Transaction Costs and the Take-up of Social Safety Net Programs: Evidence from the Combined Application Project

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Abstract

This paper studies the effect of transaction costs on the take-up and targeting of the Supplemental Nutrition Assistance Program (SNAP) by exploiting the Combined Application Project (CAP), a widespread state-level policy designed to encourage SNAP enrollment among elderly recipients of Supplemental Security Income. I show that the CAP increased SNAP take-up by 3.75 percentage points, or about 18%. The increase was suggestively larger among those with a higher probability of being food insecure. Of the various formats of the CAP, "auto-enrollment" into SNAP was most effective at increasing take-up.

Keywords: social safety net; Supplemental Nutrition Assistance Program; auto-enrollment

JEL codes: H53, I38, J14

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1 Introduction

Social safety net programs in the U.S. are known to suffer from incomplete take-up (Currie (2004), Ko and Moffitt (2022)). One explanation for why low-income Americans who are eligible for a program might fail to participate is that enrollment is not automatic: the application process for most programs involves transaction costs that could outweigh the expected benefits. Applicants might, for instance, incur monetary costs of traveling to a site to interview with a caseworker, face application forms that are time-consuming and complex, and endure social stigma (Hernanz et al. (2004)). Empirically, these various costs are difficult to distinguish.

The traditional view among the theoretical literature is that transaction costs are welfare-improving: such costs improve the targeting of safety net programs by screening out applicants who are less in-need of benefits (Nichols and Zeckhauser (1982), Besley and Coate (1992)). Work in behavioral economics, in contrast, generally argues that transaction costs are more likely to deter those most in-need of benefits from applying (Bertrand et al. (2004), Mani et al. (2013)). This tension is unresolved in the empirical literature. Some authors have found that higher transaction costs yield better targeting (e.g. Alatas et al. (2016) and Finkelstein and Notowidigdo (2019)), while others have shown the reverse to be true (e.g. Deshpande and Li (2019) and Arbogast et al. (2022)). Importantly, existing empirical work almost exclusively studies researcher-developed interventions or natural experiments in which administrative hassles were reduced by chance; little attention has been paid to the effect of policies implemented with the explicit intention of reducing transaction costs.

This paper studies a sample of elderly Supplemental Security Income (SSI) recipients[1] to evaluate the effect of transaction costs on the take-up and targeting of the Supplemental Nutrition Assistance Program (SNAP). Although SSI recipients are "categorically eligible" for SNAP[2] and food insecurity among this population is pervasive,[3] more than half of SSI recipients never enroll in SNAP. Between 1995-2012, eighteen U.S. states adopted a simplified process for enrolling SSI recipients in SNAP, called the Combined Application Project (CAP). The format of the CAP varies across states but most commonly takes one of two forms. The "Standard" CAP is a joint-filing procedure, by which individuals newly applying for SSI are simultaneously enrolled in SNAP if they agree to it and those already receiving SSI may be automatically enrolled in SNAP. The "Modified" CAP is an outreach program, by which SSI recipients are mailed simplified SNAP application forms. In either case, the CAP both reduces the complexity of the SNAP application process and waives the interview that is otherwise required as part of the application, saving the applicant both time and cognitive bandwidth.

I exploit the plausibly exogenous variation generated by the adoption of the CAP to evaluate whether

[1] I construct a target sample of "likely" SSI recipients, described in Section 3.2.

[2] That is, SSI recipients do not need to pass an income or assets test to be deemed eligible for SNAP. There are some less commonly applicable conditions that might still make an SSI recipient ineligible for SNAP, such as immigration status. See FNS (2021) for details.

[3] See Weinstein-Tull and Jones (2017) and Savin et al. (2021) for an overview. Food insecurity among seniors more generally is discussed further below.

the induced reduction in transaction costs increased the take-up of SNAP among SSI recipients ages 65 and older. To study whether the CAP improved or worsened the targeting of SNAP, I explore whether the effect of the CAP varied with recipients' predicted probability of food insecurity. My main source of data is the cross-sectional American Community Surveys (ACS) provided by IPUMS USA (Ruggles and Sobek (2022)).

My empirical strategy involves three distinct research designs that leverage variation across states, household types, and their interaction. In the first design, I restrict the sample to SSI households eligible for the CAP and use difference-in-differences where individuals residing in states that implemented the CAP are the treated units and those residing in states that did not are the control. I call this the "state-level" design. In the second, I restrict the sample to states that implemented the CAP and that limited eligibility to one-person SSI households, and I use difference-in-differences with single SSI recipients as the treated units and married SSI recipients as the control (the "singles-couples" design). In the third, I use a triple-difference estimator that compares the relative SNAP enrollment of single and couple SSI recipients living in states that adopted the CAP with the relative enrollment of single and couple SSI recipients living in states that did not adopt the CAP.

Recent developments have shown that two-way fixed effects (TWFE) estimates may be biased in the presence of heterogeneous treatment effects coupled with staggered treatment timing.[4] I account for this potential bias by complementing my TWFE estimates with the improved doubly-robust estimator in Callaway and Sant'Anna (2021). My findings are additionally supported by two placebo tests, using matched placebo "treated" states that never adopted the CAP, and enrollment in unemployment insurance rather than SNAP as the outcome variable, respectively. I also conduct two sets of robustness checks, one of which imposes a balanced panel restriction, and the other uses alternative definitions of the target sample of SSI recipients.

My average treatment effect estimates show that the introduction of the CAP caused a statistically significant, 3.75 percentage-point increase in SNAP take-up from a baseline rate of about 20%, which is a roughly 18 percent increase. This result is robust to the inclusion of individual- and state-level controls, where the latter includes controls for other SNAP-related policies aimed at simplifying the certification process. I find suggestive evidence – constrained by sample size and the limited granularity of individual-level characteristics in the data – that the increase in take-up was largest among those with a high probability of being food insecure, suggesting that the CAP improved the targeting efficiency of SNAP. More broadly, these results imply that transaction costs deter participation in SNAP, and that the burden of these costs may be more substantial for individuals most in-need of the benefits.

While there is agreement among the literature that transaction costs discourage the take-up of safety net programs, the specific channels through which these costs operate are less clear. To investigate the mechanisms by which the CAP increased SNAP take-up, I exploit variation in the format of the CAP across states to assess whether auto-enrollment, joint-enrollment, or outreach was most effective at improving SNAP take-up. I also explore whether the effectiveness of auto-enrollment into SNAP varied with the ease

[4]See Roth et al. (2022) for a review of this literature.

of accessing one's SNAP benefits, measured by the geographic accessibility of SNAP retailers: with data from the USDA, I construct a measure of the probability of living in a census tract with limited access to food stores that accept SNAP as payment, and I interact this measure with the main effect in my difference-in-differences analysis.

I find that the auto-enrollment version of the CAP – in which SSI recipients were automatically sent an Electronic Benefit Transfer card which they were simply required to activate by using it to purchase food – was most effective, suggesting an important role for inertia or default options in the decision about whether to enroll in SNAP. However, the effectiveness of auto-enrollment was constrained by the accessibility of SNAP retailers: SSI recipients living in areas in which they must travel far to reach the nearest food store were less likely to activate their SNAP benefits upon being auto-enrolled.

I make three contributions to the literature. Despite broad consensus among economists that transaction costs reduce the take-up of welfare programs (see Moffitt (1983) on stigma; Schanzenbach (2009) on application complexity; Homonoff and Somerville (2021) and Wu and Meyer (2021) on administrative burdens; and Ko and Moffitt (2022) for a review), most of the existing literature studies either researcher-developed interventions (such as Finkelstein and Notowidigdo (2019)) or variation not specifically intended to affect transaction costs (such as Deshpande and Li (2019)). I contribute to this literature by studying the effectiveness of a policy introduced on a national level and explicitly intended to reduce the costs of applying for SNAP. That the CAP took the form of automatic enrollment in many states also distinguishes it from researcher-developed interventions, which often take the form of application assistance or outreach. The behavioral effects of the CAP might be of interest in understanding the importance of default options outside the realm of welfare programs, in contexts such as retirement policy and health insurance (see Madrian and Shea (2001), Choukhmane (2019), and Choi et al. (2024) for discussions of default options in retirement savings behavior, and McIntyre et al. (2021) for the analog in health insurance enrollment).

The CAP has appeared sparsely in prior work on SNAP enrollment: a few existing papers include the CAP as one of several SNAP-related programs in the construction of a policy index to study trends in overall SNAP enrollment. Ganong and Liebman (2018) estimate the joint effect of the CAP and seven other state policies on county-level SNAP enrollment and find a positive and statistically significant effect of the eight policies combined. Jones et al. (2021) follow this approach[5] with eleven policies and additionally use fixed-effects regression to separately estimate the effect of each policy, in which they find that the CAP induced a marginally significant 1.3 percentage-point increase in SNAP enrollment among older Americans at or below 185% of the federal poverty line. I build on this literature by using a difference-in-differences design to estimate the causal effect of the CAP, and by restricting my sample to likely SSI recipients to isolate the impact of the CAP among those eligible.[6]

[5]In addition to Jones et al. (2021), several later papers follow Ganong and Liebman (2018)'s methods, creating a SNAP "policy-index" to study the effect of a number of pooled SNAP-related policies, of which one is the CAP. See Valizadeh et al. (2022) and Easterday and Ginther (2022).

[6]The consequences of the CAP have also been explored qualitatively in the sociology literature (Negoita et al. (2022)) and in various policy briefs (Dorn et al. (2014)), most of which document that SNAP enrollment increased in states that introduced

Second, my results on targeting speak to the ongoing debate in the empirical literature about the sign of the effect of transaction costs on the targeting efficiency of welfare programs (see Dupas et al. (2016) and Shepard and Wagner (2022) for examples outside the context of SNAP). I find suggestive evidence of a negative correlation between transaction costs and the targeting of SNAP, which contrasts with Finkelstein and Notowidigdo (2019) and also with much of the development literature on transfer programs (e.g. Alatas et al. (2016) and Dupas et al. (2016)).

My analysis of targeting efficiency explores heterogeneity in the effect of the CAP by level of need. However, the study population that I use throughout this paper – individuals ages 65 and older – is high-need in the first place, so the results on take-up are informative about the impact of transaction costs on those with a high marginal utility of consumption. Seniors are documented to be both particularly susceptible to food insecurity and unlikely to enroll in SNAP even if they are eligible: recent work has shown that as many as 22% of individuals experience food insecurity between ages 60-80, but the SNAP take-up rate among seniors is only half that of the general population (Levy (2022) and Heflin et al. (2023), respectively). The drivers of low SNAP take-up among seniors are not well understood, but there is evidence to suggest that hassles involved in the initial enrollment process are particularly burdensome for this population (Giordono et al. (2022)). My analysis generates new evidence on the channels through which the elderly remain unenrolled in SNAP, providing broader insights into the causes of food insecurity among this population.

Finally, this paper presents evidence on the mechanisms through which transaction costs impede the take-up of welfare programs. Despite the abundance of literature empirically evaluating the effect of transaction costs on take-up, fewer authors have studied the specific channels through which these costs discourage participation. Bhargava and Manoli (2015) use different treatment arms and survey evidence to decompose the costs of EITC receipt and find that increasing the salience of the program and clarifying beliefs about benefit size contribute more than reducing social stigma does to increases in take-up. Finkelstein and Notowidigdo (2019) similarly use different treatment arms to evaluate mechanisms and conclude that both information and assistance play important roles in increasing SNAP take-up, and Deshpande and Li (2019) find that increased congestion at the SSA offices that remain open is the main channel through which the closing of SSA offices reduces enrollment in disability benefits. I contribute to this literature by showing that default options are important in determining whether an individual chooses to enroll in SNAP, but that the effectiveness of auto-enrollment is constrained by the ease with which the enrollee can use their benefits.

The remainder of this paper proceeds as follows. Section 2 provides institutional background on the CAP. Section 3 describes my data sources and empirical strategy. Section 4 presents the results on take-up along with the related placebo tests and robustness checks, followed by the results on targeting. Section 5 explores the mechanisms by which the CAP increased SNAP take-up. Section 6 concludes.

the CAP.

2 Institutional Background

SNAP (formerly "food stamps") is a federal means-tested transfer program that provides low-income households with funds to purchase food. The program served 40 million Americans in 2010 at a cost of $78 billion. The average SNAP household in 2010 had an annual income of $8,800 (CBO (2012)). As part of the application process for SNAP, all applicants are required by federal law to interview with a SNAP caseworker, which generally necessitates that they appear in-person at a SNAP office (Giannella et al. (2022)).[7] Other details of the application process differ by state.

The SSI program is a monthly cash transfer distributed to Americans meeting both (1) income and asset limits and (2) age or disability criteria. An adult age 65 or older must possess no more than $2,000 in resources[8] to be eligible; couples applying jointly must possess no more than $3,000 (Duggan et al. (2015)). Given these stringent resource caps and the low benefits provided through the SSI program – the average benefit for elderly participants was just $426 per month in 2014[9] – many SSI recipients are susceptible to food insecurity (Savin et al. (2021)). Eligibility for SNAP among SSI recipients is categorical in most cases, in that anyone receiving SSI is guaranteed to qualify for SNAP on the basis of their level of income and assets. Nonetheless, the SNAP enrollment rate among likely SSI recipients ages 65 and older in my sample was around 20% at baseline (see Table 1)[10]. This remarkably low rate of take-up coupled with high rates of food insecurity motivates my analysis of the barriers to SNAP enrollment faced by this population.

A 1997 federal law requires that any individual applying for SSI at a Social Security Administration (SSA) office be given the opportunity to apply for SNAP at the same time. In practice, however, this is difficult to implement: caseworkers at the SSA office often lack the requisite information to file the SNAP application, and the applicant is still required to complete an interview with a SNAP caseworker (FNS (2005)). This entails future interactions between the applicant and the SNAP office, effectively making simultaneous application impossible.

The CAP seeks to resolve this issue, by providing a simplified process for enrolling SSI applicants in SNAP. To adopt the CAP, a state must apply for a series of waivers from the U.S. Department of Agriculture (USDA) allowing it to bypass the SNAP application and interview process for eligible SSI recipients.

While each state's program is slightly different, there are two main formats of the CAP: the Standard CAP and the Modified CAP. The Standard CAP is a joint-filing procedure: when an individual applies for SSI at an SSA office, the caseworker inquires whether the applicant would like to participate in SNAP. If the applicant agrees, their information is transmitted electronically to the state SNAP agency, and benefit amounts are calculated automatically. The applicant avoids having to present for a separate SNAP interview or have any direct contact with the SNAP agency. Individuals already receiving SSI who have yet to enroll

[7]By 2010, all U.S. states had received waivers from the USDA permitting SNAP interviews to take place by phone. Nine states received a waiver before 2009, only two of which are included in my sample of treated states under the singles-couples treatment design. I control for the receipt of these waivers in my regressions.

[8]Resources include cash, bank accounts, stocks, vehicles, land, and so on.

[9]See Duggan et al. (2015).

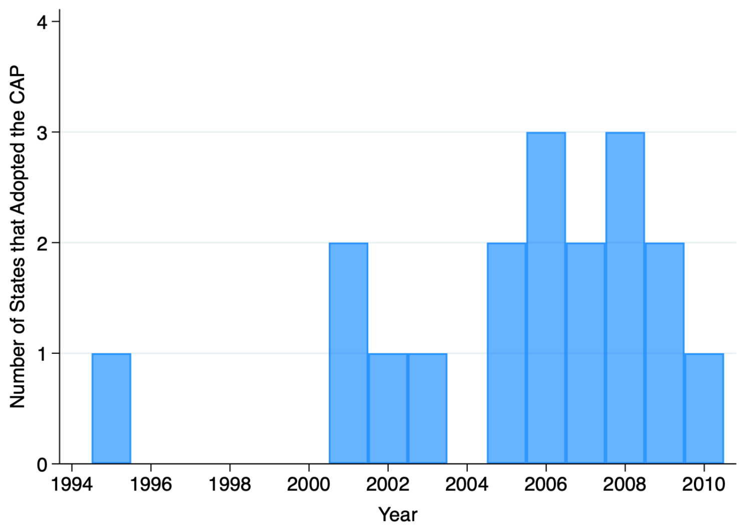
[10]See Section 3.4.1 for a comparison to SNAP participation rates in administrative data.

in SNAP at the time of the adoption of the CAP may receive a simplified form by mail – as in Mississippi – or are enrolled automatically – as in Massachusetts and Pennsylvania. One variation on the Standard CAP, implemented in New York, took the form of auto-enrollment for all SSI recipients: SNAP cases were automatically opened for eligible SSI recipients, each of whom was mailed an Electronic Benefit Transfer (EBT) card which they were simply required to activate – by using it in a store to purchase food – to begin receiving monthly benefits. Failure to activate the card within 90 days constituted opting out. The auto-enrollment of individuals already receiving SSI in Massachusetts and Pennsylvania was similar in process.

Under the Modified CAP, the state CAP agency requests contact information for new SSI recipients from the SSA and mails these individuals a simplified SNAP application form (FNS (2005), Weinstein-Tull and Jones (2017)). Some Modified CAP states, such as Maryland, re-send the form if the SSI recipient does not respond after some number of months. Like the Standard CAP, the Modified CAP also waives the interview requirement.

Eighteen states adopted the CAP between 1995-2012.[11] Figure 1 shows the distribution of years in which the CAP was adopted. Figure 2 shows which states adopted each format (Standard or Modified) of the CAP.

Figure 1: Distribution of CAP Adoption Years

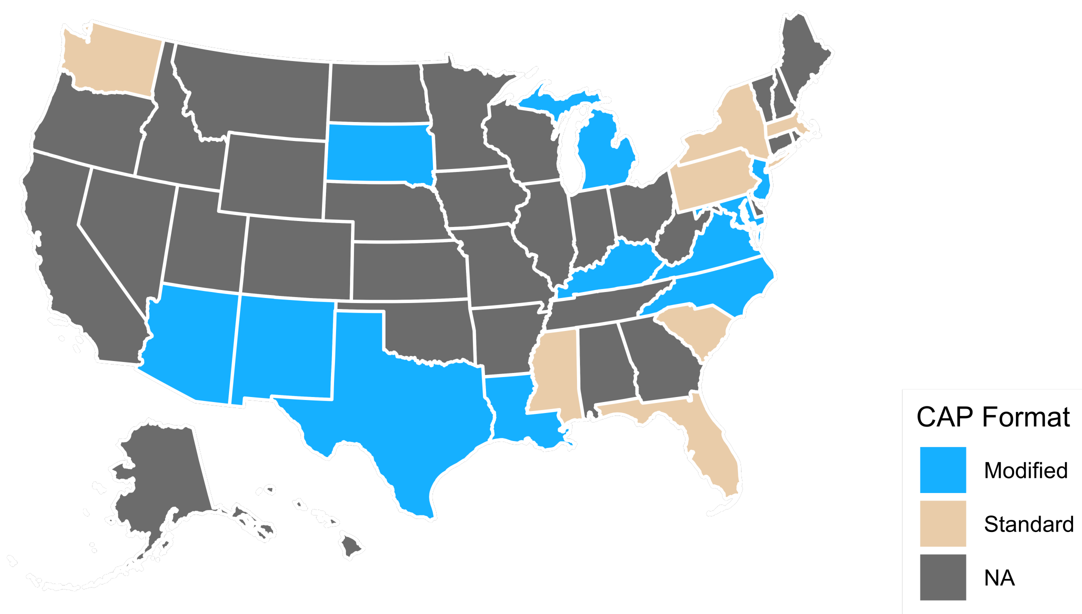


Notes: Figure shows the distribution of years in which the CAP was adopted. The sample is the set of eighteen U.S. states that ever adopted the CAP.

Eligibility for the CAP differs slightly across states; in most states, only one-person SSI households qualify. A smaller number of states additionally require that households have no earned income, and some states also have restrictions by age, often limiting eligibility to recipients of SSI for the elderly, i.e. those ages 65 and older. Details on the eligibility criteria and enrollment procedures in each state are in Appendix Table A.1.

[11] New Mexico adopted the CAP in 2009 but later suspended the program. As of 2016, the CAP remained in operation in the other seventeen states (Weinstein-Tull and Jones (2017)).

Figure 2: Format of the CAP (Standard vs Modified) in Each State



Notes: Figure shows which states adopted the CAP (those in blue or beige), and, among those states, which implemented a "Standard" CAP (beige) or a "Modified" CAP (blue). States that never adopted the CAP are shaded grey.

3 Empirical Setup

This section discusses my data sources and empirical strategy. I outline the characteristics used to define the target sample of "likely SSI recipients" and provide descriptive statistics for this sample.

3.1 Data

My main source of data is the 2000-2016 American Community Surveys (ACS) provided by IPUMS USA (Ruggles and Sobek (2022)).[12] These data are repeated cross-sections and contain individual- and household-level characteristics, including an indicator for the receipt of food stamps at any time in the prior 12 months, which I use as my outcome variable. To restrict the sample to SSI recipients, I construct a target sample of "likely" SSI recipients using household income and other characteristics, discussed in Section 3.2. Variables featured as controls in my analysis include the following demographic characteristics: highest year of school completed, race, ethnicity, sex, age, and physical disability status. The most granular geographic identifier in the data is the Public Use Microdata Area (PUMA), which I use in my calculations of the accessibility of SNAP retailers, discussed further below.

The USDA SNAP policy database lists all SNAP-related policies at the state-by-month level, from 1996-2016 (Tiehen and Jones (2018)). I use these data to confirm the start and end dates of the CAP – and other policies, which I include as controls – in each state. Other state-specific eligibility information was found on the websites of state government agencies. To define the target sample, I use documentation of federal SSI benefit levels from the Social Security Administration website.[13]

For use as controls and for the first placebo test – which uses nearest-neighbor matching to identify placebo "treated" states – I obtain state-by-year unemployment rates from the Bureau of Labor Statistics, state-level political data and median incomes from the Census Bureau's "State and Metropolitan Area Book: 2006," and state latitudes and longitudes from Kaggle's "USA Lat, Long for State Abbreviations" dataset (BLS (2023), Census.gov (2006), Ahmed (2006)). I additionally use the 2000-2016 Annual Social and Economic (ASEC) supplement from the Current Population Survey (CPS) to obtain data on enrollment in unemployment insurance, which replaces SNAP take-up as the outcome variable in my second placebo test (Flood and Westberry (2022)). For my analysis of the effect of the CAP on SNAP targeting efficiency, I use the 2014-2020 panels of the Survey of Income and Program Participation (SIPP) to construct a measure of predicted food insecurity in the ACS sample (Census.gov (2021b)).

To explore the mechanisms through which the CAP affected SNAP enrollment, I assess whether the effect of the CAP varied with the accessibility of SNAP retailers in an individual's place of residence. I use data from the USDA's 2015 Food Access Research Atlas to measure the accessibility of SNAP retailers (USDA (2023)). This dataset is identified at the census tract level and contains indicators for whether residents of a

[12] I weight all regressions by the person-weights included in the ACS to make the sample representative of the U.S. population.
[13] See `https://www.ssa.gov/OACT/COLA/SSIamts.html`.

given tract have poor access to food stores.[14] I calculate the probability that an individual in my data lives in a low-access area by taking the population-weighted mean of one such indicator across all tracts in the PUMA in which the individual lives. This calculation uses population data at the level of the census tract and PUMA from the Census Bureau and IPUMS USA, respectively (Census.gov (2021a), IPUMS (2023)).

3.2 Defining the Target Sample

Literature on welfare take-up that uses survey data often constructs a target sample of "likely" recipients of the benefit in question, using socioeconomic or demographic characteristics to predict participation.[15] To identify individuals who are likely SSI recipients, I consider the following definitions of the target sample:

1. Age 65 or older, has household income no greater than (a) 150% or (b) 120% of the federal SSI benefit amount, and has no earned income.

2. Age 65 or older, has no more than a high school education, does not own a home, and has no earned income.[16]

In my main analysis, I use target sample (1a), defined by income no greater than 150% of the federal SSI benefit amount. I reason that this sample best captures individuals whose main source of income is SSI, but whose total reported income might slightly exceed the federal SSI benefit amount due to earnings from other transfer programs or state SSI supplements,[17] for instance. Results for the other two target samples are included as robustness checks, contained in Section 4.2.

I restrict to individuals ages 65 or older and with no earned income throughout my analyses, as several states limit eligibility for the CAP to this subset of SSI recipients. As previously discussed, seniors are highly susceptible to food insecurity and have markedly low SNAP take-up rates, making this population valuable for study.

3.3 Empirical Strategy

3.3.1 Take-up Analysis

The take-up rate is defined in the literature as the proportion of the eligible population that is actually enrolled in a program (Ko and Moffitt (2022)). Consider an individual i who is eligible for SSI and for the CAP. Let p_i represent the probability that individual i enrolls in SNAP in the absence of the CAP, and let p_i' denote the probability that they enroll after their state adopts the CAP. I study the effect of the CAP by exploring the difference $p_i' - p_i$ between these two probabilities.

[14] The stores included are supercenters, supermarkets, and large grocery stores, which are collectively where 84-92% of SNAP purchases are made (USDA (2023), CBPP (2019)).

[15] Davern et al. (2019) provide an overview of one prediction method involving linked survey-administrative data. For an example in the context of SSI, see Neumark and Powers (2000). One motivation for the target sample approach, relative to using actual survey reporting of participation, is to avoid issues of selection into the program.

[16] Prior research has shown that low education and non-homeownership are highly predictive of SSI receipt (Neumark and Powers (2000), Stegman and Hemmeter (2015)).

[17] See Duggan et al. (2015) for information on state SSI supplements.

I use three research designs to study the effect of the CAP on SNAP take-up: the "state-level" design,[18] in which I restrict the sample to SSI households eligible for the CAP and compare individuals in states that adopted the CAP with those in states that did not; the "singles-couples" design, in which I restrict to states that limited eligibility for the CAP to one-person SSI households and compare single with married SSI recipients; and the triple-difference estimator that interacts the two sources of variation.

Let y_{ist} be an indicator that takes the value 1 if individual i living in state s is enrolled in SNAP in year t, and 0 otherwise. For the state-level treatment design, I estimate the following dynamic TWFE regression,

$$y_{ist} = \delta_s + \gamma_t + \sum_{\tau=-k}^{k} \beta_\tau (T_s \times \mathbb{I}\{t - F_s = \tau\}) + X_i + \tilde{X}_{st} + \epsilon_{ist}, \tag{1}$$

where δ_s and γ_t are state and year fixed-effects, respectively, T_s is an indicator that equals 1 if state s ever had the CAP and 0 otherwise, and F_s is the first year in which state s had the CAP. X_i is a vector of individual-level controls, and $\tilde{X}_{st}$ is a vector of time-varying state controls including other SNAP-related policies implemented at the state level.[19] The coefficient β_τ gives the causal effect of the CAP on SNAP enrollment τ years relative to its adoption.

To obtain average treatment effect estimates, I estimate

$$y_{ist} = \delta_s + \gamma_t + \beta D_{st} + X_i + \tilde{X}_{st} + \epsilon_{ist}, \tag{2}$$

where D_{st} is an indicator that equals 1 if the CAP was in operation in state s in year t and 0 otherwise. The difference-in-differences estimator for the effect of the CAP on SNAP enrollment is β.

The dynamic and static TWFE regressions for the singles-couples treatment design are analogous to equations 1 and 2 but with treatment defined in terms of household type h instead of state s. In particular, I restrict to states that adopted the CAP and that limited eligibility to one-person SSI households, and I estimate

$$y_{ihst} = \alpha_h + \gamma_t + \sum_{\tau=-k}^{k} \beta_\tau (T_h \times \mathbb{I}\{t - F_s = \tau\}) + X_i + \tilde{X}_{st} + \epsilon_{ihst} \tag{3}$$

and

$$y_{ihst} = \alpha_h + \gamma_t + \beta D_{ht} + X_i + \tilde{X}_{st} + \epsilon_{ihst}, \tag{4}$$

where α_h is a vector of indicators for household type,[20] T_h is an indicator that equals 1 for single SSI recipients

[18] The state-level design allows for less conservative restrictions on the sample based on eligibility criteria: in this design, I include states that allowed married SSI recipients to participate in the CAP, and I restrict to SSI recipients that meet their state's specific eligibility criteria for the CAP.

[19] These controls are indicators for whether the state had any of the following policies aimed at simplifying the SNAP certification process: broad-based categorical eligibility to waive the income or assets tests, waivers to eliminate the face-interview requirement at initial application or re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, and the elimination of the fingerprinting requirement at initial application. Figure B.1 shows the evolution of these policies in treated and control states over time, revealing little to no differential trend in adoption between treated and control.

[20] These indicators include classifications such as "married couple," "female householder living alone," and so on.

and 0 otherwise, and D_{ht} is an indicator that equals 1 for single SSI recipients surveyed in years after the adoption of the CAP and 0 otherwise. The coefficients of interest are once again β_τ and β, respectively.

The triple-difference regression used in my third research design is

$$y_{ihst} = \alpha_h + \delta_s + \gamma_t + \lambda_{ht} + \theta_{st} + \kappa_{hs} + \beta D_{hst} + X_i + \tilde{X}_{st} + \epsilon_{ihst}, \tag{5}$$

where $\lambda_{ht}, \theta_{st}$, and κ_{hs} are interacted household-type by year fixed effects, state by year fixed effects, and household-type by state fixed effects, respectively. The indicator D_{hst} takes the value 1 for single SSI recipients living in treated states after the adoption of the CAP and 0 otherwise. The triple-difference estimator is β.

Recent literature has shown that TWFE regressions may yield biased results in the context of staggered treatment timing and heterogeneous treatment effects, as treated units may receive "negative weights" (Callaway and Sant'Anna (2021), Roth et al. (2022)). To confront this issue, in my two difference-in-differences designs, I additionally use the improved doubly-robust (DR) estimator proposed by Callaway and Sant'Anna (2021), which is a weighted average of group-time average treatment effects estimated separately for states that adopted the CAP in different years.

The main identifying assumption for the TWFE and DR models is that, in the absence of the introduction of the CAP and conditional on covariates, enrollment in SNAP among SSI recipients would have evolved in parallel (1) across treated and control states, in the case of the state-level treatment design, and (2) across singles and couples in treated states, in the case of the singles-couples design. The triple-difference estimator provides additional robustness: its identifying assumption is that the *relative* SNAP enrollment of singles and couples must have evolved in parallel in treated and control states.[21] I provide evidence of parallel trends in the figures contained in Section 4.

Identification also relies on the "no anticipation" assumption, which requires that SSI recipients living in states yet to adopt the CAP did not change their SNAP enrollment decision on account of its future adoption. I use as the time of treatment the date at which a state received a waiver from the USDA to implement the CAP. The state presumably would not have announced its adoption of the CAP prior to USDA approval, making anticipatory behavior unlikely.

3.3.2 Targeting Analysis

After estimating the effect of the CAP on SNAP take-up, I explore its effect on targeting efficiency. I follow Finkelstein and Notowidigdo (2019) in defining targeting efficiency: consider a world in which individuals are of two discrete types, $i \in \{H, L\}$, where "type H" individuals are those with a high marginal utility of consumption and "type L" are those with a low marginal utility. Given probabilities of SNAP enrollment p_i and p'_i before and after the adoption of the CAP, respectively, I define an improvement in the targeting of

[21] See Gruber (1994) and Olden and Møen (2022) for discussions of the triple-difference estimator.

SNAP as

$$p'_H - p_H > p'_L - p_L,$$

which states that the change in the probability of SNAP enrollment was larger among type H than type L individuals.

To evaluate the effect of the CAP on the targeting of SNAP, I construct a measure of predicted food insecurity for each individual in my sample as a proxy for their marginal utility of food consumption, which is unobservable. Specifically, using the 2014-2020 panels of the SIPP – which contain detailed data on respondents' ability to afford food – I regress an indicator for experiencing food insecurity F_{it} on the vector of individual demographic characteristics X_{it} used in my main regression, as follows:

$$F_{it} = \phi X_{it} + \epsilon_{it}. \tag{6}$$

I then obtain predictions $\hat{F}_{it} = \hat{\phi} X_{it}$. I merge these predictions into the main ACS data by each combination of the characteristics in X_{it} to obtain predicted probabilities of food insecurity for my analysis sample.

I use these predicted probabilities to subset the sample by level of need for SNAP: in particular, I subset by quartiles of the distribution of $\hat{F}_{it}$ and estimate equation 2 separately by quartile, to detect heterogeneity in the effect of the CAP by level of predicted food insecurity.

Details on the construction of F_{it} and on the individual-level characteristics included in X_{it} are contained in Appendix C.

3.4 Descriptive Statistics

Table 1 shows descriptive statistics for the target sample of likely SSI recipients, defined according to 1(a) in Section 3.2. The sample is quite poor, with average annual income around $7,000, and yet less than a quarter is enrolled in SNAP at baseline. Just over 20% of the sample is Black or Hispanic, about half received less than a high school education, and slightly more than half report having a disability. Of note, only about 12% of the sample reports receiving SSI: I discuss concerns about under-reporting and measurement error in Section 3.4.1 below.

Table 1: Descriptive Statistics on the Treated and Control Samples of SSI Recipients

	State-level Design			Singles-couples Design		
	Treated	Control	p-value from t-test	Treated	Control	p-value from t-test
Enrolled in SNAP	0.19	0.16	0.00	0.20	0.12	0.00
Mean Personal Income	$7,045.29	$7,251.60	0.00	$7,056.30	$5,679.37	0.00
Mean Poverty Status	75.09	76.74	0.00	74.69	89.44	0.00
Mean Age	77.61	77.91	0.00	77.70	74.87	0.00
Black	0.18	0.12	0.00	0.20	0.12	0.00
Hispanic	0.06	0.04	0.00	0.05	0.07	0.00
Female	0.80	0.80	0.11	0.81	0.46	0.00
HS Dropout	0.50	0.47	0.00	0.49	0.47	0.00
HS Graduate	0.32	0.33	0.00	0.33	0.34	0.26
Some College	0.12	0.14	0.00	0.11	0.11	0.97
College Graduate	0.06	0.06	0.23	0.06	0.08	0.00
Has Disability	0.58	0.60	0.00	0.57	0.45	0.00
Lives in Metro Area	0.46	0.38	0.00	0.51	0.48	0.00
Self-reports SSI Receipt	0.12	0.12	0.01	0.11	0.06	0.00
N	65,539	82,271	147,810	52,785	28,253	81,038

Notes: Table shows summary statistics for the treatment and control groups, in the "state-level" research design (in which the treatment group is states that adopted the CAP and the control group is states that never adopted the CAP), and the "singles-couples" research design (in which the treatment group is single individuals and the control group is married individuals). In the state-level design, I restrict the treated sample to the pre-treatment period, and I restrict the control sample such that the distribution across years matches the distribution in the treatment sample. In the singles-couples design, I restrict to observations in states that adopted the CAP, prior to its adoption. Personal income is the total pre-tax income received by the respondent in the survey year from all sources, including earned income, retirement income, and any income from Social Security and cash transfers. Poverty status refers to percent of the federal poverty level. The indicator for disability status is limited to physical disabilities.

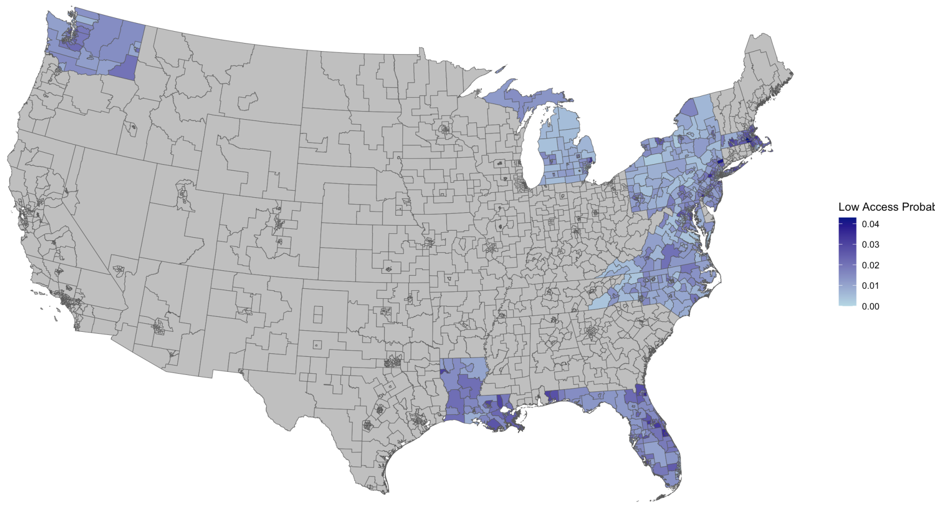
In Section 5, I evaluate whether the effect of the auto-enrollment version of the CAP varied with the geographic accessibility of SNAP retailers, i.e. food stores that accept SNAP benefits as payment. To construct a measure of the accessibility of SNAP retailers, I use the "low access at 1 and 10 miles" variable in the USDA's Food Access Research Atlas,[22] which is an indicator at the census tract level for whether at least 500 people or at least 1/3 of the population in a given census tract lives more than 1 mile away from the nearest food store if they live in an urban area, or more than 10 miles away if they live in a rural area. Since my sample of SSI recipients contains geographic data at the PUMA level, I compute the population-weighted average of this variable across all census tracts in the PUMA in which a recipient lives, and I assign this measure as the recipient's probability of having low access to SNAP retailers.

Figure 3 shows the dispersion of low accessibility to SNAP retailers across PUMAs. Individual-level statistics are contained in Table D.1. The average probability of living in a low-access area is small, at about 1%, but there is considerable variation across localities.

To motivate my two difference-in-difference designs ("state-level" and "singles-couples"), Figure 4 displays raw plots of the proportion enrolled in SNAP in each year relative to the adoption of the CAP, among (a) SSI recipients living in states that adopted ("treated") versus states that never adopted ("control") the CAP,

[22] Detailed statistics on this measure and a comparison to other variables in the USDA's Food Research Atlas are contained in Appendix D.

Figure 3: Probability of Low Access to SNAP Retailers in Auto-enrollment CAP States



Notes: Figure shows the probability of having low access to SNAP retailers in each PUMA in the U.S. (restricted to states that adopted the auto-enrollment version of the CAP), using data from the USDA's Food Access Research Atlas. The metric used is a population-weighted average of the census-tract-level variable "low access at 1 and 10 miles", which indicates whether at least 500 people or at least 1/3 of the population in a given census tract lives more than 1 mile away from the nearest food store if they live in an urban area, and more than 10 miles away if they live in a rural area. Accessibility data is from 2015 and includes data on supercenters, supermarkets, and large grocery stores. States that did not adopt the auto-enrollment version of the CAP are shaded grey.

and (b) single versus married SSI recipients. SNAP enrollment was in general increasing over time, but at a faster rate among those eligible for the CAP relative to those ineligible in the years after its adoption.

3.4.1 Mis-reporting and Mis-measurement of Social Safety Net Program Take-up

The proportion of my sample enrolled in SNAP is lower than estimates from administrative data for similar samples: estimates of the SNAP participation rate among elderly SSI recipients around the start of my study period range from 31.2% for men and 42.5% for women in 1999 to about 51.8% for the entire population in 2001 (Daly and Burkhauser (2003) and Trenkamp and Wiseman (2007)). The lower rates of reported take-up in my data are consistent with prior research which shows that survey data often produce attenuated estimates of the proportion enrolled in social safety net programs, due to under-reporting by survey respondents. In the ACS specifically, Meyer et al. (2022) shows that 35% of SNAP recipients fail to report that they are receiving SNAP.

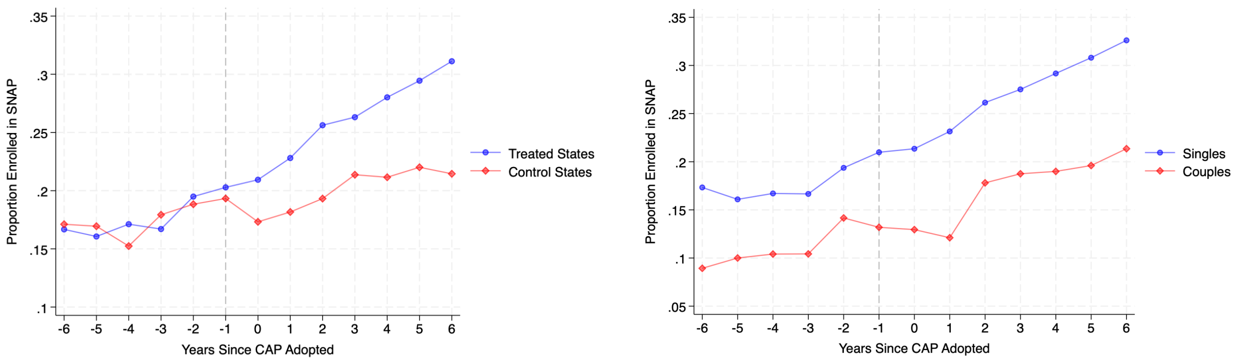
Mis-reporting of SNAP participation in my data does not seriously threaten the validity of my difference-in-differences strategy, so long as the mis-reporting is not time-varying and heterogeneous across treatment and control.[23] Since the literature finds that this mis-reporting is predominantly *under-reporting*, and since I use SNAP enrollment strictly as an outcome variable, this attenuation might increase my estimated standard errors but will not bias the point estimates.

[23]Even in this situation, my use of multiple research designs with distinct definitions of treatment and control ensures that my results are robust to mis-reporting that is correlated with demographics or geography.

Figure 4: Raw Plots of SNAP Enrollment Rates

(a) Treated vs Control States

(b) Singles vs Married Individuals



Notes: Figure shows the proportion enrolled in SNAP in each year relative to the adoption of the CAP (year 0). Sub-figure (a) compares individuals living in states that adopted the CAP (blue) with individuals living in states that never adopted the CAP (red), where the sample is restricted to likely SSI recipients eligible for the CAP in each state in which it was adopted. Sub-figure (b) compares single (blue) to married (red) individuals, where the sample is restricted to likely SSI recipients living in states that adopted the CAP. To assign values for "years since CAP adoption" for the control states in sub-figure (a), which never actually adopted the CAP, I assign placebo adoption years to match the population-weighted distribution of actual adoption years among the treated states, by randomly assigning the same population-weighted proportion of control states as treated states to each adoption year. N = 1,040,494 persons in (a), and N = 448,432 persons in (b).

A related issue is under-reporting of SSI participation, since SSI eligibility/receipt is a prerequisite for eligibility for the CAP. I address this concern by using a target sample of "likely" SSI recipients, described in Section 3.2 above. As discussed with regard to Table 1, self-reported SSI receipt is quite low among my target sample. While this may in part be attributable to under-reporting, it is plausible that a non-trivial share of my target sample is not actually enrolled in SSI, as SSI take-up is documented to be quite low among eligible seniors.[24] If this is the case, my estimates of the effect of the CAP on SNAP take-up might be attenuated.

I account for this potential attenuation in two ways. First, in robustness checks shown in Section 4.2, I re-run the main analysis on various alternative target samples of individuals who are likely to receive SSI, one of which is the sample of self-reported SSI recipients. Second, in Appendix E, I explore how the magnitudes of my estimates of the effect of the CAP change when I interpret my main regression estimates as an "intent to treat" and scale them up by the "first stage" – self-reported SSI receipt among the target sample in the pre-period – to obtain an average treatment effect on the treated.

4 Results

This section presents estimates of the effect of the CAP on SNAP take-up and targeting efficiency.

[24]Estimates of the SSI take-up rate among seniors range from 38% to 73% (Coe and Wu (2014)).

4.1 Take-up

4.1.1 Dynamic Fixed-Effects Estimates

Figure 5 shows estimates from the dynamic TWFE and DR regressions for the effect of the CAP on SNAP take-up. The two alternative treatment designs yield qualitatively similar results. SNAP enrollment evolved roughly in parallel for the treated and control groups in the years prior to the introduction of the CAP. In the second through sixth year after the CAP was adopted, enrollment was statistically significantly higher in the treatment group relative to the control group, and the magnitude of this difference was roughly increasing over relative time.

In both the state-level and singles-couples designs, none of the coefficient estimates in the pre-treatment years is significant, in either the TWFE or DR specifications. This provides supporting evidence of parallel trends in SNAP enrollment across the treatment and control groups. Analyses of the power of these tests for parallel trends are contained in Appendix F.

One feature of these plots is the slight lag in the effect of the CAP: a statistically significant effect on SNAP enrollment is only visible starting two years after the adoption of the CAP in most specifications. Some candidate explanations for this lag include administrative delays which may have prevented the CAP from being introduced in all SSA offices immediately upon approval from the USDA, and a gap between the stated adoption of the CAP and the point at which many eligible individuals received outreach. States that implemented the Modified CAP and used paper mail to contact SSI recipients might have contributed most to this lag in the effect given the time it would have taken for applications to be received, returned to the SSA office, and processed.

4.1.2 Average Treatment Effect Estimates

Panel A of Table 2 shows the average effect of the CAP on SNAP take-up, using the TWFE regressions in equations 2, 4 and 5. Columns (1)-(3) use the state-level design, Columns (4)-(6) use the singles-couples design, and Columns (7)-(8) use the triple difference design. Column (3) of Panel (A) is my preferred specification because, unlike the singles-couples and triple difference designs, the state-level design is not susceptible to control group spillovers, which could lead the estimates from these other two designs to be downward biased.[25]

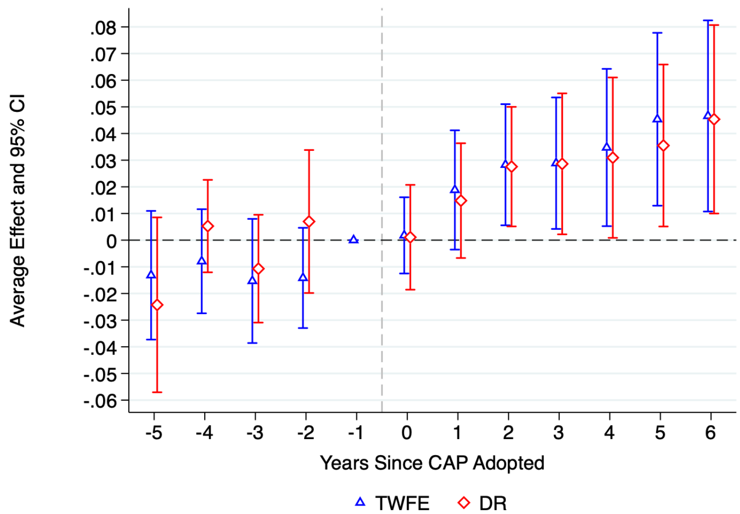
With this specification, the CAP caused a statistically significant 3.75 percentage-point increase in the probability of SNAP enrollment on average. The estimates in all other specifications are in a range of 2-6 percentage points.

[25] Although only single SSI recipients were eligible for the CAP, the expedited enrollment of these individuals in SNAP might have reduced crowding at SNAP offices in states that adopted the CAP, thus increasing the accessibility of SNAP for couples and non-SSI recipients in these states as well. In the singles-couples treatment and triple-difference specifications, the estimated treatment effect could in principle be attenuated because of increased SNAP enrollment among those ineligible for the CAP in treated states. The state-level treatment design, on the other hand, avoids this concern, as the control group is SSI recipients in states that did not adopt the CAP.

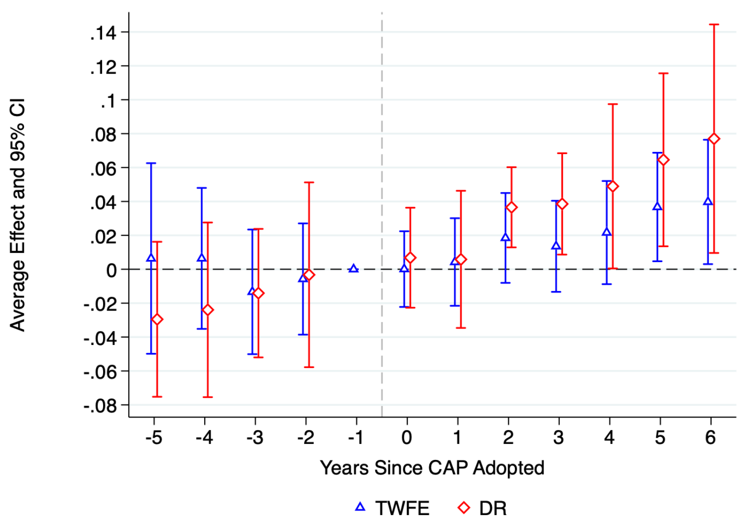
In Panel B of Table 2, I use the DR estimator from Callaway and Sant'Anna (2021), with never-treated units as the control, in the state-level and singles-couples research designs. The estimates from each specification are similar in magnitude to the TWFE estimates and are likewise significant. This consistency alleviates concerns about bias resulting from heterogeneous treatment effects and staggered treatment timing.

Figure 5: Effect of the CAP on SNAP Enrollment

(a) State-level Design



(b) Singles-Couples Design



Notes: Figure shows estimates (with 95% confidence intervals) of β_τ in equation 1 for sub-figure (a) and in equation 3 for sub-figure (b), where the blue triangles use the TWFE specification and the red diamonds use Callaway and Sant'Anna (2021)'s improved doubly-robust estimator with never-treated units as the control. The outcome variable is an indicator for SNAP enrollment. Individual-level controls included throughout are sex, race, ethnicity, age, education level, and disability status. Standard errors are clustered at the state level. N = 1,040,253 persons in (a), and N = 448,083 persons in (b).

Table 2: Average Effect of the CAP on SNAP Enrollment

	State-level Design			Singles-Couples Design			Triple Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: TWFE Estimator								
Treated x Post	0.0405***	0.0384***	0.0375***					
	(0.012)	(0.012)	(0.011)					
Single x Post				0.0568**	0.0188**	0.0382**		
				(0.026)	(0.006)	(0.015)		
Single x Treated x Post							0.0215**	0.0217***
							(0.008)	(0.008)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household Type FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	1,040,494	1,040,494	1,040,494	448,083	448,083	448,083	1,524,029	1,524,029
Panel B: DR Estimator								
Treated x Post	0.0369**	0.0274**	-				-	-
	(0.014)	(0.012)	-				-	-
Single x Post			-	0.0422**	0.0423**	0.0425**	-	-
			-	(0.020)	(0.019)	(0.020)	-	-
Year FE	✓	✓	-	✓	✓	✓	-	-
State FE	✓	✓	-				-	-
Household Type FE			-	✓	✓	✓	-	-
Individual Controls		✓	-		✓	✓	-	-
State Controls			-			✓	-	-
N	1,040,253	1,040,253	-	448,191	448,191	405,333	-	-

Notes: Panel (A) shows estimates of β in equation 2 for columns (1)-(3), in equation 4 for columns (4)-(6), and in equation 5 for columns (7)-(8), where the outcome variable is an indicator for SNAP enrollment. Panel (B) uses the doubly-robust estimator from Callaway and Sant'Anna (2021) with never-treated units as the control; this estimator by default interacts all covariates with year. The individual controls are age and indicators for sex, race, ethnicity, education level, and physical disability. State controls include 2003 median income and percent of congressional representatives that were Democrats in 2004, along with time-varying indicators for the following SNAP-related policies in Panel (A): broad-based categorical eligibility to waive the income or assets tests, waivers to eliminate the face-interview requirement at initial application or re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, and the elimination of the fingerprinting requirement at initial application. For Columns (3) and (6) of Panel (A), the state controls also include yearly unemployment rates, while Column (3) excludes the time-invariant controls (2003 median income and percent of congressional representatives that were Democrats in 2004). The triple difference specifications also include household-type by year fixed effects, state by year fixed effects, and household-type by state fixed effects. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

The 3.75 percentage-point estimate documented in Table 2 translates to an 18.85 percent increase in SNAP take-up (from the roughly 20% baseline rate). This effect is quite large – and comparable to estimates in the literature on other policies and transfer programs[26] – and indicates that transaction costs impose a substantial barrier to SNAP take-up. A back-of-the-envelope calculation suggests that the CAP saved the marginal SNAP applicant about $350, or about 5% of their annual income.[27] The capacity of programs like the CAP to reduce the burden of applying for social safety net benefits is substantial.

4.2 Take-up: Placebo Test and Robustness Checks

4.2.1 Placebo Tests

The identifying assumption of each of my difference-in-differences designs is that, in the absence of the adoption of the CAP, SNAP enrollment would have evolved in parallel across treated and control states, and across single and married SSI recipients, respectively. To verify whether the increase in SNAP take-up that I document is an effect of the CAP rather than of either coinciding SNAP policies or underlying trends in enrollment in welfare programs more broadly, I perform two placebo tests.

In the first test, I use a nearest-neighbor matching algorithm to select placebo "treated" states among the control states (i.e. those that never adopted the CAP). I then run the difference-in-differences and triple difference regressions with SNAP enrollment as the outcome variable; a null effect on SNAP take-up would indicate that SNAP enrollment did in fact evolve smoothly in the absence of the CAP. I construct the placebo treatment group by matching control to treated states by (1) state geography, (2) median income, and (3) the proportion of each state's representatives that are Democrats. Appendix H contains additional details about the matching algorithm, and Table H.1 lists the pairs of matched states.

The second placebo test discerns whether underlying trends in social safety net program participation more generally, perhaps on account of the business cycle, might explain my results. In this test, I repeat my main analysis but use enrollment in unemployment insurance (UI) rather than SNAP as the outcome variable, with a sample of working-age individuals. Since the CAP should not have affected enrollment in UI, a statistically significant effect would imply that other factors related to the underlying economy could be driving the increase in SNAP take-up that I observe.

Table 3 shows the estimates of β in equations 2, 4, and 5, in each of the two placebo tests. In the test with placebo "treated" states, most estimates are insignificant, except in the singles-couples design with no controls and the state-level design with individual and state controls, in which the estimates are marginally significant and of conflicting signs. In the test with UI enrollment as the outcome variable, most coefficients are negative and small in magnitude, and none is statistically significant. These results are evidence that the increase in SNAP take-up observed among actually treated units, as presented in Table 2, is attributable

[26] For instance, the magnitudes of my estimates are within 5 percentage points of those in Deshpande and Li (2019) and Homonoff and Somerville (2021) for the effect of transaction costs on Disability Insurance take-up and SNAP re-certification, respectively.

[27] See Appendix G for the details of this calculation.

to the CAP rather than either coinciding SNAP policies or underlying trends in social safety net program participation.

Appendix Figure H.1 shows the dynamic TWFE plots for the two placebo tests. These figures likewise suggest a null effect, further supporting this conclusion.

Table 3: Placebo Tests for the Effect of the CAP

	State-level Design			Singles-Couples Design			Triple Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effect on SNAP Enrollment with Placebo "Treated" States								
Treated x Post	0.015	0.017	0.020*					
	(0.013)	(0.013)	(0.011)					
Single x Post				-0.034*	-0.008	-0.015		
				(0.019)	(0.010)	(0.013)		
Single x Treated x Post							0.008	0.002
							(0.011)	(0.012)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household Type FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	637,826	560,227	560,227	257,427	257,427	257,427	932,137	825,153
Panel B: Effect on UI Enrollment								
Treated x Post	-0.001	-0.001	-0.001					
	(0.002)	(0.002)	(0.001)					
Single x Post				-0.001	0.00009	-0.002		
				(0.004)	(0.001)	(0.002)		
Single x Treated x Post							-0.002	-0.001
							(0.001)	(0.001)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household Type FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	860,709	860,709	860,709	470,164	470,164	470,164	1,864,839	1,864,839

Notes: Table shows estimates of β in equations 2, 4, and 5 in Columns (1)-(3), (4)-(6), and (7)-(8), respectively, in two different placebo tests. Panel (A) uses a set of placebo "treated" states (selected via a nearest-neighbor matching algorithm) and estimates the effect of the placebo "adoption of the CAP" on SNAP enrollment. Panel (B) carries out the main regression analysis (with actual treated states) but with enrollment in unemployment insurance among working-age individuals as the outcome variable. Individual controls are age and indicators for sex, race, ethnicity, education level, and physical disability. State controls are yearly unemployment rates, along with (in Panel A only) time-varying indicators for the following SNAP-related policies: broad-based categorical eligibility to waive the income or assets tests, waivers to eliminate the face-interview requirement at initial application or re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, and the elimination of the fingerprinting requirement at initial application. The triple difference specifications also include household-type by year fixed effects, state by year fixed effects, and household-type by state fixed effects. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.2.2 Robustness Checks

I next perform two sets of robustness checks. The first imposes a balanced-panel restriction: I limit the sample of treated states to those that adopted the CAP in 2005-2010, for which I have data in a range of 5 years before and after the adoption of the CAP. I then restrict to the ±5-year window around the adoption of the CAP in these states and repeat the main regression analysis, using each of my three research designs.

Table 4: Average Effect of the CAP on SNAP Take-up: Balanced Panel

	State-level Design			Singles-Couples Design			Triple Difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	0.030**	0.030**	0.031***					
	(0.012)	(0.012)	(0.010)					
Single x Post				0.027**	0.021**	0.021*		
				(0.012)	(0.007)	(0.008)		
Single x Treated x Post							0.023**	0.023***
							(0.010)	(0.008)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household Type FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		✓
N	914,857	914,857	914,857	318,681	318,681	318,681	1,392,693	1,392,693

Notes: Table shows estimates of β in equations 2, 4, and 5 in Columns (1)-(3), (4)-(6), and (7)-(8), respectively, where the set of treated states is restricted to those that adopted the CAP in 2005-2010, and I use a balanced panel with the 5 years before and after treatment. The outcome variable is an indicator for SNAP enrollment. Individual controls are age and indicators for sex, race, ethnicity, education level, and physical disability. State controls are yearly unemployment rates (in columns 3 and 6 only) along with time-varying indicators for the following SNAP-related policies: broad-based categorical eligibility to waive the income or assets tests, waivers to eliminate the face-interview requirement at initial application or re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, and the elimination of the fingerprinting requirement at initial application. The triple difference specifications also include household-type by year fixed effects, state by year fixed effects, and household-type by state fixed effects. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4 shows the results: the estimate for the effect of the CAP on SNAP take-up remains positive, statistically significant, and within a range of 2-3 percentage points, confirming that my main results are robust to this balanced panel restriction.

Next, I test the sensitivity of my estimates to the definition of the target sample of likely SSI recipients, by modifying this definition using the two alternatives proposed in Section (3.2). Namely, I define the target sample of likely SSI recipients as those with less than 120% of the federal SSI benefit amount (with no earned income), or as those with no more than a high school education who are not homeowners (and who have no earned income). As a third definition, I use survey respondents' self-reporting of SSI receipt to define the target sample: I restrict the sample to individuals ages 65 and older with no earned income who report receiving a positive amount of SSI in the survey year.

Table 5 contains the estimates of β in the state-level design, using each of these alternative target sample definitions. Columns (1)-(3) use the sample of individuals with household income no greater than 120% of the federal SSI benefit level, Columns (4)-(6) restrict to non-homeowners with no more than a high school education, and Columns (7)-(9) use self-reported SSI recipients. The coefficient estimates in columns (1)-(6) are positive, statistically significant, and very close in magnitude to the estimates in Table 2. My results are thus robust to alternative definitions of "likely" SSI recipients.

The estimates in Columns (7)-(9), while also positive and statistically significant, are notably larger in magnitude than those using all other target samples: when the sample is defined as those who self-report receiving SSI, the effect of the CAP is an 11.4 percentage-point increase in SNAP enrollment. Acknowledging that the reliability of survey data on SSI receipt, particularly among those ages 65 and older,[28] has been deemed questionable by prior literature, these results suggest that my main estimates using a target sample of "likely" SSI recipients are conservative.

Appendix H displays event study plots for the effect of the CAP on SNAP enrollment using each of these three alternative target samples. In each case, there is evidence of parallel trends in the pre-period, followed by a significant increase in SNAP enrollment among the treatment group after the adoption of the CAP.

Table 5: Average Effect of the CAP on SNAP Take-up: Robustness Checks with Alternative Target Samples

	120% of SSI Income			$\leq$ HS Ed. & Non-homeowner			Self-reported SSI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated x Post	0.052***	0.049***	0.048***	0.048***	0.046***	0.046***	0.114***	0.115***	0.114***
	(0.014)	(0.013)	(0.012)	(0.011)	(0.011)	(0.010)	(0.020)	(0.020)	(0.018)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓	✓
State Controls			✓			✓			✓
N	636,756	636,756	636,756	807,198	807,198	807,198	243,293	243,293	243,293

Notes: Table shows estimates of β in equation 2, using three alternative definitions of the target sample of SSI recipients. Columns (1)-(3) define the target sample as those with income less than 120% of the federal SSI benefit amount, columns (4)-(6) use those with no more than a high school education who do not own a home, and columns (7)-(9) use those who self-report receiving SSI. The outcome variable is an indicator for SNAP enrollment. Individual controls are age and indicators for sex, race, ethnicity, education level, and physical disability. State controls are yearly unemployment rates along with time-varying indicators for the following SNAP-related policies: broad-based categorical eligibility to waive the income or assets tests, waivers to eliminate the face-interview requirement at initial application or re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, and the elimination of the fingerprinting requirement at initial application. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

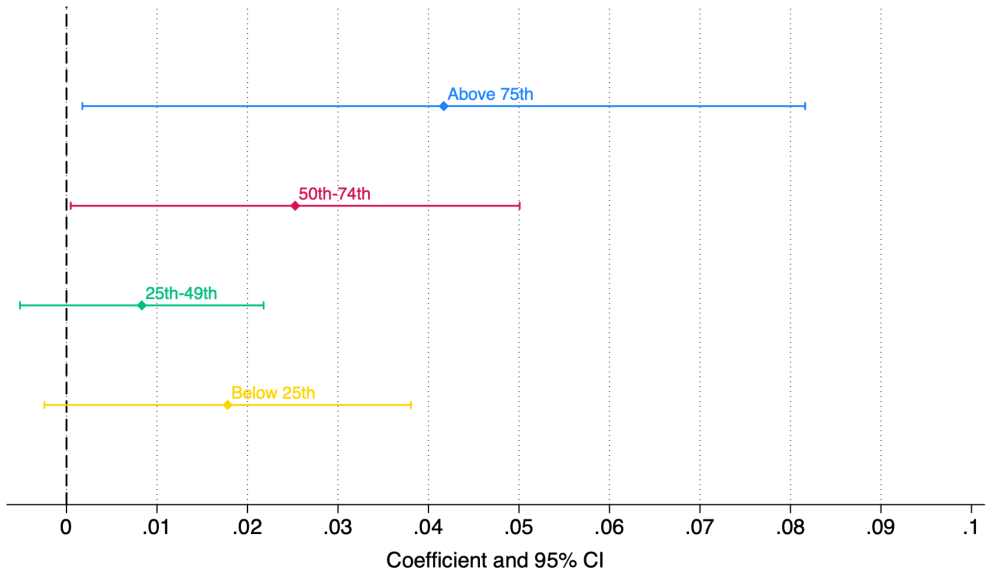
4.3 Targeting Efficiency

Given strong evidence that the CAP increased SNAP take-up, I now explore whether the CAP improved or worsened the targeting efficiency of SNAP. Specifically, I test whether the effect of the CAP on SNAP take-up varied by an individual's predicted level of food insecurity, where the calculation of these predictions is outlined in Section 3.3.2.

Figure 6 shows estimates of β in the state-level regression, where I restrict the sample by quartile of predicted probability of food insecurity. There are no statistically significant differences between the coefficients. However, only those in the highest two quartiles of the distribution – i.e. those with the highest probability of being food insecure – experienced an effect of the CAP that was significantly different from zero. Furthermore, the magnitude of the estimates is roughly increasing in predicted level of food insecurity.

[28] Neumark and Powers (2004) document a difference between SSA administrative data and self-reported SSI receipt among the elderly in the Survey of Income and Program Participation. They conjecture that individuals ages 65 and older might confuse SSI with Social Security benefits, as the two payments are distributed by the same local offices.

Figure 6: Average Effect of the CAP on SNAP Enrollment by Percentile of Predicted Food Insecurity



Notes: Figure shows estimates of β in equation 2, in regressions restricted to individuals in each quartile of the distribution of predicted food insecurity (where a higher percentile indicates a higher probability of food insecurity). Standard errors are clustered at the state level. The full sample (before subsetting by quartile) has size 471,836 persons.

Like Deshpande and Li (2019), who show, in the context of disability insurance, that transaction costs and targeting are negatively correlated (particularly with respect to education level), I find suggestive evidence that the CAP – a reduction in transaction costs – improved the targeting efficiency of SNAP. A larger sample and more granular demographic/socieconomic characteristics would be useful in confirming these results.

5 Mechanisms: An Analysis of Default Options

Recall from Section 2 that states adopted one of two formats of the CAP, either Standard or Modified. The former is a joint-filing procedure, while the latter is an outreach program in which the state mailed shortened SNAP application forms to SSI recipients.

Apart from this distinction, there is more granular variation in the details of the Standard CAP across states: since the joint-enrollment procedure would have only affected individuals newly applying for SSI, some Standard CAP states took steps to account for individuals already enrolled in SSI but yet to enroll in SNAP at the time of the CAP's adoption. New York, in particular, auto-enrolled all SSI recipients in SNAP by mailing them EBT cards, instead of using joint-enrollment at the time of the SSI application. Massachusetts and Pennsylvania used both joint-enrollment – to enroll individuals newly applying to SSI – and auto-enrollment – to enroll existing SSI recipients who had yet to apply for SNAP. The remaining Standard CAP states either sent simplified SNAP application forms to those already enrolled in SSI or did not, as far as the documentation shows, account for those already enrolled in SSI.

I exploit this variation in the format of the CAP to study the mechanisms through which the CAP increased SNAP enrollment. If the effect of the CAP was larger in states that adopted the auto-enrollment version of the Standard CAP, then transaction costs associated with manually filling out a simplified application (relative to being auto-enrolled or jointly enrolled with help from a caseworker) must be pertinent. This would indicate that "inertia" or default options were important channels through which the CAP increased SNAP enrollment.

Table 6 displays the coefficients on the interaction (or triple-interaction) terms in the TWFE regressions, in each of my three research designs, where I also interact the main effect with indicators for whether the CAP adopted in each state was Standard, or Standard with auto-enrollment, relative to Modified. The interaction with the auto-enrollment indicator is positive throughout, and is statistically significant in two of the three designs. The sum of the coefficients in the state-level design indicates that the auto-enrollment version of the Standard CAP caused a 7.72 percentage-point increase in SNAP enrollment. The interaction with the Standard indicator, on the other hand, is negative throughout and significant in one of the three designs. This suggests that auto-enrolling SSI recipients in SNAP by sending them an EBT card had an effect on SNAP enrollment above and beyond mailing them a simplified SNAP application form. Conversely, implementing a joint-enrollment procedure to enroll newly-applying SSI recipients in SNAP without accounting for those already enrolled in SNAP may have been less effective than mailing all SSI recipients a SNAP application form.

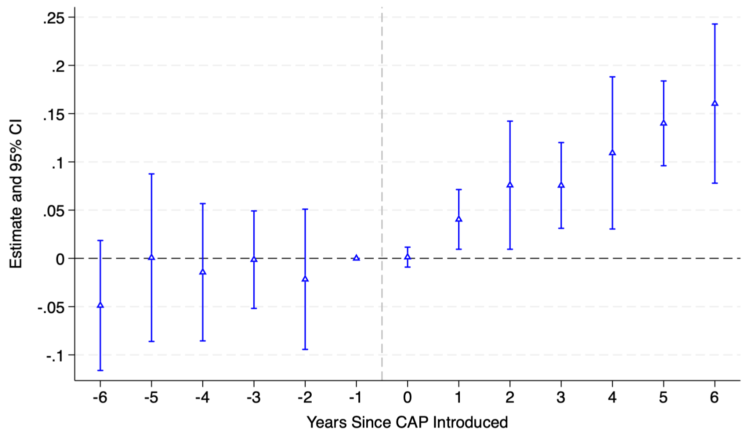
Table 6: Average Effect of the CAP on SNAP Enrollment: Standard vs Modified Format

	State-level	Singles-Couples	Triple Difference
Treated x Post	0.0319**		
	(0.012)		
Treated x Post x Standard	-0.0294**		
	(0.015)		
Treated x Post x Auto-enroll	0.0453**		
	(0.021)		
Single x Post		0.0182***	
		(0.006)	
Single x Post x Standard		-0.0163	
		(0.010)	
Single x Post x Auto-enroll		0.0129	
		(0.016)	
Single x Treated x Post			0.0165*
			(0.008)
Single x Treated x Post x Standard			-0.0105
			(0.013)
Single x Treated x Post x Auto-enroll			0.0254**
			(0.012)
N	1,040,494	448,432	1,524,029

Notes: Table shows estimates of β in equations 2, 4, and 5 in the first, second, and third columns, respectively, along with the coefficient on an interaction of the main effect with indicators for whether the format of the CAP was Standard, or Standard with auto-enrollment, relative to Modified. The outcome variable is an indicator for SNAP enrollment. The individual controls (included in all specifications) are age and indicators for sex, race, ethnicity, education level, and physical disability. Standard errors clustered at the state level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

To further explore this result, Figure 7 shows event study estimates for the effect of the CAP on SNAP enrollment, restricted to the three states that implemented the auto-enrollment version of the CAP. The magnitudes of these estimates are notably larger than those for the full sample: the effect in the sixth year of implementation surpasses a 15 percentage-point increase in SNAP enrollment. Unlike the delayed effect seen for the full sample of states – in which the estimates are only significant starting two years after implementation – the effect among auto-enrollment states was nearly immediate. This result sheds light on the source of the delay for the full sample: the paper mail applications involved in the Modified format of the CAP likely played a role.

Figure 7: Effect of the CAP on SNAP Enrollment in Auto-enrollment States



Notes: Figure shows event study estimates (the equivalent of β in equation 1 but without control states) for the effect of the CAP on SNAP enrollment, limited to the three states that implemented the auto-enrollment version of the CAP. The individual controls are age and indicators for sex, race, ethnicity, education level, and physical disability. Standard errors are clustered at the state level. N = 78,340 persons.

These results suggest that changing the "default option" was an important mechanism through which the CAP increased SNAP enrollment. Although mailing SSI recipients a simplified SNAP application form increased their probability of enrolling in SNAP to some degree, the costs of filling out this form were clearly enough to prevent more widespread participation, making auto-enrollment a more effective means of increasing take-up.

5.1 Auto-enrollment with Low Access to SNAP Retailers

The 7.72 percentage-point increase in SNAP take-up in states that adopted the auto-enrollment version of the Standard CAP exceeds the 3.75 percentage-point increase for the overall sample in states that adopted either the Standard or Modified CAP (reported in Table 2). However, this effect is still fairly small; if the CAP had been true "auto-enrollment", we would expect nearly the entire population of SSI recipients to be enrolled in SNAP after the adoption of the CAP. Only a few explanations for the failure of this policy to achieve more widespread SNAP enrollment exist: either SSI recipients did not understand the mailer containing the EBT card or how to activate the card, the mailer failed to reach many SSI recipients, or recipients received the mailer but actively chose to ignore it.

To distinguish between these possible explanations, I explore whether the effect of the auto-enrollment version of the CAP varied with the ease with which the recipient could activate their benefits, measured by the geographic accessibility of food stores that accept SNAP benefits as payment. Table 7 shows the results: using my singles-couples research design, I interact the main effect with an indicator for being in an area with low access to SNAP retailers. The sample is restricted to the three states that auto-enrolled SSI recipients in SNAP: New York, Massachusetts, and Pennsylvania.

The coefficient on the interaction is negative and significant, indicating that the effect of the CAP was about 2 percentage points smaller for individuals living in low-access areas. The sign and approximate magnitude of this effect is robust to the inclusion of controls.[29]

These results reveal an important limitation of auto-enrollment. SSI recipients with low access to food stores may have been inclined to ignore the EBT card sent to them in the mail; even absent the cost of filling out a SNAP application, the time and monetary cost of traveling to a food store to activate their card may have outweighed the benefit of receiving SNAP, especially when substitutes for purchasing food – at fast food restaurants, for instance – exist.

Table 7: Average Effect of the "Auto-enrollment" Standard CAP, By Accessibility of SNAP Retailers

	(1)	(2)
Single x Post	0.0636***	0.0386***
	(0.008)	(0.009)
Single x Post x [Low Access to SNAP Retailers]	-0.0177***	-0.0264***
	(0.007)	(0.007)
Household Type FE	✓	✓
Year FE	✓	✓
State FE		✓
Individual Controls		✓
N	127,115	104,523

Notes: Table shows estimates of β in equation 4, along with the coefficient on an interaction of the main effect with an indicator for whether the individual lives in a PUMA with greater than the median probability of having low access to SNAP retailers. The outcome variable is an indicator for SNAP enrollment. The sample is restricted to individuals in New York, Massachusetts, and Pennsylvania, the three states with an auto-enrollment component to their CAP. Data is from 2005-2016 (since PUMA is not reported before 2005), so the Single x Post indicator for New York is simply an indicator for being single (as New York adopted the CAP in 2003). The individual controls included in Column (2) are age and indicators for sex, race, ethnicity, education level, and physical disability, along with household income. Robust standard errors are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6 Conclusion

A large proportion of the poorest Americans are not availing of benefits for which they are eligible. Existing literature has shown that transaction costs constrain enrollment in social safety net programs, but whether such costs helpfully screen applicants or preclude participation among the most needy has remained unclear, as have the channels through which these costs operate.

In analyzing the effect of the CAP, I find that the associated reduction in transaction costs increased SNAP take-up by 3.75 percentage points, with suggestively larger effects for those with a higher probability of being food insecure. These findings show that transaction costs reduce the take-up of SNAP and provide

[29] This result is also robust to using the USDA's alternative measures of low-access listed in Table D.1 (not shown).

suggestive evidence that these transaction costs are negatively correlated with targeting efficiency. I use variation in the format of the CAP across states to show that changes in default options played an important role in increasing SNAP take-up, as states that adopted auto-enrollment saw the largest increases in take-up among SSI recipients. However, the effectiveness of this auto-enrollment policy was limited by the ease of use of SNAP benefits for individuals in these states: those with lower access to SNAP retailers were less likely to activate their EBT card upon being auto-enrolled.

The policy implications of these findings are as follows. Extrapolating from the sample of SSI recipients studied here, my results suggest that auto-enrollment-like programs can improve the overall take-up and targeting of SNAP and of social safety net programs more broadly. Auto-enrollment clearly has its shortcomings, though: programs designed to reduce the cost of applying for a social safety net benefit cannot overcome barriers to the use of the benefit, as in areas with low access to food stores. These programs may be best implemented in tandem with efforts to improve transportation options or to increase the availability of food stores in low-access areas.

Future work might evaluate the effect of the CAP on outcomes beyond SNAP take-up, such as food insecurity and overall consumption. Whether the CAP induced a statistically significant increase in household consumption, for instance, would be illuminating. Data on caloric intake or nutrition-related measures would allow for an assessment of the effect of the CAP on the health of those affected.

Repeating my analysis with long-run panel data might elucidate the impact of the CAP in the longer term: whether those initially enrolled in SNAP by the CAP subsequently re-enrolled after the expiration of their initial period of benefit receipt would be worth investigating. In the short-term, at least, transaction costs associated with the SNAP application process have evidently made the program inaccessible to much of the population that it is intended to serve.

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Appendix A: Supplementary Data on the CAP

Table A.1 reports adoption dates, format, and detailed eligibility criteria and enrollment procedures for the CAP in each state.

Table A.1: CAP Adoption Dates and Policy Details by State

State	Effective Date	Eligibility Criteria	Standard (S) or Modified (M)	SNAP Enrollment Procedure
South Carolina	1995	SSI recipients living alone or preparing meals separately, with no earned income	S	Joint enrollment in SNAP during SSI application; those already enrolled in SSI were mailed simplified SNAP application form
Mississippi	Oct. 2001	SSI recipients living alone or preparing meals separately, with no earned income	S	Joint enrollment in SNAP during SSI application; those already enrolled in SSI were mailed simplified SNAP application form
Washington	Dec. 2001	One-person SSI households with no earned income	S	Joint enrollment in SNAP during SSI application; those already enrolled in SSI were mailed simplified SNAP application form
Texas	Dec. 2002	SSI-eligible individuals over 60 with no earned income or a disability	M	Simplified SNAP application form mailed to SSI recipients
New York	Nov. 2003	SSI recipients living alone	S	Auto-enrollment in SNAP for all eligible SSI recipients
Massachusetts	Feb. 2005	Unmarried SSI recipients who prepare food separately from others and have no earnings	S	Joint enrollment in SNAP during SSI application; auto-enrollment for those already enrolled in SSI
Florida	Mar. 2005	One-person SSI households with no earned income	S	Joint enrollment in SNAP during SSI application
North Carolina	Feb. 2006	SSI recipients living alone and preparing meals separately who are age 65+	M	Simplified SNAP application form mailed to SSI recipients
Pennsylvania	Feb. 2006	SSI-eligible individuals who do not live with a spouse or children, prepare food separately, and have no earned income	S	Joint enrollment in SNAP during SSI application; auto-enrollment for those already enrolled in SSI
Virginia	Feb. 2006	Unmarried SSI recipients ages 65+ with no earned income	M	Simplified SNAP application form mailed to SSI recipients
Kentucky	June 2007	n/a	M	n/a
Louisiana	July 2007	SSI recipients not living with a spouse or children and preparing meals separately, ages 65+	M	Simplified SNAP application form mailed to SSI recipients
Michigan	Dec. 2008	SSI recipients living independently and preparing food separately, with no earned income	M	Simplified SNAP application form mailed to SSI recipients
New Jersey	Dec. 2008	SSI-eligible individuals ages 65+ who live alone and have no earned income	M	Simplified SNAP application form mailed to SSI recipients
Arizona	Dec. 2008	n/a	M	n/a
South Dakota	Dec. 2009	n/a	M	n/a
New Mexico	May 2009; suspended May 2014	SSI recipients with no earned income living alone and purchasing food separately, or living with a spouse who also receives SSI	M	Simplified SNAP application form mailed to SSI recipients, but SSI applicants can also apply at SSA office
Maryland	July 2010	SSI-eligible individuals ages 60+ who live alone or prepare meals separately and have no earned income	M	Simplified SNAP application form mailed to SSI recipients; mailed a second form if no response to the first

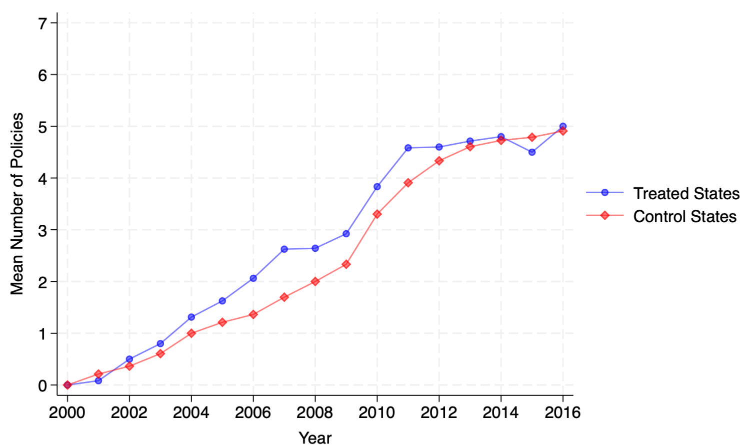
Notes: Entries of "n/a" indicate no information found; I exclude all states with entries of "n/a" from my analyses. References for this table are Weinstein-Tull and Jones (2017), FNS (2005), the USDA SNAP policy database, and state government agency websites.

Appendix B: Evolution of Other SNAP-Related Policies Over Time

My regressions control for the existence of other state-level policies besides the CAP that might have simplified the SNAP certification process. These seven policies (listed in Section 3.3.1 and in the figure notes below) were commonly introduced during my study period.

Figure B.1 displays the evolution of these other policies over time, by plotting the average number of the seven policies present in treated and control states in each year. Although there is a small gap (no larger than one policy) in levels from about 2004-2012, the slopes of the trend lines are highly similar between treated and control throughout. Even though my regressions already control for these particular policies, the lack of a differential trend is reassuring that other, unobserved SNAP-related policy differences were also likely to have evolved similarly in states with and without the CAP.

Figure B.1: Number of Other SNAP-Related Policies in Each Year



Notes: Figure shows the average number of other policies (excluding the CAP) aimed at simplifying the SNAP certification process that were present in states ever treated by the CAP (blue) and never treated by the CAP (red), in each calendar year. The set of policies is as follows: broad-based categorical eligibility to waive the income or assets tests, waivers to eliminate the face-interview requirement at initial application or re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, and the elimination of the fingerprinting requirement at initial application.

Appendix C: Details on the Targeting Analysis

As described in Section 3.3.2, to analyze the effect of the CAP on the targeting efficiency of SNAP, I subset the sample of likely SSI recipients by predicted marginal utility of food consumption – measured as food insecurity – and check for heterogeneity in the effect of the CAP across these subsets.

The ACS data that I use for my main analysis does not contain a measure of food insecurity. As such, I use the 2014-2020 panels of the SIPP to construct such a measure. I define the indicator for experiencing food insecurity F_{it} to take the value 1 for respondent i in SIPP survey wave t if and only if at least one of the following conditions is met:

1. The respondent reports that they often could not afford balanced meals.
2. The respondent reports that, in the given survey year, they ever skipped or cut the size of meals because they could not afford food.
3. The respondent reports that, in the given survey year, they ever ate less than they thought they should because there wasn't enough money for food.

With this indicator F_{it} as the outcome variable, I estimate equation 6, where the vector X_{it} contains the following demographic/socioeconomic characteristics: sex, race, ethnicity, age, education level, income bracket, and disability status. I merge the predicted values $\hat{F}_{it}$ into the ACS sample by each existing combination of values of these same demographic/socioeconomic characteristics, to obtain predicted probabilities of being food insecure in the main analysis sample.

I find that predicted food insecurity $\hat{F}_{it}$ is higher among those who are Black (or more generally, non-white and non-Asian), have less than a high school degree, or are younger than 70. See Table C.1 below, which displays regression coefficients in linear probability models where I regress F_{it} on various combinations of these characteristics.

Table C.1: Linear Probability Models of Food Insecurity by Demographic/Socioeconomic Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	-0.0111***						0.00954
	(0.003)						(0.011)
Black		0.0693***					0.0469***
		(0.003)					(0.013)
Asian		-0.0171**					-0.0215
		(0.006)					(0.019)
Other Race		0.128***					0.0953**
		(0.009)					(0.034)
Hispanic		0.0303***					0.00586
		(0.004)					(0.015)
Has Disability			0.174***				0.108
			(0.019)				(0.063)
HS Grad				-0.0504***			-0.0508***
				(0.003)			(0.011)
Some College				-0.00577			-0.0220
				(0.004)			(0.017)
College Grad				-0.0482***			-0.0568***
				(0.004)			(0.014)
Income ($10,000s)					-0.000286		0.0140
					(0.003)		(0.009)
Age 70-79						-0.0549***	-0.0568***
						(0.003)	(0.013)
Age 80+						-0.152***	-0.150***
						(0.003)	(0.012)
N	108,431	108,431	108,431	108,431	108,431	108,431	108,431

Notes: Table shows coefficient estimates in regressions where the outcome variable is the indicator F_{it} for being food insecure and the regressors are various combinations of demographic/socioeconomic characteristics. Standard errors clustered at the person level are in parentheses below each estimate. The omitted category for the race, education, and age variables are respectively White, less than high school, and age 65-69. Note that in calculating predicted food insecurity $\hat{F}_{it}$ for the targeting analysis, I use discrete income buckets (calculated within the ACS target sample), whereas here I use annual household income (in $10,000s) for illustrative purposes. The sample is drawn from the 2014-2020 panels of the SIPP and restricted to match the characteristics of the ACS target sample.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix D: Statistics on Measures of SNAP Retailer Accessibility

In Section 3.4, I describe how I construct a measure of the probability that each SSI recipient in my data lives in an area with low access to SNAP retailers, to evaluate how the effect of the CAP varies with the ease of spending SNAP benefits.

To complement Figure 3, Table D.1 shows statistics on the population-weighted average of various census-tract-level indicators in the USDA's Food Access Research Atlas for having low access to SNAP retailers. In my analysis, I use the measure in the first row, "low access at 1 and 10 miles", which indicates whether at least 500 people or at least 1/3 of the population in a given census tract lives more than 1 mile away from the nearest food store if they live in an urban area, or more than 10 miles away if they live in a rural area.

Table D.1: Statistics on the Probability of Having Low Access to SNAP Retailers

	Mean	Median	Min.	Max.	Std. Dev.
Low-Access at 1 and 10 mi.	0.0105	0.0101	0	0.0430	0.0073
Low-Access at 1/2 and 10 mi.	0.0181	0.0171	0	0.0526	0.0099
Low-Access with Vehicle	0.0093	0.0084	0	0.0417	0.0063
Share Age 65+ Low-Access at 1/2 mi.	0.0031	0.0031	0	0.0081	0.0017
Observations	476,883				

Notes: Table shows summary statistics for four probability measures of the geographic accessibility of SNAP retailers, using data from the USDA's Food Access Research Atlas. Each row is a population-weighted average of a census-tract-level variable. "Low-Access at 1 and 10 mi.", for instance, is a population-weighted average at the PUMA level constructed from an indicator for whether at least 500 people or at least 1/3 of the population in a given census tract lives more than 1 mile away from the nearest food store if they live in an urban area, or more than 10 miles away if they live in a rural area. The third row factors in vehicle access. The fourth uses a variable for the share of the census tract that is seniors living more than 1/2 mile away from the nearest store. Accessibility data is from 2015 and includes data on supercenters, supermarkets, and large grocery stores. The sample contains all residents of the following states included in my singles-couples research design, from 2005-2016: Florida, Louisiana, Maryland, Massachusetts, Michigan, New Jersey, New York, North Carolina, Pennsylvania, Virginia, and Washington.

Appendix E: "Intent to Treat" Interpretation of Main Estimates

Section 3.4.1 notes that my estimates of the effect of the CAP on SNAP take-up may be attenuated, as I use a target sample of "likely" SSI recipients, many of whom may not actually be enrolled in SSI, making them ineligible for the CAP. Apart from repeating my analysis with the sample of self-reported SSI recipients – with results shown in Section 4.2 – here I take a different approach to addressing this concern about attenuation.

Following the instrumental variables literature,[30] here I interpret my estimates of the effect of the CAP on SNAP take-up from Table 2 as an "intent-to-treat," where the reported SSI participation rate among the target sample in the pre-period is the "first stage." I therefore scale up this intent to treat (denoted ITT below) by the first stage (denoted FS) to compute an average treatment effect on the treated, as follows:

$$ATT = \frac{ITT}{FS}. \tag{7}$$

I calculate standard errors via block bootstrapping with 100 repetitions, clustered at the state level.

In the state-level research design, I obtain an intent to treat of 0.0375 (see Table 2) for the effect of the CAP on SNAP take-up, and a first stage of 0.11 (see Table 1) for the SSI participation rate among the target sample. Applying equation 7 yields an ATT estimate of 0.3125 (standard error = 0.083). Analogously, for the singles-couples research design, I obtain an intent to treat of 0.0425 and a first stage of 0.11, yielding an ATT estimate of 0.3864 (standard error = 0.147).

These estimates imply that the CAP led to an increase in SNAP take-up of between 31-39 percentage points, which is more than a doubling of the baseline SNAP take-up rate (albeit with large standard errors). The magnitude of these estimates suggests that the effect of the CAP among "actual" SSI recipients might have been somewhat larger than my main estimates.

[30]See Angrist et al. (1996).

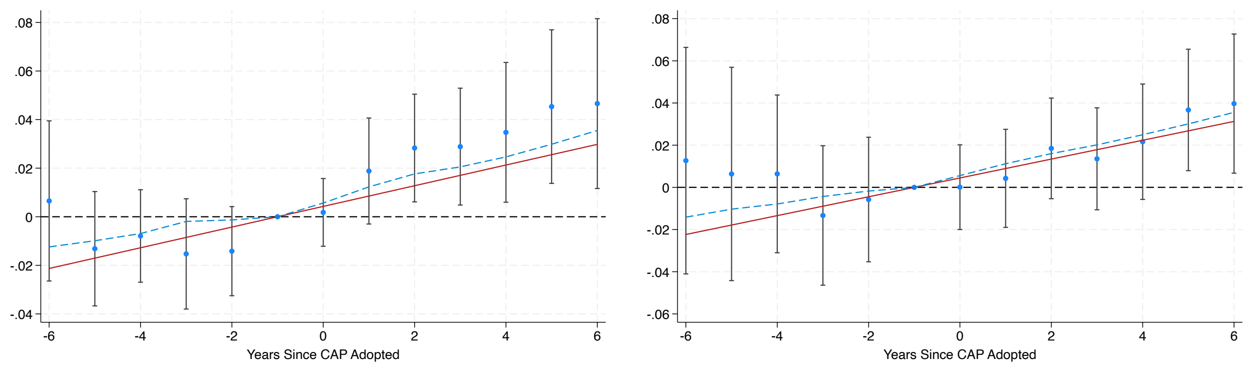
Appendix F: Power of the Tests for Parallel Trends

In Section 4, I showed that, in both the state-level and singles-couples research designs, I cannot reject the parallel trends assumptions, as none of the pre-period coefficient estimates is significantly different from zero. However, Roth (2022) finds that tests for parallel trends of this kind are often underpowered and may fail to detect violations of parallel trends that generate substantial bias. In this section I explore the power of my tests for pre-trends, following the analysis in Roth (2022) and focusing specifically on linear violations of parallel trends.

Figure F.1 shows the hypothesized trend (in red) and expected coefficient estimates conditional on pre-testing (in dotted blue) that would result from a linear violation of parallel trends with a slope of about 0.004, overlayed on the TWFE coefficient estimates and confidence intervals for the state-level and singles-couples research designs. This particular violation of parallel trends – chosen for illustrative purposes – would occur if the difference in SNAP enrollment between the treated and control groups increased by 0.004 each year in the absence of the CAP.

Figure F.1: Linear Violations of Parallel Trends with Slope 0.004

(a) State-level Design　　　　(b) Singles-Couples Design



Notes: Figure shows event study estimates using the state-level and singles-couples research designs in (a) and (b) respectively, along with the hypothesized trend (in red) and expected coefficient estimates conditional on pre-testing (in dotted blue) that would result from a linear violation of parallel trends with slope 0.004. See Roth (2022) for details.

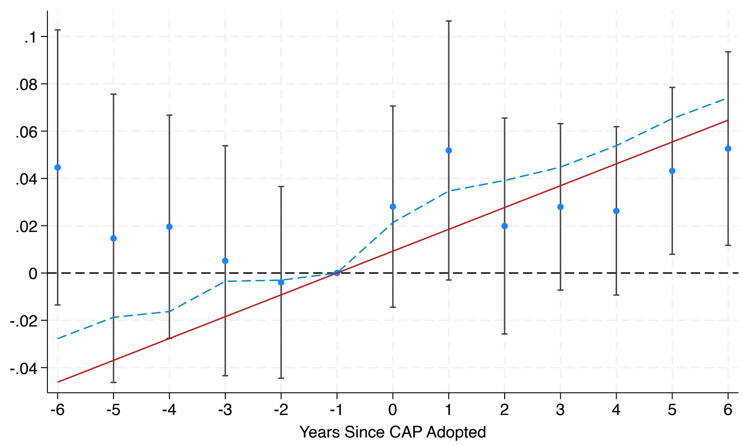
It is visually clear from sub-figure (b) that, in the singles-couples design, a linear trend of this size generates bias in magnitude roughly equal to the estimated treatment effects. Furthermore, applying the analysis in Roth (2022) confirms that my test for pre-trends has only 25% power to detect such a violation (i.e. this trend would generate at least one statistically significant pre-period coefficient only a quarter of the time). This suggests that my pre-trends test for the singles-couples design may be underpowered.

On the other hand, sub-figure (a) shows that the bias generated from a trend of this size is far below the estimated treatment effects in the state-level design: in relative year 2, for instance, the estimated treatment effect is nearly twice as large as the average bias that this trend would produce. My analysis shows that the pre-trends test for this design has 50% power to detect such a trend. Any steeper trend would be detected with even higher probability. The state-level design is thus fairly well-powered to detect linear violations of parallel trends that generate substantial bias.

Offering further robustness to violations of parallel trends is the fact that I additionally use a triple-difference design. Following Olden and Møen (2022), it is clear that even if both parallel trends assumptions for the state-level and singles-couples designs fail, if the bias in the two estimators is roughly the same, then the triple-difference estimates will be unbiased.

The triple-difference design itself comes with a parallel trends assumption – that the *relative* difference in SNAP enrollment between singles and couples in treated states and in control states trend similarly – which can be tested. Figure F.2 shows event-study estimates for the triple difference design, where the estimates are the coefficients on the interaction of [single household type] x [treated state] x [year since the CAP was adopted]. Given that none of the pre-period coefficient estimates is significant, I cannot reject the parallel trends assumption.

Figure F.2: Linear Violation of Parallel Trends with Slope 0.009 in Triple Difference Regression



Notes: Figure shows event study estimates using the triple difference design, along with the hypothesized trend (in red) and expected coefficient estimates conditional on pre-testing (in dotted blue) that would result from a linear violation of parallel trends with slope 0.009, against which the pre-trends test has 50% power. The event study estimates present coefficients on the interaction of [single household type] x [treated state] x [year since the CAP was adopted]. See Roth (2022) for details.

However, this test is similarly subject to concerns about power. Superimposed on this plot are the hypothesized trend and expected coefficient estimates that would result from a linear violation of parallel trends with a slope of about 0.009, which is the violation that this pre-trends test would have 50% power to detect. This slope would generate sufficient bias to account for most of the estimated treatment effects, but the estimated effect in relative year 1 is well above the average bias generated by this trend. The triple-difference design is thus reasonably well-powered to detect linear trends that generate considerable bias.

Ultimately, my use of three research designs – each with a distinct parallel trends assumption and with quite different estimates of the pre-period difference between treatment and control – provides robustness to a number of violations of parallel trends, even if each design alone may suffer from limitations to pre-testing.

Appendix G: Value of the CAP for the Marginal Applicant

This section performs a back-of-the-envelope calculation of the monetary value of the CAP for the marginal SNAP applicant.

Suppose for simplicity that all SNAP recipients are paid the same benefit amount each month, and that the amount of time required to apply for SNAP is constant across applicants. In particular, assume based on USDA (2018) that SNAP benefits are \$114 per month, and based on Ponza et al. (1999) that the SNAP application process takes 5 hours. Consider the marginal applicant for SNAP: since they are indifferent between applying and not applying for SNAP, their cost of applying is exactly equal to the net present value of SNAP benefits.[31] In the absence of the CAP, the marginal SNAP applicant is the one who assigns monetary value v to an hour of time such that

$$\sum_{t=1}^{12} \frac{114}{(1+r)^t} = 5 \cdot v. \tag{8}$$

Assume a monthly discount rate of $r = 0.078$, following Shapiro (2005).[32] Then the left-hand-side of equation 8 simplifies to \$868.09, and solving for v yields $v = \$173.62$.

To approximate the monetary value of the time saved for the marginal applicant by the adoption of the CAP, assume that the CAP reduced the SNAP application time by 2 hours.[33] The (previously) marginal CAP-eligible applicant for whom $v = \$173.62$ now has a cost of only $3 \cdot 173.62 = \$520.85$ of applying for SNAP, implying that they earn a surplus of $868.09 - 520.85 = \$347.23$. This is about 5% of the average annual income of an individual in my sample (see Table 1).

[31] I assume that SNAP benefits stop after one year, as recipients are required to re-certify.

[32] Shapiro (2005) finds an annual discount rate of 146% among SNAP recipients, which is equivalent to a monthly discount rate of about 7.8%. This discount rate is quite high; if I instead assume an annual discount rate of 25%, which translates to a monthly discount rate of 1.9%, I calculate that the marginal SNAP applicant saved \$485.21 due to the CAP. This value is substantially larger than the one reported in the text.

[33] The CAP removed the face interview required for the SNAP application, which alone can take roughly 30 minutes, along with any associated travel time to present at the SNAP office, which I assume takes another 30 minutes. Any remaining forms that the applicant was required to complete were vastly simplified relative to the standard SNAP application form, which I assume saves the applicant an additional hour.

Appendix H: Placebo Test Details and Supplementary Figures

H.1 Nearest Neighbor Matching Algorithm to Select Placebo "Treated" States

To conduct the first of two placebo tests, I apply the following nearest-neighbor matching algorithm to choose placebo "treated" states among the control states: I first match each treated state to the five nearest control states according to (1) state latitude/longitude coordinates, (2) median income, and (3) the proportion of each state's representatives that are Democrats. This generates up to 15 control states matched to each treated state and ranked out of five across the three categories. To choose a unique match for each treated state, I iterate over the following steps: in the first instance, I match each treated state to the control state that ranked first in at least two of the three characteristics, if such a control state satisfies this criterion. Excluding the previously matched control states, I then match control states that appeared as both a first match in one category and a second-fifth match in one or more of the other two categories.

I continue iterating on the above two steps with the second through fifth ranks. After this process, only two treated states (Kentucky and North Carolina) remain unmatched, for which I select the only two previously unmatched control states that ranked as a first match in any category. This process leads to a unique matching of each treated state to a control state. Table H.1 shows the results of the matching algorithm. The regression estimates are in Table 3.

Table H.1: Matched States for Placebo Test

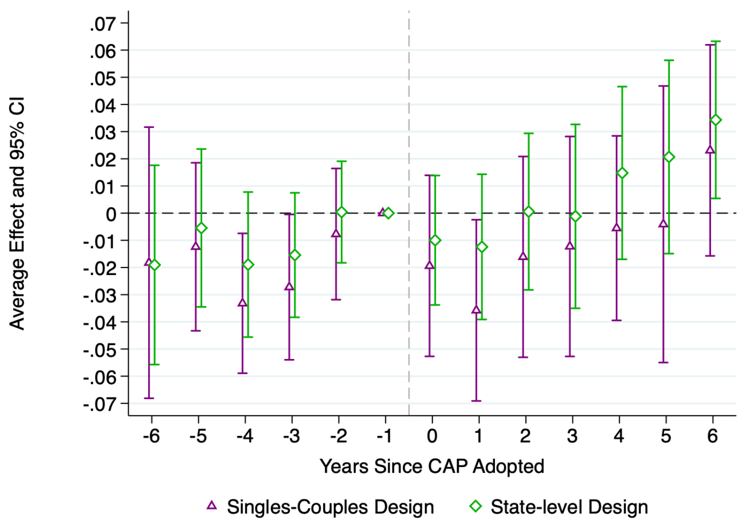
Treated State	**Matched Control State**
Arizona	Kansas
Florida	Alabama
Kentucky	New Hampshire
Louisiana	Arkansas
Massachusetts	Rhode Island
Michigan	Wisconsin
New Jersey	Connecticut
New York	Vermont
North Carolina	Colorado
Pennsylvania	Ohio
South Carolina	Tennessee
South Dakota	North Dakota
Texas	Missouri
Virginia	Delaware
Washington	Nevada

H.2 Supplementary Figures

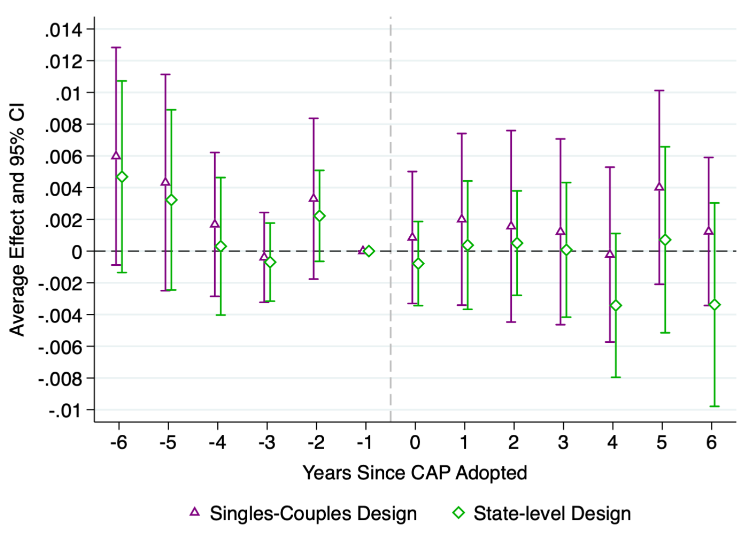
Figure H.1 displays estimates from my two placebo tests: sub-figure (a) shows the effect of a fake adoption of the CAP in a set of placebo "treated" states, and (b) shows the effect of the CAP on UI enrollment.

Figure H.1: Placebo Tests for the Effect of the CAP

(a) Placebo Test 1: Matched Placebo "Treated" States



(b) Placebo Test 2: UI Enrollment as Outcome Variable



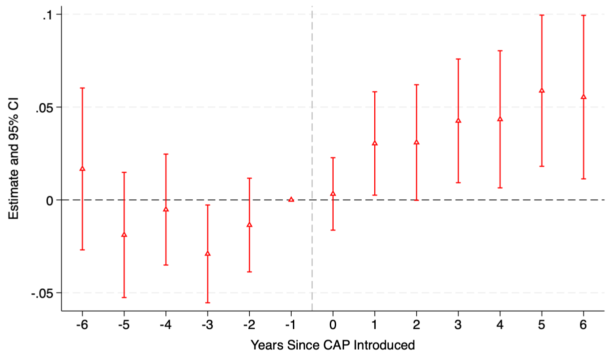
Notes: Figure shows estimates (with 95% confidence intervals) of β_τ in equations 1 and 3, where Panel (a) uses placebo "treated" states in place of the states that actually adopted the CAP and estimates the effect of the fake "adoption of the CAP" on SNAP enrollment, and Panel (b) estimates the effect of the adoption of the CAP (among actually treated states) on enrollment in unemployment insurance among working-age individuals. Standard errors are clustered at the state level. N = 560,227 persons and 257,427 persons for the state-level and singles-couples designs, respectively, in (a); and N = 860,709 persons and 470,164 persons for the state-level and singles-couples designs, respectively, in (b).

Appendix I: Robustness Checks: Supplementary Figures

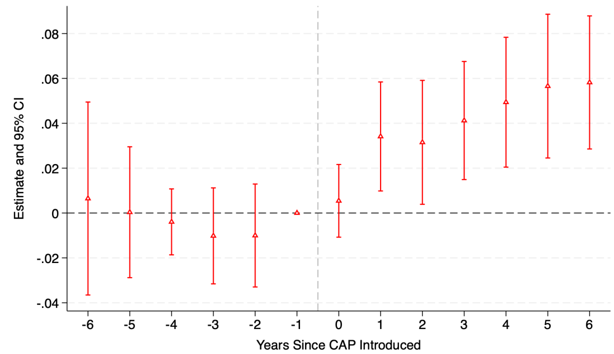
Figure I.1 shows estimates of the effect of the CAP on SNAP take-up using three alternative definitions of the target sample of likely SSI recipients, as described in Section 3.2.

Figure I.1: Effect of the CAP with Alternative Target Samples of "Likely" SSI Recipients

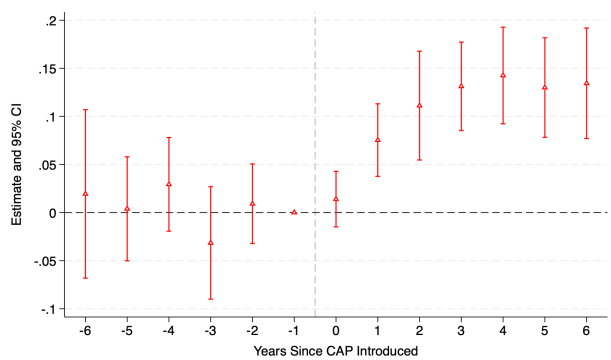
(a) 120% SSI Income



(b) ≤ HS Education & Non-homeowner



(c) Self-reported SSI Recipients



Notes: Figure shows estimates (with 95% confidence intervals) of β_τ in equation 1, using three alternative definitions of the target sample of likely SSI recipients. The outcome variable is an indicator for SNAP enrollment. Standard errors are clustered at the state level. N = 636,756 persons in (a), N = 807,198 persons in (b), and N = 243,293 persons in (c).