Targeting, Screening, and Retention: Evidence from California's Food Stamps Program

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May 3, 2023
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Abstract

Many households eligible for the Supplemental Nutrition Assistance Program (SNAP) do not enroll. Using a new dataset of monthly enrollment histories for all SNAP participants in California between 2005 and 2021, I document how procedures used to verify program eligibility lower retention and contribute to incomplete take-up. I show that the majority of households who leave SNAP appear income eligible in the months before and after their exit. I also find that these reporting requirements most deter enrollment among seemingly more advantaged households. Ineligible households, as well as those with higher earnings and low predicted food security, are more likely to exit in a reporting month. Though these eligibility verifications appear to improve targeting, I present evidence that reducing their frequency can still be an efficient way to increase program participation.

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1 Introduction

The Supplemental Nutrition Assistance Program (SNAP), commonly known as food stamps, is a critical part of the American social safety net. In 2019, before the outbreak of Covid-19 when participation reached an historic peak, around 36 million Americans were enrolled in SNAP in any given month, and altogether, received over $60 billion in assistance. Although SNAP receipt is associated with reduced food insecurity, reduced poverty, lower criminal recidivism, improved short- and long-term health outcomes, and for children, greater life expectancy and higher lifetime earnings, roughly one in six eligible individuals do not enroll (Cunnyngham et al., 2018).[1] Incomplete take-up has long concerned policymakers, and significant public and private resources have been expended to increase awareness of the program and encourage eligible households to apply.

Alongside soliciting new applications, policymakers and stakeholders can increase participation by improving program retention. In order to confirm they are still eligible, most SNAP recipients must periodically report whether their income, household composition, or expenses have changed, and the burden of these administrative processes can induce still-eligible households to leave the program. Several studies document how these eligibility verifications are associated with program exits and shortened enrollment spells (Kabbani and Wilde, 2003; Ribar, Edelhoch and Liu, 2008; Gray, 2019; Homonoff and Somerville, 2021).

Since SNAP is a means-tested program, some degree of ongoing eligibility verification is necessary. Policymakers can only choose the frequency and rigor with which these verification are administered. When they do, they balance two competing objectives: promote efficient redistribution and minimize the costs that these processes impose on enrollees and the government (Kleven and Kopczuk, 2011). Less frequent reporting might allow ineligible households to remain enrolled longer, while more burdensome ordeals risk screening out both eligible and ineligible households.

Despite the importance of this policy decision, there is little evidence about how current reporting requirements affect the composition of program caseloads or the size of these Type 1 (false rejection or incomplete take-up) and Type 2 (false award) errors. While administrative burdens clearly lower enrollment, it remains unclear to what degree they screen out eligible versus ineligible households and how any screening benefits compare to the cost of incomplete take-up. This evidence is critical for policymakers to judge whether current policy is maximally efficient and equitable.

In this paper, I study how reporting requirements affect participation in SNAP in California, the state with the highest SNAP enrollment and one of the lowest take-up rates (Cunnyngham et al., 2018). I use a new administrative dataset covering individual- and household-level enrollment in the program between 2005 and 2021, to which I merge quarterly earnings data from 2012 onward as well as monthly case-level benefit issuance records from 2010 onward. The breadth of these data allow me to document several new facts about program enrollment, churn, and the effects of administrative burdens.

I show that program exits coincide with reporting schedules, and that nearly half of new entrants leave

[1] An incomplete list of the studies documenting these benefits includes: Ratcliffe, McKernan and Zhang (2011); Mabli and Ohls (2015); Almond, Hoynes and Schanzenbach (2011); Bronchetti, Christensen and Hoynes (2019); East (2020); Gregory and Deb (2015); Oddo and Mabli (2015); Morrissey and Miller (2020); Hoynes, Schanzenbach and Almond (2016); Tuttle (2019); Bailey et al. (2020)

the program by their first eligibility screen at six months. I also show that the large majority of households who exit the program appear income eligible in the months before and after their exit. Further, I show that lengthening the period between when households must verify eligibility increases retention.

I also find that reporting requirements improve targeting by screening out seemingly more advantaged households. I document that ineligible households, households with higher earnings, and households with lower benefit amounts are all more likely to exit in a reporting month. I also conclude that household characteristics associated with higher food insecurity are negatively associated with likelihood of exit. Finally, I show how quicker rebounds in earned income after enrollment correspond with earlier exits from the program.

To identify the marginal effect of reporting requirements on the composition of the program caseload, I evaluate the impact of a reform that expanded the reporting window. In 2013, California moved from quarterly reporting (cases had to reverify every three months) to the current semi-annual reporting policy (cases must reverify every six months). This reform increased the likelihood that households remained enrolled for at least six months by around 10 percentage points. The reform most increased retention among households predicted to be the least food insecure.

These findings suggest that reducing the burden or frequency of reporting requirements would increase retention and take-up. At the same time, such a reform would also allow some households who were no longer eligible to remain enrolled for additional months and more advantaged households to continue to remain in the program when they might otherwise choose not to participate. The principal contribution of this paper is to provide evidence about the scale of this trade-off.

I conclude by estimating the marginal value of public funds (MVPF) of reducing the frequency of eligibility verifications, using the enrollment effects from the 2013 reform. I tally the additional benefits disbursed due to the increase in retention and use estimates of the fiscal externalities associated with these processes and SNAP receipt. I allow for these benefits and costs to vary between recipient types in order to account for the relevance of worse targeting. I find that eliminating this reporting requirement improved welfare and efficiently boosted enrollment.

The paper makes multiple contributions to the study of enrollment dynamics in and administrative burdens of safety net programs. First, I contribute to a growing literature studying the incomplete take-up of means-tested programs (Moffitt, 1983; Currie, 2006; Bhargava and Manoli, 2015; Finkelstein and Notowidigdo, 2019). Of the three commonly cited explanations for incomplete take-up – learning, compliance and stigma costs – this paper underscores the importance of compliance costs. I find that limited retention is a significant source of non-participation among eligible households, and retention is low largely due to the burdens associated with reporting requirements.[2]

Second, I build on many studies investigating enrollment patterns in SNAP, in particular those studying trends in total participation, enrollment duration, and characteristics which predict program entry and exit (Blank and Ruggles, 1996; Jolliffe and Ziliak, 2008; Ganong and Liebman, 2018; Mills et al., 2014; Burstein and Siegel, 2009). A persistent issue plaguing this literature has been limited access to reliable, individual-

[2]Interactions with government during reporting months might make stigma costs more salient, which could drive exits during these months. At the same time, one could then just as easily consider this experience of stigma as a form of compliance cost fostered by an administrative burden.

level, and longitudinal enrollment data. Public survey data documenting enrollment in safety net programs is prone to misreporting (Meyer, Mok and Sullivan, 2009; Meyer and Mittag, 2019) and rarely follows the same individuals and households over time or with sufficient frequency (Ganong and Liebman, 2018; Leftin et al., 2014). Most studies investigating the effect of policies and practices on enrollment and take-up evaluate changes in aggregate flows into and out of enrollment (Kabbani and Wilde, 2003; Ganong and Liebman, 2018; Heflin and Mueser, 2010; Schwabish, 2012; Shiferaw, 2019). These studies generally do not measure actual enrollment duration, assess the importance of changes in retention, or how take-up and enrollment patterns vary across different populations.[3]

A subset of this literature considers the importance of reporting requirements on enrollment and retention. Using aggregate enrollment data and variation in state policy, Klerman and Danielson (2011), Currie et al. (2001), Kabbani and Wilde (2003), McKernan, Ratcliffe and Gibbs (2003), and Hanratty (2006) all show that shorter reporting periods are associated with lower program enrollment. A handful of papers use state- or county-level micro-data to document how reporting policies and practices affect retention (Staveley, Stevens and Wilde, 2002; Ribar, Edelhoch and Liu, 2008; Ribar and Swann, 2014; Hastings and Shapiro, 2018; Gray, 2019; Homonoff and Somerville, 2021; Wu and Meyer, 2021).

Like these authors, I find that reporting requirements lower program retention. This paper adds to and diverges from other papers' findings in several key respects. By linking administrative data on program enrollment and quarterly earnings, I can identify the likely eligibility status of households who exit SNAP. Similar to Gray (2019), I estimate that a majority of households who exit are income eligible. I show that this finding is robust to using several definitions of eligibility. I also show that enrollment spells are shorter and retention is lower in California than those documented elsewhere, and that earnings play a significant role in explaining households' likelihood of exiting the program in reporting months. I also present the broadest evidence to date about these processes' effects on targeting and caseload composition.

Third, this paper contributes to an ongoing debate about the merits and effects of administrative burdens (Currie, 2006; Kleven and Kopczuk, 2011; Herd and Moynihan, 2019). Early models of the optimal design of safety net programs proposed constructing significant barriers to enrollment (Akerlof, 1978; Nichols, Smolensky and Tideman, 1971; Nichols and Zeckhauser, 1982; Moffitt, 1983; Besley and Coate, 1992), motivated by the assumption that the deterrent effect of administrative "hassles" can facilitate more efficient redistribution to the most vulnerable households, assuming that hassles screen out those less willing to spend the time required to apply or remain enrolled. In this way, hassles are able to elicit the opportunity cost of potential enrollees' time and reveal their otherwise unobservable need for assistance. Alternative models propose that hassles screen out those less able to navigate these ordeals, thereby deterring exactly the individuals policymakers most want to help (Bertrand, Mullainathan and Shafir, 2004; Mani et al., 2013; Mullainathan and Shafir, 2013). Empirical evidence supporting either explanation remains relatively limited (Alatas et al., 2016; Waldinger, 2021; Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019; Homonoff and Somerville, 2021; Wu and Meyer, 2021). Indeed, the few studies cited here reach contradic-

[3]There are a few notable exceptions, including Mills et al. (2014), who use the SIPP and state program data to document the costs of program "churn," Leftin et al. (2014), who also the SIPP to document a number of facts about SNAP enrollment patterns, and Klerman and Danielson (2011), who use the USDA SNAP Quality Control files to study how composition of SNAP caseloads change during large increase in enrollment surrounding the Great Recession. Neither the SIPP nor the SNAP QC files allow researchers to observe enrollment spells as long as those represented in the MEDS data that I use.

tory findings.[4]

I conclude that reporting requirements serve a targeting purpose. Holding an array of other case characteristics constant, income eligible households are, on average, three times more likely to complete their reporting requirement and remain enrolled than ineligible households. There is also a strong negative relationship between retention and earnings and a positive relationship between retention and benefit amounts. The likelihood of exiting in a reporting month increases by approximately four percentage points for each additional $500 in earned income. And households receiving more than $500 in benefits each month are 30 percentage points less likely to leave than households receiving less than $50.

While earnings play a key role in predicting exit, other case characteristics that proxy for relative disadvantage appear less important. For example, an individual's race, language, and previous enrollment in the Temporary Assistance for Needy Families program (TANF) are all only somewhat related to likelihood of exit. I use the combination of these other characteristics to relate each household to similar households in the December Current Population Survey (CPS), which asks respondents about their ability to access and afford food. Considering only demographic characteristics, SNAP households most similar to CPS households who report being food insecure are slightly more likely to reverify and remain enrolled. When I incorporate earnings, I recover a much stronger relationship.

Fourth, I contribute to the new literature estimating the marginal value of public funds (MVPF) for expansions to public programs (Hendren and Sprung-Keyser, 2020). I provide the first estimate of the MVPF associated with increasing enrollment by widening the reporting interval, and one of the first estimates to integrate targeting effects. I find that, in this setting, administering fewer recertifications increases social welfare. The personal and public cost savings from avoiding another report, plus the limited net welfare costs associated with increased SNAP enrollment, aggregate to an MVPF ratio above 1. This result is important, because it suggests that compositional changes in program caseloads should not be the sole measure by which we judge whether administrative burdens are worthwhile. Even if a given ordeal correctly deters more advantaged and ineligible individuals on the margin, lessening the frequency or rigor with which that burden could still improve welfare.

The paper proceeds as follows. In Section 2, I describe the administrative data. In Section 3, I provide background information on program eligibility and reporting requirements. In Section 4, I provide descriptive statistics on enrollment patterns and evidence of the impact that reporting requirements have on retention. In Section 5, I calculate the share of households who exit the program despite appearing eligible. In Section 6, I document how households' earned income evolves before, during, and after enrollment. In Section 7, I identify how individual and household characteristics predict likelihood of exiting SNAP in reporting and non-reporting months. In Section 8, I calculate the MVPF associated with extending the reporting interval. In Section 9, I conclude.

[4]These contradictory results may indicate that targeting effects simply vary across contexts, programs and processes studied. If so, it's not clear what researchers or policymakers can learn about the broader welfare consequences of these burdens from findings of compositional changes in program caseloads documented in each study on this topic. It's similarly unclear that worse targeting implies that the studied ordeal is necessarily worthwhile, as I argue in this paper.

2 Data

I use individual-level monthly enrollment data collected by the California Department of Social Services (CDSS).[5] Along with enrollment indicators, these panel data contain basic demographic information about each recipient, including their date of birth, race and ethnicity, language, and sex. I also observe the county in which individuals are enrolled and their case number. Table 1 summarizes basic characteristics of enrollees for a select number of years in my sample.

I identify the start date, end date, and length of every continuous enrollment spell for all recipients between 2005 and 2021.[6] To account for censoring issues, I exclude cases that were enrolled as of January 2005. Using county identifiers and case serial numbers, I group enrollees into common households in each enrollment month. I assign each household one of six household types, according to the ages of their case members: children-only,[7] working-age adults with no children, single working-age adults with children, multiple working-age adults with children, seniors, and seniors with children. These different households are subject to different reporting requirements, and we might expect that they have different levels of need for food assistance.

All adults are matched to their available quarterly wage earnings records, including all quarters in which the adult was enrolled in SNAP, as well as the six quarters before their enrollment started and six quarters after their enrollment ended.[8] I sum quarterly earnings within each case. I also match each individual to their households' SNAP benefit amount from 2010 through December 2021.[9]

The administrative data I rely on are not the official records of SNAP enrollment in the state. California is unique in that counties, as opposed to the state, administer SNAP, and the 58 separate county offices retain their own official enrollment data. Counties report aggregate enrollment counts, through the state, to Food and Nutrition Service (FNS) at the US Department of Agriculture. **??** plots total monthly enrollment as recorded in MEDS and the FNS reports. Comparing my data to FNS counts, MEDS records appear to overstate enrollment each month by nearly 100,000 individuals (or two to three percent of the official caseload) each year. This difference is partially explained by MEDS data recording participation in a state-

[5]These data originate from California Department of Health Care Services' Medi-Cal Eligibility Data System (MEDS) files. This data system is primarily used for the administration of the state's Medicaid program (known as Medi-Cal), but it also captures monthly enrollment information in other safety net programs including CalFresh (California's instantiation of SNAP) and CalWORKs (California's Temporary Assistance for Needy Families [TANF] program). Based on guidance from staff at CDSS, we identify an individual as enrolled in SNAP if s/he is recorded as enrolled in both data systems. The original version of this paper, released in early 2021, did not apply this restriction, leading us to slightly overstate SNAP enrollment and the number of eligible non-claimants in this population.

[6]Other work studying similar enrollment trends "fill in" one month enrollment gaps, assuming these gaps more likely reflect data errors than actual breaks in enrollment (Burstein, 1993; Gleason, Schochet and Moffitt, 1998; Cody et al., 2005, 2007; Mabli et al., 2011; Ratcliffe, 2016; Gray, 2019). Leftin et al. (2014) find that these gaps could very well be instances of churn, as opposed to misreporting, but still choose to fill them in. I identify enrollment spells both ways, filling in these one-month gaps and not. I choose to use the version in which I do not fill in these gaps, because my measures of churn and total enrollment better align with what the state reports when I do not fill them in, but my results are robust to using either approach.

[7]Children-only households are generally households in which adults are not eligible for SNAP due to their immigration status, but their children are.

[8]The division at the state agency responsible for administering unemployment insurance (UI) and which helped to facilitate this match does not retain earnings records for more than seven years, which precluded me from matching earnings records to participants before 2012.

[9]California Department of Social Services does not have issuance histories for cases before 2010.

run food assistance program.[10]

Enrollment increased significantly in the aftermath of the Great Recession, as it did nationally (Ganong and Liebman, 2018), and enrollment fell between 2015 and 2019 as the economy recovered. It increased again in June 2019 when Supplemental Security Income (SSI) recipients in California became eligible for SNAP; total enrollment increased by 330,000 in the first three months after expansion. Enrollment spiked again in Spring 2020 amidst the Covid-19 crisis. The economic disruption wrought by the pandemic resulted in more Californians enrolling in SNAP than at any other point the program's history (4.73 million in May 2020). It also resulted in many individuals enrolling in SNAP for the first time. Of the roughly one million Californians who enrolled between March and June 2020, one-third had never been enrolled in any other month between 2005 and 2020.

Along with facilitating spell-based analyses, these panel data also allow me to count the number of unique Californians who have ever interacted with the program over the last decade and a half. The program has a much wider reach than cross-sectional counts might suggest. SNAP has assisted over 14 million Californians since 2010, over 11.5 million since 2015, and more than 4.2 million since the onset of the Covid-19 pandemic.

3 Policy Background

3.1 Eligibility

The rules used to determine SNAP eligibility are largely set at the federal level. Generally, a household is income eligible for SNAP if: (1) its gross income is below 130 percent of the households' federal poverty level (FPL); (2) net income (gross income minus taxes, 20 percent of earned income, a $100 to $200 standard deduction, and a portion of the cost of shelter, utility, medical, and care expenses) is less than 100 percent of its FPL, and (3) total assets are worth less than $2,250, or $3,500 for households with seniors or disabled members (CBPP, 2020). Households can also be categorically eligible if they receive assistance from TANF, SSI or a state-financed general assistance program.

States have some ability to expand eligibility. For example, California, along with many other states, allows households with seniors, disabled persons, or a member eligible for a TANF-funded program to qualify for SNAP even if their gross income is up to 200 percent of FPL (LSNC, n.d.*a*; USDA, 2020). California also allows any households containing a member who qualifies for Medicaid to be categorically eligible for SNAP. Additionally, households with only seniors or disabled members need only meet the net income test. A small number of households in which every member is enrolled in cash assistance are exempt from both income tests.

SNAP cases are defined as a group of individuals who prepare and eat meals together. The income eligibility limits, and benefit amount credited to households based on that income, are applied according to each SNAP case's total size, regardless of the age of the members. Nearly all forms of earned and unearned income count towards these income tests, and income received by all members of a household

[10]The California Food Assistance Program is a state-run program for recent immigrants who are not yet eligible for SNAP. There were roughly 30,000 individuals enrolled in this program in FY 2019-2020.

counts towards eligibility.

In many states, households must also demonstrate that they have sufficiently low assets in order to qualify for SNAP. In California, households who qualify for SNAP under broad-based categorical eligibility are exempt from the asset test, meaning I can infer eligibility using data on income. This is a particular advantage of my study relative to other studies of this issue, since researchers rarely have access to information about household wealth.

Some individuals are categorically ineligible for SNAP, including: non-citizens, workers on strike, students (except in particular circumstances), and until 2019, Californians receiving Supplemental Security Income (SSI). These exemptions are generally not a concern in my setting, as I mainly consider continuing eligibility among individuals who were already deemed eligible.

3.2 Reporting Requirements

The federal government sets minimum intervals within which households must verify their eligibility, but states are permitted to administer more frequent verifications. Most SNAP recipients in California must confirm eligibility twice a year. Six months after enrolling and every 12 months thereafter, most households need to complete a two-page semi-annual report (known as a SAR-7), on which they relist all household members, all sources of income, how that income might change over the next six months, and their expenses. Then, twelve months after initial enrollment (and every 12 months thereafter) most recipients need to complete a full recertification (known as a RRR). The annual recertification resembles initial enrollment in its length and complexity. In addition to completing a four-page form, households must also complete an in-person or phone interview with county staff. If a household fails to meet any of these requirements before the last day of the reporting month, their benefits can be cut off. Households can remain enrolled without reapplying if they submit any missing paperwork or complete their interview within 30 days of their initial reporting deadline. If they do not, and they wish to re-enroll, they must undertake a full re-application. In between these scheduled reporting months, households must also notify their county office if their gross income ever exceeds 130 percent of its FPL, or their household composition changes such that they may no longer be eligible.

The six month cycle of semi-annual report and full recertification describes the reporting process for most households in California, but some face different timelines. For example, households with only seniors or individuals with disabilities only need to complete the semi-annual report every 12 months. If anything about their status has changed, they might also have to submit the semi-annual report in the intervening months. Households including only seniors or individuals with disabilities and who have no earned income only need to recertify every 36 months (LSNC, n.d.*b*). For now, these households must still complete a semi-annual report every 12 months, but this requirement was eliminated starting in 2022. Figure 2 illustrates the reporting schedule for these three household types.[11]

[11] There are a handful of exceptions to this common schedule. For example, in six counties between 2018 and 2020, working-age adults with no children had to demonstrate that they were working or looking for work at least 20 hours a week; otherwise, these individuals were limited to receiving benefits for only three months over the course of three years. The Trump Administration planned to institute these benefit limits and work requirements on so-called Able-Bodied Adults without Dependents (ABAWDS) nationwide starting in March 2020, but implementation was postponed indefinitely due to the COVID-19 pandemic. California received a waiver from implementing this rule in any county through October 2024.

Even though when households must report and what information they need to submit is determined federally, county offices have some discretion over how these reports are administered. They can decide how and when to conduct interviews with recipients, whether and how often they remind enrollees about their reporting deadlines, and whether they use third-party information to verify what enrollees report.

The reporting requirements described above have applied to SNAP in California since October 2013. Before then, households were required to submit eligiblity reports every quarter.[12] These quarterly reports required cases to report an estimated income amount for each month in the quarter; the semi-annual report only asks for current earnings and potential future changes in earnings. Hereafter, I reform to this policy change as the "2013 reform." In the following section, I document the impact this reform had on program enrollment.

4 Enrollment Durations

Figure 3 summarizes the most frequent enrollment spell lengths. Panel A includes spells that began at least two years before October 2013, when reporting requirements shifted from every three months to every six. Panel B includes spells that began after October 2013. In both, it is clear that enrollment spells are commonly in intervals that coincide with when households must verify eligibility.

Figure 4 provides additional evidence that individuals exit SNAP in the month a report is due. I plot the share of cases that remain enrolled through each month after initial enrollment up to 24 months. Again, I consider these patterns before and after the 2013 reform. In either case, after 2013, roughly 40 percent of SNAP recipients exit by six months, and almost 65 percent exit by 12 months. Notably, the share of individuals who exit by six months is fairly similar before and after the 2013 reform. Indeed, the share of cases that exited at three months before the reform appear to now remain enrolled up to six months.

Figure 5 summarizes differences in exit rates for each enrollment duration.[13] Conditional on remaining enrolled for six months, when the first semi-annual report is due, the average household has a one-in-five chance of exiting the program in that month. The likelihood of exiting at month 12 is nearly the same. Conditional on remaining enrolled until month 18 and month 24, the probability of exiting are both approximately 10 percent.

Together, this evidence suggests that most individuals will remain enrolled in SNAP until they are required to recertify. Then, because they are no longer eligible, believe they are no longer eligible, or the costs of reporting eligibility outweigh the benefits of remaining enrolled, many exit. I distinguish between these

[12]This reform was allowed by a series of regulatory changes dating back to 1999, which also permitted states to decrease not only the frequency of these reports, but also the amount of information that families had to submit (Danielson et al., 2011). Between 2003 and 2011, USDA FNS authorized a series of waiver requests from California to continue administering quarterly reporting, all the while urging the state to move to semi-annual reporting. State policymakers insisted the transition was complicated by legislative, political, and technological obstacles (CDSS, 2010). Finally, the California legislature passed AB 6 in 2011, directing CDSS and the counties to adopt semi-annual reporting by October 2013.

[13]To identify the per-month hazard rates, I estimate a logit regression, in which the dependent variable is an indicator for exit and independent variables are vector of dummies α, representing each enrollment period $(d = 1, \dots, D)$, as well as controls for month and year ϕ, county θ, and household type η fixed effects. I cluster standard errors at the person-spell level. Estimating these hazard rates is computationally intensive, so I use a five percent sample of all spells. I restrict to cases that began after 2013 so as to focus on enrollment patterns under current reporting policy. Using the estimated coefficients from this logit, I identify the predicted probability of exit for each covariate, including each level of d. These are the estimates reported in Figure 5.

competing explanations in the next section.

5 Measuring Eligibility Among Leavers

CDSS infers the degree to which reporting requirements burden eligible households by tracking the share of cases who exit SNAP at their recertification but reapply to the program within one to three months. The assumption is that households who leave but quickly re-enroll were never actually ineligible, but simply failed to complete their semi-annual report or recertification on time. Counties report these "churn" rates to CDSS, and CDSS publishes them every quarter. In any given quarter, about 10 percent of cases reapply for benefits within one month after failing to complete their recertification, and 15 percent reapply within three months. These rates are fairly constant over time (Figure 6) and are similar to national figures reported by Mills et al. (2014).

I replicate and extend these tabulations using my panel data. Table 2 reports the share of individuals who exited SNAP at some point between 2014 and 2020, but returned to the program within six different timelines. From 2014 onward, 10 and 18 percent of individuals who exit SNAP re-enroll within one and three months, respectively.[14] Roughly 40 percent who exit re-enroll within one year, and about half re-enroll within two years. These rates are similar to those reported by Leftin et al. (2014). Figure 6 reports the shares by each individuals' exit date going back to 2005. It is clear that the 2013 reform also reduced the churn rate. Fewer eligibility verifications reduced not only the number of leavers in each month, but also the share of those leavers who would quickly re-enroll.

That nearly one-in-six individuals return to the program within three months after exiting suggests that a significant share of exits are not due to ineligibility. However, this measure potentially underestimates the share of leavers who are eligible, because it does not count eligible individuals who exit the program and never return or return after three months.

I use the state's earnings records to measure the actual fraction of households that exit but appear income eligible. I identify each households's total earned income in the quarter and after their exit, and then count the number of exiting households whose total income is above or below their respective eligibility threshold.[15]

Determining actual eligibility for SNAP is complicated. It's an imperfect process even for the government agencies that administer the program and which have more information than I observe. My approach, which relies mainly on wage earnings, is likewise imperfect. Below, I discuss how my limited information about alternative sources of income and household expenses might bias my estimates and how I address each of these concerns. At the end of the section, I present estimates of eligibility using multiple, alternative definitions.

First, eligibility for SNAP is determined monthly, but I observe only quarterly earnings. I assume that each person's monthly earned income is equal to one-third of their quarterly earnings. In order to not misassign income earned while on or off the program, I restrict this analysis to individuals who exited SNAP

[14] This estimate is slightly below counties' reports. This discrepancy is likely due to how the Medicaid records are updated relative to the county SNAP case files. This might also help to explain why MEDS tends to overstate total enrollment.

[15] Since I am unable to match children-only households (i.e. mixed immigration status families) to their parent's earned income, I exclude these households from this analysis. I also only consider cases that exit for at least two months, meaning my estimates tend to be lower bounds on the true share of eligible leavers.

at the end of a calendar quarter. In a separate simulation, I assume that households receive all their quarterly earnings in the one month they must verify eligiblity, which means I compare their quarterly earnings to their respective monthly income eligibility threshold.

Second, I do not observe all forms of earned and unearned income. EDD records only capture in-state wage earnings.[16] I also do not observe cases' unearned income. To test the relevance of unearned income, I supplement my analysis using case records from San Francisco county as well the SNAP Quality Control files. I assign each household in the CDSS data the average level of unearned income reported among similar households in these data. I then recalculate the share of households who appear eligible assuming that they each have this simulated level of unearned income, in addition to their actual earned income. Refer to Appendix B for more information about this procedure.[17]

Third, I do not observe each household's deductible expenses, like housing, child care, and medical costs, which determine the net income test against which their income is compared. I account for this concern by estimating the share whose income is below 200 percent of FPL and 130 percent of FPL – the approximate net income thresholds assuming households' have high and low levels of deductions, respectively.

Fourth, I do not observe household composition after a household exits the program. For example, if a household loses a member after exiting, then their earnings would be applied to a different eligibility threshold. I account for this concern by identifying the share of households whose total earnings are below 130 percent of FPL even if their last-observed household size was reduced by one person.

Figure 7 reports the share of cases who appear eligible under these various definitions of eligibility. I calculate these shares by counting the number of cases that leave at the end of each calendar quarter between December 2013 and December 2021,[18] and among those cases, the number eligible under each definition.

That the churn rate severely underestimates the rate of unwanted exit is robust to any of these alternative definitions. The share of cases with zero earned income in the quarter following exit (around 50 percent) is three times higher than the 90-date churn rate. Over 70 percent of cases have earnings that would still qualify them for SNAP, assuming their household size remains the same, which is almost five times higher than the 90-day churn rate. Removing a household member and adding in households' average unearned income amounts barely affect the estimated eligibility rates. Assigning all quarterly earnings to just one month and using the 130 percent threshold affects matters more, but it remains the case that the majority of exiting cases appear eligible. These eligibility rates are nearly the same for every quarter over the last six years.

[16]EDD data captures the sum of three-months' worth of each individual's earnings from all jobs that are covered by the unemployment insurance program. Self-employment income, employment by the military and the federal government, and under-the-table wages are not covered by the state's unemployment insurance program, and so are not captured in these records. Kornfeld and Bloom (1999) conclude that UI records cover roughly 90 percent of workers and their earnings. See also Czajka, Patnaik and Negoita (2018). BDT (2020) report that less than 5 percent of SNAP recipients receive self-employment income.

[17]Large increases in unearned income after a household exits SNAP could result in my overstating eligibility after exit. Neither the San Francisco case records nor the Quality Control files capture changes in unearned income after a household leaves the program. To account for this concern, I use the Survey of Income and Program Participation (SIPP) to track SNAP households before, during and after SNAP enrollment. I find no evidence of any significant change in unearned income around program exits. Refer to Appendix A for a summary of this analysis.

[18]I limit to these cases because I have earnings data for all of these quarters and all of these quarters occur after the 2013 reform.

6 Earnings Trends

In the preceding section, I showed that most households that exit SNAP have earnings that would still qualify them for the program. In the next two sections, I investigate potential explanations for exit in a reporting month.

First, I consider whether households exit because their earnings have changed since they enrolled. Even if households are still eligible, their earnings might have recovered enough that the stigma and compliance costs of remaining enrolled exceed the value of their SNAP benefits. I identify these earnings trends relative to the start of SNAP enrollment by estimating the following equation, similar to Hastings and Shapiro (2018).

$$
\begin{aligned}
\mathrm{E}_{it} = \gamma_l \underbrace{\Lambda_{il}}_{\substack{\text{spell}\\ \text{length}}} + \underbrace{\sum_{q=-5[q\neq 0]}^{8} \mathbb{1}[Q_{it} - Q_{i,0} = q]\, \rho_q}_{\substack{\text{quarter relative to}\\ \text{enrollment start}}} \\
+ \sum_{q=-5[q\neq 0]}^{8} (\Lambda_{il} \times \mathbb{1}[Q_{it} - Q_{i,0} = q])\delta_q + \underbrace{\eta_h + \phi_t + \theta_c}_{\substack{\text{year/month, county,}\\ \text{and household type}\\ \text{fixed effects}}} + \varepsilon_{iym}
\end{aligned}
$$

The model identifies the average difference in earnings in each quarter q relative to the quarter before the case's enrollment spell began. I interact the leads and lags for quarters with the indicator for spell length to identify how earnings trends vary between cases that exit soon after enrolling versus those that remain enrolled through multiple reporting periods. I account for year and month fixed effects, ϕ, county fixed effects, θ, and household type, η. Post-estimation, I use estimates of γ, ρ, and δ to predict earnings in each quarter for each spell length and at the mean value of the other covariates.

Figure 8 plots the average predicted quarterly earnings for each quarter relative to the beginning of an enrollment spell for spell lengths of 6, 12, 18, and 24+ months. On average, patterns are the same for each spell length: earnings are fairly constant in the year before an individual enrolls in SNAP, enrollment coincides with a sharp decline in earned income, and households tend to exit the program around when their earnings have recovered. For those who exit at six months, earnings recover by the first quarter after enrollment. For those who exit at 12 months, earnings rebound to the average predicted pre-enrollment earnings by the third quarter after enrollment starts and are well above pre-enrollment earnings by the fourth quarter. The same pattern follows for those who exit at 18 or after 24 months. Earnings remain depressed in the quarters in which these individuals are still enrolled and recover only three or four quarters after enrollment starts. These trends suggest that SNAP serves the intended purpose of an income support program, cushioning family income during periods of acute financial need, at least among those who enroll.

On average, households whose earnings recovery more quickly appear more likely to exit earlier, as one might expect. I cannot rule out the possibility that the causality runs in the opposite direction – earnings rebound because households must replace income they lost from leaving SNAP, or households increase their earnings when they no longer face the steeper tax rate imposed by the SNAP benefit schedule. However, households who exit at six months experience a recovery in earnings before they exit, which suggests the decision to exit or remain follows from changes in earnings.

The main takeaway is that enrollment in and exit from SNAP coincides with important changes in households' earned income. If cases tend to still be income eligible after exit, as shown above, but they exit after their earnings rebounded to a pre-enrollment average, this implies that many households were eligible for many months before they enrolled. I test this implication by identifying the share of households who appear income eligible (using the 130 percent FPL threshold) in the quarters preceding, during and after their enrollment. I re-estimate **??** but replace the outcome variable with an indicator for whether the case appears income eligible.[19] Again, I distinguish between cases enrolled for 6, 12, 18 and more than 24 months, and I use the estimates to identify the average predicted eligibility level in each quarter relative to the start of enrollment. Figure 9 summarizes the results. Enrollment coincides with a sharp uptick in the likelihood of eligibility, mirroring the drop in earnings illustrated in Figure 8.[20] As predicted, the vast majority of households who enroll in SNAP are eligible for many months before and after their exit.

7 Who Leaves in Reporting Months?

Next, I identify whether participants who are no-longer eligible or less in need of food assistance are more likely to exit in reporting months. Since I do not observe individuals' latent "ability" or need for food assistance, I evaluate whether several individual and household-level characteristics that typically correlate with economic and food insecurity (e.g., current earnings, past earnings, race, language status, household composition) are predictive of exit.

I estimate the marginal effects of these characteristics on program exits in reporting months using a discrete time hazard model (Kalbfleisch and Prentice, 2011; Hoynes, 2000). The model identifies the the transition probability $P(d, \mathbf{Z})$, or the likelihood that a subjects exits the program in period d, conditional on remaining enrolled until period $d-1$ and covariates $\mathbf{Z}$. The hazard rate is modeled as a logit probability.

$$P(d, \mathbf{Z}_{it}) = \frac{\exp(\alpha_d + \mathbf{Z}_{it}\delta)}{1 + \exp(\alpha_d + \mathbf{Z}_{it}\delta)} \tag{1}$$

The vector of dummy variables, α_d, captures each potential period of participation $(d = 1, \ldots, D)$. These dummies non-parametrically account for underlying duration patterns and identify the baseline hazard. Additional covariates, $\mathbf{Z}$, include a series of fixed effects as well as demographic and household characteristics.

$$\mathbf{Z}_{it}\delta = \mathbf{X}_i'\beta + \mathbf{X}_i' \times (\text{Report}_{id})\gamma + \phi_t + \theta_c + \eta_h \tag{2}$$

The fixed effects include calendar year and month ϕ_t, which vary within each individual's enrollment spell, county effects θ_c, which tend not to vary within spells, and household type η_h, which also tends not to

[19] I define a case as eligible if their quarterly earned income is below 130% of the FPL for their household size. I use the household composition as of when their enrollment begins.

[20] This share might not reach 100 percent for at least two reasons: (1) I measure eligibility against the 130% FPL gross income test, and many households will still qualify if their earnings are below 200%; and (2) the verification process is imperfect, and a small share of households who have incomes above the eligibility threshold for some month during the quarter will be able to remain enrolled.

vary within spells.

I estimate this model separately for different sets of characteristics, including: demographic characteristics (race, preferred language, household type, and previous enrollment in TANF), an indicator for eligibility, and levels of earnings or benefit amounts. Demographic characteristics are constant throughout all individuals' spells and increase or lower baseline hazards for all enrollment spell lengths, while earnings and benefit levels can change each month. Finally, I identify whether the effect of those characteristics vary between reporting and non-reporting months by interacting the relevant characteristic with an indicator for whether the period d is a month in which the case would have to complete a semi-annual report or a recertification. The key parameters in the logit model are β and γ; these capture the separate effects that characteristics have on likelihood of exit in reporting and non-reporting months.

I restrict this analysis to spells that started between December 2013 and December 2019 to avoid confusing effects between two reporting systems and to ensure I have earnings data for all months enrolled and up to 24 months after enrollment. Individuals may enroll in SNAP multiple times over the six year period; I treat these spells as independent. I cluster standard errors at the individual-spell level. Since this analysis is highly computationally intensive, I again rely on a five percent random sample of all individual spells.

After estimating Equation 1, I transform the estimated log odds to the predicted marginal effect of each characteristic on the likelihood of exit in reporting and non-reporting months. Table 3 and Figures 10 to 13 summarize these effects across each model.[21]

Table 3 reports the average likelihood of exit in reporting and non-reporting months by imputed eligibility status. Both eligible and ineligible households are roughly six times more likely to exit in reporting months (11 compared to 2 percent and 32 compared to 5 percent, respectively).[22]

Figure 10 summarizes the effect of earned income and benefit amounts received at time d on likelihood of exit. There is little to no effect of benefit levels on exit in non-reporting months, but there is a clear effect in months when households must verify eligibility. Every \$50 in additional benefits is associated with 3 to 5 percent point decrease in the likelihood of exit, up to about \$400 in benefit levels at which point the effect plateaus. There is also a clear relationship between earnings and likelihood of exit, especially in reporting months. Every \$500 is associated with a 3 to 5 percentage point increase in the likelihood of exit. Relative to households with zero earned income, households with more than \$5,000 in estimated monthly earnings are 42 percentage points more likelihood to exit. There is also a relationship between earnings and exit in non-reporting months, which reflects the fact that households can leave the program in between reporting periods if their income increases enough that they become ineligible.

The associations summarized in Figure 10 are not necessarily evidence of targeting effects. Present earnings might not reflect households' latent need for SNAP, and the association between present earnings and likelihood of exit might capture a mechanical effect of an eligibility verification. I further explore these processes' screening effects by testing whether likelihood of exit varies with other indicators of households' need for food assistance. Figure 11 summarizes the relationship between likelihood of exit and earnings twelve months before one's SNAP enrollment starts. Again, I document a relationship between these earn-

[21]Appendix Tables 4 to 10 summarize estimates from each logit model and the transformation to average and marginal effects.

[22]That just one-in-three ineligible households exit SNAP in a reporting month might reflect both Type 1 errors and an imperfect measure of eligibility.

ings and likelihood of exit in a reporting month, but this effect is more muted. For every $500 increased earnings, the likelihood of exit increases by just one percentage point. Households with monthly earnings of more than $5,000 a year before enrolling are 10 percentage points more likely to exit in a reporting month than households with no earnings a year before enrolling. Comparing this result with those from Figure 10, eligibility verifications appear to lower retention among households with higher current income, as one might expect. This effect is still present, but not as large, when using previous earnings.

The relationship between demographic characteristics and exit in reporting months is even less clear. There is no relationship between any individual demographic characteristic and likelihood of exit in non-reporting months (Figure 12, Panel A). In reporting months, I observe some limited variation (Panel B). Black recipients are slightly less likely to exit relative to white enrollees, but effects for other groups relative to white recipients are insignificant. Individuals who were enrolled in TANF before their current enrollment in SNAP started are also slightly more likely to remain enrolled. Non-English speakers appear just as likely to exit as English speakers. Seniors and households with children are clearly less likely to exit than single adults without children.

It is not obvious how these characteristics correspond with actual need for food assistance. Indeed, there might be important interactive effects between one's race, household composition, language status and earnings in predicting economic insecurity. Next, I identify how combinations of demographic and household characteristics are associated with food insecurity and relate this imputed measure of need for food assistance to likelihood of exit. I use the December Current Population Survey (CPS) from 2010 to 2018, which asks respondents about their ability to access and afford food. I identify how respondent demographics and household characteristics relate to this reported food insecurity.[23] I assign each SNAP recipient the predicted level of food insecurity estimated for their counterpart in the CPS, and run a version of Equation 1 in which the vector of characteristics is the binned values of predicted likelihood of food insecurity. As above, I use the coefficients estimated in this regression to identify the marginal percentage point effect of the imputed levels of food insecurity on likelihood of exit.

Figure 13 summarizes the results from this analysis. When I rely only on demographic characteristics and ignore earnings, I find a limited relationship between food insecurity and likelihood of recertifying. Households that are most likely to be food insecure are just 10 percentage points more likely to recertify than households with the lowest food insecurity level. The former have about a 9 percent chance of exiting in a reporting month, whereas the latter have a 19 percent chance. When I control for earnings, I find a clearer relationship between predicted food insecurity and likelihood of exit. Households with the highest level of food insecurity are about 39 percentage points more likely to recertify than households with the lowest level; the latter households have nearly a 1-in-2 chance of exiting in a reporting month, while the former exit only 5 percent of the time. In both versions, there is almost no relationship between imputed food insecurity and likelihood of exit in non-reporting months.

[23] Specifically, I estimate a logit model of respondents' reported food insecurity on binned values of their age, race, number of children, presence of other adults, state, survey year, and earnings. I then use the estimated coefficients to predict each respondent's likelihood of being food insecure, resulting in a measure of predicted food insecurity for every observation that ranges from zero to one. For all possible combinations of these characteristics, I then identify the average predicted level of food insecurity for all combinations of characteristics included in the prediction exercise. Refer to Appendix Section 1 for more information about this procedure.

Finally, I evaluate whether the 2013 reform increased retention differently between households with higher or lower levels of imputed food insecurity. I compare cases exposed to the reform (those who enrolled in SNAP between August 2013 and December 2013) to those who were not (those who enrolled in SNAP between February and July 2013). The latter cases would have had to submit a quarterly report before October 2013, while the former would only have to submit the new semi-annual report.

To evaluate the effect of the reform, I compare the survival rates between these two groups of cases. Figure 14 summarizes the results of this analysis. Panel A illustrates how the the reform decreased exit rates at three months and increased the likelihood that households remain enrolled for up to six months. On average, treated cases were 11 percentage points more likely to remain enrolled up to six months. Panel B illustrates how this effect differs between cases assigned high and low predicted food insecurity.[24] The effect was largest for households with the lowest level of food insecurity, since these were the households most likely to exit by six months before the reform. High food insecurity cases also exhibit increased retention, and the difference in the effect between the two types of cases is fairly modest (1 to 2 percentage points each month). However, the difference in longer-term retention is larger. Among high food insecurity cases, treated cases are just as likely to remain continuously enrolled for 12 or more months. The treated, low food insecurity cases are 5 percentage points more likely to remain enrolled past 12 months. Reducing by half the total number of eligibility reports that a household expects they would have to complete as long as they are enrolled induces more marginal cases to remain continuously enrolled for longer.

8 Welfare Effects

I conclude with a stylized calculation of the marginal value of public funds (MVPF) associated with eliminating the quarterly reporting requirement (Hendren and Sprung-Keyser, 2020). The common MVPF formulation is:

$$\text{MVPF} = \frac{\text{WTP}}{\text{Net Cost}} = \frac{\text{WTP}}{\text{B + C + FE}}$$

The marginal value of public funds for a public program is the ratio of the recipient's willingness to pay for the benefits of that program and the net public cost of administering it. In this setting, the numerator represents participants willingness to pay to eliminate the reporting requirement, plus the personal costs required to complete the report. The denominator represents the total cost to government of eliminating the reporting requirement, including additional benefits disbursed, direct administrative costs saved, and participants' behavioral responses. Both numerator and denominator are calculated over the set of participants who would remain enrolled because the reporting requirement was eliminated.

I extend the standard calculation by accounting for the reform's effects on targeting, borrowing a framework from Finkelstein and Notowidigdo (2019). I distinguish between benefits and costs associated with increased retention that vary over recipient type. Suppose there are two types of participants $j \in \{L, H\}$ with latent wage θ_j where $\theta_H > \theta_L$. The following variation of the MVPF formula identifies the net benefits

[24] "High" food security cases are those whose predicted food insecurity value is less than .25. "Low" food security cases are those whose value is greater than .25. Thes sample is nearly evenly split between these two groups.

and costs of this reform across these two recipient types.

$$\text{MVPF}_{\text{reform}} = \frac{(\bar{B}_L + C_L)\frac{dE_L}{dR} + (\bar{B}_H + C_H)\frac{dE_H}{dR}}{(\bar{B}_L + \kappa_L - C_g)\frac{dE_L}{dR} + (\bar{B}_H + \kappa_H - C_g)\frac{dE_H}{dR}}$$

In the numerator, I identify the additional benefits delivered to each recipient type, scaled by the change in that type's enrollment induced by the reform.[25] $\bar{B}_j$ indexes the average monthly benefit received by type j. C_j indexes the private cost of completing an eligibility report for type j. $\frac{dE_j}{dR}$ is the change in enrollment for type j induced by the reform, which is the product of type j's change in retention and the fraction of type j in the population. In the denominator, I identify the net cost to government of type j's increased enrollment. C_g is the public cost of administering an additional eligibility verification. κ_j indexes the net fiscal externality associated with SNAP receipt for type j. Typically, the relevant cost is the income tax revenue lost due to labor supply responses to SNAP enrollment.

With this framework, I identify the net benefits and costs associated with the 2013 reform. I use imputed food insecurity levels to represent the two types of recipients. Individuals with seemingly less need for SNAP are represented by type H, and individuals with seeming greater need are type L. Since the reporting reform changed the likelihood that recipients remain enrolled beyond the month that the report was due, I modify the framework above to sum the benefits and costs associated with the multiple additional months of enrollment the reform induces, allowing retention effects to vary each month over recipient type. For simplicity, I consider the reform's effects on each type's retention from four to six months after enrollment, but one could easily extend this calculation beyond six months.

$$\text{MVPF}_{\text{reform}} = \frac{\sum_j \left(\left[\sum_m \bar{B}_{jm} \frac{dE_{jm}}{dR} \right] + C_j \frac{dE_j}{dR} \right)}{\sum_j \left(\left[\sum_m (\bar{B}_{jm} + \kappa_{jm}) \frac{dE_{jm}}{dR} \right] - C_g \frac{dE_j}{dR} \right)}$$

In the numerator, $\bar{B}_j$ is now the sum of benefits paid out in months 4 through 6, scaled by the increased enrollment of type j in each of those months, $\frac{dE_{jm}}{dR}$. In the denominator, there is the same summation of $\bar{B}_j$, as well as the net fiscal externality associated with benefit receipt in each of those months. The personal and public savings from not having to administer the quarterly report are outside the monthly summations, since they are only realized once.

Among cases that initially enrolled in 2013 before the reform took palce, $\bar{B}_L$ was \$416 and $\bar{B}_H$ was \$348. I multiply these benefits by the increased enrollment in each month for each type. The change in retention for each type j in each month is summarized in Panel B of Figure 14.

I define C_j, the time cost to the recipient of completing the quarterly report.[26] Assuming that the report takes two hours to complete,[27] and, following Finkelstein and Notowidigdo (2019), the opportunity cost

[25]The numerator is typically equal to the total benefits received by the marginal recipient, since behavioral responses to the reform are assumed to have no impact on utility, according to the envelope theorem. However, if recipients value a dollar of SNAP benefits less than a dollar in income, then this numerator is overstated. Hoynes and Schanzenbach (2009) conclude that recipients spend SNAP benefits as if they're cash, but Hastings and Shapiro (2018) and Whitmore (2002) find that recipients value a dollar from SNAP at only \$0.50 and \$0.80, respectively. Hendren and Sprung-Keyser (2020) use a WTP for SNAP of \$.69 for every dollar in benefits.

[26]This term is positive because the recipient places positive value on the time required to complete a report.

[27]As a benchmark, Isaacs (2008) finds that it takes recipients about five hours to complete an initial application.

of that time for recipients is twice the national minimum wage, the average personal cost of completing a quarterly report is roughly \$30. I assume the public cost of administering a quarterly report is roughly \$80, and the cost of reviewing a submission is the same for each type.[28]

Identifying the tax revenue consequences to SNAP receipt is more complicated. I follow Hendren and Sprung-Keyser (2020)'s calculation of the net fiscal cost associated with the introduction of SNAP, in which they distinguish this effect between adult recipients (due to their labor supply reductions) and children (due to their increase in lifetime earnings). For adults, Hendren and Sprung-Keyser (2020) report a fiscal externality $\kappa_a = \$0.16$ for every \$1 in SNAP receipt, identified from the labor supply response estimated in Hoynes and Schanzenbach (2012). For young children, they report a fiscal externality $\kappa_c = -.11$ for every \$1 in SNAP benefits, identified from the long-term earnings effects estimated in Bailey et al. (2020). Following Finkelstein and Notowidigdo (2019), I assume that SNAP receipt among seniors imposes no indirect revenue consequences. In order to scale the fiscal costs associated with the additional benefits paid out due to the reform, I multiple the average benefits, $\bar{B}_j$, by the share of enrollees of each type j that are adults, children, and seniors and their respective fiscal externalities.[29]

$$\begin{aligned} \kappa_L &= \bar{B}_L(\pi_{La}\kappa_a + \pi_{Lc}\kappa_c + \pi_{Ls}\kappa_s) \\ &= \$416(.434(.16) + .56(-.11) + .006(0)) \\ &= 3.24 \end{aligned} \qquad \begin{aligned} \kappa_H &= \bar{B}_H(\pi_{Ha}\kappa_a + \pi_{Hc}\kappa_c + \pi_{Hs}\kappa_s) \\ &= \$348(.537(.16) + .452(-.11) + .012(0)) \\ &= 12.60 \end{aligned}$$

With estimates for each term, I calculate an overall MVPF for this reform:

$$\text{MVPF}_{\text{reform}} = \frac{\$416(.24) + \$348(.29) + \$30(.14)}{\big(\$416 + \$3.24\big)(.24) + \big(\$348 + \$12.60\big)(.29) - \$80(.14)} = 1.06$$

The MVPF associated with this reform is 1.06.[30] More liberal choices regarding the cost of completing and administering these verifications, and a more conservative estimate of the labor supply response would push this estimate even higher. SNAP receipt has also been shown to improve short- and long-term health outcomes, increase life expectancy, reduce criminal recidivism, and decrease use of other public programs. Including these externalities in the denominator would also increase the estimate. At the same time, if recipients value SNAP benefits at only half their cost, my estimate could be severely overstated.

Hendren and Sprung-Keyser (2020) (Table II) report estimates of MVPF for SNAP from two program expansions: Finkelstein and Notowidigdo (2019)'s randomized outreach effort, and the program's initial

[28] Mills et al. (2014) reports that the average administrative cost of program churn across six states is \$80. Isaacs (2008) estimates that the annual administrative cost associated with SNAP enrollment, including all reporting costs, is about \$178 per recipient.

[29] Since I assume labor supply effects are constant across type and benefits decline with net income, the fiscal externality associated with increased enrollment could be higher for type L, which implies a decrease in targeting increases social welfare. As Finkelstein and Notowidigdo (2019) point out, this violates the standard intuition that delivering more assistance to individuals with greater need and higher marginal utility of consumption should increase social welfare. Estimates of labor supply response to SNAP benefits that vary with income or characteristics of ability would improve the accuracy of estimates of MVPF and might yield results more in line with the standard intuition. Incorporating welfare weights into calculations of MVPF would also change the welfare consequences of targeting. By allowing fiscal externalities to vary over adults and children, and because food security is lower for households with more children, my estimate is more in line with a standard intuition.

[30] Note that I compute estimates using unrounded values for each input, so reader's calculations using the arithmetic in the text may differ from what's reported.

quasi-random rollout (Bailey et al., 2020). Aggregating estimates of direct and indirect effects from multiples studies of SNAP, the authors conclude that the MVPF for increasing take-up of SNAP among seniors is between .89 and .92, and the MVPF for the program's initial introduction was 1.04.[31] Gray et al. (2023) estimate an MVPF between 1 and 1.32 for eliminating ABAWD work requirements.

The MVPF for this reform is comparable to these other estimates, which suggests that widening the reporting interval may be an efficient way to expand SNAP and increase take-up, despite worse targeting. In part, this type expansion is attractive from a MVPF perspective because it involves eliminating costly requirements for both recipients and government. This is in contrast to outreach efforts that can be expensive to administer. Unless particular outreach efforts are shown to be highly cost-efficient and effective at eliciting applications among the most disadvantaged non-participants, lowering administrative burdens and increasing retention may be a more attractive policy option to increase take-up.

9 Conclusion

This paper provides new evidence that administrative burdens lower participation in SNAP. Using enrollment data for 14 million unique individuals over 15 years from the country's largest food stamps program, I show that SNAP exits are concentrated in reporting months and lengthening the period in between when households must verify eligibility increases retention. I also show that Type 2 errors are widespread. Most households who exit in these months appear eligible before and after they leave, a finding that is robust to multiple definitions of eligibility. For every one ineligible household induced to leave in a reporting month, two eligible households also leave.

At the same time, reporting requirements do appear to lower Type 1 errors. These reporting requirements lessen participation at higher rates for no-longer eligible participants and households with higher earnings. Other measures of disadvantage, including earnings from one year before enrollment and characteristics predictive of food insecurity, are also predictive of whether a household will remain enrolled through a reporting month.

Whether these screening effects justify lower take-up depends on the net costs of redistribution and administering these procedures (Kleven and Kopczuk, 2011; Hendren and Sprung-Keyser, 2020). Relying on others' estimates of those costs and benefits, I present evidence that less frequent recertifications can efficiently improve take-up.

This paper does not address whether alternative procedures can more efficiently screen for eligibility. Recent work finds that business processes and simpler procedures can affect retention (Gray, 2019; Homonoff and Somerville, 2021; Wu and Meyer, 2021). Policymakers might consider limiting the information and documentation required in these reports, and how state administrative data could be used to screen out no longer eligible households, instead of soliciting this information from recipients themselves. Measuring the impact of these procedures and comparing their effects to even longer reporting intervals is an important avenue for future work.

[31] Bailey et al. (2020) report their own estimate of the MVPF associated with SNAP's introduction, which is 56. The massive difference is due to how the authors value the expected difference in life expectancies due to SNAP receipt.

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10 Tables and figures

Table 1: Demographic characteristics for primary taxpayer in SNAP sample

	2006	2009	2012	2014	2019	2021
Age						
0-18	.607	.578	.536	.501	.478	.388
19-65	.380	.410	.441	.458	.458	.457
65+	.023	.026	.038	.056	.078	.171
Household type						
Children only	.170	.169	.150	.133	.120	.076
Working-age adults only	.120	.148	.179	.204	.220	.244
Single working-age adult w/ children	.436	.378	.354	.345	.346	.305
2+ working-age adults w/ children	.249	.277	.276	.259	.231	.196
Seniors only	.019	.021	.032	.047	.069	.158
Seniors with children	.004	.004	.004	.005	.006	.007
Race						
White	.220	.220	.220	.214	.210	.203
Hispanic	.461	.498	.504	.503	.494	.434
Black	.167	.140	.124	.118	.121	.118
Asian/NH/PI	.033	.032	.031	.034	.029	.040
SE Asian	.049	.039	.037	.037	.036	.046
AI/AN	.008	.007	.006	.006	.006	.005
Other	.062	.064	.077	.088	.105	.155
Language						
English	.731	.712	.718	.724	.735	.734
Spanish	.210	.241	.240	.234	.221	.191
Other	.060	.047	.043	.042	.044	.075
Earnings						
On case with earnings	–	–	.397	.554	.560	.308
Average earnings ($)	–	–	6,141	11,916	12,720	8,826
Observations	2,877,915	4,108,240	5,557,976	6,039,948	5,447,710	6,103,451

Notes. Table 1 summarizes the composition of the SNAP caseload in California for select years in my sample. I define the caseload to be all unique individuals enrolled for at least one month in the calendar year. Among these individuals, I identify the share in each of three age bins; the share in six different household types; the share in each of seven race codes; the share who speak English, Spanish or neither; and the share in cases with non-zero versus zero earned income.

Table 2: Comparing reentry rates in MEDS to CDSS's reported churn rates

Months	CDSS churn rate	MEDS reentry rate
1	11.8	10.9
3	14.3	18.2
6	–	30.2
12	–	42.2
18	–	48.3
24	–	52.6

Notes. Table 2 summarizes the share of individuals who, after exiting, reenter SNAP within six different timelines, limited to individuals who exited after 2014. I calculate the share of individuals who exit the program and then re-enroll within t months, restricting attention to uncensored observations. I calculate the overall averages for the counties by averaging the quarterly rates.

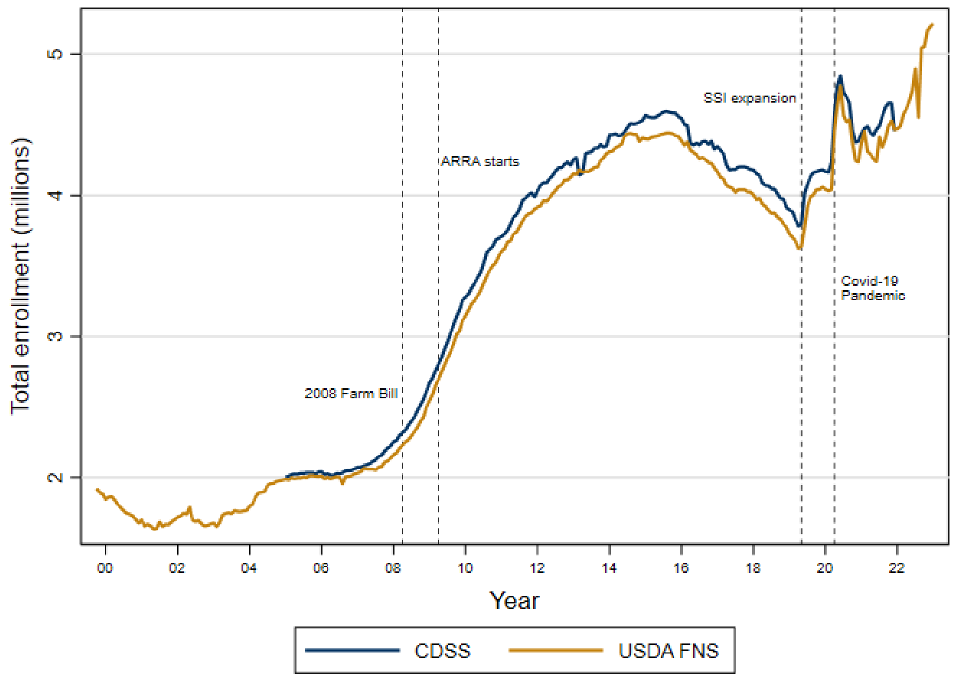
Table 3: Average likelihood of exiting SNAP in reporting and non-reporting months by eligibility status

	Non-reporting months	Reporting months
Eligible	.022	.117
	(.000)	(.001)
Ineligible	.053	.324
	(.000)	(.000)

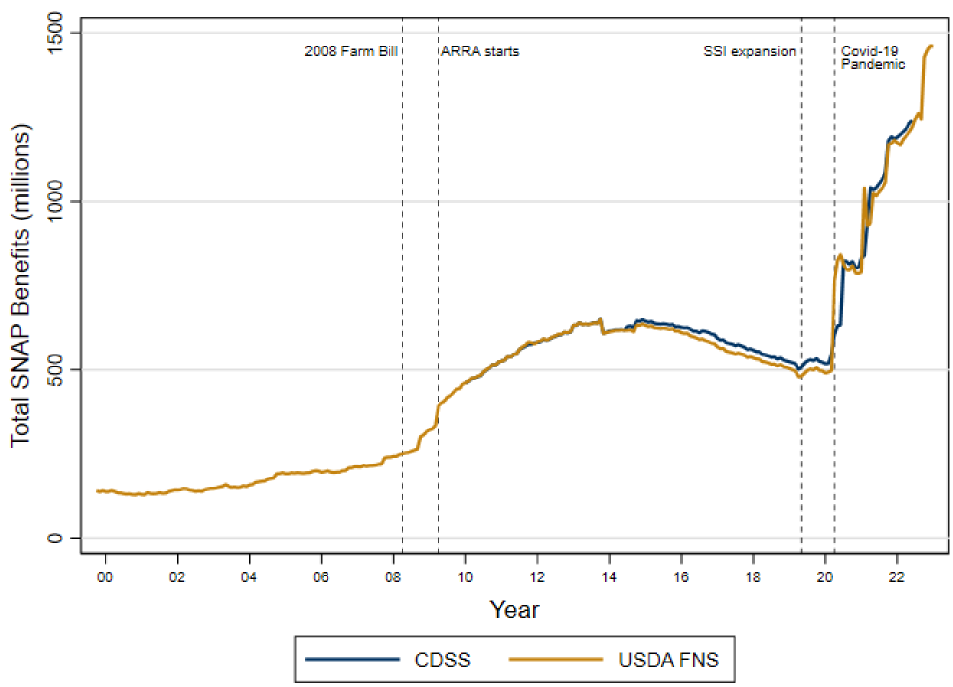
Notes. Table 3 summarizes the likelihood of exit by eligibility status in reporting and non-reporting months, limited to cases that started enrollment after 2014. I calculate these averages by estimating Equation 1, using an indicator for eligibility as the characteristics, and then transforming the estimated log-odd ratios into average effects. Values in the parentheses represented Delta-method estimated standard errors.

Figure 1: Total monthly SNAP enrollment and disbursements in California, 2000-2021

(a) Client-level enrollment



(b) Disbursements



Notes. **??** plots total SNAP enrollment for California from two data sources. The USDA counts are the official figures reported by the counties to the state, which are then reported to FNS at USDA. The MEDS count is the sum of individuals recorded as being enrolled in SNAP each month in the Medicaid Monthly Eligibility Files.

Figure 2: SNAP reporting schedule in California

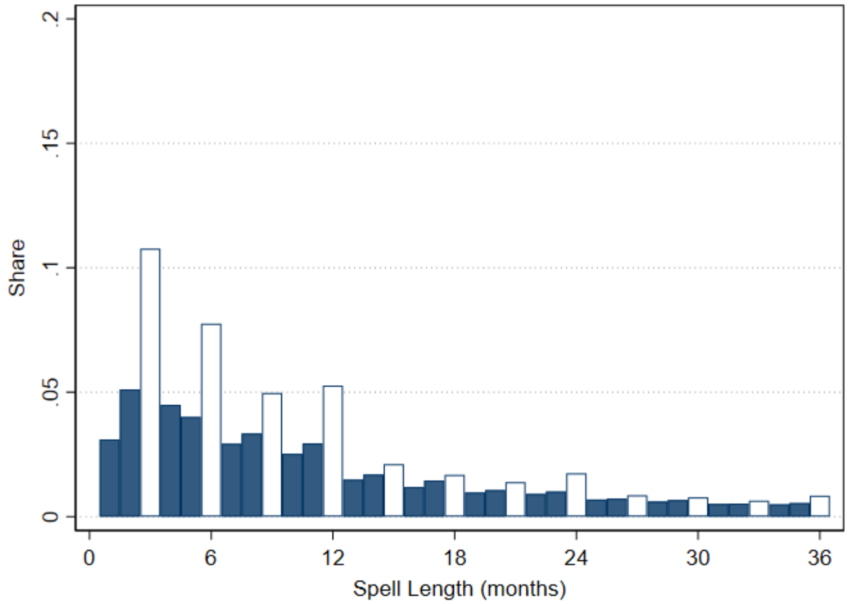
Months since enrollment

	0	6	12	18	24	30	36
Working age households	Enroll	SAR-7	RRR	SAR-7	RRR	SAR-7	RRR
Seniors or disabled persons w/ earnings	Enroll		SAR-7		RRR		SAR-7
Seniors or disabled persons w/o earnings	Enroll		SAR-7		SAR-7		RRR

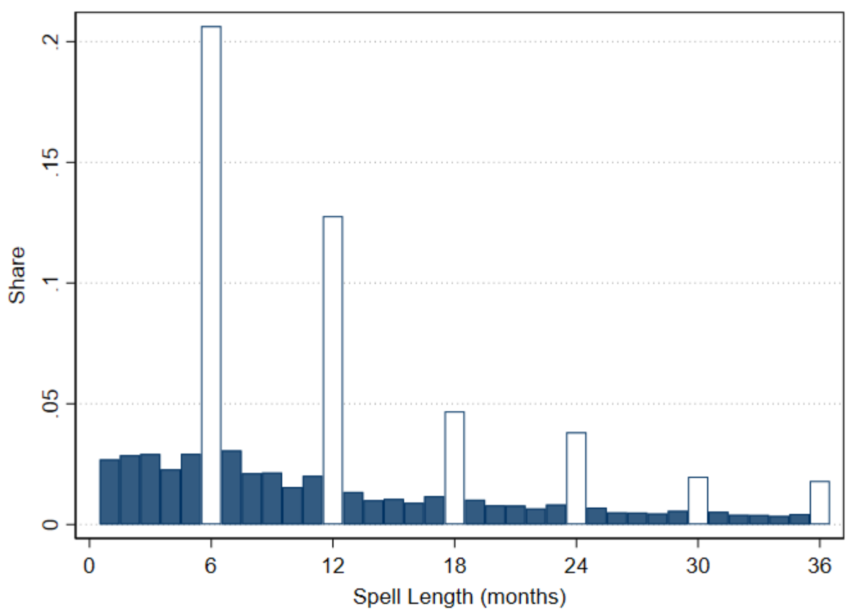
Notes. Figure 2 illustrates the reporting schedule for three types of households. Most households must complete a periodic report (known as a Semi-Annual Report, or a SAR-7) six months after enrolling, and every twelve months thereafter. The household must complete a short form, identifying whether household members, sources of income, and deductible expenses have changed, and if so, how. Six months later, and twelve months after enrolling, the household must complete a full recertification (known as a RRR). This entails completing a longer form (known as a CF-37), including much of the same information, providing proof of earnings, and completing an interview with county staff. Households with seniors or individuals with a disability and without working-age adults, but who have some earned income, are allowed to extend the recertification schedule, such that they complete the SAR-7 twelve months after enrolling, and the RRR twenty four months after enrollment. Finally, households with seniors or disabled persons but no earned income only need to complete the RRR every 36 months and the SAR-7 every 12 months.

Figure 3: Frequency distribution of SNAP enrollment durations

(a) Spells beginning 2005 - 2011

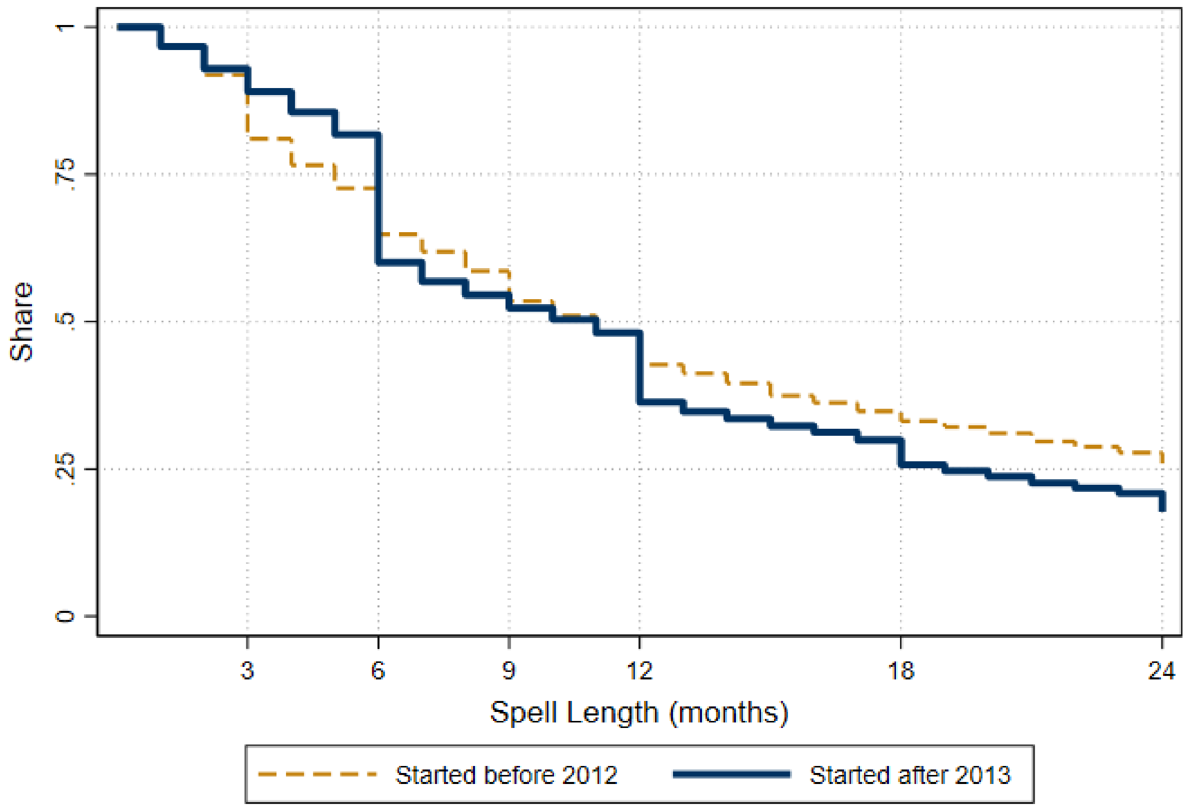


(b) Spells beginning 2014 - 2018



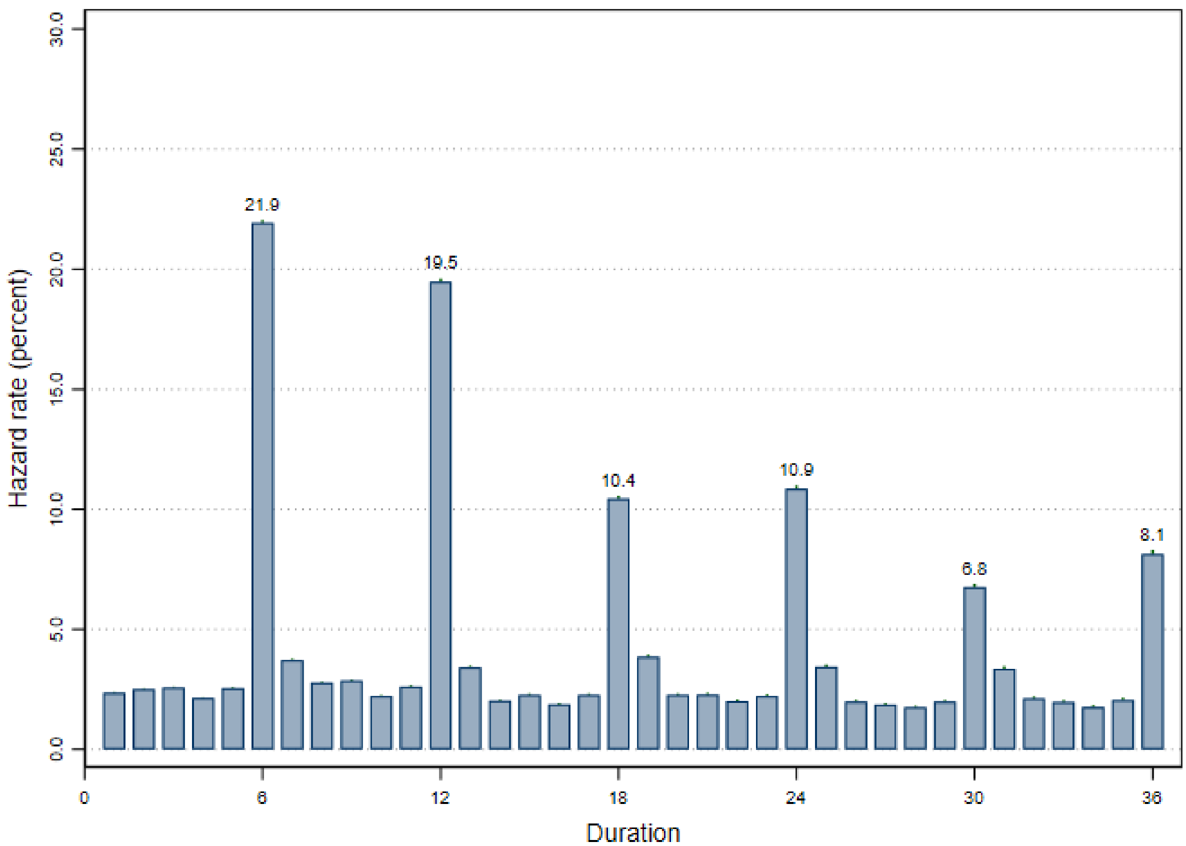
Notes. Figure 3 summarizes the frequency of enrollment spell lengths – periods of consecutive months in which individuals are receiving SNAP. I plot two versions of this distribution. Before October 2013, households had to recertify every three months, and every six months since then. Panel A includes spells that started at least two years before October 2013, and Panel B includes spells that began after October 2013. The white bars represent spell lengths that align with reporting periods. Before 2013, the most common enrollment spell was three months, which is when households had to submit their first quarterly report. Now, less than five percent of cases end at three months, and the most common spell length is six months, again, when households must first recertify.

Figure 4: Survival rates for SNAP recipients up to 24 months after enrollment



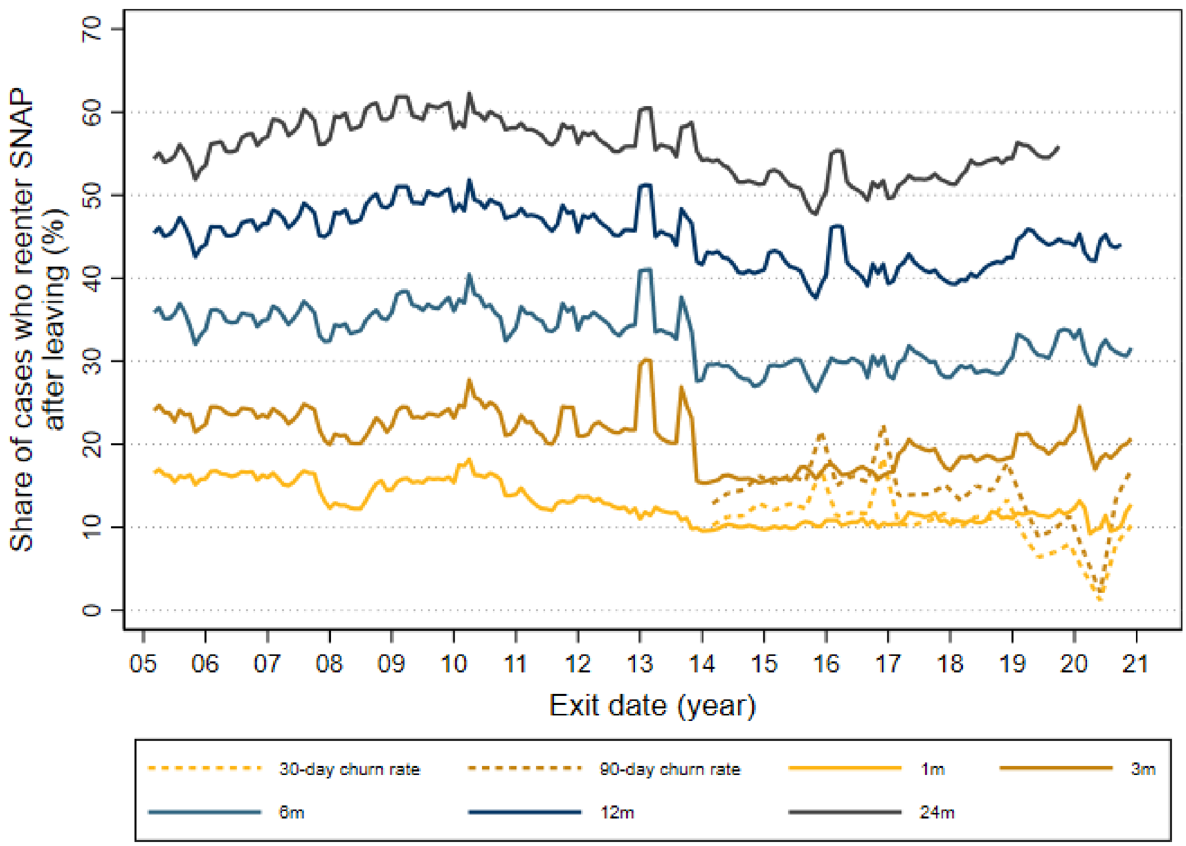
Notes. Figure 4 illustrates the share of recipients who remain enrolled in SNAP each month after their enrollment begins, up to 24 months. Again, I consider these patterns before and after the 2013 reform. The blue lines illustrate survival rates assuming that one-month gaps in the CDSS are true drops in participation, and the gold lines illustrate survival rates after I fill in one month gaps. The largest drops in participation occur in reporting months. Since 2013, around 40 percent of households leave SNAP within six months after enrolling. The difference in retention rates between the pre- and post-reform periods suggest that individuals will generally remain enrolled in SNAP until they are required to recertify. Filling in one month gaps in participation does not dramatically change these results, suggesting that low retention is neither a data issue nor a function of short breaks in enrollment.

Figure 5: Hazard rates of program exit by enrollment duration



Notes. Figure 5 reports the hazard rates by enrollment month, or the likelihood that any given person will leave SNAP in that month conditional on remaining enrolled up to that month. I estimate these hazard rates by estimating a logit regression on dummies for enrollment duration, as well as county, date and household type fixed effects. I restrict to cases that started between 2014 and 2018, as these cases were only exposed to the post-2013 reporting policy and had the possibility of remaining enrolled for at least 24 months. After estimating the logit, I identify average marginal effect of enrollment duration on exit.

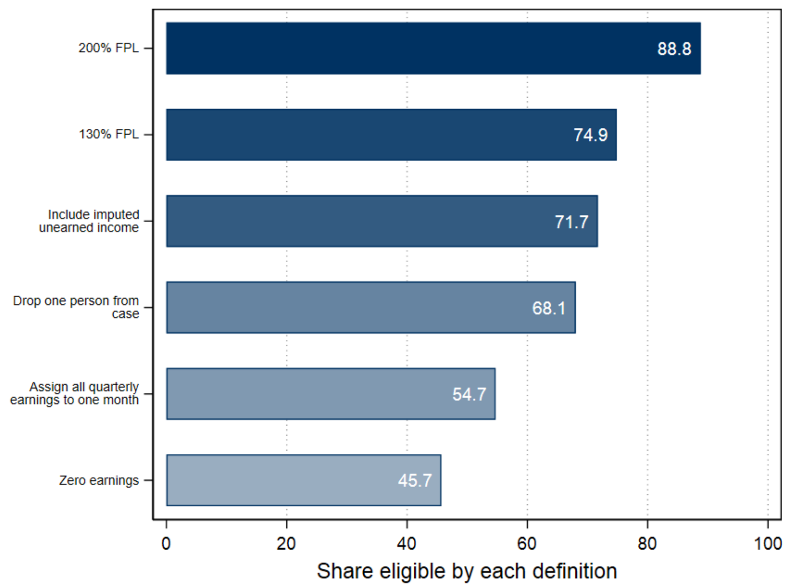
Figure 6: Share of cases that reenter SNAP by five different timelines over exit dates



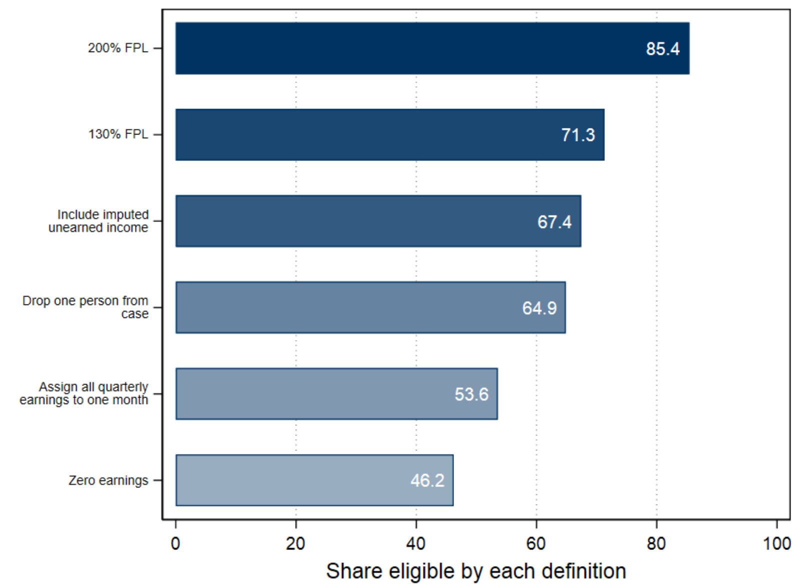
Notes. Figure 6 plots the share of cases ending in each month who re-enroll within each of the identified timelines. I calculate these reentry measures in three steps. First, I count the number of instances in which an individual re-enrolls in SNAP within 1, 3, 6, 12, 18 or 24 months after exiting. Next, I count the number of enrollment spells that ended with enough time such that I can observe reentry within the relevant timeline. I measure the share of reentries within each timeline by dividing the first count by the second. For clarity, the trend lines reported here represent the three-month moving average over each year-month. There is a mechanical relationship among these trends: a one-month reentry also qualifies as reentry within 24 months, meaning that spikes and dips will reverberate upward.

Figure 7: Share of cases exiting SNAP that appear income eligible

(a) Earnings from the quarter in which the case leaves SNAP

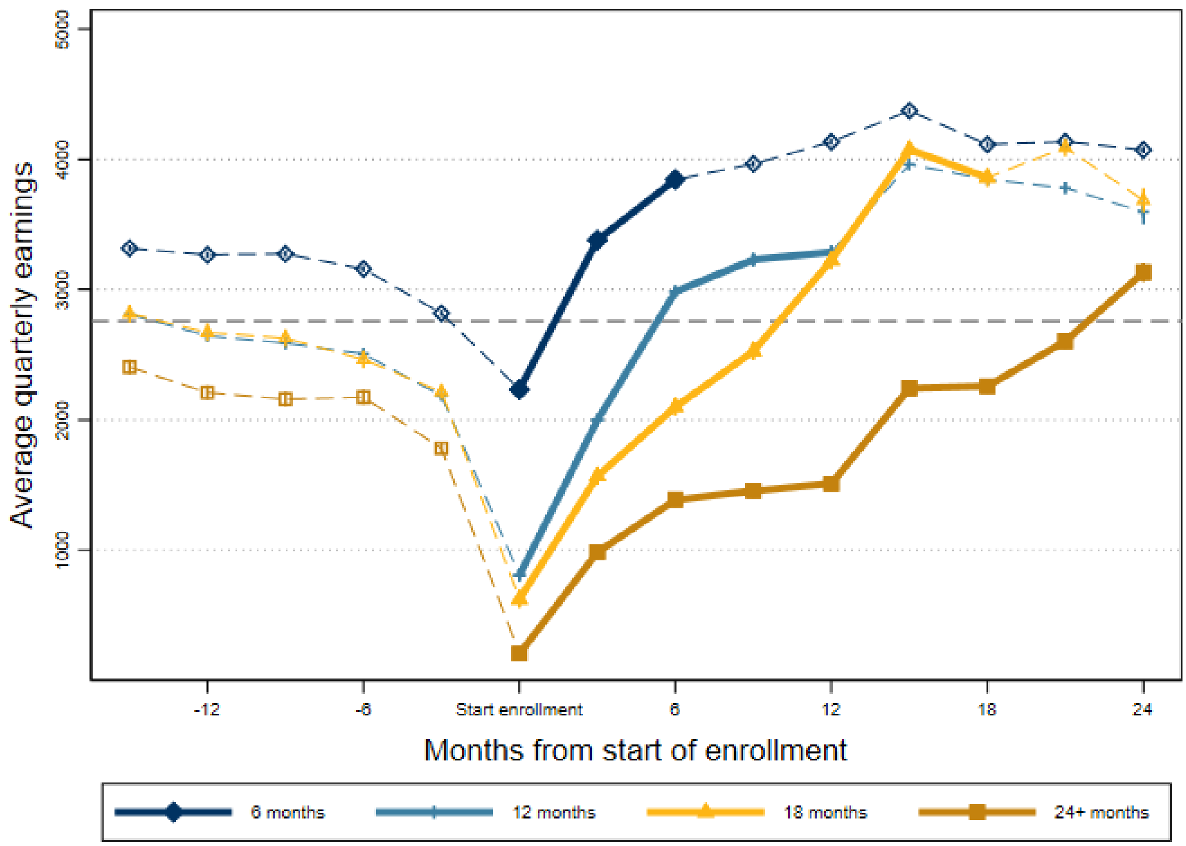


(b) Earnings from the quarter after the case leaves SNAP



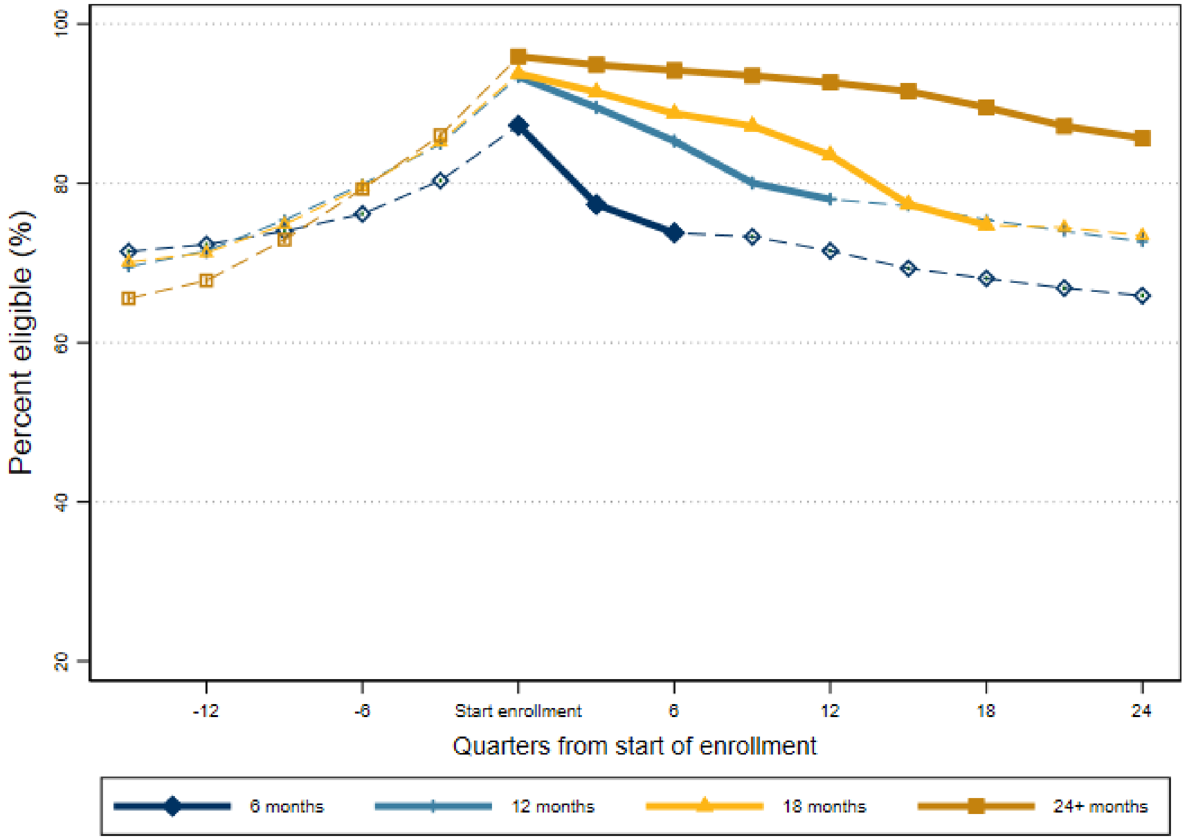
Notes. Figure 7 reports the share of cases who exit SNAP but appear income eligible according to various eligibility definitions. I restrict to cases that leave SNAP at the end of a calendar quarter between December 2013 to December 2019. Panel A uses earned income from the quarter in which the case leaves SNAP, and Panel B uses earned income from the quarter immediately after the case leaves SNAP. In the first definition, I compare one-third of a household's total earned income to 200 percent of its monthly FPL. In the second, I use 130 percent of the households' FPL. Third, I identify whether one-third of a household's total quarterly income, plus the average unearned income for its households type assigned using the procedure described in the appendix, exceeds 130 percent of the household's FPL. Fourth, I identify whether one-third of a household's total quarterly income exceeds 130 percent of the household's FPL, assuming their household size was reduced by one person. Fifth, I identify whether a households' total quarterly income exceeds 130 percent of the household's FPL; this test is equivalent to assuming that the household receives all of their quarterly income in the month of, or immediately following, their exit.

Figure 8: Average monthly earnings before, during, and after SNAP enrollment by spell length



Notes. Figure 8 plots average household earnings for each quarter relative to the quarter before enrollment starts. I separate these estimates between cases exiting SNAP at six, twelve, eighteen and more than 24 months. I identify these averages by regressing quarterly earnings on a vector of dummies for each quarter relative to the quarter before enrollment in SNAP starts. I also include an indicator for spell length and an interaction between spell length and the relative quarter dummies, as well as fixed effects for calendar quarter, demographic characteristics and household type. I limit to spells between 2014 and 2019, for whom I have complete earnings information and the standard reporting window was 6 months. I also restrict to cases that begin at the start and end at the close of quarters, so that I am able to distinguish between income earned while enrolled and not enrolled. Finally, I restrict to spells in which the enrollee does not return to SNAP within 12 months after exiting. Post regression, I predict average earnings for each relative quarter separately for each spell length, and at the means of the other covariates. The solid lines and markers indicate quarters in which the case is still enrolled in SNAP, while hollow markers and dashed lines represent quarters in which the case is not enrolled. The dotted horizontal line identifies the average quarterly earnings ($2,759) in quarters within one year on either side of when enrollment starts.

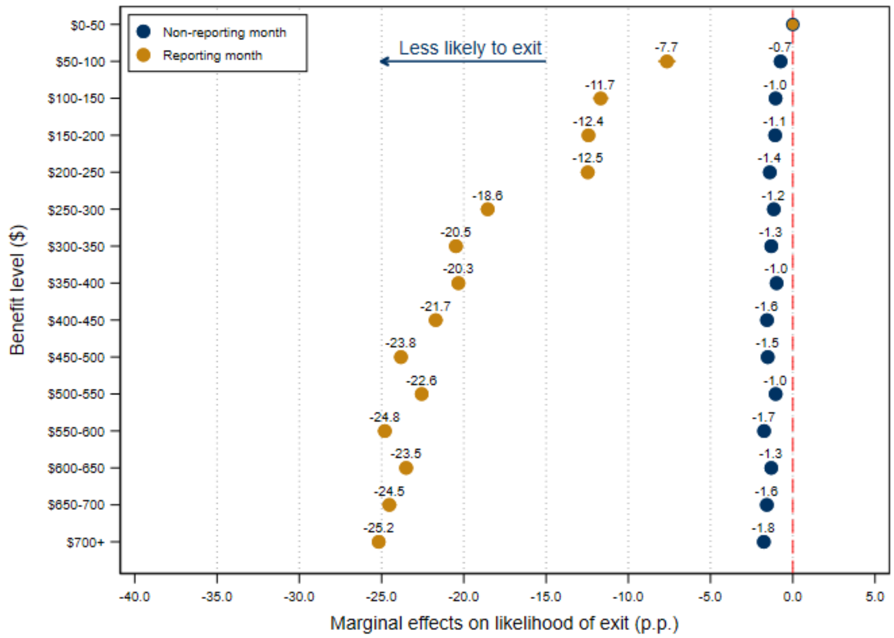
Figure 9: Share of cases that appear income eligible each quarter relative to case's initial enrollment in SNAP



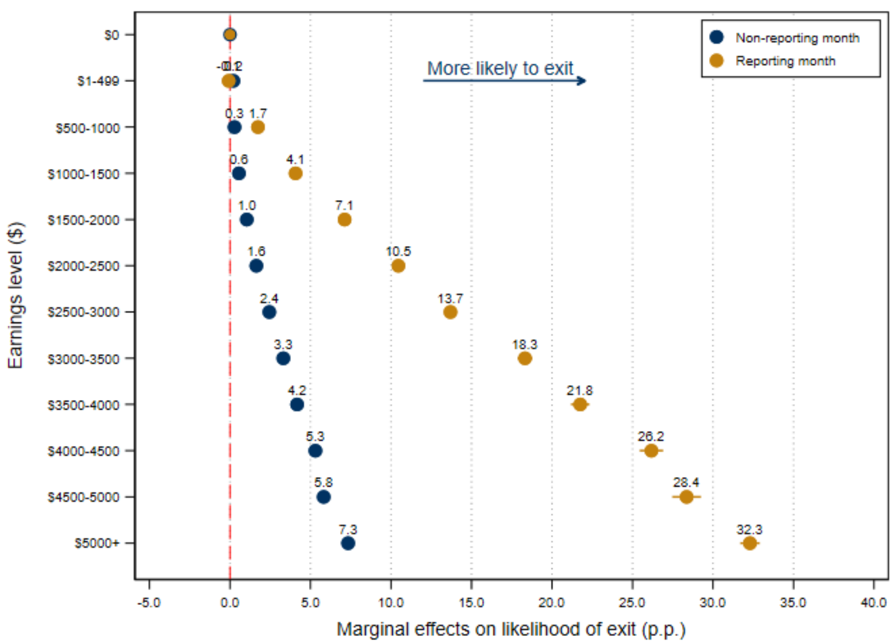
Notes. Figure 9 plots the share of cases that appear income eligible each quarter relative to when they first enroll separated by spell length. Analysis is restricted to spells between 2014 and 2019, for which I have complete earnings information and the standard reporting window was 6 months. I also restrict to cases that begin at the start and end at the close of quarters, so that I am able to distinguish between income earned while enrolled and not enrolled. As noted in the body of the paper, these shares might not reach 100 percent, as expected, because a number of households will still qualify even if their income exceeds 130 percent FPL, because they are able to deduct the cost of numerous expenses. It is also the case that the verification process is imperfect, and a small share of households who have incomes above the eligibility threshold for some month during the quarter will be able to remain enrolled.

Figure 10: Likelihood of exiting SNAP by household benefit amount and earned income

(a) Benefit amount

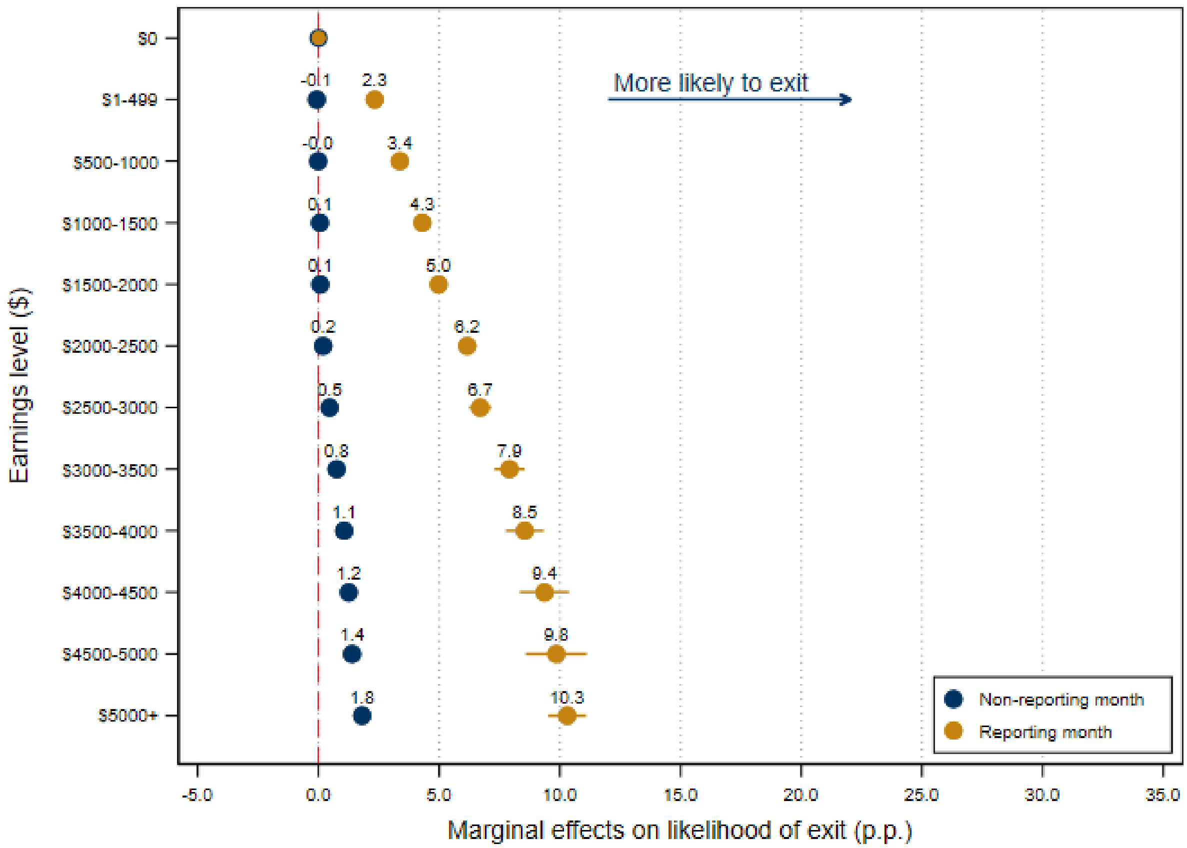


(b) Earned income



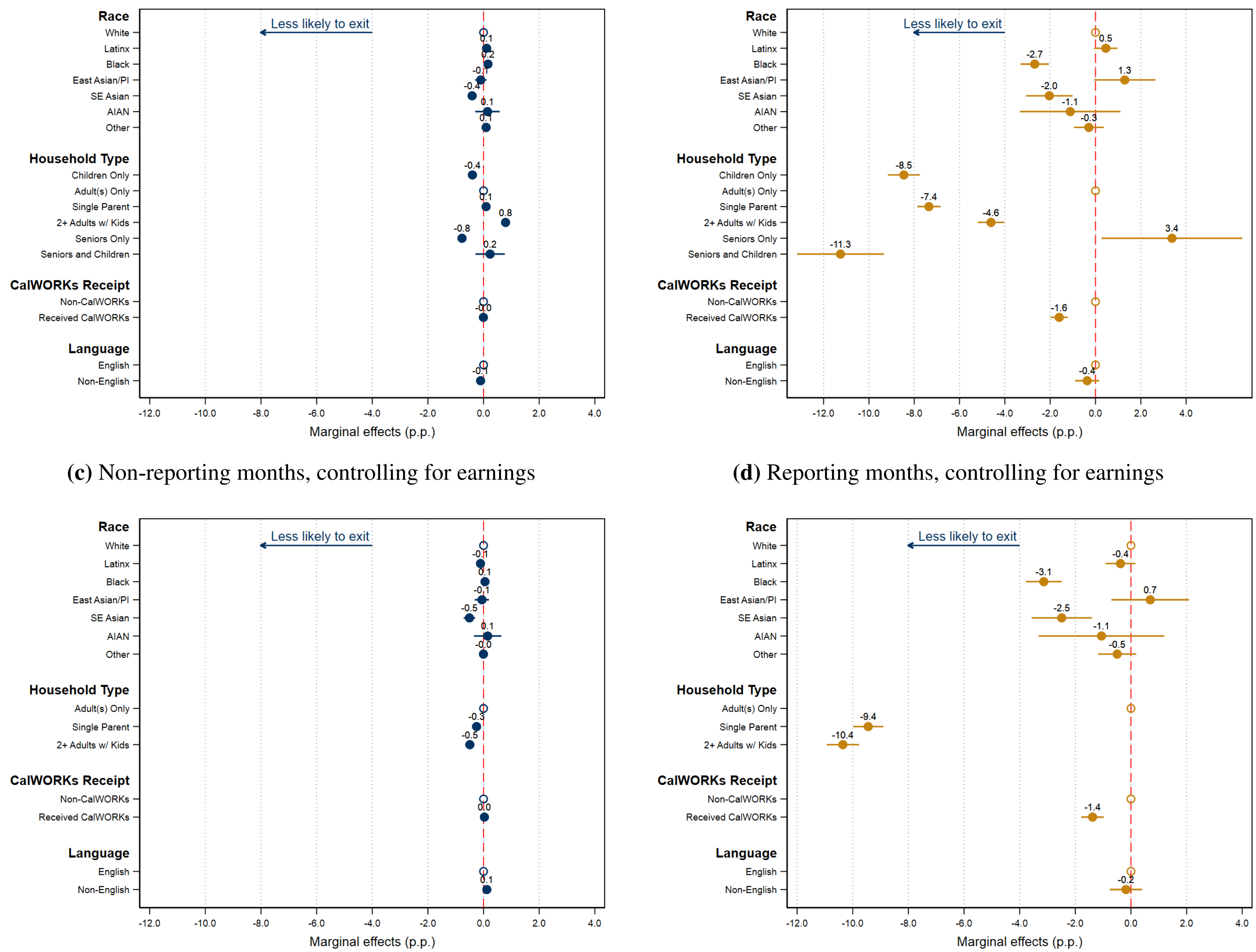
Notes. Figure 10 reports the marginal effect on likelihood of exit by earnings levels and benefit amounts in reporting and non-reporting months. I calculate these effects by first estimating Equation 1 and then identifying the difference between the predicted probabilities of exit for each benefit and earnings level, relative the baseline, at the mean effect of all other covariates in that model. The baseline likelihood of exit for households with $0-50 in SNAP benefits is 4 percent in non-reporting months and 37 percent in reporting months. The baseline likelihood of exit for households with no earnings is 2 percent in non-reporting months and 10 percent in reporting months.

Figure 11: Likelihood of exiting SNAP in a reporting month by household earnings 12 months before initial enrollment



Notes. Figure 11 reports the marginal effect on likelihood of exit in reporting and non-reporting months by earnings levels 12 months before enrollment starts. I calculate these effects by first estimating Equation 1 and then identifying the difference between the predicted probabilities of exit for each benefit and earnings level, relative to the baseline, at the mean effect of all other covariates in that model. The baseline likelihood of exit for households with $0 in earnings one year before enrollment starts is 3 percent in non-reporting months and 13 percent in reporting months.

Figure 12: Likelihood of exiting SNAP in a reporting month by demographic characteristics



(a) Non-reporting months

(b) Reporting months

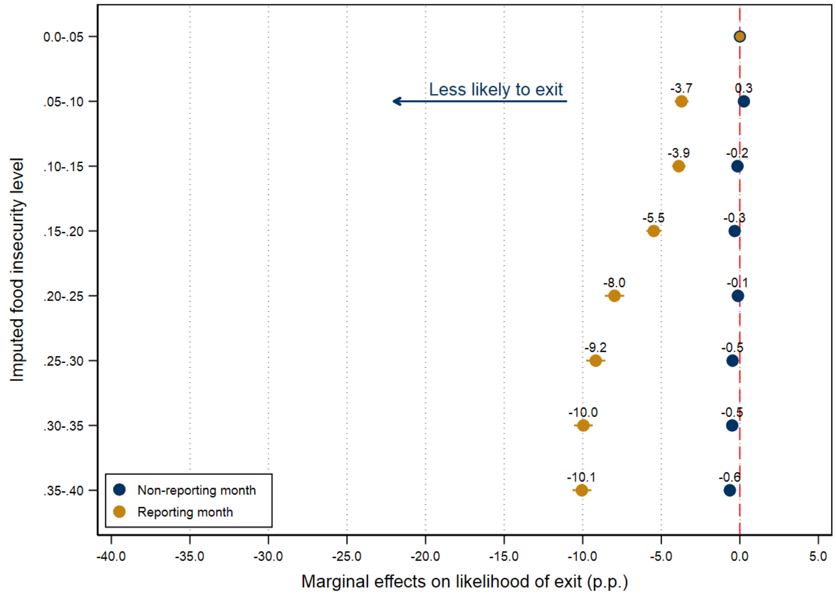
(c) Non-reporting months, controlling for earnings

(d) Reporting months, controlling for earnings

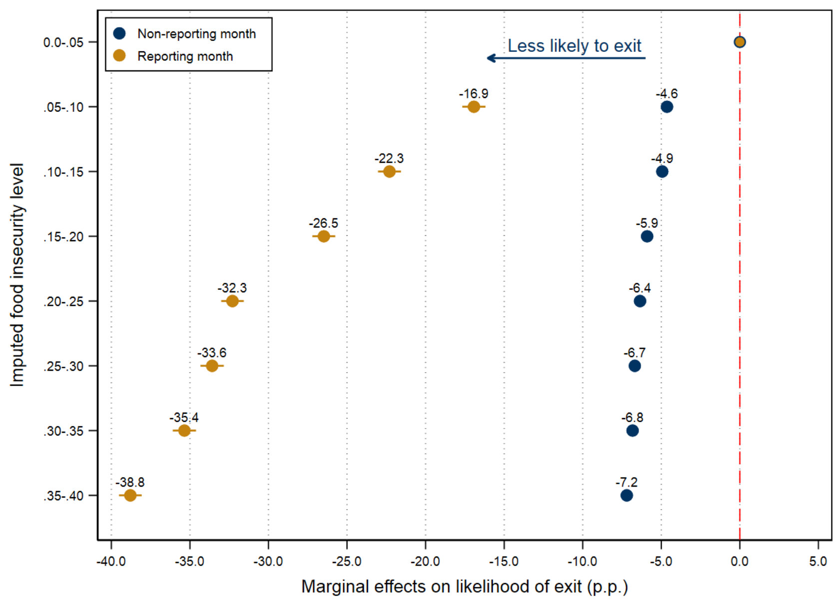
Notes. Figure 12 reports the marginal effect on likelihood of exit in reporting and non-reporting months by listed demographic characteristics. I calculate these effects by estimating Equation 1 and then identifying the difference between the predicted probabilities of exit for each benefit and earnings level, relative to the baseline, at the mean effect of all other covariates in that model.

Figure 13: Relative likelihood of exiting SNAP in a reporting month by imputed food insecurity level

(a) Without earnings



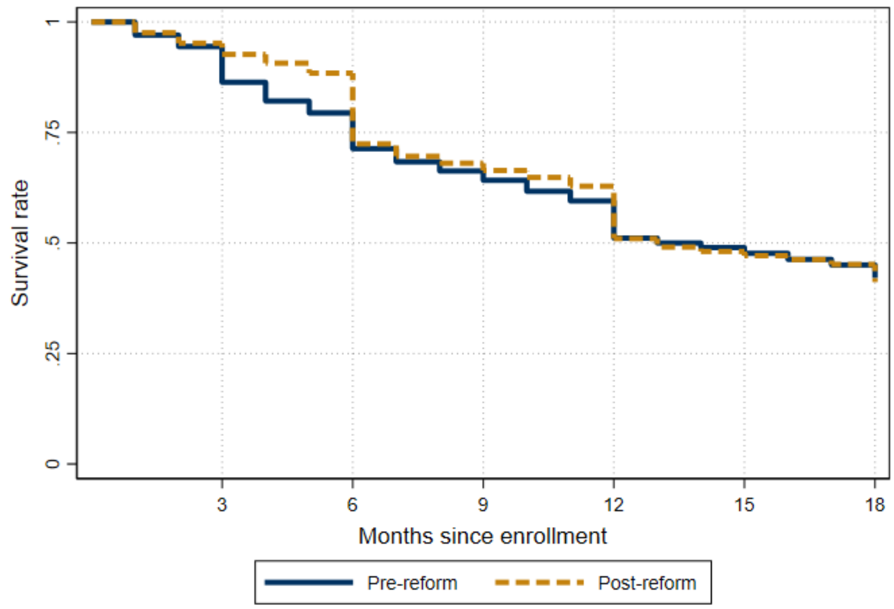
(b) With earnings



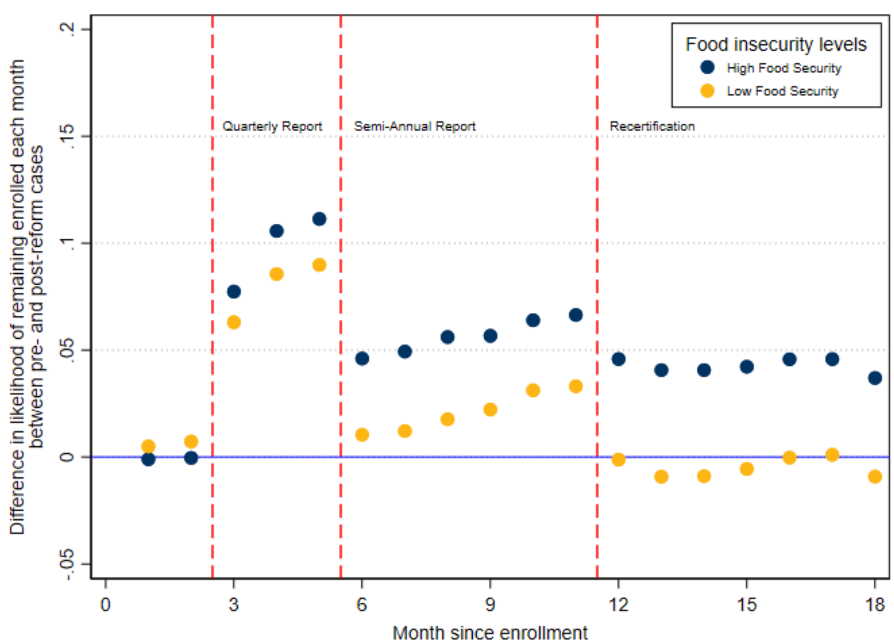
Notes. Figure 13 reports the marginal effect on likelihood of exit in reporting and non-reporting months by levels of imputed food insecurity. These effects come from estimate Appendix Equation 1, and I then transform the estimated log odds to average marginal effects relative to the same baseline and holding all other covariates at their means. In order to demonstrate the important of earnings to food insecurity, and to isolate the relevance of demographic characteristics like race and household composition by themselves, I estimate these effects using earnings in the food insecurity assignment and not. For Panel A, I assign households a predicted level of food insecurity without using their earned income. For Panel B, I incorporate households' earnings. See **??** for more information about this imputation. The baseline likelihood of exit for households with lowest level of imputed food insecurity (not including earnings) is 3 percent in non-reporting months and 22 percent in reporting months. The baseline likelihood of exit for households with lowest level of imputed food insecurity (including earnings) is 9 percent in non-reporting months and 46 percent in reporting months.

Figure 14: Survival rate for SNAP recipients before and after reporting reform

(a) Average survival rate for pre-reform and post-reform cases among lowest food insecurity cases



(b) Differences in survival rates between pre-reform and post-reform cases by levels of predicted food insecurity



Notes. Figure 14 illustrates the effect that the 2013 reporting reform had on survival rates. In Panel A, plot survival rates for cases that began between January and June 2013 (pre-reform) and those that began between July 2013 and December 2013 (post-reform). The reform decreased the exit rate at three months, but the average survival rates converge after six month. Panel B distinguishes this effect between cases identified as high and low food insecurity.