2023). Unlike SNAP, TANF grants states considerable discretion over eligibility requirements and administrative procedures, potentially introducing additional barriers to enrollment.

We study one such barrier imposed by states in the form of drug testing and screening policies. In particular, we examine 14 state policies requiring some form of drug screening or testing for TANF applicants. Most of these policies involve screening for drug use rather than direct testing, although some require mandatory drug testing. Leveraging the staggered adoption of these policies across states, we use a stacked difference-in-differences design to estimate their effects on TANF participation. We use data from the Office of Family Assistance to examine changes in the number of families receiving assistance, as well as participation among adults, children, and different racial and ethnic groups. We also consider potential spillover effects to SNAP.

We find a 21.9% decrease in families enrolled in TANF per 100,000 households, which is likely driven by a drop in new applications or recertifications. While the policies targeted adult drug users, we find 24.0% and 21.1% reductions in participation among adults and children, respectively, suggesting substantial spillover effects on children who are not directly subject to drug screening requirements. We find that the effects are not concentrated among a single racial or ethnic group, with 21.8%, 17.0%, and 20.1% reductions in participation

among White, Black, and Hispanic recipients, respectively.[1] While the estimated effects are relatively large, we believe they are consistent with findings from other studies of public assistance requirements.

Our findings contribute to a substantial body of work on the impact of screening and barriers to public assistance for both enrollment and targeting of benefits, spanning a range of public programs, including TANF, SNAP, Medicaid, Social Security Income, and Free Application for Federal Student Aid (FAFSA). In theory, screening should improve targeting and efficiency (Nichols and Zeckhauser, 1982; Besley and Coate, 1992). However, in practice, the screening tools often become barriers in the form of a lack of information and numerous administrative hurdles (e.g., Heinrich et al., 2022; Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019). For example, Gray (2019) estimates low retention in SNAP at the time of recertification, with half of those disenrolled still remaining eligible. Similarly, Homonoff and Somerville (2021) finds that late recertification timing, which leaves less time for rescheduling, leads to large disenrollment effects, with unenrolled cases reporting similar need to the average enrolled household in SNAP. Gray (2019) finds that reducing recertification barriers for SNAP in Michigan leads to a 12% increase in retention. Similarly, Finkelstein and Notowidigdo (2019) find an 80% increase in take-up of SNAP benefits from an information notification and a 200% increase if information and assistance are provided for enrollment.

Our paper is most closely related to studies examining the effects of increased sanctions and compliance costs for public assistance. Richard and Bart (2026) examine the effects of longer sanctions for failing to comply with TANF work requirements and find a 35% reduction in the likelihood of returning to TANF. They also document substantial declines in SNAP and Medicaid participation following sanctions, including among children who remain eligible for assistance. Relatedly, Gray et al. (2023) find over a 50% decrease in SNAP participation due to work requirements that change sharply when participants turn 50 years old. Cook

[1]Participation by race and ethnicity is measured by fiscal year rather than calendar year, which may attenuate the estimated effects. See Section 4 for further discussion.

and East (2026) find that referrals to an Employment and Training program reduce SNAP receipt among household heads by 57% and household SNAP benefits by 23%, highlighting the role of administrative burdens in reducing program participation.

We are the first, to our knowledge, to estimate the effects of drug testing and screening on program participation. In doing so, we highlight an important screening barrier that has substantial effects on enrollment. Consistent with prior literature, these reductions may reflect additional barriers to enrollment and recertification. Moreover, we document important spillovers within households, particularly among children. Given that many of these policies remain in place, our findings have important implications for ongoing policy discussions.

2 Background and data

2.1 Institutional background

The Temporary Assistance for Needy Families (TANF) program provides cash assistance to low-income families with children and is jointly administered by federal and state governments. TANF replaced Aid to Families with Dependent Children (AFDC) in 1996, shifting from joint funding with the federal government to block grants for states. The state block grants allowed states to shift money away from TANF recipients to other buckets of funding.[2] For example, in FY97, states were allocating 70.9% of the funding toward basic assistance (i.e., TANF), but only 24.6% in FY23. The transition to TANF also allowed states significant discretion in designing eligibility requirements, time limits, and administrative procedures.

States have tried to exercise this discretion in the form of drug testing as a condition of program eligibility. Michigan first passed mandatory drug testing for TANF recipients in

[2]The federal money could be spent on one of four categories: "1. Provide assistance to needy families so that children can be cared for in their own homes or in the homes of relatives. 2. End the dependence of needy parents on government benefits by promoting job preparation, work, and marriage. 3. Prevent and reduce the incidence of out-of-wedlock pregnancies 4. Encourage the formation and maintenance of two-parent families".

1999, but the law was determined to be unreasonable search in *Marchwinski v. Howard* and thus unconstitutional. Florida attempted a similar policy in 2012, which was again deemed unconstitutional in *Lebron v. Secretary, Florida Department of Children and Families*. In both cases, courts deemed unilateral, suspicionless drug testing to be unreasonable search.

However, states have been permitted to implement alternative methods of screening applicants for drug use. In many states, TANF applicants are required to complete questionnaires about current or past drug use during the application or recertification process. Some states use standardized instruments, such as the Substance Abuse Subtle Screening Inventory (SASSI), while others use simple yes/no questions about drug use. In other states, caseworkers and agency employees may establish reasonable suspicion through observations or other information obtained during the application process. Under either approach, applicants may be required to undergo drug testing if reasonable suspicion of drug use is established.

Importantly, states impose consequences for those who test positive after a drug test or refuse to be tested. Some states permit individuals to continue receiving benefits conditional on participation in substance abuse treatment. Others impose periods of ineligibility before applicants may reapply, coupled with participation in or completion of treatment. Refusal to undergo required testing or failure to comply with treatment requirements can also result in the suspension or loss of benefits. Individuals may face substantial consequences for knowingly providing false information on government benefits applications, including fines, repayment of benefits, disqualification from future assistance, and even criminal prosecution. Consequently, applicants may be discouraged from misrepresenting their drug use on screening questionnaires.

2.2 Data

We use annual state-level data from 2000 to 2019 to examine the impact of TANF drug testing policies on public assistance participation. Specifically, we measure TANF caseloads from the Office of Family Assistance's public records on the number of families receiving TANF

benefits each year, the number of enrolled adults and children, and the racial composition of enrolles.[3] We also consider SNAP caseloads to measure potential spillover effects across programs, with annual state-level SNAP caseloads obtained from the Food and Nutrition Service's SNAP Program Data portal.[4]

Population estimates are drawn from the U.S. Census Bureau's annual state population files, while household counts are proxied by the number of tax returns filed per year from the IRS Statistics of Income (SOI) data series.[5] We use state population estimates by race from the National Center for Health Statistics, which includes four racial categories (White, Black, Asian, and Native American), and a separate measure for Hispanic ethnicity. This combination allows us to distinguish between individual-level and household-level participation effects when analyzing potential differential effects on participation by race and for adults and children.

We build several measures of public assistance take-up. Our primary outcome measures enrolled TANF families per 100,000 households, calculated as the average number of families receiving TANF assistance divided by the number of household tax filings per 100,000. We construct an analogous outcome for SNAP using the average annual number of participating households. We also examine TANF participation separately for adults and children, measured as the number of recipients divided by the corresponding state population per 100,000 adults and children, respectively. Across all outcomes, we use the log transformation, allowing us to estimate proportional changes in program participation across states and over time.

We also measure TANF participation across four racial and ethnic groups: White, Black, Hispanic, and Other. The underlying data distinguish White, Black, Hispanic, Native American, Hawaiian, Asian, Multiracial, and Unknown recipients. However, the first three groups account for the vast majority of recipients, representing 93.5% of adult TANF recipients

[3] `https://acf.gov/ofa/programs/tanf/data-reports`.

[4] `https://www.fns.usda.gov/pd/supplemental-nutrition-assistance-program-snap`

[5] IRS Data Book Table 3

in 2010. Additionally, some states report zero recipients in smaller racial categories in certain years, which poses challenges when using the log transformation (CITE ROTH). We therefore combine the remaining categories into an Other group, measured as total recipients minus White, Black, and Hispanic recipients. Importantly, these data are reported by fiscal year rather than calendar year; for example, the fiscal year 2010 covers October 2009 through September 2010. We measure each outcome as the number of recipients divided by the corresponding racial or ethnic group's state population per 100,000, with the population for the Other group constructed by subtracting the White, Black, and Hispanic populations from the total population.

Next, we supplement this data with a combination of national reports and original research to identify the passage and effective dates of drug testing policies targeting TANF recipients, National sources include the Congressional Research Service report, *Drug Testing and Crime-Related Restrictions in TANF, SNAP, and Housing Assistance*, and the 2019 report by the Center for Law and Social Policy (CLASP).[6] To improve the precision of announcement timing, we supplement these sources with local news coverage from the NewsLibrary archive to identify when a drug testing proposal first appeared in the local media. Our primary treatment timing is based on the passage date of a drug testing policy. This ensures that the treatment reflects the first moment when households and policy actors would have been impacted by eligibility changes or increased monitoring. When the CRS and CLASP reports conflicted, or if those reports conflicted with local news coverage, we deferred to local media coverage as the primary indicator of treatment onset.

Appendix Table A1 provides the month and year of each policy enactment, as well as its effective date. For Georgia, we did not identify an effective date from the data. In general, most policies were enacted between 2009 and 2015. We use the enactment date as our primary treatment event, as it is plausible that TANF recipients preemptively responded to coverage of legislation prior to its effective date. In Section 4, we show results are comparable

[6] `https://www.clasp.org/wp-content/uploads/2022/01/2019_drug-testing-and-public-_0.pdf`

when using the effective date as our treatment event. In Florida, Michigan, and Oklahoma, we identified policies from both states that were effective in the late 1990s but subsequently repealed. We later show the results are robust to dropping these three states. Notably, most states adopted some type of policy between 2009 and 2015. As discussed in Section 3, these are the states we will primarily analyze given our identification strategy.

3 Methodology

The staggered rollout of policies across states provide natural experiments to study the effect of drug testing policies on access to public programs. We estimate the treatment effect on the treated, hereafter referred to as the effect, using a stacked difference-in-differences framework, following Cengiz et al. (2019); Wing, Freedman, and Hollingsworth (2024). This method improves upon traditional staggered treatment designs by avoiding negative weighting and implicit comparisons across already-treated units. The core idea is to treat each policy adoption as its own event and construct event-specific control groups from states that have not adopted a TANF drug testing policy.

We first define notation for our model. Let t index the year and T_s denote the enactment year for state s. States that share the same treatment year are grouped into a treatment cohort c, such that all treated states in cohort c satisfy $T_s = T_c$, where T_c is the common enactment year. For each cohort, we examine an event window $t \in [T_c - \kappa_{pre}, T_c + \kappa_{post}]$, where κ_{pre} and κ_{post} denote the number of years before and after the policy enactment, respectively. We set $\kappa_{pre} = 5$ and $\kappa_{post} = 4$, which provides five pre-treatment years and five post-treatment years when including the enactment year. For each treatment cohort c, we construct a sub-experiment comparing treated states to states that remain untreated through year $T_c + \kappa_{post}$. These sub-experiments are then stacked to form a pooled dataset, with event time defined relative to each cohort's enactment year ($\tau = t - T_c$).

Formally, we estimate the following specification:

$$Y_{cst} = \beta_1 Post_{cst} + \gamma_{cs} + \delta_{ct} + \epsilon_{cst}.$$

where Y_{cst} is the outcome of interest (e.g., log TANF caseloads per 100,000 households) for state s in year t and cohort c. $Post_{cst}$ is an indicator variable equal to one for treated states in cohort c during years $t \geq T_c$, and zero otherwise. γ_{cs} denotes cohort by state fixed effects, which capture time-invariant characteristics of states within each cohort. δ_{ct} represents cohort by event-time fixed effects, which absorb time-specific shocks common to all states within a cohort. In our preferred specification, we include cohort by Census-region by event-time fixed effects, denoted by λ_{crt}, to account for regional shocks that vary over time. β_1 estimates the average treatment effect on treated states over the post-period. Standard errors are clustered at the state level to account for correlation within states over time and across sub-experiments.

The identifying assumptions for our model are that treated states would have experienced similar changes in caseloads as untreated states in the absence of a drug testing policy (parallel trends), treated states did not begin changing their behavior prior to the policy change (no anticipation), and untreated states are not affected by treatment in other states (stable unit treatment value assumption). We believe potential SUTVA violations are a second-order concern, as cross-state spillovers are less likely given that TANF and SNAP eligibility is tied to state residence. Furthermore, we assess the plausibility of the parallel trends assumption by visualizing dynamic treatment effects of the policy. Specifically, we estimate the following:

$$Y_{cst} = \sum_{\substack{\tau=-5 \\ \tau \neq -1}}^{4} \beta_\tau \Big[Treat_{cs} \times \mathbb{1}[t - T_c = \tau] \Big] + \gamma_{cs} + \lambda_{crt} + \epsilon_{cst}. \tag{1}$$

with similar indexing to Equation (1). Here, $Treat_{cs}$ indicates that state s is treated in cohort c, and τ denotes the year relative to the enactment year for cohort c within a given cohort. Each β_τ coefficient reflects the treatment effect at event time τ relative to $t = -1$,

the year prior to adoption. While the parallel trends assumption is not directly testable, estimates of β_τ from $\tau = -5$ to -2 that are near zero provide suggestive evidence that treated and control states were on similar trends prior to policy enactment.

4 Results

4.1 Main results

We start by estimating the effect of TANF drug testing policies on the number of families receiving TANF assistance per 100,000 households, our primary outcome. Figure 1a plots the dynamic coefficients from our preferred model, which includes cohort by Census region by event-time fixed effects, as described in Equation (2). The coefficients in the five years preceding the enactment of a TANF drug policy appear stable and close to zero, consistent with our identifying assumption that treated states and control cities trended similarly within Census regions prior to the enactment. In the year of the policy adoption, there is approximately a 9.5% decrease in the TANF caseload per 100,000 households. This effect becomes larger in the following years after the enactment date, stabilizing at around a 22% decrease.

Table 2 presents the average treatment effects on the treated over the five-year post period. Column 1 is the baseline specification, which includes cohort by state and cohort by event-time fixed effects, and estimates a 21.8% reduction in TANF assistance per 100,000 households. Our preferred specification with cohort by Census region by event-time fixed effects is presented in Column (2), where the effect size slightly grows to a 21.9% decrease. In Column 3, we test for divergence in pretrends between treated and control states by including an indicator for the two years prior to the policy for treated states, and the results remain unchanged.

Next, we assess whether public assistance participation differs across age, specifically between adults and minor children enrolled in TANF. Officially, drug testing policies should

only be applicable to adults. However, if adults enroll both themselves and their children when applying for public assistance, both groups may be affected by drug testing policies. We first present the dynamic effects on adult enrollees and child enrollees in Figure 1. For adults, there is a slight upward trend prior to adoption, but a large decrease in the year of adoption and the years following. Similarly, the pre-period estimates for take-up among children are also relatively flat prior to the policy adoption, with a large reduction in the post-period.

Table 2 presents the estimates for these two outcomes. Again, using Column (2) as our preferred specification, there is a slightly larger 24.0% decrease among adults compared to a 21.1% decrease among children. The results remain similar and statistically significant across Columns (1) and (3). Despite children being exempt from drug testing policies, our results suggest there are substantial spillovers to the enrollment of children in TANF. Given similar findings related to the expansion of Medicaid eligigility to already eligible children (Hudson and Moriya, 2017) or household spillovers for SNAP and TANF work requirements (Richard and Bart, 2026; Cook and East, 2026), this result is perhaps not surprising.

We also assess differential effects by race, given notably distinctive experiences by race in the public welfare system and the disproportionate share of Black enrollees represented in the TANF population (Table 1). Additionally, Black individuals are overrepresented in the criminal justice system (Finlay, Mueller-Smith, and Street, 2023), perhaps in part by systematic racial biases (Tuttle, 2026; Sloan, 2026). We examine four racial groups: White, Black, Hispanic, and recipients of any other race. Table 3 provides the estimates using Equation (1) with cohort by region by event-time fixed effects (analogous to Column (2) in Table 2). We find the strongest effects among White recipients, with an estimated 21.8% reduction, and comparable 17.0% and 20.1% decreases among Black and Hispanic recipients. The dynamic treatment effects are presented in Appendix Figure A2. Across White, Black, and Hispanic populations, the dynamic treatment effects prior to adoption are near zero and smooth, with large reductions in the post-period. For the remaining race population,

however, the treatment effects remain near zero but noisy.

Across racial and ethnic groups, the estimated treatment effects are generally smaller in magnitude than the effects for households, adults, and children. One explanation is that race-specific enrollment data are reported by fiscal year, which runs from October of the preceding calendar year through September of the current calendar year. For example, the reported White enrollment in 2015 would capture the average enrollment from October 2014 through September 2015. As shown in Figure 1, the estimated dynamic treatment effects become larger in magnitude in the years following policy adoption. Consequently, because fiscal-year measures partially capture participation in the prior calendar year, the estimated effects may be attenuated, including the average effect over the first five years from the policy adoption. Regardless, the effect sizes by White, Black, and Hispanic are still comparable to the prior estimates and meaningfully large.

Last, we assess spillovers to other public assistance programs, namely SNAP. While SNAP was exempt from drug testing policies, some states unsuccessfully attempted to pass similar policies alongside TANF drug testing policies.[7]. Furthermore, many states allow for automatic eligibility for SNAP once enrolled in TANF, creating natural logistical spillovers to changes in application barriers. Figure 1d presents the dynamic treatment effects for families enrolled in SNAP per 100K households. Although none of the treatment effects in the pre-period are statistically significant, there may be an increasing trend prior to the policy adoption. In the years after the enactment, the treatment effects are largely negative, but none are statistically significant. Column (2) of Table 2 presents our preferred estimate, which is a statistically insignificant 3.0% decrease in take-up of SNAP.

In total, we find meaningful results that the enactment of TANF drug testing policies decrease the number of families enrolled in TANF per 100,000 households. These effects persist among both adults and children, despite children not being targeted by the policy

[7]Georgia passed a law to drug test for SNAP in 2014, Wisconsin's Attorney General sued for clarification on drug testing for SNAP in 2015, and Kentucky brought drug testing law to the floor in 2019, but it did not passAssociated Press (2014); FOX6 Milwaukee (2015); Kentucky General Assembly (2019)

design. By race, we find the strongest decreases among White recipients. We next assess the sensitivity of these results.

4.2 Sensitivity

We first examine sensitivity for our four primary outcomes. Primarily, the results may depend on how we define treatment of drug testing policies, and they may also depend on model specification. We extend this analysis to our constructed race outcomes. Last, our study also centers on analyzing fourteen different state policies, which are not all uniformly the same, so we examine the effects when dropping one treated state at a time.

Figure 2 presents treatment effects using alternative specifications for our four primary outcomes. First, we report the treatment effect when interacting the first year of treatment with the share of coverage in the given year (i.e., how many months of the year since the policy had been passed). As expected, the treatment effects grow slightly larger under this specification. Next, we use the effective date to classify treatment status, as our primary treatment variable uses the passage date, and the effect sizes increase in magnitude.

We then examine whether our results are sensitive to model specification, particularly for addressing the staggered adoption of state policies. First, we apply the weighting procedure proposed in Wing, Freedman, and Hollingsworth (2024) to account for different compositions of control states across sub-experiments in a stacked difference-in-differences design, and our estimates remain unchanged. We then use the Callaway and SantAnna (2021) estimator as an alternative method of estimating effects in a staggered adoption design, and our results remain similar. Additionally, when using this estimator, there is a statistically significant 8.1% decrease in SNAP enrollment per 100,000 households, which may provide suggestive evidence of spillover effects onto other assistance programs. Last, we compare our primary estimates to a basic two-way fixed effects (TWFE) specification, as Hull (2026) finds TWFE estimates are highly positively correlated with modern difference-in-difference estimates. The TWFE estimates are somewhat larger in magnitude but all have the same sign compared to

our preferred stacked DiD estimates.

We provide an analogous comparison for estimates by race in Appendix Figure A3. The estimates remain similar and statistically significant for both the White and Black subpopulations. The estimated effects on Hispanics remain comparable, though the results vary in statistical significance. Again, we find mostly null but noisy results across the combined group of other races.

Appendix Figures A4 and A5 provide the dynamic treatment effects across alternative model specifications for our primary outcomes of interest and TANF caseload by race, respectively. Looking first at our primary outcomes of interest, the dynamic treatment effects at each event-time remain quite similar, and no estimate is statistically significantly different than the enactment date treatment effect. Similarly, the dynamic treatment effects for White and Black are similar across model specifications. The estimated treatment effects for Hispanic and Other groups are similar, though the treatment effects three years after the effective date diverge somewhat. In total, the dynamic treatment effects demonstrate that across alternative specifications, we still examine smooth pre-trends prior to the treatment date and a persistent decline in the post-period.

Furthermore, we assess sensitivity to dropping one treated state at a time for our four primary outcomes in Appendix Figure A6. While most policies have similar language, we do examine policies that were short-lived or revoked, and in the case of Florida, the policy outright tested all applicants. For TANF families enrolled per 100,000 households and TANF participation per 100,000 children, all estimates remain statistically significant and comparable to the respective primary estimate. For adults, the estimates remain mostly statistically significant but become marginally significant when dropping Arizona and Utah. Last, all leave-one-out estimates are statistically insignificant when examining SNAP families enrolled per 100,000 households. Additionally, three states—Florida, Michigan, and Oklahoma– adopted some type of drug screening or testing policy in the 1990s that was soon after repealed or revoked, and our estimates remain similar when dropping each state

at a time. In total, the exercise provides strong evidence that not one state is driving the reported aggregate treatment effect.

Last, we can use the estimates from our sensitivity checks to gauge a range of effect sizes. In our baseline specification using the enactment date for treatment, we estimate a 21.9% reduction in TANF families per 100,000 households. Our smallest estimated reduction is 21.5% when using stacked weights, and our largest is 28.2% under the TWFE specification. Thus, our preferred specification can be interpreted as a more conservative estimated effect of drug testing policies on TANF participation.

5 Conclusion and discussion

Many states have added barriers to receiving public assistance with the goal of targeting only those most deserving or most in need of benefits. Our paper is the first, to our knowledge to document the effects of drug-screening policies on TANF caseloads. Using the staggered rollout of drug screening policies across states, we document a 21.9% decrease in families enrolled in TANF per 100,000 households. We find similar effects among adults and children, with 24.0% and 21.1% decreases, respectively. Thus, while these policies were aimed at reducing participation among potential drug users, they produced a meaningful spillover effect on children. We also find comparable results across racial and ethnic groups, with 21.8%, 17.0%, and 20.1% reductions in participation among White, Black, and Hispanic recipients, respectively. Further, our results are robust to several estimation methods and classifications of policy timing.

While the estimated effect sizes are quite large, we believe they are reasonable compared to other findings on public assistance barriers. For example, prior research finds that work requirements generate substantial reductions in SNAP participation, ranging from 10% to 64% decreases.[8] A separate consideration is whether drug screening policies achieve their intended purpose of reducing public assistance participation among active drug users. We

[8]See Gray et al. (2023); Stacy, Scherpf, and Jo (2018); Vericker et al. (2023); Bauer and East (2025).

believe this is unlikely to explain our findings for two reasons. First, from the 2010 National Survey on Drug Use and Health, we find that only 12.4% of adults aged 18-64 living in households with children under 18 and reporting family income below $30,000 used illicit drugs in the preceding month (Substance Abuse and Mental Health Services Administration, 2015). Using this measure as a proxy for drug use among eligible TANF enrollees, even if all drug users were to disenroll due to drug testing policies, they would explain approximately half of the estimated 24% decrease among adults. Second, and perhaps most importantly, we find substantial reductions in TANF participation among children, who were not directly subject to drug screening requirements. Taken together, these findings suggest that the effects of drug screening policies extend beyond individuals directly disqualified for drug use.

Instead, the decline in participation may be attributed to several factors, such as confusion over the policy requirements and additional administrative burdens. For example, news coverage in Missouri around the time of its policy adoption described the legislation as "requir[ing] drug testing for all work-eligible welfare recipients," despite the law only requiring drug testing when screening established reasonable cause of drug use (St. Louis Public Radio, 2011). Additionally, screening questionnaires and the possibility of subsequent testing may increase the time, effort, and uncertainty associated with applying for benefits, which could further discourage participation.

Altogether, our findings suggest that drug screening and testing policies have meaningfully reduced TANF participation, including among children who are not directly subject to these requirements. While our analysis cannot directly identify mechanisms driving these reductions, the magnitude of the effects suggests that the consequences of these policies extends beyond excluding active drug users. Given that many of the policies observed are still in place, our findings have important implications for how states design eligibility for public assistance.

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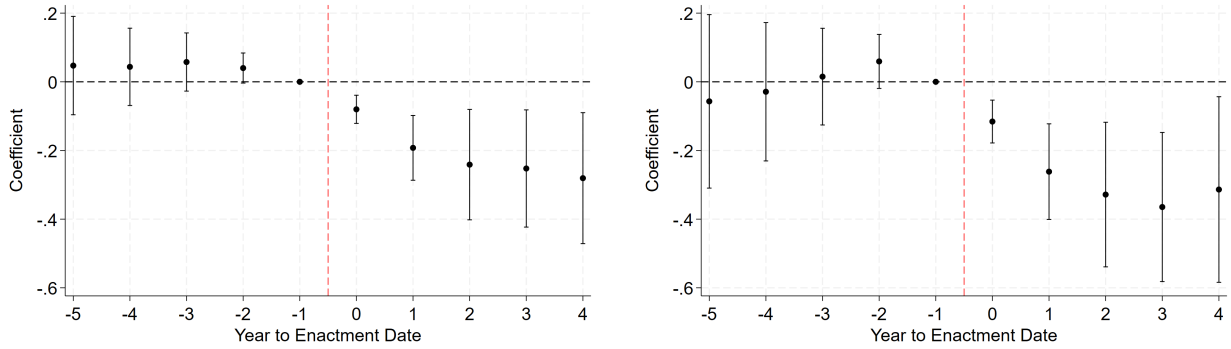
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6 Figures and Tables

Figure 1: Dynamic DiD plots of primary outcomes

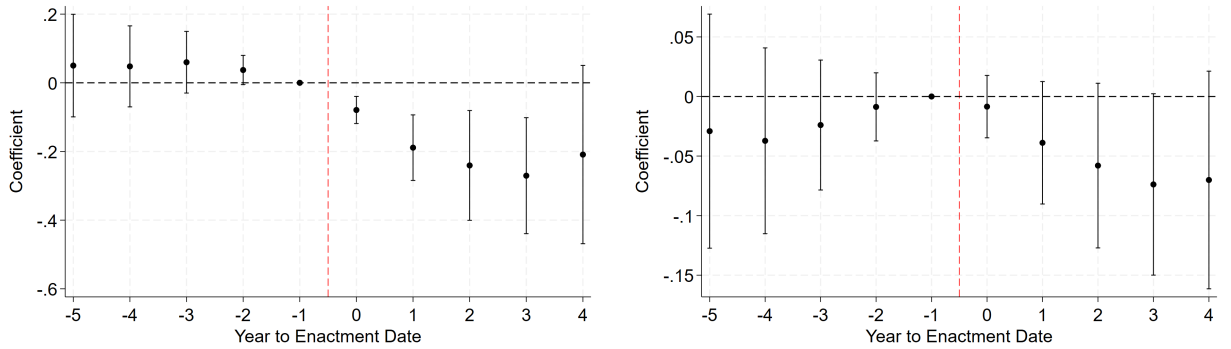
(a) log(TANF/100K HH)

(b) log(TANF adult recipients / 100K)



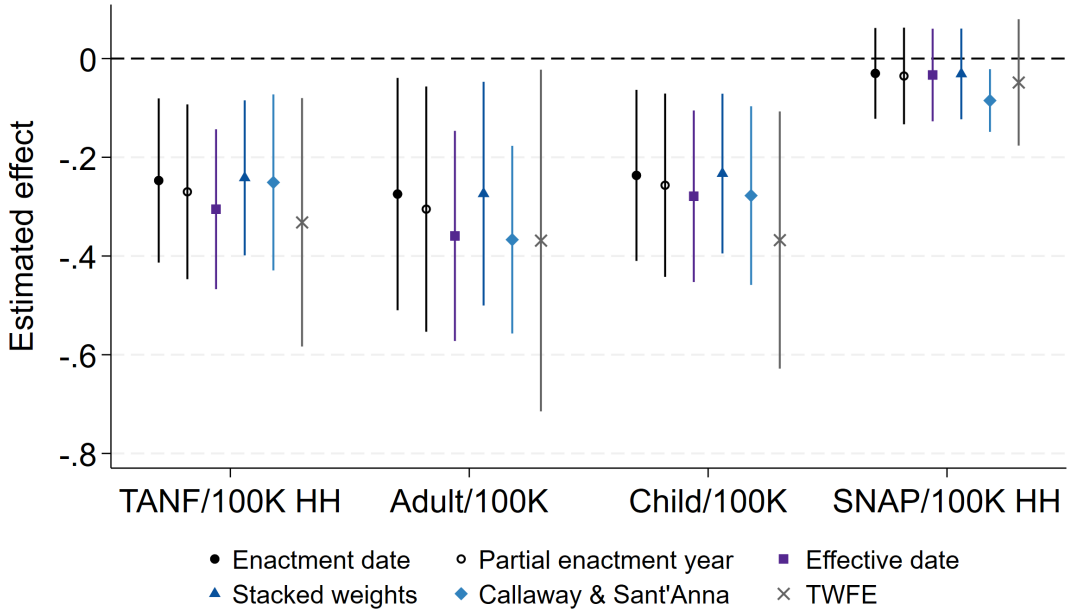
(c) log(TANF child recipients / 100K)

(d) log(SNAP/100K HH)



Notes: The unit of observation is a state-year. The plotted stacked difference-in-differences estimates include cohort by state and cohort by Census-region by event-time fixed effects, as described in Equation 2. Standard errors are clustered by state. 95 percent confidence intervals are shown. TANF/ 100K HH denotes families enrolled in TANF per 100,000 households. TANF adult receipients / 100K denotes adult enrollees in TANF per 100,000 adults. TANF child recipients / 100K denotes child enrolles in TANF per 100,000 children. SNAP/ 100K HH denotes families enrolled in SNAP per 100,000 households. All measures are expressed using the log transformation. Caseload data are collected from the Office of Family Assistance and the Food and Nutrition Service's SNAP Program Data. Household counts are approximated by tax returns from IRS Statistics of Income, and population estimates by age are drawn from the U.S. Censes Bureau's annual population files.

Figure 2: Sensitivity to alternative specifications



Notes: The unit of observation is a state-year. TANF/ 100K HH denotes families enrolled in TANF per 100,000 households. TANF adult receipients / 100K denotes adult enrollees in TANF per 100,000 adults. TANF child recipients / 100K denotes child enrolles in TANF per 100,000 children. SNAP/ 100K HH denotes families enrolled in SNAP per 100,000 households. All measures are expressed using the log transformation. Enactment date refers to our primary specification using Equation (1) with cohort by region by event-time fixed effects and treatment defined by the enactment date. Partial enactment year interacts treatment with the months of the year the policy has been enacted. Effective date uses the policy effective date as the defined treatment date. Stacked weights incorporates the weighting procedure proposed by Wing, Freedman, and Hollingsworth (2024). Callaway & Sant'Anna uses the Callaway and SantAnna (2021) estimator. TWFE uses the entire sample with state and Census region by year fixed effects. 95 percent confidence intervals are shown.

Table 1: Summary statistics of treated and control states

	Treated		Control	
	Pre-Period	Post-Period	Pre-Period	Post-Period
Population (millions)	6.548	6.816	6.263	6.493
	(4.174)	(4.432)	(7.752)	(8.115)
Share adult	0.754	0.762	0.765	0.773
	(0.022)	(0.023)	(0.016)	(0.017)
Share Black	0.156	0.159	0.081	0.085
	(0.103)	(0.104)	(0.073)	(0.074)
Share White	0.713	0.697	0.731	0.713
	(0.094)	(0.098)	(0.174)	(0.177)
TANF: Child share of caseload	0.756	0.787	0.746	0.760
	(0.069)	(0.062)	(0.055)	(0.063)
TANF: Adult share of caseload	0.244	0.213	0.254	0.240
	(0.069)	(0.062)	(0.055)	(0.063)
TANF: Black share of caseload	0.483	0.461	0.268	0.258
	(0.222)	(0.211)	(0.230)	(0.214)
TANF: White share of caseload	0.342	0.356	0.391	0.393
	(0.133)	(0.139)	(0.233)	(0.235)
TANF cases per 100,000 households	945	586	1102	882
	(480)	(368)	(586)	(509)
TANF adult recipients per 100,000	339	178	434	330
	(232)	(159)	(246)	(219)
TANF child recipients per 100,000	2993	1905	3836	3160
	(1687)	(1252)	(1974)	(1754)
SNAP cases per 100,000 households	14547	16137	10852	13419
	(4893)	(4214)	(4436)	(4445)
Observations	70	70	1000	1000

Notes: The unit of observation is a state-year. The table presents sample means for states adopting TANF drug screening and testing laws and control states, separately for the pre-treatment and post-treatment periods. The sample is constructed using the stacked difference-in-differences framework, with each cohort centered on the year of policy enactment. The pre-treatment period includes five years preceding enactment, while the post-treatment period includes the year of enactment and three subsequent years. Control states are assigned the event-time of the corresponding treated cohort, and a state-year observation may appear in multiple cohorts. TANF caseload measures are collected from the Office of Family Assistance, and SNAP household counts are collected from the Food and Nutrition Service. Household counts are approximated using IRS Statistics of Income tax returns. Population and demographic measures are calculated from annual population estimates.

Table 2: Effects on TANF and SNAP take-up

	(1)	(2)	(3)
log(TANF/100K HHs)	-0.2462***	-0.2471***	-0.2588**
	(0.0763)	(0.0826)	(0.0966)
log(TANF adult recipients per 100K)	-0.2981***	-0.2745**	-0.2532*
	(0.1075)	(0.1169)	(0.1418)
log(TANF child recipients per 100K)	-0.2456***	-0.2367***	-0.2503**
	(0.0784)	(0.0861)	(0.1008)
log(SNAP/100K HHs)	-0.0537	-0.0301	-0.0198
	(0.0405)	(0.0457)	(0.0559)
Cohort × State FE	Y	Y	Y
Cohort × Year FE	Y	Y	Y
Cohort × Region × Year FE	N	Y	Y
1-2 Years Prior Indicator	N	N	Y
N	2140	2140	2140

Notes: The unit of observation is a state-year. The table reports average treatment effects of TANF drug screening and testing laws estimated using the stacked difference-in-differences model described in Equation (1). Column (1) includes cohort by state and cohort by event-time fixed effects. Column (2) additionally includes cohort by Census region by event-time fixed effects and represents the preferred specification. Column (3) adds an indicator for treated states in the one to two years preceding policy enactment. Standard errors are reported in parentheses and clustered by state. TANF outcomes are measured as families per 100,000 households, adult recipients per 100,000 adults, and child recipients per 100,000 children. SNAP participation is measured as enrolled families per 100,000 households. See Table 1 for data sources and additional information on the outcome measures. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: TANF recipients by race

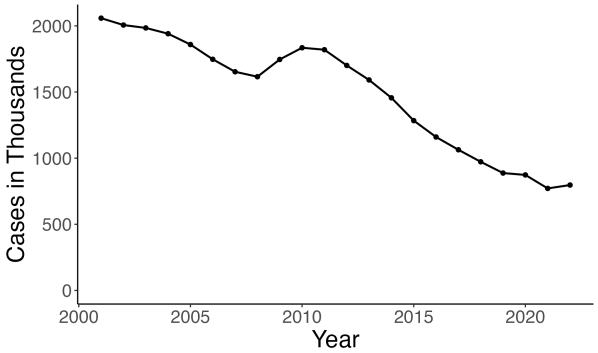
	(1) White	(2) Black	(3) Hispanic	(4) Other
log(TANF recipients per 100K)	-0.2463**	-0.1867**	-0.2238**	-0.0228
	(0.1003)	(0.0823)	(0.1112)	(0.1948)
Event Year by Cohort FE	Y	Y	Y	Y
State by Cohort FE	Y	Y	Y	Y
Region x Cohort x Event Year FE	Y	Y	Y	Y
N	2140	2140	2140	2140

Notes: The unit of observation is a state-year. The table reports average treatment effects of TANF drug screening and testing laws on TANF participation by race and ethnicity using the preferred stacked difference-in-differences specification described in Equation (1). The dependent variable is the log of TANF recipients per 100,000 individuals in the corresponding racial or ethnic population. Columns (1) through (4) present estimates for White, Black, Hispanic, and other race recipients, respectively. All specifications include cohort by state and cohort by Census region by event-time fixed effects. Standard errors are reported in parentheses and clustered by state. TANF recipient counts by race and ethnicity are collected from the Office of Family Assistance, with population denominators drawn from annual population estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

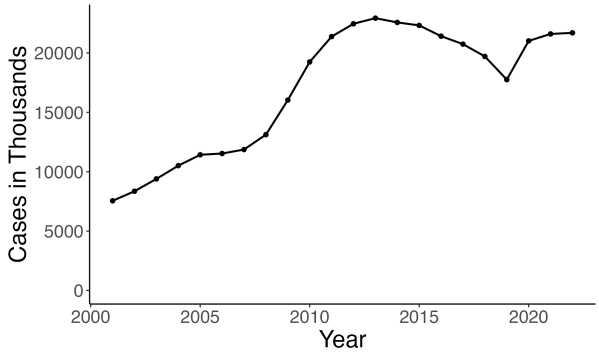
Appendix A Figures and Tables

Figure A1: National Trends in SNAP and TANF Cases

(a) TANF Cases Nationwide



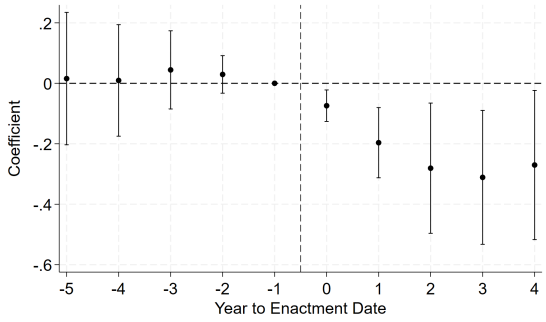
(b) SNAP Cases Nationwide



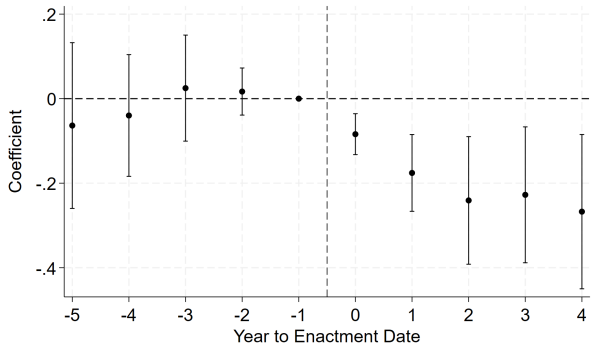
Notes: This figure plots the aggregate number of TANF and SNAP households nationally from 2001 to 2019. TANF households represent families receiving assistance under the Temporary Assistance for Needy Families program, as reported by the U.S. Office of Family Assistance. SNAP households represent participating households in the Supplemental Nutrition Assistance Program, as reported by the U.S. Department of Agriculture, Food and Nutrition Service. Each series is the sum across all states for each year.

Figure A2: Dynamic DiD plots of TANF caseload by race

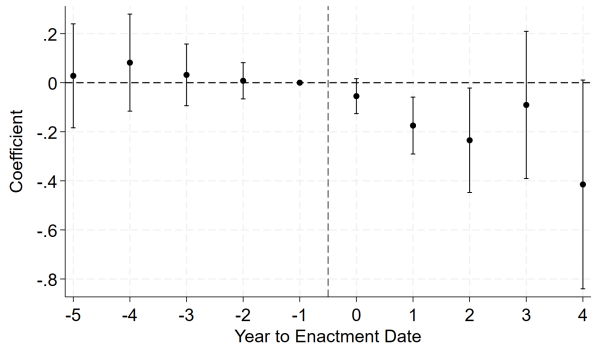
(a) White



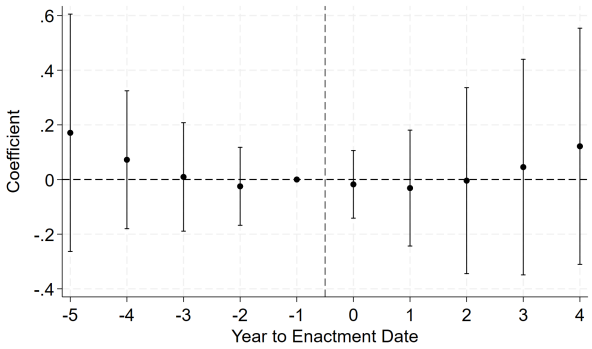
(b) Black



(c) Hispanic

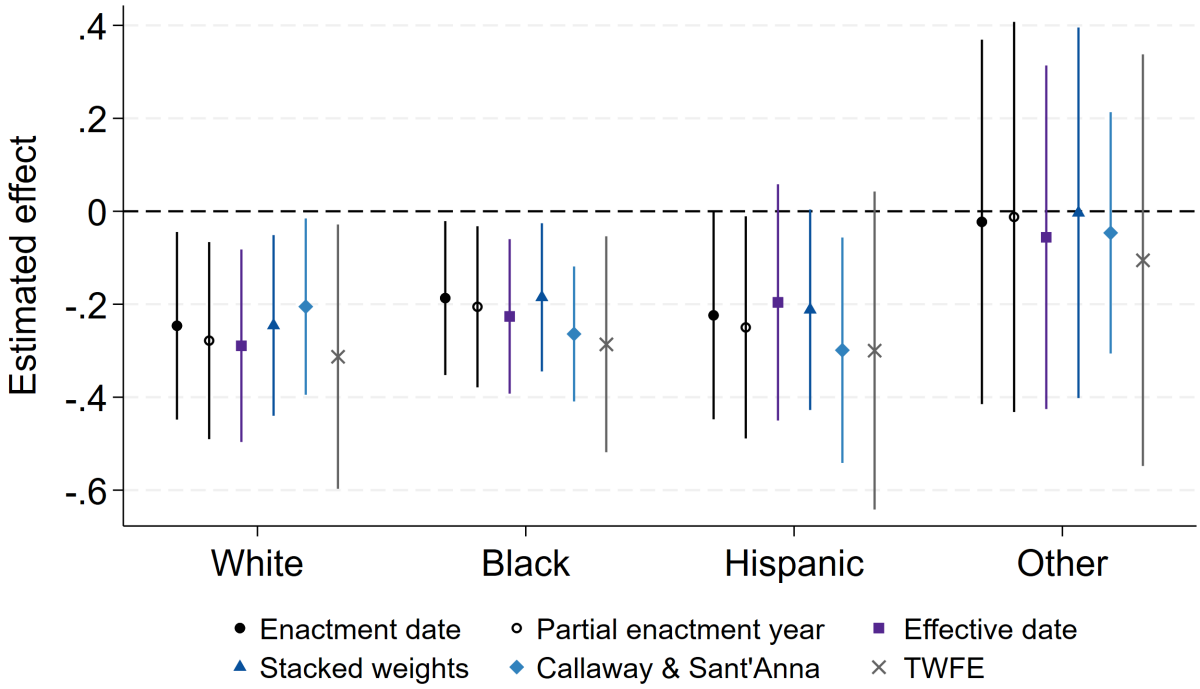


(d) Other



Notes: The unit of observation is a state-year. The figure presents dynamic treatment effects of TANF drug screening and testing laws on TANF participation by race and ethnicity using the stacked difference-in-differences model described in Equation (2). Each dependent variable is the log of TANF recipients per 100,000 individuals in the corresponding racial or ethnic population. All specifications include cohort by state and cohort by Census region by event-time fixed effects. Standard errors are clustered by state, and 95 percent confidence intervals are shown. TANF recipient counts are collected from the Office of Family Assistance.

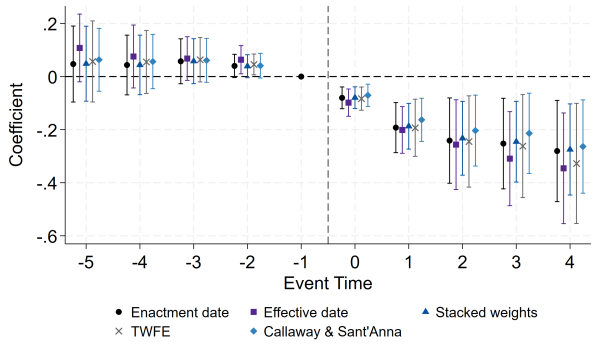
Figure A3: Sensitivity to alternative specifications for race estimates



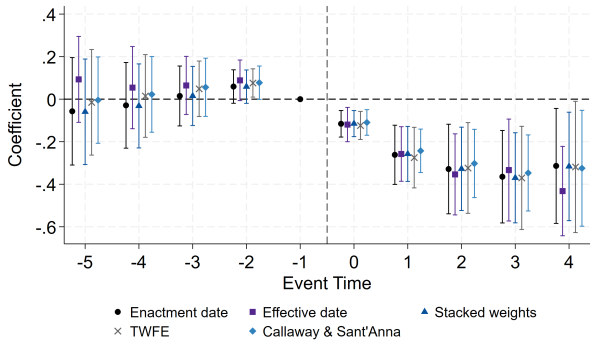
Notes: The unit of observation is a state-year. The dependent variables are the log of TANF recipients per 100,000 individuals in the corresponding racial or ethnic population. Enactment date refers to our primary specification using Equation (1) with cohort by region by event-time fixed effects and treatment defined by the enactment date. Partial enactment year interacts treatment with the months of the year the policy has been enacted. Effective date uses the policy effective date as the defined treatment date. Stacked weights incorporates the weighting procedure proposed by Wing, Freedman, and Hollingsworth (2024). Callaway & Sant'Anna uses the Callaway and SantAnna (2021) estimator. TWFE uses the entire sample with state and Census region by year fixed effects. 95 percent confidence intervals are shown.

Figure A4: Event studies for alternative specifications by main outcomes

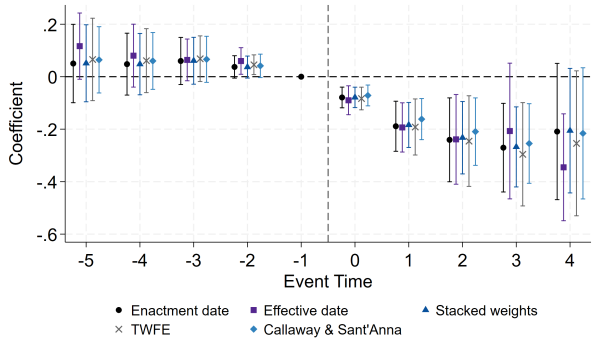
(a) log(TANF/100K HH)



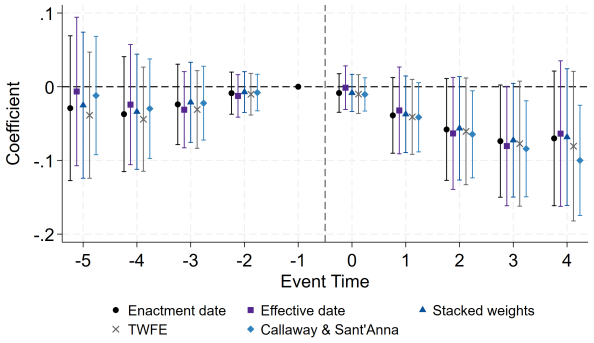
(b) log(TANF adults/100k)



(c) log(TANF child/100K)

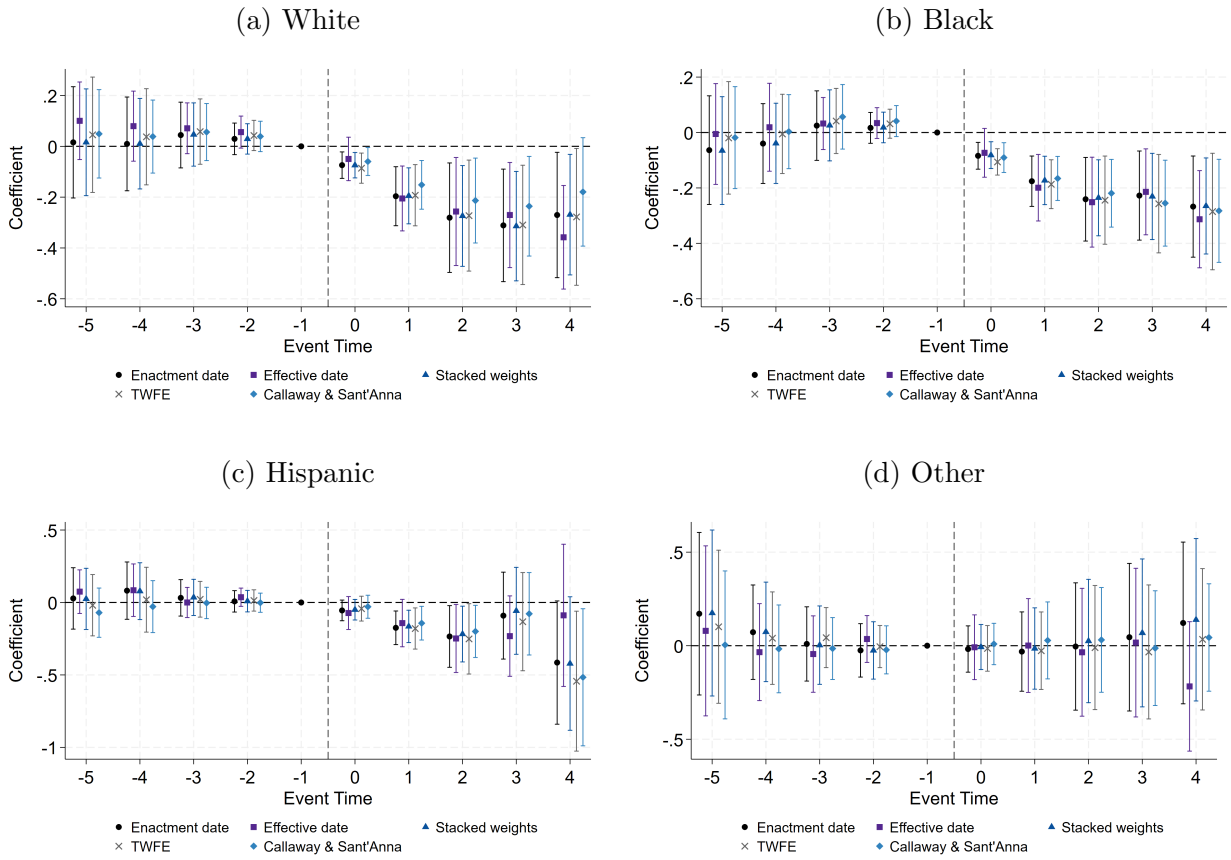


(d) log(SNAP/100K HH)



Notes: The unit of observation is a state-year. TANF/ 100K HH denotes families enrolled in TANF per 100,000 households. TANF adult receipients / 100K denotes adult enrollees in TANF per 100,000 adults. TANF child recipients / 100K denotes child enrolles in TANF per 100,000 children. SNAP/ 100K HH denotes families enrolled in SNAP per 100,000 households. All measures are expressed using the log transformation. Enactment date refers to our primary dynamic DiD specification using Equation (2) with cohort by region by event-time fixed effects and treatment defined by the enactment date. Effective date uses the policy effective date as the defined treatment date. Stacked weights incorporates the weighting procedure proposed by Wing, Freedman, and Hollingsworth (2024). Callaway & Sant'Anna uses the Callaway and SantAnna (2021) event study framework. TWFE uses the entire sample with state fixed effects and Census region by year fixed effects. Standard errors are clustered by state. 95 percent confidence intervals are shown.

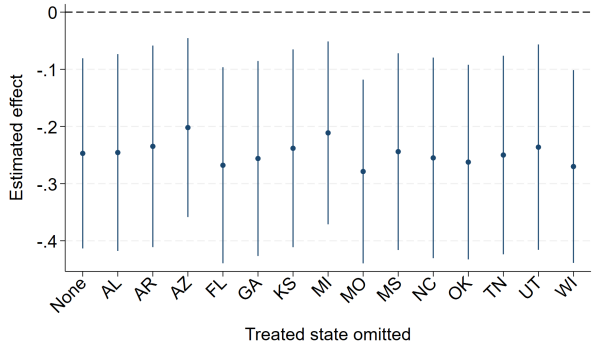
Figure A5: Event studies for alternative specifications by race



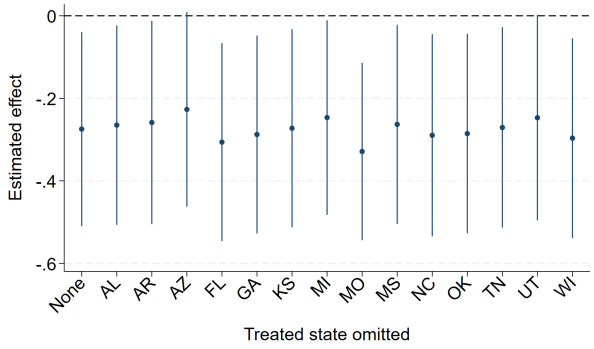
Notes: The unit of observation is a state-year. The dependent variables are the log of TANF recipients per 100,000 individuals in the corresponding racial or ethnic population. Enactment date refers to our primary dynamic DiD specification using Equation (2) with cohort by region by event-time fixed effects and treatment defined by the enactment date. Effective date uses the policy effective date as the defined treatment date. Stacked weights incorporates the weighting procedure proposed by Wing, Freedman, and Hollingsworth (2024). Callaway & Sant'Anna uses the Callaway and SantAnna (2021) event study framework. TWFE uses the entire sample with state fixed effects and Census region by year fixed effects. Standard errors are clustered by state. 95 percent confidence intervals are shown.

Figure A6: Leave-one-out analysis by primary outcome

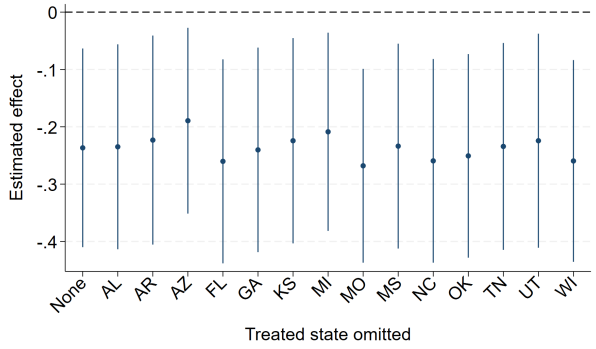
(a) log(TANF/100K HH)



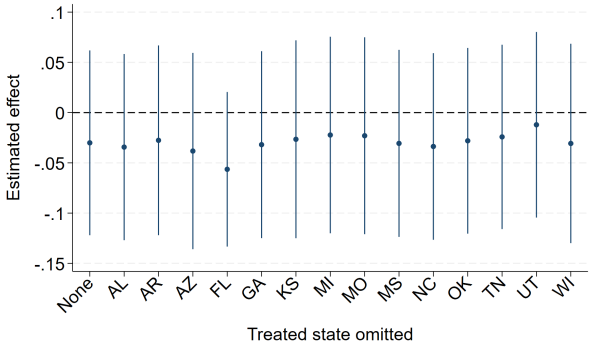
(b) log(Adults/100K)



(c) log(Child/100K)



(d) log(SNAP/100K HH)



Notes: The unit of observation is a state-year. TANF/ 100K HH denotes families enrolled in TANF per 100,000 households. TANF adult receipients / 100K denotes adult enrollees in TANF per 100,000 adults. TANF child recipients / 100K denotes child enrolles in TANF per 100,000 children. SNAP/ 100K HH denotes families enrolled in SNAP per 100,000 households. Our baseline specification using Equation (1) with cohort by Census region by event-time fixed effects and no dropped states is expressed as "None." The leave-one-out exercise drops each treated state entirely from the sample. Standard errors are clustered by state. 95 percent confidence intervals are shown.

Table A1: TANF drug-testing policy dates

State	Enactment date	Effective date
Alabama	04/2014	10/2015
Arizona	04/2009	11/2009
Arkansas	03/2015	04/2015
Florida	05/2011	07/2011
Georgia	04/2014	—
Idaho	04/2000	07/2001
Kansas	04/2013	07/2013
Michigan	12/2014	10/2015
Mississippi	03/2014	09/2014
Missouri	05/2011	03/2013
North Carolina	09/2013	08/2014
Oklahoma	05/2012	11/2012
Tennessee	05/2012	07/2014
Utah	03/2012	08/2012
West Virginia	03/2016	10/2017
Wisconsin	11/2015	07/2016

Notes: Policy data was gathered from McCarty et al. (2013) and Thompson (2019). Georgia has a confirmed enactment date, but we did not uncover any effective date for the policy; it is therefore used as a control state in all specifications using effective date as treatment. Louisiana and South Carolina adopted drug testing policies prior to the start of the sample and are dropped. Florida, Michigan, and Oklahoma both adopted drug screening policies prior to 2000 that subsequently ended within years and we consider the later adoption in our analysis.