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 social safety net programs in the context of multi-program enrollment by exploiting the Combined Application Project (CAP), a widespread state-level policy designed to encourage enrollment in the Supplemental Nutrition Assistance Program (SNAP) among elderly recipients of Supplemental Security Income. I show that the CAP increased SNAP take-up by 8-13 percentage points, or about 17-24%. The increase was suggestively larger among those with a higher probability of being food insecure. Exploiting heterogeneity in the format of the CAP across states, I find that "auto-enrollment" most effectively increased SNAP take-up.

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

JEL codes: H53, I38, J14

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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 disentangle.

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 (see e.g. Finkelstein and Notowidigdo (2019) and Deshpande and Li (2019)).

While the economics literature has consistently shown that transaction costs reduce program take-up, the vast majority of this literature studies a single program in isolation. In reality, the U.S. social safety net is a complicated agglomeration of distinct cash and in-kind transfer programs administered at all levels of governance and with different eligibility criteria and application processes (Schmidt et al. (2025)). Analyses of the effects of administrative burdens that focus on a single program in isolation can obscure interaction effects across programs (Schmidt et al. (2023)). Since the existing empirical work almost exclusively studies either researcher-developed interventions or natural experiments in which administrative hassles were reduced by chance, the effects of real-world policies implemented with the explicit intention of reducing transaction costs in the context of multi-program enrollment are little understood.

This paper studies a sample of elderly Supplemental Security Income (SSI) recipients 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[1] and food insecurity among this population is pervasive,[2] more than half of SSI recipients never enroll in SNAP. Between 1995-2010, 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 broadly takes one of two forms. The "Standard" CAP is a joint-filing procedure, in 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, in which SSI recipients are mailed simplified SNAP application

[1] 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.

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

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 the induced reduction in transaction costs increased the take-up of SNAP among SSI recipients ages 60 and older. To study whether the CAP generated a positive or negative change in 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.[3] 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 that check whether the CAP affected enrollment in other social safety net programs among the low-income elderly. I also conduct two sets of robustness checks, one of which imposes a balanced panel restriction, and the other uses alternative definitions of the analysis sample of SSI recipients.

My average treatment effect estimates show that the introduction of the CAP caused a statistically significant, 8-13 percentage-point increase in SNAP take-up from a baseline rate of about 40%, which is a roughly 17-24 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 change in take-up generated by the CAP was well-targeted. 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. Policies like the CAP can effectively increase the take-up of

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

in-kind transfers among individuals already receiving or applying for cash benefits.

While there is agreement among the literature that transaction costs discourage the take-up of social safety net programs, the specific channels through which these costs operate—particularly in the context of multi-program enrollment—are less clear. To investigate the mechanisms through 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 increasing SNAP take-up. I also explore whether the effectiveness of auto-enrollment into SNAP varied with the ease 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,[4] suggesting an important role for default options in the decision about whether to enroll in SNAP. However, the effectiveness of auto- and joint-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 enrolled.

I make three contributions to the literature. Despite broad consensus among economists that transaction costs reduce 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 program enrollment in a multi-program context; the effects of administrative burdens in such contexts are nuanced and understudied (Schmidt et al. (2023)). Unlike many researcher-developed interventions, the CAP took the form of automatic enrollment in several states. The behavioral effects of the CAP suggest insights about the importance and limitations of default options that might extend to diverse 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, and McIntyre et al. (2021) 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 constructing a policy index to study trends in overall SNAP enrollment. Ganong and Liebman (2018) estimated the joint effect of the CAP and seven other state policies on county-level SNAP enrollment and found a positive and statistically significant effect of the eight policies combined. Jones et al. (2022) followed this approach[5] with eleven policies and additionally use

[4]Note that my estimates of the auto-enrollment effect (relative to other CAP formats) are correlational, as the format of the CAP in each state was not necessarily exogenously chosen.

[5]In addition to Jones et al. (2022), several later papers followed Ganong and Liebman (2018)'s method, creating a SNAP

fixed-effects regression to separately estimate the effect of each policy, in which they found 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.[6] 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 SSI recipients to isolate the impact of the CAP among those eligible. By isolating the effect of the CAP from that of other SNAP-related policies included in broad indices used in the literature, I provide evidence on the specific channels through which policies of this nature lead to increased SNAP enrollment. For instance, this evidence is useful for identifying the behavioral responses to joint-enrollment programs specifically.

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 that programs that reduce transaction costs generate a positive change in targeting, 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 60 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 particularly susceptible to food insecurity—as many as 22% of individuals experience food insecurity between ages 60-80—and yet the SNAP take-up rate among seniors is only half that of the general population (Levy (2022), Heflin et al. (2023)). 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 correlational evidence on the mechanisms through which transaction costs impede the take-up of welfare programs. Among the smaller subset of existing studies of transaction costs and program take-up that have isolated specific mechanisms, Bhargava and Manoli (2015) used different treatment arms and survey evidence to decompose the costs of EITC receipt and found 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 used different treatment arms to evaluate mechanisms and concluded that both information and assistance played important roles in increasing SNAP take-up, and Deshpande and Li (2019) found that increased congestion at the SSA offices that remained open is the main channel through which the closing of SSA offices reduced enrollment in

"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 the CAP.

disability benefits. I contribute to this literature by exploiting variation in the format of the CAP across states to disentangle "learning costs"—related to lack of information about SNAP or misperceptions of eligibility[7]—from other transaction costs that are differentially reduced by auto- and joint-enrollment. My findings contribute new evidence that default options are important determinants of SNAP enrollment.

The remainder of this paper proceeds as follows. The "Institutional Background" section provides institutional details on the CAP. The "Empirical Setup" section describes my data sources and empirical strategy. The "Results" section presents the results on take-up along with the related placebo tests and robustness checks, followed by the results on targeting. The "Mechanisms" section explores the channels through which the CAP increased SNAP take-up. The "Conclusion" section discusses implications and next steps.

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 often necessitates that they appear in-person at a SNAP office (Giannella et al. (2022)).[8] 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[9] 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[10]—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 SSI recipients ages 60 and older in my sample was around 40% at baseline (see Table 1)[11]. 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

[7] See Ko and Moffitt (2022) for a detailed discussion of the role of imperfect information in incomplete take-up of social safety net programs. Lack of knowledge about benefits is commonly cited in survey responses as the reason for being unenrolled in a program.

[8] By 2010, all U.S. states had received waivers from the USDA permitting SNAP interviews to take place by phone. I control for the staggered receipt of these waivers in my regressions.

[9] Resources include cash, bank accounts, stocks, vehicles, land, and other items that could be converted to cash (SSA (2025)).

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

[11] See the "Takeup: Addressing Measurement Error with Administrative Data" section for a comparison to SNAP participation rates in administrative data.

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 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.

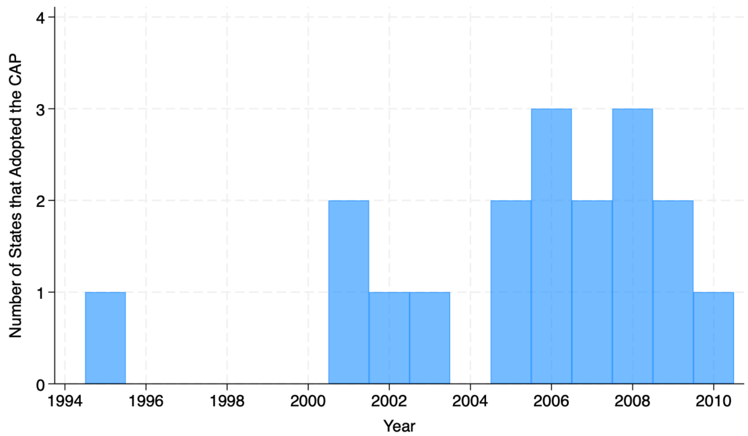
Under the Modified CAP, the state CAP agency requests contact information for SSI recipients from the SSA and mails these individuals a simplified SNAP application form (FNS (2005), Weinstein-Tull and Jones (2017)). Some Modified CAP states, such as Maryland, re-send the form if the SSI recipient does 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 1995-2010.[12] 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.

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 60 and older, or, in some states, 65 and older. Details on the eligibility criteria and enrollment procedures in each state are in Appendix Table A.1.[13]

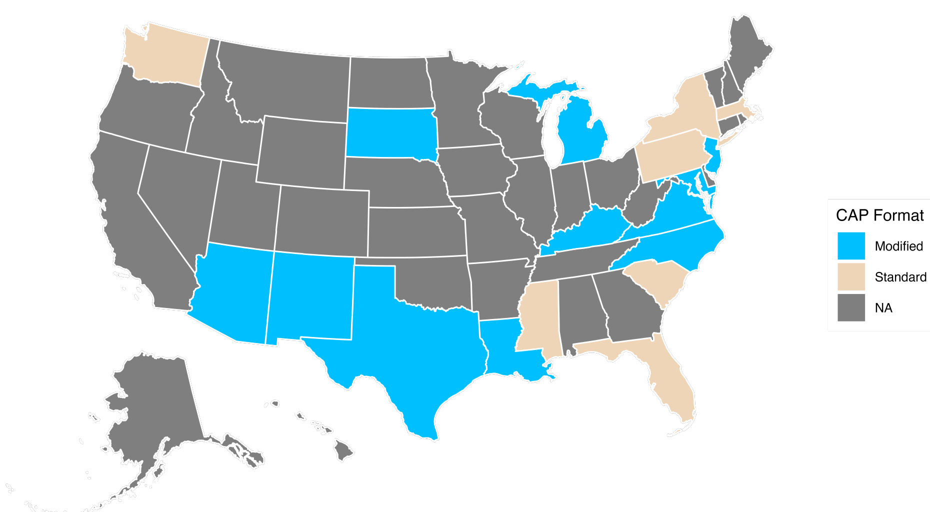
[12] New Mexico adopted the CAP in 2009 but later suspended the program. As of 2016, the CAP remained in operation in the other seventeen states (Weinstein-Tull and Jones (2017)).

[13] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at `http://onlinelibrary.wiley.com`.



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.

Figure 1. Distribution of CAP Adoption Years.



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.

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

EMPIRICAL SETUP

This section discusses my data sources and empirical strategy, and provides descriptive statistics for my analysis sample.

Data

My main source of data is the 2000-2016 American Community Surveys (ACS) provided by IPUMS USA (Ruggles and Sobek (2022)).[14] These data are repeated cross-sections and contain individual- and household-level characteristics, including an indicator for the receipt of SNAP at any time in the prior 12 months, which I use as my outcome variable, and a variable that records the amount of income received from SSI, which I use to construct my analysis sample of SSI recipients. 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 alternative samples of "likely" SSI recipients for my robustness checks, I use documentation of federal SSI benefit levels from the Social Security Administration website.[15]

For use as controls, I obtain state-by-year unemployment rates from the Bureau of Labor Statistics, and state-level political data and median incomes from the Census Bureau's "State and Metropolitan Area Book: 2006" (BLS (2023), U.S. Census Bureau (2006)). I additionally use the 2000-2016 Annual Social and Economic Supplement (ASEC) from the Current Population Survey (CPS) to obtain data on enrollment in Medicaid and the Low Income Home Energy Assistance Program (LIHEAP), which replace SNAP take-up as the outcome variables in my placebo tests (Flood and Westberry (2022)). For my analysis of the targeting efficiency of the CAP, 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 (U.S. Census Bureau (2021c)).

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 given tract have low access to food stores.[16] I calculate the probability that an individual in my data lives

[14] All summary statistics and regressions throughout the paper are weighted by the person-level survey weights included in the ACS to make the sample representative of the U.S. population.

[15] See `https://www.ssa.gov/OACT/COLA/SSIamts.html`.

[16] 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)).

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, and also requires a crosswalk between census tracts and PUMAs, which I construct using an application provided by the Missouri Census Data Center (U.S. Census Bureau (2021a), IPUMS (2023), MCDC (2018)).

As a robustness check to confirm that my estimates of the effect of the CAP are not driven by mismeasurement of SNAP or SSI participation in the ACS data, I repeat my analysis at the state-year level using publicly available administrative data on joint SNAP-SSI enrollment. These data are drawn from USDA annual reports on the characteristics of SNAP households, taken from the Food Stamp Program Quality Control sample (FNS (2025)).[17] For this analysis, I also use state population data from the Census Bureau (U.S. Census Bureau (2021b), U.S. Census Bureau (2025)).

Empirical Strategy

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.

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 one if individual i living in state s is enrolled in SNAP in year t, and zero 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 one if state s ever had the CAP and zero 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

[17] See, for instance, Table B-6 of the 2001 report: `https://fns-prod.azureedge.us/sites/default/files/2001CharReport.pdf`. I extract the text from the corresponding table in each fiscal year report to obtain SNAP-SSI counts for each state.

[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.

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 one if the CAP was in operation in state s in year t and zero 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 one for single SSI recipients and zero otherwise, and D_{ht} is an indicator that equals one for single SSI recipients in survey years after the adoption of the CAP and zero 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 one for single SSI recipients living in treated states after the adoption of the CAP and zero 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

[19] These controls are indicators for whether any part of the state had each 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. In Appendix B, I show that these policies evolved roughly in parallel between treated and control states.

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

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 discuss these assumptions in more detail in the "Testing for Selection into Adopting the CAP" section, and I provide evidence of parallel trends in the "Results" section.

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.

Targeting Analysis

After estimating the effect of the CAP on SNAP take-up, I explore its 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 a positive change in targeting efficiency 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 targeting efficiency of the CAP, 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.

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

I use these predicted probabilities to subset the sample by level of need for SNAP: in particular, I subset by above-below median and quartiles of the distribution of $\hat{F}_{it}$ and estimate equation (2) separately for each group, to detect heterogeneity in the effect of the CAP by level of predicted food insecurity.[22]

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

Descriptive Statistics

Table 1 shows descriptive statistics for my analysis samples of SSI recipients. Average annual income is only around $12,000, and yet less than half of the sample is enrolled in SNAP at baseline. About 30% of the sample is Black or Hispanic, a bit over half received less than a high school education, and about 70% report having a disability.

Table 1. Descriptive statistics on pre-period treatment and control samples.

	State-level design				Singles-couples design			
	Treated	Control	Difference	(SE)	Treated	Control	Difference	(SE)
SNAP Enrollment	0.391	0.317	0.074	(0.009)	0.425	0.290	0.135	(0.011)
Personal Income	$12,024	$12,689	-$665	($241)	$12,467	$14,448	-$1,982	($433)
Poverty Status	144	134	11	(2)	132	266	-134	(3)
Age	73.705	73.689	0.016	(0.176)	74.118	72.683	1.435	(0.197)
Black	0.212	0.182	0.030	(0.008)	0.270	0.143	0.128	(0.010)
Hispanic	0.171	0.076	0.094	(0.007)	0.116	0.124	-0.008	(0.009)
Female	0.743	0.762	-0.019	(0.009)	0.774	0.510	0.264	(0.011)
HS Dropout	0.626	0.549	0.077	(0.010)	0.586	0.510	0.076	(0.011)
HS Graduate	0.217	0.239	-0.022	(0.008)	0.243	0.269	-0.026	(0.010)
Some College	0.097	0.150	-0.053	(0.006)	0.103	0.117	-0.014	(0.007)
College Graduate	0.061	0.062	-0.002	(0.004)	0.068	0.103	-0.035	(0.006)
Disability	0.706	0.710	-0.004	(0.009)	0.697	0.656	0.041	(0.011)
Lives in Metro Area	0.155	0.185	-0.030	(0.005)	0.199	0.219	-0.020	(0.007)
N	14,005	10,219	24,224		8,411	7,621	16,032	

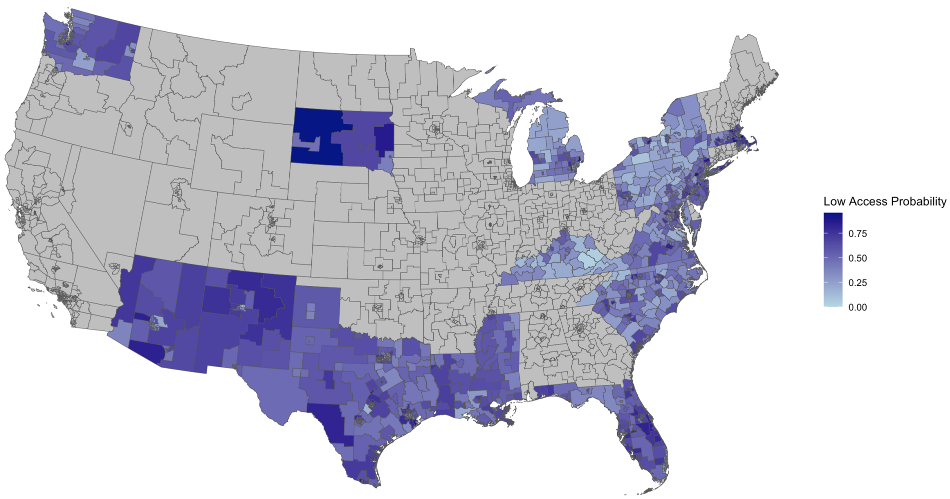
Notes: Table shows summary statistics for the treated and control groups, in the "state-level" research design (in which the treatment group is states that adopted the CAP and the control group is states that never adopted the CAP), and the "singles-couples" research design (in which the treatment group 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 the "Mechanisms" section, I evaluate whether the effect of the auto- and joint-enrollment versions of the CAP varied with the geographic accessibility of SNAP retailers, i.e. food stores that accept SNAP

[22]This definition of targeting, which focuses on the characteristics of new SNAP enrollees following the adoption of the CAP, is common in the literature (see e.g. Deshpande and Li (2019) for a similar analysis). An alternative definition might compare the characteristics of those newly enrolled via the CAP with the characteristics of *prior* enrollees.

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, 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 one 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 probability of having low access to SNAP retailers in each PUMA in states that adopted the CAP. There is considerable dispersion across locations.



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 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 one 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 CAP are shaded grey.

Figure 3. Probability of Low Access to SNAP Retailers in CAP States.

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, and (b) single versus married SSI recipients. As shown in panel (a), SNAP enrollment remained roughly constant over the entire sample period in states that never adopted the CAP, while it increased rapidly in years 1-3 after the introduction of the CAP in treated states. Panel (b) shows that SNAP enrollment increased gradually among both singles (who were eligible for the CAP) and couples (who were ineligible) in states that adopted the CAP, but at a faster rate among singles in the years after its adoption.

Testing for Selection into Adopting the CAP

As discussed in the "Take-up Analysis" subsection, my empirical strategy relies on the assumption that SNAP enrollment would have evolved in parallel between treated and control states absent the adoption of the CAP. More concretely, this assumption implies that the adoption of the CAP did not coincide with other changes to the SNAP policy environment, or with changes to the accessibility of social safety net programs more generally. This assumption would be violated if the adoption of the CAP in treated states occurred in tandem with the adoption of other policies that altered the SNAP application process or eligibility criteria. It would also be violated if states commonly introduced the CAP following a change in partisan power, say from Republican to Democrat, which might co-occur with broader changes to social safety net program structure and accessibility.

I find evidence in favor of these identifying assumptions in Appendix B, where I test whether CAP adoption can predict either the adoption of other SNAP-related policies or which political party is in power in the adopting state. Both sets of tests, performed with various alternative specifications, yield insignificant estimates, which suggests that selection into adopting the CAP does not drive my results on take-up presented below. My placebo tests—with enrollment in other social safety net programs used as the outcome in place of SNAP enrollment—provide additional evidence in this regard.

To further address the potential for lack of comparability of treated and control states in their underlying policy environments, my singles-couples research design, which compares single to married SSI recipients in states that adopted the CAP, uses an alternative identification strategy that does not rely on this assumption of comparability at the state level. As discussed below, the state-level and singles-couples designs yield estimates of the effect of the CAP that are highly similar in magnitude and significance, further indicating that my results are not driven by selection.

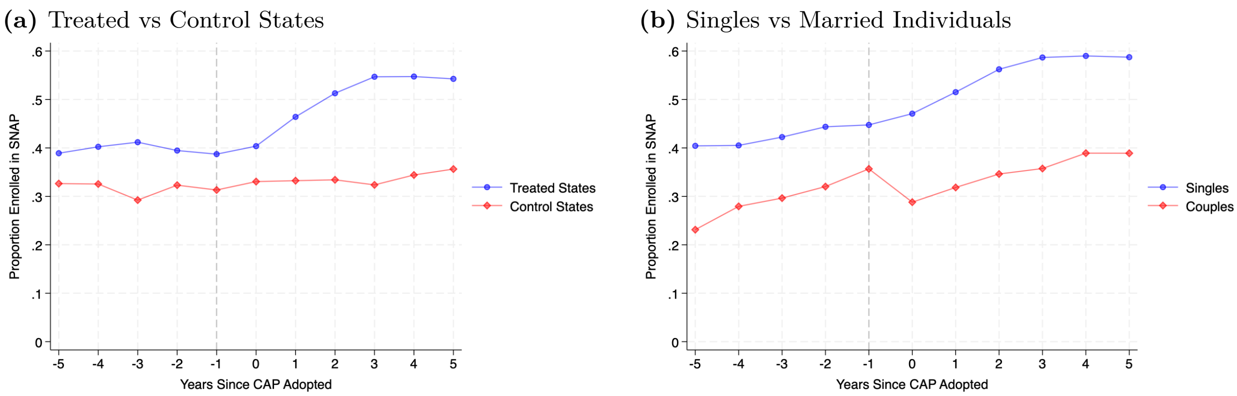
RESULTS

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

Take-up

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, as reflected in the small and insignificant pre-period coefficient estimates. In the years after the CAP was adopted, SNAP enrollment was statistically significantly higher in the treatment group relative to the



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 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 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 proportion of control observations as treated observations to each adoption year. Observations more than 5 years before or after the adoption of the CAP are omitted. N = 62,974 persons in (a), and N = 42,780 persons in (b).

Figure 4. Raw Plots of SNAP Enrollment Rates.

control group; the magnitude of this difference was increasing over relative years one through three and then remained constant through the fifth year after adoption.

These estimates show that the CAP generated a near-immediate increase in SNAP enrollment among eligible SSI recipients, but that the full effect of the CAP was not realized until about three full years after its adoption, perhaps due to administrative delays in setting up the CAP at all SSA offices across the state or in mailing simplified forms to all eligible SSI recipients. Once in place, the effect of the CAP was persistent, causing SNAP enrollment to remain at a higher level among treated than control units.

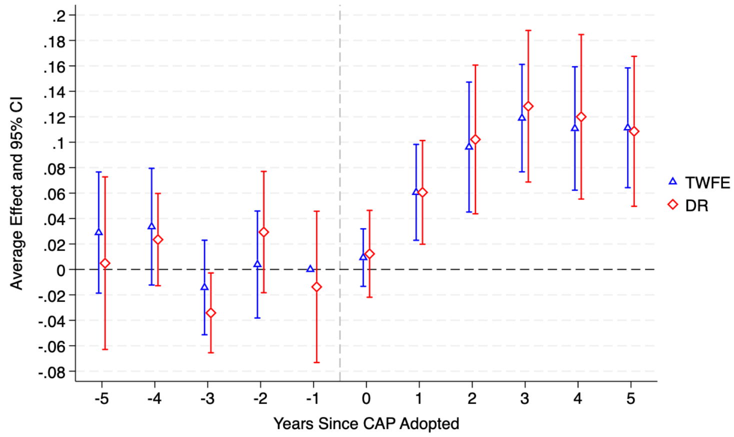
Nearly all pre-period coefficient estimates across specifications are insignificant, which provides supporting evidence of parallel trends in SNAP enrollment across the treatment and control groups. Analyses of the power of these tests for parallel trends are contained in Appendix D.

Average Treatment Effect Estimates

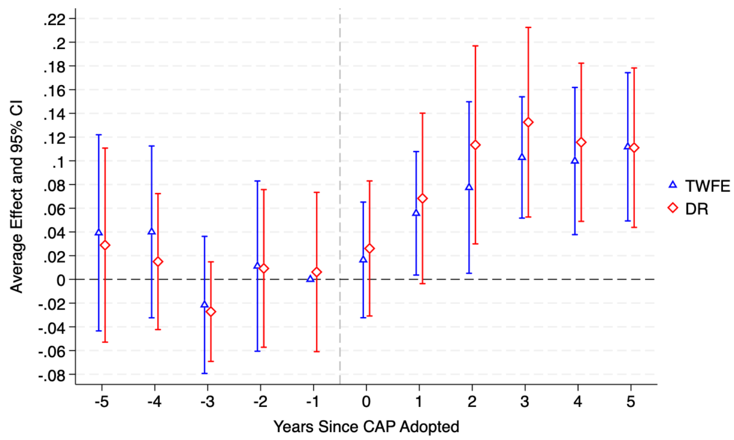
Panel A of Table 2 shows the average effect of the CAP on SNAP take-up, using the TWFE regressions in equations (2), (4) and (5). Columns (1)-(3) use the state-level design, columns (4)-(6) use the singles-couples design, and columns (7)-(8) use the triple difference design. The use of three distinct estimators provides additional confidence in the approximate magnitude of the effect; the state-level design is arguably most reliable since it is not susceptible to control group spillovers, which could attenuate the estimates from the other two designs.[23] As such, in all following analyses for which I present only one set of estimates, I use

[23] 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 reducing administrative barriers to applying

(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 not-yet-treated units as the control. The outcome variable is an indicator for SNAP enrollment. Individual-level controls are age and indicators for sex, race, ethnicity, education level, and physical disability. Observations more than 5 years after the adoption of the CAP are binned into a single indicator, and observations more than 5 years before the adoption of the CAP are binned into a separate indicator; the coefficients on these indicators are not plotted here. Standard errors are clustered at the state level. N = 141,461 persons in (a), and N = 90,353 persons in (b).

Figure 5. Effect of the CAP on SNAP Enrollment.

the state-level design.

The estimates convey that, after controlling for covariates, the CAP induced a significant, 8-10 percentage point increase in SNAP enrollment, which translates to a 17-24% increase from the roughly 40% enrollment rate at baseline.

In panel B of Table 2, I use the DR estimator from Callaway and Sant'Anna (2021), with not-yet-

for SNAP for couples and non-SSI recipients in these states as well. In the singles-couples 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.

treated[24] 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.

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	0.0947***	0.0974***	0.0967***					
	(0.021)	(0.021)	(0.020)					
Single x Post				0.125***	0.0897***	0.101***		
				(0.030)	(0.019)	(0.021)		
Single x Treated x Post							0.0742***	0.0768***
							(0.019)	(0.020)
Percent Change	0.234	0.241	0.239	0.276	0.197	0.223	0.163	0.169
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	141,461	141,461	141,461	90,353	90,353	90,353	216,741	216,741
Panel B: DR Estimator								
Treated x Post	0.122***	0.110***	-				-	-
	(0.030)	(0.026)	-				-	-
Single x Post			-	0.118***	0.131***	0.141***	-	-
			-	(0.034)	(0.043)	(0.048)	-	-
Percent Change	0.302	0.271	-	0.261	0.288	0.309	-	-
Year FE	✓	✓	-	✓	✓	✓	-	-
State FE	✓	✓	-				-	-
Household FE			-	✓	✓	✓	-	-
Individual Controls		✓	-		✓	✓	-	-
State Controls		✓	-			✓	-	-
N	139,718	139,718	-	88,610	88,610	85,560	-	-

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 not-yet-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. Percent change is calculated relative to the baseline SNAP enrollment rate among treated units in the year before the CAP was adopted. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

[24] Baker et al. (2025) argue that the use of not-yet-treated (rather than never-treated) units as the control group can reduce the likelihood of bias from violations of parallel trends. Not-yet treated units are plausibly more similar than never-treated units are to the treatment group in their unobservable characteristics.

The 17-24% increase in SNAP enrollment induced by the CAP is quite large—and comparable to estimates in the take-up literature on other policies and transfer programs[25]—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 3% of their annual income.[26] The capacity of programs like the CAP to reduce the financial burden of applying for social safety net benefits and to increase take-up is substantial.

My estimates convey that combinations of auto-enrollment, joint-enrollment, and reduced application complexity lead to increased SNAP take-up among those already receiving or applying for SSI. In the context of the fractured, multi-program nature of the U.S. social safety net, this finding suggests that even for existing program beneficiaries for whom eligibility for other programs is categorical, either the costs of applying for or lack of information about these other programs can preclude enrollment. The effect of the CAP thus has implications for policies intended to increase program enrollment among existing social safety net beneficiaries, and in particular, policies focused on in-kind transfer programs for existing cash transfer recipients. I explore the mechanisms underlying the effect of the CAP in a later section.

Take-up: Placebo Tests and Robustness Checks

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 underlying trends in enrollment in welfare programs more broadly, I perform two placebo tests, in which I investigate whether the CAP had an effect on enrollment in other social safety net programs among elderly SSI recipients. Specifically, I estimate my main specifications but with indicators for enrollment in Medicaid and in the Low Income Home Energy Assistance Program (LIHEAP) as the two outcomes. Neither of these programs was intended to be influenced by the CAP, but enrollment in these programs is common among elderly SSI recipients, making these two programs ideal outcomes for this analysis.

Table 3 shows the estimates of β in equations (2), (4), and (5), in each placebo test. The coefficient estimates across all specifications are uniformly small in magnitude and insignificant.[27] This result provides reassurance that the significant estimate for the effect of the CAP on SNAP take-up was in fact driven by the CAP rather than by coinciding policy changes or trends in social safety net take-up more generally.

[25]For instance, my range of estimates is within 7 percentage points of the estimate in Deshpande and Li (2019) and precisely includes the estimate in Homonoff and Somerville (2021) for the effect of transaction costs on Disability Insurance take-up and SNAP re-certification, respectively. In Appendix E, I reconcile the difference in magnitudes between my estimates of the effect of the CAP on SNAP take-up and the smaller estimate in Jones et al. (2022); conditioning on SSI eligibility/receipt turns out to be crucial to this difference.

[26]See Appendix F for the details of this calculation.

[27]Event study plots presented in Appendix G likewise show small and insignificant estimates across all post-period years.

Table 3. Placebo tests for effect of the CAP on enrollment in other programs.

	State-level design			Singles-Couples design			Triple difference	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Medicaid Enrollment								
Treated x Post	0.0164	0.0166	0.0159					
	(0.012)	(0.012)	(0.012)					
Single x Post				0.00898	-0.00416	0.00438		
				(0.008)	(0.007)	(0.011)		
Single x Treated x Post							-0.00117	0.000238
							(0.018)	(0.018)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	13,007	13,007	13,007	5,747	5,747	5,747	15,130	15,130
Panel B: LIHEAP Enrollment								
Treated x Post	-0.0221	-0.0202	-0.0186					
	(0.015)	(0.015)	(0.015)					
Single x Post				-0.0144	0.00755	-0.00657		
				(0.017)	(0.015)	(0.020)		
Single x Treated x Post							-0.0217	-0.0225
							(0.040)	(0.039)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	13,007	13,007	13,007	5,747	5,747	5,747	15,130	15,130

Notes: Panel (A) 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 using data on elderly SSI recipients surveyed in the CPS March ASEC. Panel (A) uses an indicator for whether the individual is enrolled in Medicaid as the outcome, and panel (B) uses an indicator for whether the individual is enrolled in LIHEAP. Individual controls are age and indicators for sex, race, and education level. State controls are yearly unemployment rates (and, additionally in column (6), 2003 median income and percent of congressional representatives that were Democrats in 2004) 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.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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	0.0755***	0.0751***	0.0741***					
	(0.021)	(0.021)	(0.018)					
Single x Post				0.0769**	0.0724**	0.0583**		
				(0.023)	(0.022)	(0.018)		
Single x Treated x Post							0.0581***	0.0603***
							(0.021)	(0.021)
Percent Change	0.177	0.176	0.173	0.170	0.160	0.129	0.128	0.133
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓		✓		✓	✓
Household FE				✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓		✓
State Controls			✓			✓		
N	82,725	82,725	82,725	45,733	45,733	45,733	157,347	157,347

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 (and additionally in column (6), 2003 median income and the percent of congressional representatives that were Democrats in 2004) 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. Percent change is calculated relative to the baseline SNAP enrollment rate among treated units in the year before the CAP was adopted. 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 roughly 6-8 percentage points, confirming that my main results are robust to this balanced panel restriction.

Next, I test whether I obtain similar estimates for the effect of the CAP using a sample of low-income elderly individuals who are "likely" to receive SSI, but who are not necessarily self-reported SSI recipients. This test addresses concerns about mis-reporting of SSI receipt in survey data and about selection into the program.[28] I use two methods to predict SSI enrollment using other characteristics in the ACS data. In the first, I define the target sample of likely SSI recipients as those whose reported income is less than 120% of the maximum federal SSI benefit amount. This sample aims to capture individuals whose main source

[28]The use of a "target" sample of "likely" recipients of a program is common in the literature on social safety net participation. Davern et al. (2019) provide an overview of one method to predict enrollment involving linked survey-administrative data. For an example in the context of SSI, see Neumark and Powers (2000).

of income is SSI. In the second, I define the sample as non-homeowners with no more than a high school education.[29]

Table 5 contains the estimates of β in the state-level design, using each of these alternative analysis samples. All coefficient estimates are positive and statistically significant. Most notably, while the percentage point effects are smaller in magnitude, the *percent* effects—which account for the smaller baseline level of SNAP enrollment among these samples—fall precisely within the 17-24% range obtained using my main analysis sample. This result provides support for the accuracy of the magnitudes of my estimates.

Appendix H displays event study plots corresponding to these robustness checks. In each case, there is evidence of parallel trends in the pre-period, followed by an 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 to alternative samples.

	120% of SSI income			$\leq$ HS education & non-homeowner		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	0.0432***	0.0422***	0.0414***	0.0408***	0.0390***	0.0379***
	(0.016)	(0.014)	(0.014)	(0.013)	(0.012)	(0.012)
Percent Change	0.180	0.176	0.173	0.222	0.212	0.206
Year FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Individual Controls		✓	✓		✓	✓
State Controls			✓			✓
N	352,060	352,060	352,060	400,892	400,892	400,892

Notes: Table shows estimates of β in equation (2), using two alternative analysis samples consisting of "likely" SSI recipients. Columns (1)-(3) define likely SSI recipients as those with income less than 120% of the federal SSI benefit maximum, and columns (4)-(6) use those with no more than a high school education who do not own a home. 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. Percent change is calculated relative to the baseline SNAP enrollment rate among treated units in the year before the CAP was adopted. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Takeup: Addressing Measurement Error with Administrative Data

The proportion of my sample enrolled in SNAP (shown in the first row of Table 1) is slightly lower than estimates from administrative data: administrative 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 slightly lower rates of reported take-up in my data are consistent with prior research which

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

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) showed that 35% of SNAP recipients fail to report that they are receiving SNAP.

Existing research shows that, in the presence of mis-reporting of participation in SNAP and other programs, models that use self-reported receipt as the dependent variable tend to generate attenuated treatment effect estimates that are robust in sign if not magnitude (Celhay et al. (2021); see also Courtemanche et al. (2015) for an example with a mismeasured dependent variable outside the context of program participation).[30] However, patterns of SNAP mis-reporting may be nuanced and non-random (Nguimkeu et al. (2019), Courtemanche et al. (2019)), and my empirical strategy crucially relies on the assumption that the CAP did not itself affect SNAP reporting, via increasing the salience of SNAP, for instance. Violation of this assumption could lead to bias in either direction.

A related issue is under-reporting of SSI participation, since SSI eligibility/receipt is a prerequisite for eligibility for the CAP.[31] Since I condition on self-reported SSI receipt in my analyses, misreporting of SSI enrollment is potentially more problematic, as it could threaten the validity of my analysis sample (Bollinger and Tasseva (2023)). Moreover, less is known about self-reporting of joint enrollment in multiple programs (in this case, both SNAP and SSI), with some work showing that joint program participation tends to be more strongly understated than enrollment in one program alone (Celhay et al. (2021)).

I address these concerns in two ways. The first is my robustness check with two target samples of "likely" SSI recipients, with results shown in Table 5: this analysis showed that the positive and significant effect of the CAP remains when I use other self-reported characteristics of survey respondents to infer SSI receipt.

While the target sample approach is useful in the case that survey respondents are mis-reporting SSI receipt, it relies on the assumption that survey respondents are accurately reporting their household income and demographic characteristics, where the former is subject to a similar critique (see, for instance, Bollinger and Tasseva (2023)), and it does not address mis-reporting of SNAP receipt. Hence, to more conclusively show that the effect of the CAP withstands measurement error related to survey reporting, I estimate the effect of the CAP at the state-year level using publicly available administrative data on joint SNAP and SSI enrollment.

Specifically, as an alternative to equation (1), I run the following regression at the state-year level,

$$y_{st} = \delta_s + \gamma_t + \sum_{\tau=-k}^{k} \beta_\tau (T_s \times \mathbb{I}\{t - F_s = \tau\}) + \tilde{X}_{st} + \epsilon_{st}, \tag{7}$$

where y_{st} is the number of individuals jointly enrolled in SNAP and SSI in state s in year t, and the other

[30] On the other hand, when SNAP enrollment is used as an independent variable, endogenous mis-reporting may bias the point estimates in either direction (Nguimkeu et al. (2019)). See Almada et al. (2016) for an example in the context of research on the effect of SNAP receipt on obesity.

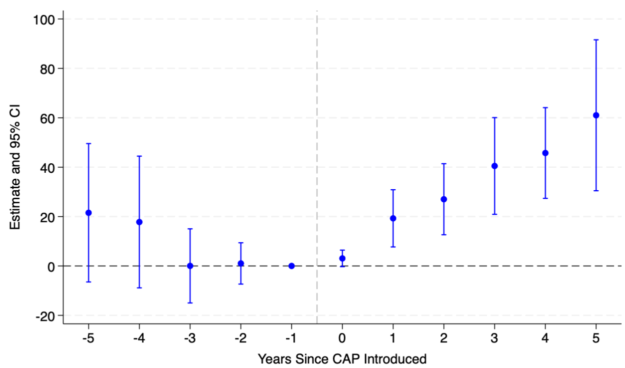
[31] Neumark and Powers (2004) documented a difference between SSA administrative data and self-reported SSI receipt among the elderly in the Survey of Income and Program Participation. They conjectured that individuals ages 65 and older might confuse SSI with Social Security benefits, as the two payments are distributed by the same local offices. This finding is consistent with broader evidence of confusion about income sources (Bollinger and Tasseva (2023)).

variables are as defined in equation (1). Analogously, to compute an average treatment effect, I estimate the following in place of equation (2),

$$y_{st} = \delta_s + \gamma_t + \beta D_{st} + \tilde{X}_{st} + \epsilon_{st}, \tag{8}$$

where y_{st} is once again the number of individuals jointly enrolled in SNAP and SSI in state s and year t.

Figure 6 and Table 6 display the results. The insignificant pre-period coefficients in Figure 6 provide evidence of parallel trends. Within the first year after the adoption of the CAP, the number of joint SNAP-SSI enrollees was significantly higher in treated than control states, and this gap persisted through the fifth year after its adoption. Table 6 shows that the CAP increased the SNAP-SSI caseload by about 40 individuals. This increase is on top of a baseline of 61 joint SNAP-SSI participants in treated states on average in the year before the CAP was adopted, which suggests that the CAP increased joint SNAP-SSI enrollment by upwards of 65%.



Notes: Figure shows estimates of β in equation (7), using publicly available administrative data from the USDA. The outcome is the number of individuals enrolled in both SNAP and SSI in each state-year. The regression is weighted by state populations. Standard errors are clustered at the state level. N = 829 state-years.

Figure 6. Effect of the CAP on SNAP Enrollment Using State-Year Administrative Data.

Table 6. Average effect of the CAP on SNAP enrollment using state-year administrative data.

	(1)	(2)
Treated x Post	45.02***	40.94***
	(11.184)	(8.458)
Percent Change	0.511	0.464
Year FE	✓	✓
State FE	✓	✓
State Controls		✓
N	829	829

Notes: Table shows estimates of β in equation (8) using publicly available administrative data from the USDA on enrollment in SNAP and SSI in each state-year between 2000-2016. The outcome variable is the number of individuals enrolled in both SNAP and SSI. State controls are 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. Regressions are weighted by state population in each year. Percent change is calculated relative to the baseline SNAP-SSI count among treated states in the year before the CAP was adopted. Standard errors clustered at the state level are in parentheses.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

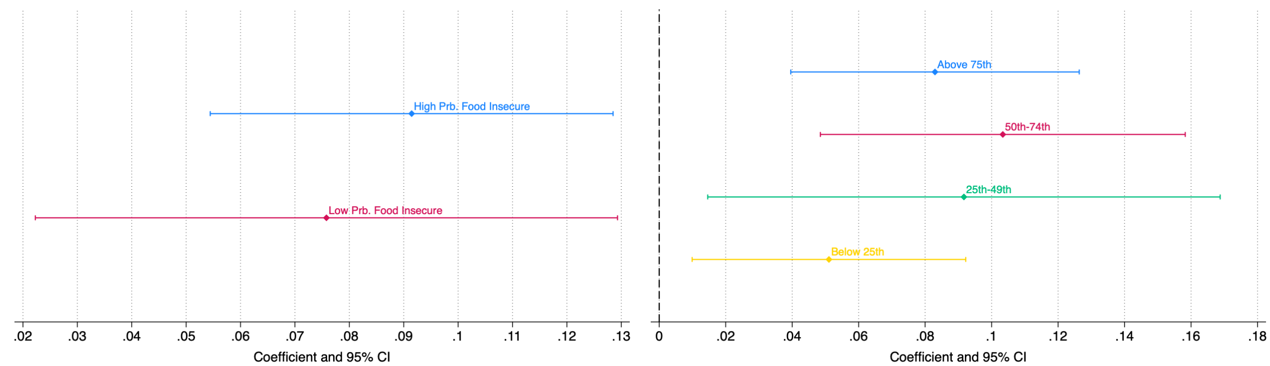
While the use of aggregate state-year data necessitates a loss of granularity in the individual-level characteristics that can be controlled for, this analysis with administrative data provides reassurance that my estimates of the effect of the CAP are not driven by misreporting of SSI or SNAP receipt in the ACS.

Targeting Efficiency

Given strong evidence that the CAP increased SNAP take-up, I now explore whether the CAP was well-targeted, in the sense that the increase in SNAP take-up was larger among those most in need. 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 the "Targeting Analysis" subsection of my "Empirical Setup" section above.

Figure 7 shows estimates of β in the state-level regression, where I restrict the sample by above/below median level of predicted probability of food insecurity in panel (a), and by quartile in panel (b). There are no statistically significant differences between the coefficients. However, the magnitude of the estimate is about 20% higher for those above the median probability of food insecurity than for those below the median, and between 60% higher to about double in size for those in the top three relative to the bottom quartile.

(a) Above/Below Median **(b)** Quartiles



Notes: Figure shows estimates of β in equation (2), in regressions restricted to individuals (a) above vs below the median level of predicted food insecurity, and (b) 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 has size 112,214 persons.

Figure 7. Average Effect of the CAP on SNAP Enrollment by Percentile of Predicted Food Insecurity.

While purely suggestive, these estimates provide evidence that the CAP was well-targeted. This finding is in line with Deshpande and Li (2019)'s results in the context of disability insurance[32] and would indicate that, among those already receiving or applying for SSI, the transaction costs associated with SNAP enrollment are most burdensome for the types of individuals that would benefit most from the program. A larger sample and more granular demographic/socioeconomic characteristics would be useful in confirming these results. Furthermore, food insecurity is just one dimension of targeting; my analysis leaves open the question of whether the CAP was well-targeted on other dimensions, which future work might explore.

MECHANISMS

The Role of Default Options

The unique design of the CAP, which involved three distinct formats adopted by different states, provides an ideal setting for probing the mechanisms through which this policy increased SNAP take-up.

Recall from the "Institutional Background" section above 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. The Standard CAP can be further decomposed into two formats; since the joint-enrollment procedure 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

[32] These authors found a negative change in targeting in the context of an increase in transaction costs, particularly with targeting measured in terms of education level.

SSI application. Massachusetts and Pennsylvania used both joint-enrollment—to enroll individuals newly applying for 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 evidently did not account for those already enrolled in SSI.

This setup yields three CAP formats: Standard with auto-enrollment, Standard without auto-enrollment, and Modified. Importantly, both types of Standard CAPs to some extent changed the "default option" for SSI recipients: by auto-enrolling or jointly-enrolling SSI recipients and applicants into SNAP, the CAP effectively shifted the default for these individuals from non-participation to participation in SNAP. On the other hand, all three CAP formats provided information about SNAP and likely increased SSI recipients' confidence in their eligibility for the program, so heterogeneous effects across formats can help rule out that the effect of the CAP operated entirely through information provision or a reduction in learning costs.

I exploit the variation in which of these three formats of the CAP was adopted in each state to explore the importance of default options in driving the CAP's effect on SNAP enrollment. I do so by interacting the main effect in my specifications with an indicator for whether the CAP adopted in each state was Standard without auto-enrollment, or Standard with auto-enrollment, relative to Modified.

Table 7 shows the results. The uninteracted treatment effect, which represents the effect of the Modified CAP, is positive and significant throughout. Notably, the interaction with the auto-enrollment indicator is positive throughout and significant in the state-level design. Its magnitude suggests that the effect of the auto-enrollment CAP was double the size of the Modified CAP: auto-enrollment CAP states saw a 15 percentage point—or about 33%—increase[33] in SNAP enrollment. The coefficient on the "Standard without auto-enrollment" indicator is, on the other hand, small and insignificant in all three designs.[34]

These estimates suggest that pure joint-enrollment that fails to account for existing SSI recipients is less effective than mailing simplified application forms to the full set of SSI recipients. However, a pure auto-enrollment procedure, or the combination of auto-enrollment for existing SSI recipients and joint-enrollment for new SSI applicants, generates the largest increase in SNAP take-up. Default options appear to play an important role in determining whether an individual will enroll in SNAP.

[33] The baseline SNAP enrollment rate in auto-enrollment CAP states one year before CAP adoption was about 45%.

[34] Appendix I shows that the positive and significant difference between the auto-enrollment and Modified CAP formats is robust to the use of subsample models, which flexibly allow for differential effects of the controls.

Table 7. Average effect of the CAP on SNAP enrollment, by CAP format.

	State-level design	Singles-Couples design	Triple difference
	(1)	(2)	(3)
Treated x Post	0.0734***		
	(0.022)		
Treated x Post x [Standard w/o Auto-enroll]	0.0183		
	(0.033)		
Treated x Post x [Auto-enroll]	0.0792**		
	(0.039)		
Single x Post		0.0878***	
		(0.015)	
Single x Post x [Standard w/o Auto-enroll]		-0.0137	
		(0.019)	
Single x Post x [Auto-enroll]		0.0321	
		(0.031)	
Single x Treated x Post			0.0635**
			(0.024)
Single x Treated x Post x [Standard w/o Auto-enroll]			-0.0133
			(0.037)
Single x Treated x Post x [Auto-enroll]			0.0399
			(0.028)
N	141,461	90,353	216,741

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 without auto-enrollment, 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. State controls (included in columns (1) and (2)) 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; column (2) additionally controls for 2003 median income and the percent of congressional representatives that were Democrats in 2004. The triple difference specification also includes 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 Limits to Auto-enrollment

As discussed in the preceding section, the auto-enrollment format of the CAP induced a 15 percentage point increase in SNAP enrollment, implying that SNAP enrollment among SSI recipients in auto-enrollment CAP states increased from about 45 percent in the year before CAP adoption to about 60 percent. Thus, while its effect was quite large in magnitude, the auto-enrollment CAP was far from inducing 100% take-up, which would be the expected effect of a true auto-enrollment policy.

A number of candidate explanations for this failure to achieve complete take-up exist: administrative errors could have prevented the full set of SSI recipients from receiving an EBT card in the mail, or some combination of difficulty understanding the mailer and concerns about stigma might have prevented recipients from activating the card despite receiving it.

An alternative explanation is that, even once the cost of *enrolling* in SNAP is precisely zero, the cost of *using* one's SNAP benefits remains non-trivial. As shown in Figure 3, in many areas of the U.S., individuals must travel several miles from home to reach the nearest food store that accepts SNAP benefits. For SSI recipients in these areas, acquiring food through other means—such as at fast-food restaurants—might be less costly than incurring this repeated travel expense. This explanation for incomplete take-up would imply that the costs associated with enrolling in and using SNAP are quite nuanced, but that these costs may be sufficient to rationalize the decision to "opt out" of auto-enrollment.

To test this explanation, I explore whether the effectiveness of the auto-enrollment version of the CAP—or, to increase power, the combination of the auto- and joint-enrollment versions of the CAP, both of which involved a change in SSI recipients' default option with regard to SNAP enrollment—varied with the geographic accessibility of food stores that accept SNAP benefits as payment. Using my state-level research design, I interact the main effect with an indicator for being in an area with low access to SNAP retailers.

Table 8 contains the results. When I restrict to auto-enrollment CAP states in panel (A), the coefficient on the interaction of the main effect with the indicator for low access is negative and large in magnitude, but not statistically significant. When I broaden the sample to include both auto- and joint-enrollment CAP states (i.e. all Standard CAP states) in panel (B), this coefficient is significant across all three specifications. Its magnitude implies that having low access to SNAP retailers reduced the CAP's effect to 20-30% of its original magnitude.

These results reveal an important limitation of auto- and joint-enrollment. Even absent the cost of filling out a SNAP application, the time and monetary cost of traveling to a food store to activate an EBT card might have outweighed the benefit of receiving SNAP, thus preventing complete take-up in spite of a change in default options. This finding has broad implications: to accurately identify how an individual rationalizes whether to enroll in a social safety net program given the costs and benefits, a researcher must consider both the fixed cost of enrolling in a program and the repeated cost of using the benefit, where the latter might vary with location. Policymakers aiming to implement true auto-enrollment into SNAP might need to consider pairing this program with one that reduces travel costs or otherwise increases the accessibility of SNAP retailers.

Table 8. Average effects of the auto/joint-enrollment CAPs, by accessibility of SNAP retailers.

	(1)	(2)	(3)
Panel A: Auto-enrollment CAP States			
Treated x Post	0.182***	0.171***	0.176***
	(0.033)	(0.032)	(0.030)
Treated x Post x [Low Access to SNAP Retailers]	-0.0960	-0.0806	-0.0807
	(0.059)	(0.058)	(0.058)
Year FE	✓	✓	✓
State FE	✓	✓	✓
Individual Controls		✓	✓
State Controls			✓
N	36,335	36,335	36,335
Panel B: Standard (Joint- or Auto-enrollment) CAP States			
Treated x Post	0.154***	0.137***	0.138***
	(0.023)	(0.031)	(0.028)
Treated x Post x [Low Access to SNAP Retailers]	-0.127***	-0.0940***	-0.0938***
	(0.042)	(0.035)	(0.034)
Year FE	✓	✓	✓
State FE	✓	✓	✓
Individual Controls		✓	✓
State Controls			✓
N	42,522	42,522	42,522

Notes: Table shows estimates of β in equation (2), 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. Panel (A) restricts to auto-enrollment CAP states plus control states, and panel (B) restricts to standard CAP states (both joint- and auto-enrollment standard CAPs) plus control states. The outcome variable is an indicator for SNAP enrollment. Data is from 2005-2016 (since PUMA is not reported before 2005). Individual controls are age and indicators for sex, race, ethnicity, education level, and physical disability, and 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.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

CONCLUSION

A large share of low-income 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 the true scale and implications of these costs in a multi-program context are little understood, as are 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 17-24%, 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 among existing SSI recipients and provide suggestive evidence that the change in SNAP targeting generated by the CAP was positive. 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 the auto- and joint-enrollment versions of the CAP 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 enroll.

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 in-kind transfer programs like SNAP among individuals already enrolled in a cash transfer. Auto- and joint-enrollment appear to be limited in their efficacy, 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 household consumption. 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.

The longer-term effects of the CAP would be worth investigating: future work could repeat my analysis with long-run panel data to discern whether those initially enrolled in SNAP by the CAP subsequently re-enrolled after their SNAP certification period ended. In the short-term, at least, transaction costs associated with the SNAP application process have evidently made the program inaccessible even to many of those granted categorical eligibility via their receipt of SSI. The costs of program enrollment in the complex, multi-program U.S. social safety net are nuanced and burdensome to those it is intended to serve.

Data Availability

The data that support the findings of this study were derived from the resources listed in the "Data" subsection of the paper. Restrictions apply to the use of the IPUMS USA and IPUMS CPS data, which were used under license for this study; to request access to the data provided by IPUMS, please use the following website: `https://www.ipums.org`. All other data sources are available in the public domain.

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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.

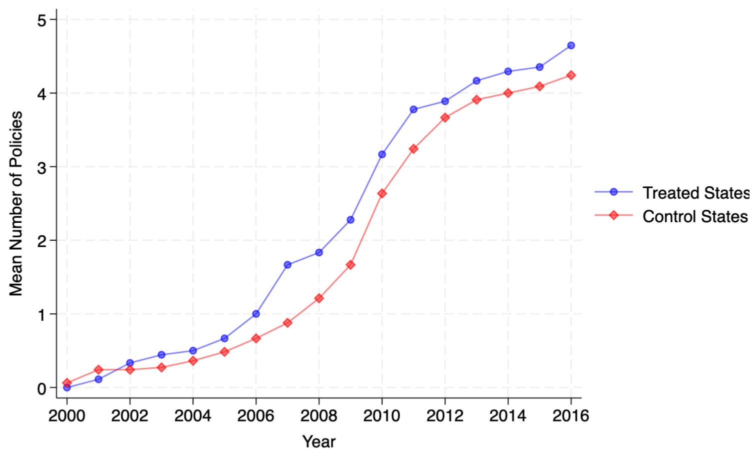
State	Effective date	Eligibility criteria	Standard (S) or Modified (M)	SNAP enrollment procedure
South Carolina	1995	SSI recipients living alone or purchasing/preparing meals separately, with no earned income	S	Joint enrollment in SNAP during SSI application; those already enrolled in SSI were mailed simplified SNAP application form
Mississippi	Oct. 2001	SSI recipients living alone or purchasing/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 recipients ages 50 and older	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	SSI recipients living alone (without a spouse or children) and purchasing/preparing meals separately from others, with no earned income	S	Joint enrollment in SNAP during SSI application; auto-enrollment for those already enrolled in SSI
Florida	Mar. 2005	SSI recipients who purchase/prepare meals alone, with no earned income	S	Joint enrollment in SNAP during SSI application
North Carolina	Feb. 2006	SSI recipients who purchase/prepare meals alone and are age 65+	M	Simplified SNAP application form mailed to SSI recipients
Pennsylvania	Feb. 2006	SSI recipients living alone, with 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	SSI recipients ages 60 and older who live alone or with a spouse that also receives SSI	M	Simplified SNAP application form mailed to SSI recipients
Louisiana	July 2007	SSI recipients not living with a spouse or children, and purchasing/preparing meals separately from others, ages 60 and older	M	Simplified SNAP application form mailed to SSI recipients
Michigan	Dec. 2008	SSI recipients who purchase/prepare food separately from others, with no earned income	M	Simplified SNAP application form mailed to SSI recipients
New Jersey	Dec. 2008	SSI recipients living alone, ages 65+, with no earned income	M	Simplified SNAP application form mailed to SSI recipients
Arizona	Dec. 2008	SSI recipients living alone (cannot live with a spouse), ages 65+	M	Simplified SNAP application form mailed to SSI recipients
South Dakota	Dec. 2009	SSI recipients living alone or with a spouse who also receives SSI	M	Simplified SNAP form mailed to SSI recipients
New Mexico	May 2009; suspended May 2014	SSI recipients living alone or with a spouse who also receives SSI, with no earned income	M	Simplified SNAP application form mailed to SSI recipients, but SSI applicants can also apply at SSA office
Maryland	July 2010	SSI recipients who purchase/prepare meals separately from others, ages 60 and older, with no earned income	M	Simplified SNAP application form mailed to SSI recipients; mailed a second form if no response to the first

Notes: References for this table are Weinstein-Tull and Jones (2017), FNS (2005), the USDA SNAP policy database, and state government agency websites.

APPENDIX B. STATE POLITICS AND OTHER SNAP-RELATED POLICIES

My identification strategy relies on the exogeneity of the adoption of the CAP. This exogeneity assumption would be threatened if states' adoption of the CAP was correlated with the introduction of other SNAP-related policies at the state level, or with underlying state politics more generally.

My regressions control for other state-level policies besides the CAP that might have simplified the SNAP certification process. These six policies (listed in the "Take-up Analysis" subsection of the paper 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 in treated versus control states, by plotting the average number of the seven policies present in each year. Although there is a small gap (no larger than one policy) in levels from about 2006-2012, the slopes of the trend lines are highly similar between treated and control throughout.



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.

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

To formally test whether CAP treatment status can predict the adoption of other SNAP-related policies, I regress the number of other SNAP-related policies present in a given state-year on an indicator for whether that state ever adopted the CAP (plus year fixed effects), with standard errors clustered at the state level. The coefficient on the CAP adoption indicator is positive but insignificant (coefficient = 0.309, standard error = 0.245). The same can be said of a regression of the number of other SNAP-related policies on an indicator for whether that state had the CAP in place *that year* (coefficient = 0.467, standard error = 0.311). This result confirms that both the time-invariant characteristics that led a state to adopt the CAP and the year-specific conditions that determined whether the state had the CAP in any given year cannot predict how many other SNAP-related policies it had in place.

These six policies may not be an exhaustive list of state-level efforts to increase SNAP take-up, and they are unlikely to fully capture state policy environments. As a more conclusive test, I regress the share of state representatives that are Democrats, along with an indicator for the lower house of the state legislature being majority Democrat, on an indicator for whether the state had the CAP in place that year.[35] These outcomes are broader measures of state policy environment: they capture both voter preferences and the partisan affiliation of those in control of policymaking at the state-level.

Table B.1 shows the results. The coefficient estimate on the indicator for whether the CAP was in place is small and insignificant in all specifications. This finding suggests that the presence of the CAP cannot predict a state's underlying political characteristics, which provides reassurance against the concern that my estimates of the effect of the CAP are confounded by other features of states' policy environments.

Table B.1. Predictive power of having the CAP in place on state politics.

	Share Democrat	Share Democrat	Majority (>50%) Democrat	Majority (>50%) Democrat
	(1)	(2)	(3)	(4)
CAP in Place	0.0290	-0.00326	0.0909	0.0210
	(0.029)	(0.011)	(0.101)	(0.051)
State FE		✓		✓
Year FE		✓		✓
N	833	833	833	833

Notes: Table shows estimates from a state-year-level regression of state political outcomes on an indicator for whether the CAP was in place in that state-year. The sample is a balanced panel of state-years from 1995-2011. The outcome in columns (1) and (2) is the share of state representatives (specifically, elected representatives in the lower house of the state legislature) that are Democrats, and the outcome in columns (3) and (4) is an indicator for whether the majority of representatives are Democrats. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

[35] Data on the composition of state legislatures is taken from U.S. Census Bureau (2021).

APPENDIX C. DETAILS ON THE TARGETING ANALYSIS

As summarized in the "Targeting Analysis" subsection of the paper, to analyze the targeting efficiency of the CAP, I subset the sample of 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 an indicator for experiencing food insecurity F_{it} to take the value one 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, and income quintile. 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.

To explore the correlation between F_{it} and the demographic/socioeconomic characteristics used to construct predicted food insecurity, Table C.1 shows regression estimates from linear probability models in which I regress F_{it} on various combinations of these characteristics. The probability of being food insecure is significantly higher among those who are Black, Hispanic, or another race relative to those who are White; among women relative to men; among high school dropouts relative to those with higher levels of education; among those with lower incomes; and among those between age 60-70 relative to those who are older.

Table C.1. Linear probability models of food insecurity by demographic/socioeconomic characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.00870***					0.00539**
	(0.002)					(0.002)
Black		0.0650***				0.0485***
		(0.005)				(0.004)
Asian		-0.0106**				-0.0121***
		(0.004)				(0.004)
Other Race		0.0775***				0.0662***
		(0.010)				(0.010)
Hispanic		0.0431***				0.0170***
		(0.004)				(0.004)
HS Grad			-0.0563***			-0.0505***
			(0.004)			(0.004)
Some College			-0.0569***			-0.0521***
			(0.005)			(0.005)
College Grad			-0.0866***			-0.0724***
			(0.004)			(0.004)
Income ($10,000s)				-0.00228***		-0.00190***
				(0.000)		(0.000)
Age 70-79					-0.0234***	-0.0286***
					(0.002)	(0.002)
Age 80+					-0.0367***	-0.0487***
					(0.002)	(0.002)
N	1,194,154	1,194,154	1,194,154	1,194,154	1,194,154	1,194,154

Notes: Table shows coefficient estimates in regressions where the outcome variable is an indicator F_{it} for being food insecure and the regressors are various combinations of demographic/socioeconomic characteristics. The omitted category for the race, education, and age variables are respectively White, less than high school, and age 60-69. Note that in calculating predicted food insecurity $\hat{F}_{it}$ for the targeting analysis, I use income quintiles, 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 sample. Standard errors clustered at the person level are in parentheses below each estimate.

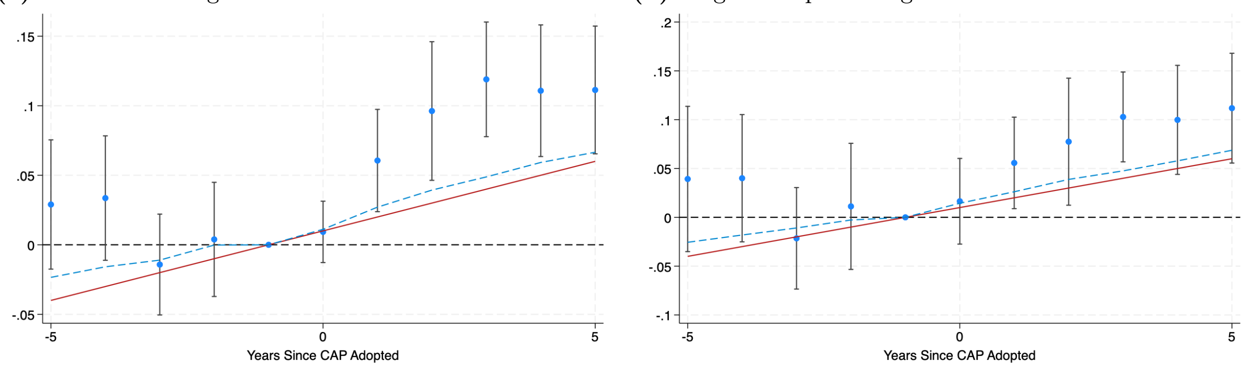
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

APPENDIX D. POWER OF THE TESTS FOR PARALLEL TRENDS

In the "Results" section of the paper, I showed that, in both the state-level and singles-couples TWFE designs, I cannot reject the parallel trends assumptions, as none of the pre-period coefficient estimates is significantly different from zero. However, Roth (2022) found 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 D.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 0.01, overlayed on the TWFE coefficient estimates and confidence intervals for the state-level and singles-couples research designs. This particular violation of parallel trends would occur if the difference in SNAP enrollment between the treated and control groups increased by one percentage point each year in the absence of the CAP, which is an economically plausible violation if treated units are more commonly targeted for other interventions aimed at increasing SNAP take-up than control units.

(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 0.01. See Roth (2022) for details.

Figure D.1. Linear Violations of Parallel Trends with Slope 0.01.

Applying the analysis in Roth (2022) to the singles-couples design in panel (b) confirms that, for this specification, my test for pre-trends has about 28% power to detect such a violation (i.e. this trend would generate at least one statistically significant pre-period coefficient 28% of the time). Notably, this violation cannot explain the pattern of the event study coefficients, as the confidence interval on the relative year 3 coefficient is entirely above the hypothesized trend.

For my state-level design in panel (a), Roth (2022)'s analysis confirms that my test for pre-trends has 52% power to detect a violation in parallel trends of this magnitude. Furthermore, the confidence intervals on more than half of the post-period coefficient estimates are strictly above this hypothesized trend, indicating that such a violation could not explain the observed pattern in the event study coefficients. Any larger violation of parallel trends would be detected with even higher power.

These computations suggest that my tests for parallel trends—particularly in the state-level research design—are well-powered.

APPENDIX E. RELATING ESTIMATES TO JONES ET AL. (2022)

This section relates the magnitudes of my estimates of the effect of the CAP to existing literature on SNAP policies and take-up. The setting of my analysis is closest to that of Jones et al. (2022), whose study focused on the effect of a broad index of SNAP-related policies on SNAP enrollment, but who also used fixed effects regression to study the effect of the CAP on SNAP enrollment among seniors. As I note in the "Introduction" section of the paper, these authors found 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. This 1.3 percentage-point effect, which translates to a 2.7% increase from the 48% baseline rate for seniors reported in their Introduction, is much smaller in magnitude than my estimates. There are several methodological differences between my paper and Jones et al. (2022) which might explain this discrepancy in magnitudes: my empirical strategy uses a difference-in-differences specification with a control group of individuals ineligible for the CAP, my analysis sample is a set of SSI recipients rather than elderly Americans below a particular poverty threshold, and I include a different set of individual- and state-level controls.

To discern how much of the discrepancy between my estimates and the one in Jones et al. (2022) is attributable to the fact that I use a sample of SSI recipients, which is intended to precisely identify those eligible for the CAP, I approximate the share of Jones et al. (2022)'s sample that receives SSI and "scale up" their estimates by this proportion. Jones et al. (2022) used the 2001-2014 CPS Food Security Supplement (CPS-FSS) as their main data source; I extract an indicator for SSI enrollment from the 2001-2014 CPS March ASEC,[36] impose sample restrictions parallel to those in Jones et al. (2022),[37] and find that about 9.9% of this sample reports receiving a positive amount of SSI benefits. Scaling up Jones et al. (2022)'s 1.3 percentage-point treatment effect by this SSI participation rate yields a 13 percentage-point treatment effect, which translates to a 27% increase in SNAP enrollment. This effect size is much closer to the 17-24% range that I estimate with my TWFE specifications.[38] This similarity provides further support for the magnitude of my estimates.

To separately investigate the extent to which the distinction between my estimates and the one in Jones et al. (2022) might be explained by the different control variables included in my regression versus theirs, I re-estimate my main TWFE regression with the full vector of ten SNAP-related policies[39] apart from the CAP that are controlled for in Jones et al. (2022).

Table E.1 shows the results: column (1) should be compared to column (3) of Table 2 of my paper, and column (2) should be compared to column (6) of Table 2. These estimates with the additional controls

[36] These data are provided by Flood and Westberry (2022).

[37] Specifically, I restrict to Americans ages 60 and older who are below 150% of the federal poverty line, and I remove residents of Alaska, Hawaii, and California from the sample. The CPS ASEC does not provide poverty measures more granular than above versus below 150%, so I could not precisely replicate the 185% restriction in Jones et al. (2022).

[38] The fact that the "amplified" Jones et al. (2022) treatment effect is slightly higher than my estimates may be explained by my restriction to those below 150% of the poverty line rather than 185% in calculating the share receiving SSI to scale up their estimate, as noted above.

[39] I use the USDA's SNAP Policy Database to construct these 10 controls; in a few instances, they differ slightly from the ones in Jones et al. (2022), who used additional data sources to construct their SNAP policy index.

included in Jones et al. (2022) are essentially unchanged from my main estimates—equivalent up to 0.5 percentage points—which suggests that the larger effects that I document in my paper are not attributable to omitted variable bias related to other SNAP-related policies.

Table E.1. Effect of the CAP on SNAP enrollment, using controls from Jones et al. (2022).

	State-level design	Singles-Couples design
	(1)	(2)
Treated x Post	0.0922***	
	(0.019)	
Single x Post		0.0975***
		(0.016)
N	141,461	90,353

Notes: Table shows estimates of β in equations (2) and (4) in columns (1) and (2) respectively, where the outcome 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 (and additionally in column (2), 2003 median income and the percent of congressional representatives that were Democrats in 2004), 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 re-certification, the existence of call centers to provide application assistance, provision of the option to apply online rather than in person, the elimination of the fingerprinting requirement at initial application, the exclusion of vehicles from the SNAP assets test, simplified options for reporting changes in household circumstances for households without earnings, the use of any state or federal grants toward SNAP outreach, and whether all noncitizen seniors who meet other eligibility requirements are eligible for SNAP; the state SNAP-related controls also include a continuous variable for the proportion of elderly SNAP recipients with no earnings who are subject to 1-6 month recertification periods. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

APPENDIX F. 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.[40] 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

$$\sum_{t=1}^{12} \frac{114}{(1+r)^t} = 5 \cdot v. \tag{9}$$

Assume a monthly discount rate of $r = 0.078$, following Shapiro (2005).[41] Then the left-hand-side of equation (9) 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.[42] 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 3% of the average annual income of an individual in my sample (see Table 1).

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

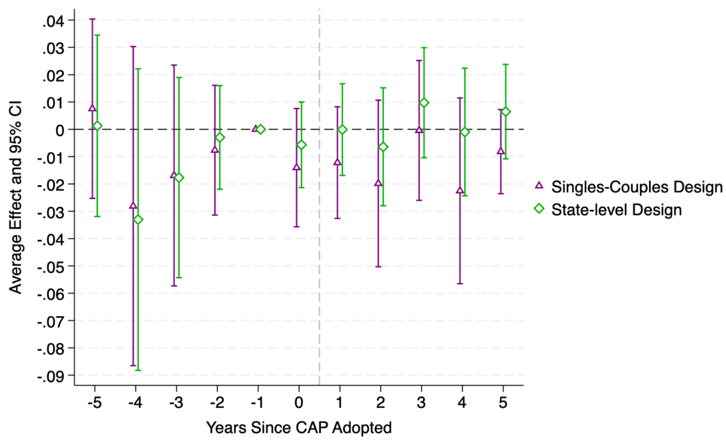
[41] Shapiro (2005) found an annual discount rate of 146% among SNAP recipients, which is equivalent to a monthly discount rate of about 7.8%. This discount rate is quite high; if I instead assume an annual discount rate of 25%, which translates to a monthly discount rate of 1.9%, I calculate that the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

[42] 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.

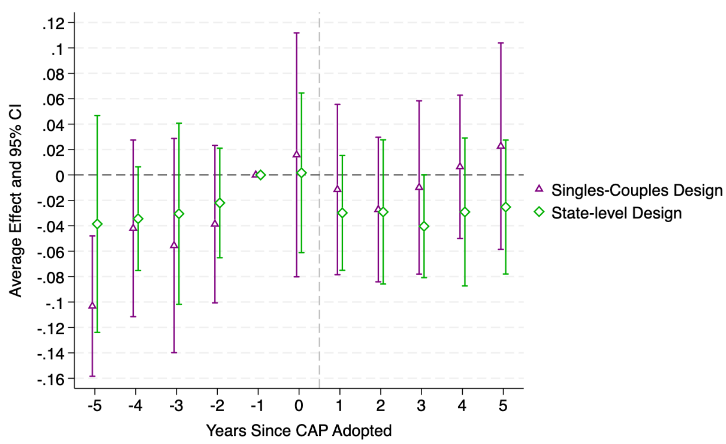
APPENDIX G. PLACEBO TESTS: SUPPLEMENTARY FIGURES

Figure G.1 displays event study estimates for my two placebo tests of the effect of the CAP on the take-up of other social safety net programs, with average effects reported in the "Take-up: Placebo Tests and Robustness Checks" subsection of the paper.

(a) Medicaid



(b) LIHEAP



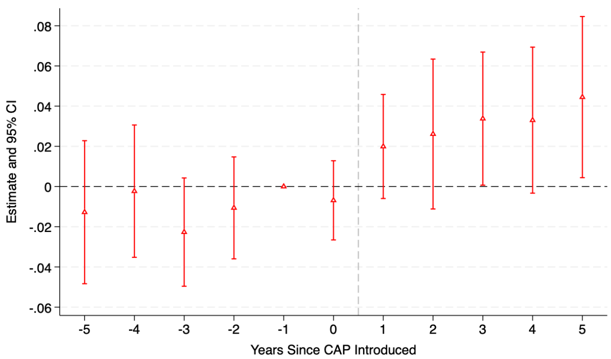
Notes: Figure shows estimates (with 95% confidence intervals) of β_τ in equations (1) and (3), in two placebo tests using data on elderly SSI recipients surveyed in the CPS March ASEC. Panel (A) uses an indicator for whether the individual is enrolled in Medicaid as the outcome, and panel (B) uses an indicator for whether the individual is enrolled in LIHEAP. Individual controls are age and indicators for sex, race, and education level. Observations more than 5 years after the adoption of the CAP are binned into a single indicator, and observations more than 5 years before the adoption of the CAP are binned into a separate indicator; the coefficients on these indicators are not plotted here. Standard errors are clustered at the state level. N = 13,007 persons for the state-level design, and N = 5,747 for the singles-couples design.

Figure G.1. Placebo Tests for the Effect of the CAP on Take-up of Other Social Safety Net Programs.

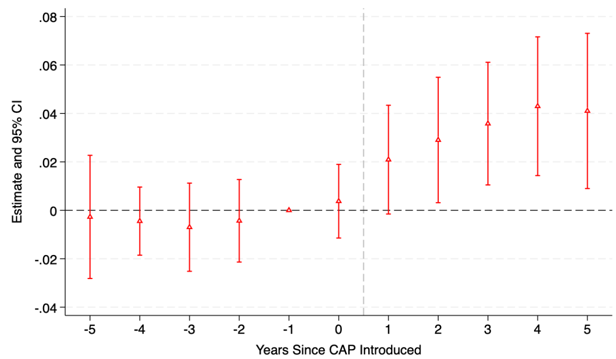
APPENDIX H. ROBUSTNESS CHECKS: SUPPLEMENTARY FIGURES

Figure H.1 shows estimates of the effect of the CAP on SNAP enrollment in my two robustness checks using alternative analysis samples of "likely" SSI recipients, with average effects reported in the "Take-up: Placebo Tests and Robustness Checks" subsection of the paper.

(a) 120% SSI Income



(b) $\leq$ HS Education & Non-homeowner



Notes: Figure shows estimates (with 95% confidence intervals) of β_τ in equation (1), using two samples of "likely" SSI recipients instead of the main analysis sample of self-reported SSI recipients. Panel (a) restricts to individuals with household income no greater than 120% of the federal maximum SSI benefit amount that year, and panel (b) restricts to non-homeowners with no more than a high school education. The outcome variable is an indicator for SNAP enrollment. Observations more than 5 years after the adoption of the CAP are binned into a single indicator, and observations more than 5 years before the adoption of the CAP are binned into a separate indicator; the coefficients on these indicators are not plotted here. Standard errors are clustered at the state level. N = 352,060 persons in (a), and N = 400,892 persons in (b).

Figure H.1. Effect of the CAP on SNAP Enrollment: Robustness Check with "Likely" SSI Recipients.

APPENDIX I. CAP FORMAT REGRESSIONS: SUBSAMPLE MODELS

In Table 7, I showed the results of testing for heterogeneous effects of the CAP across CAP formats, by interacting the main effect with indicators for the format of the CAP adopted in each state. As an alternative heterogeneity test, I estimate subsample models: specifically, I estimate equation (2) separately for states that adopted the auto-enrollment version of the Standard CAP, states that adopted the Standard CAP with no auto-enrollment component, and states that adopted the Modified CAP. Estimation via subsamples allows for differential effects of the controls and is thus more flexible.

Table I.1 shows the coefficient estimate in each regression, and Table I.2 shows the results of testing for differences in the coefficient estimates across subsamples. Consistent with Table 7, the effect of the auto-enrollment version of the Standard CAP is significantly larger than the effect of the Modified version, where the difference in magnitude—7.7 percentage points—is substantial. There are no significant differences between the other pairs of CAP formats.

Table I.1. Effect of the CAP on SNAP enrollment, by CAP format.

	AE Standard CAPs	Non-AE Standard CAPs	Modified CAPs
	(1)	(2)	(3)
Treated x Post	0.128***	0.0263	0.0507**
	(0.010)	(0.085)	(0.020)
N	18,915	13,132	51,833

Notes: Table shows estimates of β in equation (2), where the outcome is an indicator for SNAP enrollment. Column (1) restricts the sample to states that adopted the auto-enrollment version of the Standard CAP, column (2) restricts to states that adopted Standard CAPs with no auto-enrollment component, and column (3) restricts to states that adopted the Modified CAP. 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.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table I.2. Differences in effects of the CAP on SNAP enrollment across CAP formats.

	AE Standard - Non-AE Standard	AE Standard - Modified	Non-AE Standard - Modified
	(1)	(2)	(3)
Difference	0.101	0.077***	-0.024
(SE)	(0.071)	(0.022)	(0.073)

Notes: Table shows the difference between the β estimates from equation (2) for each format of the CAP as displayed in Table I.1. Column (1) compares the coefficient estimate for states that adopted the auto-enrollment version of the Standard CAP with the estimate for states that adopted the Standard CAP with no auto-enrollment component, column (2) compares the estimate for states that adopted the auto-enrollment version of the Standard CAP with the estimate for states that adopted the Modified CAP, and column (3) compares the estimate for states that adopted the Standard CAP with no auto-enrollment component with the estimate for states that adopted the Modified CAP. Standard errors clustered at the state level are in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

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