Work Requirements and Employment: Evidence from SNAP

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Abstract

Work requirements to receive public assistance are widely implemented and debated policies. Despite this fact, there is not yet consensus on how work requirements affect employment outcomes. I use administrative data from a state's Supplemental Nutrition Assistance Program (SNAP) and Unemployment Insurance (UI) records to study this question. Exploiting the reintroduction of work requirements and the fact that only certain adults are subject to them, I find that work requirements reduce participation in SNAP and increase employment. I also find evidence that the work requirements have stronger effects on employment for long-term users of SNAP.

JEL Codes: H53, H75, I18, I32, I38, J22

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1 Introduction

In designing means-tested public assistance programs, policymakers face a difficult trade-off. These types of programs are meant to reach the needy, but without disincentivizing work. In order to address the latter, policymakers often institute work requirements necessary to fulfill in order to receive benefits. The Supplemental Nutrition Assistance Program (SNAP) is one such program. The policy debate on work requirements in SNAP often splits into two positions. Some argue that SNAP is a critical social insurance that delivers invaluable nutrition assistance to millions of needy households, and that work requirements pose unnecessary barriers to that assistance while being ineffective at increasing employment (Wething, 2025; Pavetti et al., 2023). Others argue that programs like SNAP may discourage work and create dependency on public assistance, while work requirements may increase employment at least for some groups (Rachidi, 2025; Dickerson, 2024). While work requirements for SNAP date back to the 1970s, the more stringent modern work requirements originated with the 1996 welfare reform. Since then, several proposals have been made to expand work requirements for SNAP. In particular, there is a focus on expanding work requirements for Able-Bodied Adults Without Dependents (ABAWDs) with the argument that work requirements are an effective tool for increasing employment, labor force attachment, and earned income for this group. In accomplishing these, advocates argue that work requirements promote self-sufficiency and lower reliance on public assistance. This paper provides causal estimates for the efficacy of work requirements in achieving these goals.

Using administrative data from one state in the mountain-plains region (to be referred to as the "mountain-plains state"), I implement a difference-in-differences design which uses plausibly exogenous variation in exposure to ABAWD work requirements. I exploit the fact that only able-bodied adults without dependents who are aged 18-49 are subject to work requirements along with the reintroduction of work requirements after the Great Recession in September of 2012 to estimate their effects. Those who are at or above the age of 50 and/or who have children under the age of 18 in the household are not subject to the ABAWD work requirements.

This latter group serves as the control group.

My results show that being subjected to work requirements decreases program participation by 21.2% within one year. This effect is significantly larger than is estimated in other studies. I also find that employment increases by 6% within two years for ABAWDs as compared to those who are not subject to the work requirements, and increases employment by 11.1% in some samples. I estimate increases in earned income of 17.5% to 26.5% depending on the sample. I estimate smaller effects, both for program participation and for employment outcomes, for short-term users of SNAP, those who had participated in SNAP for less than two years before the reintroduction of work requirements, and larger effects for long-term users of SNAP, those who have been on the program for at least two years before the reintroduction. My results are robust to alternative samples, control groups, as well as traditional two-way fixed-effects estimates in place of my preferred doubly robust difference-in-difference estimates. I also observe similar patterns across both men and women. Using LASSO to categorize SNAP participants by predicted participation intentions, I also find evidence that work requirements increase labor supply for those who wish to replace income lost from SNAP, and those who wish to comply with work requirements to maintain SNAP benefits. Finally, I calculate the marginal value of public funds (MVPF) of eliminating work requirements and determine this value to be 0.73. I conclude that work requirements can be an effective tool for increasing employment and self-sufficiency.

I make three main contributions. First, this paper contributes to work conducted on work requirements and their effects on program participation and employment for individuals on SNAP. Cook and East (2024) study the effects of general work requirements in SNAP (as opposed to ABAWD work requirements studied in this paper) by exploiting the fact that a head of a household ages into general work requirements when their youngest child turns 6. They find no effect on employment. Ritter (2018), Harris et al. (2020), Han (2022), and Cuffey et al. (2022) study the effects of ABAWD work requirements on program participation and/or employment outcomes, but rely primarily on public use survey data such as the American

Community Survey (ACS) and Current Population Survey (CPS) which have been shown to nonrandomly underreport participation in SNAP (Meyer et al., 2022). This paper uses administrative data and so does not suffer from the same problem. In addition, these studies inevitably include individuals who would not participate in SNAP regardless of whether or not work requirements are in place, likely attenuating estimated effects. By only including known SNAP participants, this paper is able to recover a treatment effect for those who would actually be treated by the reinstatement of work requirements. Stacy et al. (2018) use administrative SNAP data linked to a restricted-use version of the ACS, and Gray et al. (2023) use administrative SNAP data from Virginia linked to unemployment insurance (UI). However, these two papers exploit the fact that ABAWDs age out of the work requirements at age 50, and so use difference-in-discontinuities and regression discontinuity designs respectively. A limitation of this design is that it can only identify effects for those close to the threshold, when it is possible for effects to be different across ages as I show in this paper. This paper provides estimates which are generalizable to a much broader age range as it does not rely on the age-50 threshold for identification.

A second contribution to this literature is the distinction I find between types of SNAP participants, finding larger effects of work requirements for what I call long-term users than for short-term users. While previous literature on work requirements has focused on heterogeneity by demographics such as race, I provide evidence of heterogeneous effects by length of time spent on SNAP, which I take as a proxy for intended length of time planned to stay on SNAP. These results suggest that work requirements may better be suited for participants who have been participating in SNAP for long periods of time. In terms of policy implications, these results suggest that work requirements may not be necessary until an individual has been on the program for an extended period of time, but that for those who have been on the program for such a period of time, work requirements may be helpful in removing the disincentive effects SNAP has for employment.[1]

[1]Causal estimates on the disincentive effects of SNAP are rare. Hoynes and Schanzenbach (2012) find reductions in employment and hours worked due to food stamp introduction. East (2018) finds that eligibility for

Third, this paper contributes to the literature on ordeal mechanisms and administrative burden in SNAP and social safety net programs more generally. Herd and Moynihan (2025) provide a review and summary of several sources of administrative burden including renewal and recertification, bureaucrat discretion, and work requirements. Several papers emphasize the importance of program and eligibility information in affecting participation in various programs such as SNAP, EITC, and disability insurance (Finkelstein and Notowidigdo, 2019; Bhargava and Manoli, 2015; Armour, 2018). Others show the relationship between access to help with applications and program participation (Rossin-Slater, 2013; Deshpande and Li, 2019; Schanzenbach, 2009). Another group of papers discusses the relationship between stigma and participation in safety-net programs (Celhay et al., 2022; Anders and Rafkin, n.d.; Currie et al., 2001). Wu and Meyer (2023) find that an initiative in Indiana which automated much of the work done by caseworkers and increased administrative burden resulted in lower enrollment for SNAP, TANF, and Medicaid. This paper similarly finds that increasing the costs of participating in a safety-net program reduces participation, though at the same time increase employment.

2 Background and Related Literature

2.1 Background on SNAP

SNAP is among the largest means-tested public assistance programs in the United States. In 2024, the program provided over $114 billion in benefits, and served a monthly average of 41 million individuals (USDA, 2025). In order to obtain benefits, individuals must certify that they are eligible based on a gross income test, a net income test, and an asset test.[2] The gross income test requires that the household's gross income, that is income before any deductions

food stamps decreased labor supply for immigrants. Cook and East (2023) find no negative effects of SNAP on earnings in the long-run.

[2]Some states waive the asset test, though the mountain-plains state does not. The asset test currently allows for households to have up to $3,000 in countable resources like cash and still remain eligible for SNAP ($4,500 for households with a member aged 60 or up or who is disabled).

are made, be under 130% of the federal poverty level. The net income test requires that the household's income after deductions be below 100% of the federal poverty level.

After eligibility is determined, benefit amounts are determined based on household size and income. Benefits increase in household size, and decrease in income at a rate of $0.30 per extra dollar of income. This decrease from income is deducted from the maximum allotment for the given household size. The maximum allotment is based on what the USDA called the Thrifty Food Plan (TFP), which is the lowest cost food plan which provides a nutritious diet according to the USDA. After benefits are determined, households are assigned a recertification date. Just as the household had to certify they were eligible for SNAP when initially applying, they must regularly recertify that they are still eligible for SNAP in order to maintain benefits, and to adjust benefit amounts based on any changes in the household's income, size, etc. These recertification periods typically lie anywhere between three and twelve months, though in the mountain-plains state are almost always six months for working-age households. Aside from means testing and recertifications, individuals may also have to satisfy work requirements in order to receive benefits.

There are two types of work requirements: general work requirements, and ABAWD work requirements, which this paper focuses on. ABAWD work requirements apply to adults aged 18-49 who have no dependents nor disabilities. For the sake of being considered an ABAWD, having a dependent means having someone in the household who is under 18 years old.[3] If considered an ABAWD without some other exemption (pregnancy, in school or a training program for at least half-time, etc.), then an individual must satisfy the ABAWD work requirements in order to receive SNAP benefits. The requirements can be met by working at least 80 hours per month, which can be paid or volunteer. Individuals can also participate in a work program for at least 80 hours a month, or a combination of work and a work program for at least 80 hours a month. ABAWDs are allowed 3 months within a 36 month period where they do not have to satisfy work requirements, however, which is known as the ABAWD time limit.

[3]With the recent passage of the One Big Beautiful Bill Act (OBBBA), ABAWD work requirements have expanded to a broader range of people, including those up to 65 years old, and those with dependents as young as 14.

These requirements must be met on top of what are known as general work requirements, which are much lighter. The general work requirements only mandate that the individual register for work, accept a suitable job if offered, participate in SNAP Employment and Training (E&T) if assigned, and not voluntarily reduce working hours below 30 hours per week without reason. The general work requirements apply to those aged 16-59 who are not in school, do not have a child under 6 years of age, and do not have a disability preventing them from working. Exemption from the general work requirements also exempts someone from the ABAWD work requirements. This paper focuses on ABAWD work requirements and so uses the term work requirements to refer to them as opposed to the general work requirements.

2.2 Work Requirements During and After the Great Recession

Work requirements in the mountain-plains state were waived across the entire state at the outset of the Great Recession (as they were across most of the country at this time as part of the American Recovery and Reinvestment Act (ARRA) of 2009). The ARRA initially suspended work requirements from April 2009 to September of 2010 due to high unemployment across the country. After that, states were able to apply for continued waiver status in parts of the state or the entire state depending on economic conditions. These waivers would typically last for periods of one year. The mountain-plains state continued to apply and receive work requirements waivers until the last application for a statewide waiver was sent to the USDA in March of 2011 which extended the waivers one final time state-wide from October 1, 2011 to September 30, 2012. This means that upon this final waiver request expiring, work requirements were reintroduced state-wide at the end of September of 2012 (effectively the beginning of October 2012). Because the reinstatement of work requirements relies on an expiration of a waiver rather than an active decision to reinstate work requirements, it is difficult to conclude concretely when people became aware that work requirements would come back into effect at the end of September 2012. The two extensions which the mountain-plains state petitioned for were filed in February 2010 and March 2011 respectively, and so it may have been clear

that the state would not apply for a new waiver by the time March 2012 had passed as this would have broken the pattern of previous waiver applications.

Because of the ABAWD time-limit, I define treatment as taking effect in January of 2013 to take into consideration that even at the outset of work requirements, ABAWDs would be able to take advantage of the 3 months they are allotted to receive SNAP without having to fulfill the work requirements. The definition of ABAWDs means that upon the reintroduction of work requirements in the mountain-plains state, only able-bodied adults without dependents who are between the ages of 18 and 49 are subject to them. Thus, those who are at least 50 years of age, or who have children under the age of 18 living with them are unaffected by the reintroduction of work requirements. This fact forms the basis of the identification strategy, using the timing of the reintroduction of work requirements along with the fact that only certain groups are subject to them to identify the effects of work requirements on program participation and employment.

2.3 Related Literature

Despite the long history of work requirements for SNAP, researchers have only recently began to evaluate their effects on labor supply. This growing literature generally exploits two sources of variation in exposure to work requirements. The first source of variation used is geographic by time variation introduced by work requirement waivers. Based on economic conditions, states may apply for waivers for parts of the state or the entire state which exempt ABAWDs from having to meet the work requirements in order to receive benefits. Using the variation in time and geography from these waivers in a difference-in-differences framework, Harris et al. (2020) evaluates the effect of work requirements on program participation and employment using data from the American Community Survey (ACS). He finds that work requirements both reduce program participation and increase employment. This result is robust to the use of a triple-differences strategy, where the third difference uses the fact that those above the age of 50 are not subject to work requirements regardless if they live in an area with

a waiver or not, while those under 50 are. This triple difference strategy also yields negative estimates for program participation and positive estimates for employment.

Han (2022) uses a very similar strategy, relying on ACS data with a similar triple difference approach to study the introduction of work requirement waivers (the removal of work requirements as opposed to the introduction). A key difference is that Han (2022) uses data on additional waivers which states can grant to ABAWDs to exempt them from work requirements.[4] He finds that work requirement waivers increase participation, but have no effect on employment. However, he does find modest negative effects on hours worked.

This use of the age 50 threshold in these two papers brings us to the second set of papers which rely instead on regression discontinuities. Ritter (2018) and Cuffey et al. (2022) both use Current Population Survey (CPS) data and a regression discontinuity to find the effects of aging out of work requirements at age 50. Ritter (2018) finds no effect on employment while Cuffey et al. (2022) find that aging out of work requirements decrease employment on the intensive and extensive margin, at least for low income and low education adults. This difference in findings despite similar data sources and strategies may be attributed to the different selection criteria made by each set of researchers, particularly with regards to the states included.[5] Cuffey et al. (2022) use data from most states while Ritter (2018) uses data from 25-29 states depending on the sample period based on his sample selection criteria.

The final set of papers which use the age 50 threshold in a regression discontinuity are Stacy et al. (2018) and Gray et al. (2023). Both of these papers primarily use some form of restricted data. Stacy et al. (2018) uses ACS restricted use data linked to administrative SNAP data from 9 states, while Gray et al. (2023) use administrative SNAP data from Virginia linked to UI data. Stacy et al. (2018) use a difference-in-discontinuities, leveraging the age 50 threshold along with waiver and non-waiver counties to control for other factors which may affect SNAP participants at 50. Gray et al. (2023) use a donut regression discontinuity

[4] States are allowed to exempt an 15% of ABAWDs which live in areas not under a waiver from the work requirements.

[5] Cuffey et al. (2022) find heterogeneous effects of aging out of work requirements across groups of states depending on state policies.

to capture effects only for ABAWDs who were fully exposed to work requirements as Virginia reinstated them in a staggered fashion based on the recertification months of the ABAWDs. Both papers find decreases in SNAP participation as a result of work requirements, and neither concludes that work requirements increase employment. Gray et al. (2023) do find suggestive evidence that work requirements increase earnings, however, for those who are potentially close to the threshold for hours necessary to fulfill the requirements.

One potential reason this paper finds positive employment effects when Stacy et al. (2018) and Gray et al. (2023) do not is because this paper shows the effects of work requirements on individuals who have been participating in SNAP for long periods of time (at least two years) while the previous literature does not. Gray et al. (2023) do show estimates for individuals who have been on SNAP for at least 18 months and have been exposed to work requirements for an additional 6 months, and they estimate positive effects on earnings around ranges they estimate would be needed in order to satisfy the work requirements. It is also worth noting that in their RD estimates for employment, the effects they estimate after 18 months of participating in SNAP are often large and at times statistically significant in some specifications, even larger that what I estimate at times.[6] These long-term estimates are not necessarily comparable to the estimates I present here because I estimate effects for those who have been on SNAP for at least 2 years in some samples, while they estimate effects for individuals who have been on SNAP for at least 18 months and have then been exposed to work requirements for some time further than that. This is only to say that the previous literature has not focused on SNAP participants who have been on SNAP for as long of periods as the long-term sample I define in section 3, and so may have missed a population where work requirements increase employment. I discuss this reason in more depth later in the paper.

Overall, the literature is in agreement that work requirements reduce program participation. Effects on employment outcomes are mixed however, with conclusions ranging from no effect on employment, to only effects on the intensive margin, to positive effects on employ-

[6] See figure C.13 in their appendix.

ment. It should also be noted that the results do not follow identification strategies closely. That is to say, similar identification strategies at times yield different results. It also seems to be the case that the choice of data and regions covered could affect results. This variation emphasizes the importance of building upon the literature with a high quality dataset which has not been used to study ABAWD work requirements thus far combined with a strong empirical design which can yield results that are generalizable to a wider age range.

3 Data

The data come from a single unidentified state in the mountain-plains region. The data consist of administrative SNAP data which allows me to observe applicants and beneficiaries along with benefit amounts at the monthly level between 2011 and 2020. The data also contain demographic information for members of the household. Linked to these data is employment information from the state's UI database. These data are reported at the quarterly level and contain information on whether the head of the household is employed, the industry of employment, along with the wages earned in the quarter. Although these data only cover the head of the household and for jobs which are covered by UI, they yield employment information for household heads who have merely applied for or received SNAP at some point. That is, I can observe labor supply outcomes regardless of whether the head or household is currently on SNAP. This feature allows me to track heads of households after and even before they participate in SNAP.

I restrict the data to households that have never reported any type of federal exemption from work requirements, reported a disability, reported the head was in school, or that had a characteristic that would automatically qualify them for SNAP such as participation in Temporary Assistance for Needy Families (TANF). I also exclude anyone who would have changed ABAWD status during the time frame studied. For example, an individual who is 49 years of age just before the reintroduction of work requirements would age out of ABAWD status within

just a year, so this individual would be removed from the data. Since the number of people removed from the data increases with the scope of the post-treatment period, I limit the primary employment estimates to only 8 quarters after the reintroduction of work requirements so as to not reduce the sample too heavily.[7]

To be a part of the sample, households must have been receiving benefits at some point before the work requirements were reintroduced. My primary sample dictates that a household must have been receiving benefits at some point on or between January 2011 and February 2012. January 2011 is the beginning of the sample, and February 2012 is about 6 months before work requirements are reintroduced. This gap between the end of the eligible sample period and the reintroduction of work requirements is to ensure that the estimates are not affected by households who join close to the reintroduction (and so with the knowledge that work requirements will soon be reintroduced), which may be different than other households. Table 1 shows summary statistics for each of the cohorts broken down by the ABAWD and non-ABAWD population.

To examine heterogeneity by length of time spent on SNAP, I also define two other cohorts. The first cohort are "long-term" SNAP users, who I observe as participating in SNAP *at least as of* January 2011. Because the data begin in January 2011, many of these SNAP users were actually receiving benefits before January 2011, and so I call them long-term users. Though I cannot precisely determine when each of the long-term users began receiving benefits, due to the data starting in January 2011, I impute their starting months using a procedure described in appendix A. The second cohort of "short-term" SNAP users were observed to *start* receiving SNAP benefits on or between February 2011 and February 2012. Because these households joined SNAP only after January 2011, the beginning of the sample, I determine that they have been on SNAP for a shorter period of time leading up to treatment.[8] As table 1 shows, the short-

[7] For SNAP outcomes, I limit the estimates to only use 10 months after treatment for the sake of more manageable event study plots, and because treatment effects appear to be fully realized by this point.

[8] It is possible that some of these short-term users were actually on SNAP at some point before 2011, and only rejoined after January 2011. This would still mean that their current SNAP spell is shorter than the long-term users, however.

term sample has an average start month of July 2011 for both ABAWDs and non-ABAWDs, while the long-term sample has an average start month of May and April of 2010 for ABAWDs and non-ABAWDs respectively, approximately 14 more months of SNAP participation. Only about 7.6% of individuals in the long-term sample began their first observable SNAP spell in January 2011, with all others beginning earlier. The primary sample is the combination of the short-term and long-term cohorts.

One important note is that, because households are allowed to stop participation in SNAP at various points in time, they may not still be receiving SNAP benefits by the time of treatment. Figures 1-3 show participation in SNAP as a rate by the ABAWD and non-ABAWD populations for the three cohorts. Because these plots follow participation of the chosen cohorts, new participants are not allowed to join the sample after February 2012 (time -11 on the graphs) for the Feb 2011 - Feb 2012 and Jan 2011 - Feb 2012 cohorts, and new participants do not join the January 2011 cohort at all. This explains the gradual decline in participation over time as people do not recertify (perhaps due to the costs of recertification or due to becoming ineligible) and lose benefits. These plots also show descriptive evidence of the work requirements lowering participation among ABAWDs. In figure 3 for example, we see that participation rates for ABAWDs and non-ABAWDS generally follow parallel and linear trends until January 2013 (the first month work requirements are fully implemented) where a noticeable dip in ABAWD participation can be observed. The non-ABAWDs, however, seem unaffected, continuing on their gradual linear decline.

To provide evidence for the external validity of the data, I compare the primary sample of interest to households from the SNAP Quality Control (QC) system. The SNAP QC data contains information on SNAP households from a nationally representative sample. Table 2 compares my sample to a sample of households from the SNAP QC from the same time frame. Apart from lower proportion of black head of households, my sample of interest is largely similar to the nationally representative sample from SNAP QC.

4 Empirical Strategy

To identify the effects of work requirements on program participation and employment outcomes, I exploit the timing of reintroduction of work requirements along with the fact that only ABAWDs are subject to them. This setting makes for a natural use of a difference-in-differences estimator. I use non-ABAWDs, those who are either at the age of 50 or above or have children in the household (or potentially both) who are not subject to the work requirements as a comparison group for the ABAWDs. Formally, one could estimate the following equation:

$$Y_{it} = \beta ABAWD_i * Post_t + \alpha_i + \tau_t + \epsilon_{it} \tag{1}$$

where Y_{it} denotes the outcome of interest for household i at time t.[9] $ABAWD_i$ is a binary variable indicating whether the head of the household would be considered an ABAWD or not. $Post_t$ is a binary variable indicating whether work requirements have taken effect yet or not. α_i and τ_t are household and time period fixed effects respectively. I provide estimates using this TWFE specification, however, for my preferred estimation method I implement the doubly robust difference-in-differences (DRDID) estimator proposed by Sant'Anna and Zhao (2020). This estimator allows for some advantages over a more traditional two-way fixed-effects (TWFE) specification.

While the data contain rich demographic information on households, the variables available are generally time-invariant and so would be collinear with the household fixed effects included in a normal TWFE specification as in equation (1). The DRDID estimator allows researchers to use time-invariant pre-treatment covariates by combining the outcome regression approach proposed by Heckman et al. (1997) and the inverse propensity weighting (IPW) estimators proposed by Abadie (2005). The DRDID estimator improves on the outcome regression and IPW approaches by requiring only one of the functional forms for either approach to be correctly specified in order to identify the average treatment effect on the treated (ATT). This

[9] Because the SNAP data is at the monthly level and the employment data is at the quarterly level, t may refer to either months or quarters depending on the outcome.

estimator thus allows me to include various household characteristics in my estimation.[10] This inclusion helps create better balance between treatment and control groups, and this may be important as previous research has found heterogeneous effects of work requirements and different participation patterns in different types of people. Harris (2021), for example, finds larger employment effects for blacks than for whites as well as larger employment effects for people living in urban versus rural counties. Argibay et al. (2024) finds that immigrants eligible for SNAP participate in the program at lower rates compared to non-immigrants. I also observe a large difference in the proportion of head of households who are male as opposed to female between the ABAWD and non-ABAWD households as seen in table 1, and so include gender as a variable in the DRDID process. Conditioning on these covariates may improve balance between the treated and comparison groups.

Relatedly, the DRDID estimator strengthens the validity of the parallel trends assumption which is central to DID estimators. It does so by only requiring that the parallel trends assumption is not violated conditional on covariates. I provide event studies in order to provide evidence for this conditional parallel trends assumption. The other identifying assumptions for the DRDID estimator are that the data are identically and independently distributed, and that for every value of the covariates included, there is at least a small probability of non-treated units.

As a third approach intended as a robustness check, I implement the DRDID approach on data limited to households with no children and in which the head of the household is between 40 and 55 years old at the start of the data. This strategy removes households with children entirely, meaning the resulting estimates will not rely on whether or not such households make for good comparisons to ABAWD households with no children. This approach will also be more comparable to previous research which often makes use of the age 50 threshold for aging out of work requirements. In all regressions I cluster standard errors at the household level.

[10] I include race, ethnicity, gender, citizenship, county, and baseline employment as variables used in the DRDID estimates.

5 Results

5.1 Program Participation and Benefit Levels

Table 3 shows the effects of reintroducing the work requirements on SNAP participation. For the broadest sample in the middle column, I estimate a decrease in participation of 21.2% after dividing the estimated coefficient by the baseline mean of SNAP participation. These estimates are somewhat similar across the different samples, but grow larger from the short-term users (18.5% decrease in participation) to the long-term users (23.3% decrease in participation). Figure 4 shows the corresponding event studies for each of the cohorts, and displays evidence towards parallel trends between the treatment and control groups prior to treatment. In general, I estimate much larger declines in participation than most of the previous literature.

Using the log of benefits levels plus one as the outcome, table 4 shows estimates for the impact of work requirements on SNAP benefit levels. Here I see similar patterns, where SNAP benefit levels drop across all three samples, but the magnitude of the estimates grow from the short-term users (a decrease of 29.1%) to the long-term users (a decrease of 40.7%). The largest cohort from January 2011 to February 2012 experiences a drop in benefit levels of 35.7%. Event studies for these estimates are shown in figure 5 and mirror the event studies of the extensive margin. The extensive and intensive margin results suggest that most of the effects on the intensive margin can be explained by households ceasing participation rather than continuing participation and receiving fewer benefits.[11]

5.2 Employment and Earnings

Table 5 shows the effects of reintroducing work requirements on employment. For the broadest sample in the middle column, I estimate a 6% increase in employment, after dividing the estimated coefficient by the baseline mean of employment. Short-term users experience essentially no effect (a statistically insignificant 0.3% increase) while the long-term users ex-

[11] If the case was the latter, we would expect to see small or no effects on program participation.

perience a much larger 11.1% increase. Figure 6 shows event studies for each of the cohorts to show that trends before treatment are generally parallel between treatment and control groups.

Table 6 shows analogous effects of work requirements on earnings. Once again, short-term users experience a small and statistically insignificant increase in earnings, while long-term users experience a statistically significant 26.5% increase in earnings. The mixed sample including both types of users experiences a 17.5% increase in earnings. Figure 7 shows the corresponding event study which closely mirrors the employment event study in figure 6 and once again shows support for the parallel trends assumption holding. The extensive and intensive margin employment results suggest that most of the increases in earnings can be explained by becoming employed at all as opposed to increasing earnings in an attempt to meet the 20 hour per week minimum which the work requirements mandate to participate in SNAP.[12]

5.3 Young ABAWDs

One of the primary advantages of my approach that builds upon previous literature is the generalizability of my results to ages away from 50. Most of the previous literature on this topic makes use of the age-50 threshold in work requirements and in doing so limit identification to ages around 50. While my primary estimates make no such restriction, I further show that the effects I find are applicable to young ABAWDs as well. Tables 7 and 8 show the effects of work requirements on program participation and employment for ABAWDs who begin in my sample at the age of 30 or below. The control group is kept the same and includes all non-ABAWDs found in my primary estimates. Figures 8 and 9 show the corresponding event studies.

The patterns shown in these results are similar to the results from the more general sample in that young ABAWDs also see declines in program participation, increases in employment, and effects which are stronger for long-term users of SNAP. Effects on participation are some-

[12] If the only increases in earnings were due to working more hours to meet the minimum eligibility threshold, we would not expect to see positive effects on the extensive margin for employment.

what smaller than in the more general sample, while effects on employment are larger. This is potentially evidence that young ABAWDs are more likely to increase their employment in order to maintain benefits, but the differences for young ABAWDs and the more general group are not so large that this conclusion is clear. Later I show estimates for households without children where the head of the household is aged between 40 and 55 at the start of my sample which show larger declines in participation and smaller gains in employment. This is perhaps further evidence that younger ABAWDs are more likely to increase their labor supply to comply with work requirements and maintain eligibility than older ABAWDs. More generally, larger employment effects for young ABAWDs show that previous literature focusing on individuals aged around 50 may not be able to detect effects on employment because they are smaller for this group and the methods which limit data to ages around 50 may not be sufficiently powered. The results for household heads aged 40-55 are discussed further in section 6.1.

5.4 Heterogeneity by Gender

Table 8 shows the effects of work requirements on employment by gender of the head of household. The table yields two interesting results. First, the pattern of employment effects becoming more positive the more long-term the sample becomes remains true for both men and women. Second, women have stronger employment responses in the short-term sample than men (3.8% increase vs a statistically insignificant 1.1% decrease), whereas the opposite becomes true in the long-term sample (6.5% increase vs a 11.8% increase). It is possible that while both male and female long-term users experience stronger effects of the policy, the factors which drive this difference between long and short-term users may be more prominent in men. I discuss this further in the discussion section.

The event studies across all three cohorts can be found in figures 10 through 12, and collectively provide evidence that the conditional parallel trends assumption is not violated for neither men nor women. One note is that for the January 2011 cohort, both men and women appear to have positive effects on employment, however, the effect for women seems to be

more delayed than for men. This may partially explain the smaller coefficient estimated for women in this sample, though even after the delay the effect for women does not appear to grow as large as for men until about five quarters after treatment.

6 Robustness

6.1 Heads Aged 40-55

One potential concern is the inclusion of households with children in the control group. If households with children are not comparable to households without children in their evolution of the outcomes examined, my estimates may be misleading. While the parallel trends shown in the relevant event studies provide evidence that the set of control households are comparable, at least conditional on covariates, to the treated households, I offer an additional method to address this concern. I estimate program participation and employment effects for households without children whose head is aged between 40 and 55 at the beginning of the sample (January 2011). This still excludes any heads who would age into work requirements during any point of the period of examination (so up through two years of treatment), along with all other exclusion criteria discussed in the data section. In this specification, all households are childless, and ABAWD status is determined by the head of household being below 50 years old. This exploits the fact that work requirements would not apply to those above the age of 50 as is common in the literature. I continue to use the doubly robust methods used in the main results presented earlier on this sample.

The results are qualitatively similar to what I estimate using the primary sample which includes households with children in the control group. Event studies for program participation and employment can be found in figures 13 and 14. They both largely mirror the event studies in the primary samples, providing evidence that the conditional parallel trends assumption is not violated, while showing negative effects for program participation and positive effects for employment. Tables 10 and 11 show the corresponding doubly robust difference in difference

estimates for these samples. Effects on program participation appear to be even larger in this sample compared to the main estimates. Compared to the main results in table 5, effects on employment are larger for the short-term users (though statistically insignificant, in part due to the substantially smaller sample size) while the effects for long-term users are somewhat smaller. The effects on the largest sample, combining short and long-term users, are the same between both the preferred results in table 5 and the results limiting to childless households with heads around age 50. Still, for both program participation and employment, long-term users are again affected more by the policy change than short-term users. Overall, these results provide further evidence that the effects I estimate in my preferred specifications/samples are not driven by unobserved differences between households with children and those without.

One important note is that the estimates for program participation in this group are large, while the estimates for employment are somewhat smaller, especially in comparison to the effects estimated for young ABAWDs in section 5.3. Because so much of the previous literature focuses on individuals aged around 50, this further demonstrates the value in the methods presented here which generalize to a broader age range. Methods such as regression discontinuities which examine individuals aged around 50 may have sufficient statistical power to detect the large program participation effects, but not enough to detect the more modest employment effects for this group. For example, Ritter (2018) and Gray et al. (2023) both using RDs around age 50 estimate employment effects in multiple samples/specifications which are as large if not larger than the effects I estimate for the 40-55 group. These estimates are unfortunately noisy, however, and so the authors do not take them as evidence that work requirements increase employment.

6.2 Traditional Difference-in-Differences

In addition to the doubly robust methods, I also present estimates using a simple two-way fixed-effects difference-in-differences framework without any type of weighting strategy. That is, I estimate equation (1) using OLS. This specification does not include the demographic

controls used in the DRDID estimates however, due to their collinearity with the fixed-effects. I estimate this equation for both employment and program participation to compare to the doubly robust estimates. The event studies for program participation are found in figure 15, and the resulting estimates from the difference-in-differences are found in table 12. Both are qualitatively similar to the results found using the preferred doubly robust methods. The coefficient for the short-term users is roughly equal, though the coefficient for the long-term users is somewhat smaller (-0.1048 for DRDID vs -0.0712 for TWFE). The event studies for both estimation methods are also similar, showing deeper declines in participation for the long-term group than for the short-term group.

For employment, I also find qualitatively similar results. Figure 16 shows the event study for effects on employment, while table 13 shows the resulting difference-in-difference estimates. The event studies for employment have larger and statistically significant estimates in the pre-treatement period for some samples, suggesting that the covariates used in the DRDID estimates provide value in creating better matches between the treated and control groups. Regardless, the TWFE and DRDID estimators have very similar estimates for the long-term group's employment outcomes (0.024 and 0.0267 respectively). The short-term users however have larger and statistically significant estimates for employment (0.015 for TWFE vs a statistically insignificant estimated coefficient of 0.0011 for DRDID). Despite the significantly larger coefficient in the TWFE estimates, the long-term users still yield an effect nearly twice as large as the short-term users. In sum, the previous results show that negative participation effects, positive employment effects, and larger magnitudes of estimated coefficients for the long-term cohort are present whether I use traditional TWFE or my preferred DRDID estimator.

7 Estimating Types of SNAP Participants

In the interest of determining if work requirements lead to employment to replace SNAP, or if work requirements only lead to employment to remain eligible for benefits, researchers may

naturally ask what drives the increases in employment and earnings I estimate. For policymakers this is an important question because if work requirements only increase employment to remain eligible for SNAP then they indeed increase employment and earned income but do not entirely lead to self-sufficiency. I attempt to explain if the increases in employment and earnings I observe are due to participants leaving the program and working to compensate for the loss of income, or if these increases are better explained by participants working more in order to try to remain eligible for participation. Instead of conditioning on the endogenous outcome of having remained on SNAP after work requirements have been in effect and using the DRDID approach on this sample, I follow a similar approach to Gray et al. (2023), who refer to the two aforementioned effects as income and incentive effects respectively. To avoid conditioning on an endogenous outcome, I categorizing SNAP participants into different categories which should respond to work requirements in different ways and then estimate the effects of work requirements using the DRDID on the different samples.

There are three behavior categories that SNAP users can fall into. The first category, always-leavers, are participants who would have left the program regardless of the work requirement regime. The second category, induced-leavers, participate when work requirements are not in place, but leave when they are in place. The final category, never-leavers, are those who would remain in the program regardless of work requirements. One could place SNAP participants in the never-leavers category by determining if they stayed on SNAP in the presence of work requirements, but again, this involves conditioning on an endogenous outcome. Furthermore, the induced-leavers cannot be simply identified as those who left the program after work requirements came into place, as this could also be the behavior of an always-leaver. To distinguish between the three categories, I use a two-step algorithm using two LASSO regressions to predict which of the three behavior categories SNAP participants fall into.

The first step in the algorithm is to train a LASSO model on non-ABAWDs with 10-fold cross validation using an indicator for whether the household was no longer participating in

the program in June 2013.[13] By using non-ABAWDs in this model, I identify features (and estimate their corresponding coefficients) which are predictive of leaving the program despite not being subject to the work requirements.[14] The idea is that if a LASSO is trained to identify features that lead to program exit for individuals *not* subject to the work requirements (non-ABAWDs), then these features could be predictive of exiting the program regardless of the work requirement regime (and so identify always-leavers). Using these predictive features, I predict the probability of leaving SNAP for *all* households (non-ABAWDs and ABAWDs) using their own characteristics.[15] This predicted probability of exit can be interpreted as a probability of being an always-leaver, as it was produced using a model which should predict exit among those who do not have reason to leave the program due to work requirements. Once these probabilities are calculated, a cutoff is determined by finding the minimum estimated probability such that the percent of participants with a probability above that score would equal the empirical exit rate I observe among non-ABAWDs.[16] Households with a predicted probability above or equal to the cutoff are then determined as always-leavers. I am then left with ABAWDs and non-ABAWDs who are considered always-leavers.

The second step in the algorithm is to predict those who are induced-leavers (and by process of elimination, never-leavers). At the beginning of this step, I remove anyone in the sample who was identified as an always-leaver in the first step. This means that for both ABAWDs and non-ABAWDs, only induced-leavers and never-leavers should remain, though they are yet to be

[13] June 2013 is chosen as it appears to be the point where the treatment effects become fully realized in the program participation event studies. I grid search over values of λ between 0.0005 and 0.1 in increments of 0.0005 to find the lambda which minimizes prediction error.

[14] The features I use in training the LASSO are indicators for female, US citizenship, Hispanic status, race, county, age of the head of household, age of the youngest in the household, county, the first month observed to participate in SNAP, months on SNAP since January 2011, household size, lags of unearned income in the two years before treatment, dependent care expenses in the last two years before treatment, total earnings within four years before treatment, lags of earnings in the 16 quarters before treatment, lags of number of jobs in each quarter and the single-digit NAICS code of the primary job worked.

[15] I do this using five folds, running the tuned LASSO on 80% of the sample and use the selected features to predict values for the 20% of the sample remaining. Each of the non-ABAWDs is thus given a single score, whereas the ABAWDs are given 5 predictions and I take the average of these to compare to the cutoff.

[16] For example, if I empirically observe that 40% of non-ABAWDs are no longer participating in the program in June 2013, then I choose a cutoff C such that the percent of individuals with exit probabilities above C would equal 40%.

identified. I separate them by running another LASSO similar to the first using participants who are ABAWDs (and who are not predicted to be always-leavers) as the training set. The logic in this step is that if an individual is an ABAWD who left SNAP after the reinstatement of work requirements despite not being an always-leaver, then the LASSO can identify features which are predictive of exiting the program due to work requirements and not just natural attrition. These features are again used to generate a probability of leaving SNAP for all individuals and a cutoff to determine leavers. Because the only two groups of people remaining are those who would be induced to leave and those who would never leave, those who are above the cutoff in this step are determined as induced-leavers, while those below are determined to be never-leavers.[17] This procedure leaves us with induced and never-leavers for both the ABAWDs and non-ABAWDs. To reiterate, though non-ABAWDs are not subject to work requirements, the algorithm is meant to identify non-ABAWD households who *would* have been induced to leave under work requirements had they been ABAWD households.

Once participants have been placed into one of the three categories, I estimate a doubly-robust DID on the induced-leavers and never-leavers to estimate the income and incentive effects. I estimate my preferred DRDID for the largest sample which includes participants from January 2011 to February 2012. Results for employment can be found in table 14. I see positive and significant effects for both induced and never leavers. The point estimate for never-leavers is larger than for induced leavers, though both are significantly larger than the employment estimates I show in column 2 of table 5. This difference is likely due to the fact that the always-leavers who should largely be unaffected by the policy have been removed from these samples, concentrating these samples with those who have incentives to increase their labor supply. The always leavers serve as a pseudo-placebo check, where I do not expect to see large effects on employment as these households are expected to leave SNAP regardless of the work requirement regime. Here, I see a statistically significant but small increase in employment. In the case of always-leavers, it is important to note that while the LASSO model

[17] I assume that there are no participants who would participate in SNAP under work requirements but would leave under no work requirements, similar to assuming there are no defiers in the potential outcomes framework.

may have high predictive power, it does not classify always-leavers with certainty, so there may be small numbers of true induced-leavers in the always-leavers category, which may drive the small positive estimates I observe there. Figure 17 puts these estimates in perspective and tests for parallel trends between the ABAWDs and non-ABAWDs among the three categories of participants. I see no evidence of a violation of the parallel trends assumption, and a stark difference in effect size between the three groups, with the induced and never-leavers showing strong positive effects.

Table 15 shows analogous estimates for earnings and reveals the same pattern as for employment. There are large earning gains for both induced and never-leavers, with larger gains for the never-leavers, which again are larger than the corresponding estimates in column 2 in table 6. Figure 18 shows the corresponding event study, which reveals similar patterns as shown for the employment event study in figure 17. This exercise reveals that there appear to be both income and incentive effects among SNAP participants, meaning that work requirements seem to increase employment to replace benefits lost from SNAP, and also increase employment in order to remain eligible for SNAP.

This exercise also sheds light on the difference in the short-term and long-term samples. Table 16 shows that the short-term sample is composed of a higher percent of always-leavers than the long-term sample (76% vs 62%) and also has a lower percent of never-leavers (6% vs 13%) and induced-leavers (17% vs 25%). Always-leavers can be thought of as more transitory participants, those who do not plan to be on the program long and so would not be affected by work requirements. This is essentially the type of participant I argue is more likely to be in the short-term sample. Never and induced-leavers on the other hand may have their work plans affected by the work requirements, and would plan to stay on the program indefinitely or as long as work requirements are waived respectively. These types are more common in the long-term sample. This provides further evidence that the LASSO sorting algorithm is correctly placing participants into their correct categories.

8 Marginal Value of Public Funds

In this section I estimate the marginal value of public funds (MVPF) of eliminating work requirements. MVPF in this context can be interpreted as the benefit eliminating work requirements yields to ABAWDs in dollars for each dollar in net cost to the government for doing so. The equation for MVPF can be written as:

$$MVPF = \frac{WTP}{C + FE} \tag{2}$$

where WTP denotes the ABAWD's willingness-to-pay for the elimination of work requirements, C is the direct cost of doing so to the government, and FE is the fiscal externality from doing so. The numerator captures the value of eliminating work requirements to ABAWDs, and the denominator captures the net cost to the government. Thus, an MVPF greater than 1 implies that a single dollar of government spending on eliminating work requirements benefits ABAWDs by more than one dollar, and an MVPF of less than 1 implies the opposite.

The numerator is straightforward to calculate with the estimates already provided. I assume that the willingness to pay to eliminate work requirements is equal to the change in SNAP benefit amounts which would be caused by this policy. Using the largest sample in column 2 of table 4, I estimate that the reintroduction of work requirements leads to an average decrease of $52.92 per recipient. Only 42% of the sample is still receiving benefits in the month prior to the reintroduction of work requirements, so I scale the average decrease by this amount to get a decrease of $129.07 per ABAWD. This assumes that ABAWDs value SNAP benefits dollar for dollar with cash. However, due to the limitations placed on what SNAP can purchase, this may not hold true. Whitmore (2002) for example estimates that food stamp recipients may only value one dollar of food stamps as much as $0.75 to $0.9 of cash in some samples. To account for this, I provide a range of WTP values using this range of values for how much SNAP benefits may be valued relative to cash.

The denominator for MVPF involves a few different values. The direct cost, C, can be

broken up into the cost of the benefits provided, and the cost of administering those benefits. The former is simply the amount calculated above, $129.07. For the latter, administrative costs reflect primarily costs for certification and recertification, but may also include other costs such as outreach. Eliminating work requirements would increase administrative costs through increasing caseloads and thus increasing the number of cases which need to be certified and recertified, along with increasing the number of households benefits need to be issued to. Data from the 2012 fiscal year show that federal certification and issuance costs for SNAP in the mountain-plains state are 68% of the total administrative costs paid by the federal government in the state (USDA, 2013). Since states and the federal government split administrative costs equally, I go forward under the assumption that the mountain-plains state also spends 68.32% of its administrative costs on certification and issuance costs. Using the cost per case per month provided from the same fiscal year, this leaves me with an estimated $22.35 per case per month increase in administrative costs for eliminating work requirements. However, it is possible that eliminating work requirements reduces the handling costs of each case as applications may be easier to approve or reject because work does not have to be verified. At present, I do not have a reliable way to estimate this, and so this cost per case per month should be considered an upper bound on the administrative costs of eliminating work requirements.

The second component of the denominator is the fiscal externality. This is the tax revenue lost due to the reduction of labor supply. Using the earnings estimates from the second column of table 6, I estimate that work requirements increased earnings by $\frac{0.1616*1209}{0.42} = \$465.18$ per quarter, or $155.06 per month. For an unmarried individual living in the mountain-plains state making the average amount of an employed person in my sample, the effective tax rate for 2013 is approximately 16.2%.[18] Thus, the amount lost in tax revenue from eliminating work requirements per ABAWD is $155.06*0.162 = $25.12.

Now that I have all of the necessary components, I calculate the MVPF of eliminating work requirements to be $\frac{129.07}{129.07+22.35+25.12} = 0.73$. This implies that for every dollar spent on elimi-

[18] I calculate that the average annual earnings for an employed individual in my sample after the work requirements are reintroduced is $22,320.62.

nating work requirements, ABAWDs receive about $0.73 in benefits. This value is lower than the MVPF of eliminating work requirements estimated by Gray et al. (2023), who estimate an MVPF ranging from 0.9 to 1.4. It is also lower than some MVPFs of different aspects of the SNAP program, such as application information and application information plus assistance which have estimated MVPFs of 0.89 and 0.92 respectively (Finkelstein and Notowidigdo, 2019). It is not surprising to find that a redistributive program like SNAP has an MVPF of below 1 however. Okun (1975) notes that redistribution can carry inefficiencies, leading to less value received by the targeted party than was spent. Using a cash-equivalent value of SNAP estimated by Whitmore (2002) of 0.82, the MVPF I calculate falls to 0.6. This cash equivalent value of SNAP was estimated for a specific subgroup however, and so may not be appropriate for the entire group of ABAWDs. That being said, there is reason to believe that individuals do not value SNAP and cash equivalently. For example, several papers estimate the marginal propensity to consume food (MPCF) out of SNAP to be significantly higher than for cash.[19] If ABAWDs do value SNAP benefits less than nominally equivalent amounts of cash, then the MVPF of eliminating work requirements is lower than 0.73, but finding an acceptable range of MVPFs relies on having accurate estimates for the cash-equivalent value of SNAP for ABAWDs. Without this, I default to my original estimate of 0.73 for the MVPF of eliminating work requirements.

9 Discussion

I find both decreases in program participation as well as increases in employment due to the reinstatement of work requirements. My estimates are larger than most of the literature on this topic, likely in part because the data do not include individuals who would not participate in SNAP regardless of the presence of work requirements, and so avoids a sample which is too broad. An outlier is Gray et al. (2023) which use similar administrative data and find large

[19] Hastings and Shapiro (2018) estimates a MPCF out of SNAP of 0.5 to 0.6 and an MPCF of no more than 0.1 for cash. Beatty and Tuttle (2015) estimate MPCFs of 0.53 to 0.64 for SNAP and 0.15 for cash. Song (2022) estimates the MPCF of SNAP to be 0.48 and rejects the fungibility of SNAP.

effects on program participation (a 38% reduction in participation) but no effects on employment on the extensive margin. My main results show declines in participation of 21.2% and increases in employment of 1.69 percentage points off of a baseline of 28%. My more moderate declines in participation may be explained in part by greater compliance with the work requirements. That is, individuals may work more to maintain benefits and so participation does not fall as severely. The results of the classification exercise in section 7 provide some evidence towards this conclusion.

This naturally raises the question of why my employment estimates are higher than those of Gray et al. (2023). One possible reason is that while they also construct a "stock" of participants to follow, this stock is quite different from my own stocks. They follow participants who have been on SNAP for at least 18 months, whereas my primary sample consists mostly of households which have been on SNAP for at least two years by the time work requirements are effectively reinstated.[20] Limiting to the sample which has participated in SNAP anywhere from 10 to 22 months (the short-term users) yields no employment effects in my estimates, but still negative effects for participation. This finding holds true even when examining the household heads aged 40-55, which are likely more comparable to their estimates as they both limit the sample to childless households where the head is around the age of 50. In this strategy, participation estimates for the short-term sample becomes much larger, with a decrease of 29.3%, much closer to their participation estimates. Given that the short-term sample using the aged 40-55 strategy is the closest to the main estimates of Gray et al. (2023), my results are perhaps not so different, but rather encompass a wider range of participants which reveal different effects. Considering the importance of length of time spent on SNAP shown here, it is possible that focusing on a sample with likely shorter SNAP tenure could lower estimated effects.[21] If

[20] About 58.2% of my primary sample consists of households who were participating in SNAP at least as of January 2011. In the month before treatment two years later, 62.3% of the sample left participating in SNAP is from the January 2011 cohort.

[21] Gray et al. (2023) do estimate effects of work requirements over longer periods of time in some analyses, which do show possible evidence of positive employment and/or earnings effects. While some of these effects may not be statistically significant, they at times estimate economically large effects, upwards of 3 to 4 percentage point increases in employment, compared to their control mean of 0.273. These estimates may not be exactly comparable to mine however, since their long-term estimates become longer than their baseline by how long work

policymakers are concerned about long-term dependence on SNAP and if work requirements can lower that dependence, then the analysis I present here is perhaps more appropriate in addressing these concerns and questions. This brings us to the next major finding of this paper.

That long-term users of SNAP tend to be more responsive in finding employment than short-term users of SNAP may reflect differences in their underlying labor supply plans. Short-term users may participate in SNAP temporarily, due to transitory shocks such as job or income loss or unexpected expenses. These individuals may generally intend to receive benefits only temporarily and plan to gain employment soon regardless of program rules. If this is the case, then the reintroduction of work requirements would do little to change their employment decisions. Long-term users however may be more likely to have incorporated SNAP benefits as a stable component of their income, essentially substituting earned income with program benefits. For this group, the reintroduction of work requirements presents a significant change in their budget constraint, reducing the value of remaining without employment by tying benefits to employment. These individuals thus face stronger incentives to gain employment in order to stabilize income.

There is descriptive evidence that long-term users of SNAP have planned for the program's benefits to be a stable part of their budget. Long-term users of SNAP tend to be less likely to be employed in the quarter before treatment than short-term users (24% vs 34%) and are more likely to be participating in SNAP in the month before treatment, despite having been on the program longer by then (45% vs 38%). The higher propensity to be on SNAP despite having spent more time participating is notable given the program's low retention rates.[22] An alternative explanation for this pattern is that long-term users are just those who face more difficult barriers to employment, and thus persist more in their participation in SNAP due to a lack of earned income. This is somewhat at odds with the strong employment effects found for this group however.

requirements have been in effects, whereas my long-term estimates are longer in the time that participants have been on SNAP by the time work requirements are reinstated.

[22] Gray (2019) finds that about half of entering cases leave the program in the first year of participation, despite about half of those who exit remaining eligible.

The evidence presented here thus suggests that work requirements may be effective at inducing at least long-term users of SNAP into employment, but they also impose costs on short-term users who may plan to leave the program regardless, but who are expelled from the program for not meeting the work requirements on time. Since an individual's expected duration of SNAP participation is unobservable at the time of enrollment, policymakers could instead alter the ABAWD time limit to screen for long-term users. In theory, there is an ABAWD time limit which achieves two goals: those with short expected durations will exit the program before the exemption period ends, while long-term users will self-select into the requirement by remaining on SNAP for the duration of the exemption. This revealed-preference sorting allows work requirements to target the population for which they are most effective while also avoiding unnecessary burdens on short-term users. Of course, this requires knowing the amount of time that short-term users expect to remain on the program so as to not set the ABAWD time limit to less than this. Given that the short-term users in my sample stay on SNAP typically longer than 3 months, it may be that this value is larger than the current 3 month time limit.

10 Conclusion

This paper has provided new evidence on the effects of work requirements on program participation and employment. Using administrative SNAP and UI data from an entire state, I show that SNAP work requirements both reduce program participation and increase employment. I also find that the effect of work requirements on program participation and employment seem to be larger for long-term users of SNAP versus short-term users. These findings are consistent with the idea that short-term users of SNAP are less affected by work requirements because they do not plan to be on SNAP for long in any case. As such, work requirements are unlikely to factor in to their program participation and labor plans. For long-term users of SNAP who may plan to be on the program for an extended period of time however, work requirements

pose a serious obstacle that may induce them into working when they otherwise may not have. This provides evidence that work requirements may be useful for increasing employment and earnings, and for lowering dependence on public assistance.

The results on program participation are qualitatively in line with previous literature, but my results on employment are contrasted by some. Most notable is Gray et al. (2023) which uses similar quality data from Virginia but with a donut regression discontinuity design. While they find suggestive evidence that SNAP work requirements increase hours worked by those close to meeting the 20 hour requirement, they find no effects on the extensive margin for employment. This difference in findings however, may be due to the likely longer duration my sample has spent on SNAP by the time work requirements are reinstituted. Another possible reason, mentioned by Gray et al. (2023) themselves, is that different states may have different reporting burdens or more stringent work verification than Virginia. The difference in results I find here highlights this possibility and raises the importance of studying this topic further using different data sources which may provide a more complete picture of work requirements across different states.

This work also has potential implications for redesigning the ABAWD time-limit scheme. If only long-term users of SNAP are induced into working from work requirements, then there may be cause to extend the ABAWD time-limit to beyond three months. My results suggest that individuals participating in SNAP for even longer time periods than three months do not increase their employment when subject to work requirements, but do drop in their participation in the program. This means that the ABAWD time-limit could potentially be increased without meaningfully disincentivizing individuals from working, at least relative to eliminating work requirements. Future research may be able to investigate this possibility further.

11 Tables

Table 1: Summary Statistics

Sample	Feb 2011 - Feb 2012		Jan 2011 - Feb 2012		Jan 2011	
Group	ABAWDs	Control	ABAWDs	Control	ABAWDs	Control
Proportion White	0.4	0.49	0.48	0.62	0.57	0.69
Proportion Male	0.72	0.33	0.71	0.29	0.7	0.27
Proportion US Citizen	0.98	0.96	0.98	0.96	0.98	0.97
Proportion Hispanic	0.06	0.09	0.08	0.1	0.09	0.11
Avg Age of Head	29.86	41.61	30.53	42.58	31.29	43.12
Proportion Employed in Base Period	0.34	0.34	0.31	0.27	0.27	0.22
Avg Earnings in Base Period	1117.18	1531.7	1088.5	1272.2	946.4	1034.8
Proportion on SNAP in Base Period	0.32	0.43	0.34	0.46	0.36	0.48
Mean Starting Month	2011-07	2011-07	2010-9	2010-12	2010-5	2010-04
Proportion of Sample	0.44	0.56	0.34	0.66	0.28	0.72

Statistics calculated from administrative UI and SNAP data from mountain-plains state.
*Mean Starting Month is predicted for the Long-Term sample using the methodology provided in the appendix. For the Short-Term sample, it is the mean starting month observed in the data. For the Primary sample, it is the average starting month using the observed starting months for the short-term users and the imputed starting months for the long-term users.

Table 2: Summary Statistics: National Quality Control vs State Administrative Data

Sample	SNAP Quality Control	Administrative Data
Mean Household Size	2.151	2.426
Proportion Households with Children	0.482	0.444
Mean Household Head Age	41.013	37.724
Proportion Household Head Male	0.309	0.436
Proportion Household Head Black	0.235	0.021
Proportion Household Head Asian	0.018	0.013
Proportion Household Head US Citizen	0.855	0.964
Proportion Household Head Employed	0.249	0.263

The first column uses SNAP Quality Control data from households from January 2011 to February 2012. We use the weights provided by the Quality Control Data. Administrative data from the Mountain-Plains State is from households observed to be on SNAP at some point in the same time range.

Table 3: Effects on SNAP Participation DRDID

	SNAP Participation Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	-0.0703***	-0.089***	-0.1048***
	(0.0071)	(0.0046)	(0.0063)
Outcome Mean 1 Qtr Prior	0.38	0.42	0.45
Observations	475,358	1,138,462	663,104
Household fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. The dependent variable is an indicator for whether the household is participating in SNAP or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4: Effects on Benefit Levels DRDID

	Log Benefits + 1		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	-0.3439***	-0.441***	-0.5229***
	(0.0383)	(0.0237)	(0.0329)
Outcome Mean 1 Qtr Prior	104	120	131
Observations	475,358	1,138,462	663,104
Household fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. The dependent variable is the log transformed amount of benefits plus one for the household. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5: Effects on Employment DRDID

	Employment Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	0.0011	0.0169***	0.0267***
	(0.0048)	(0.0029)	(0.0039)
Outcome Mean 1 Qtr Prior	0.34	0.28	0.24
Observations	255,962	613,018	357,056
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. The dependent variable is an indicator for whether the head of the household is employed or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6: Effects on Earnings DRDID

	Log Wage Income + 1		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	0.0367	0.1616***	0.2351***
	(0.0425)	(0.0246)	(0.0331)
Outcome Mean 1 Qtr Prior	1486	1209	1010
Observations	255,962	613,018	357,056
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. The dependent variable is the log transformed amount of quarterly earnings plus one. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7: Effects on SNAP Participation DRDID (Young ABAWDs)

	SNAP Participation Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	-0.0616***	-0.0733***	-0.082***
	(0.0074)	(0.0053)	(0.0075)
Outcome Mean 1 Qtr Prior	0.39	0.42	0.45
Observations	398,450	979,706	581,256
Household fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. Observations are limited to those where the ABAWD heads of household are at most 30 years of age, with no further restrictions for the non-ABAWDs. The dependent variable is an indicator for whether the household is participating in SNAP or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 8: Effects on Employment DRDID (Young ABAWDs)

	Employment Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	0.0033	0.0213***	0.0328***
	(0.0055)	(0.0037)	(0.0053)
Outcome Mean 1 Qtr Prior	0.34	0.28	0.24
Observations	214,550	527,534	312,984
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. Observations are limited to those where the ABAWD heads of household are at most 30 years of age, with no further restrictions for the non-ABAWDs. The dependent variable is an indicator for whether the head of the household is employed or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 9: Effects on Employment by Gender DRDID

	Employment Binary					
Sample	Feb 2011 - Feb 2012		Jan 2011 - Feb 2012		Jan 2011	
	Men	Women	Men	Women	Men	Women
ABAWD X Post	-0.0038	0.0127**	0.0174***	0.0147***	0.0306***	0.0142**
	(0.0061)	(0.0068)	(0.0042)	(0.0044)	(0.0053)	(0.006)
Outcome Mean 1 Qtr Prior	0.35	0.33	0.31	0.26	0.26	0.22
Observations	128,198	127,764	267,400	345,618	139,202	217,854
Household fixed effects	✓	✓	✓	✓	✓	✓
Quarter fixed effects	✓	✓	✓	✓	✓	✓

Each column uses a different cohort as defined in the data section, split between male and female heads of household. The dependent variable is an indicator for whether the head of the household is employed or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 10: Effects on SNAP Participation DRDID (Heads Aged 40-55)

	SNAP Participation Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	-0.1259***	-0.1577***	-0.1849***
	(0.0166)	(0.011)	(0.0138)
Outcome Mean 1 Qtr Prior	0.43	0.47	0.5
Observations	68,146	159,354	92,208
Household fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. Households are limited to those without children, and where the head of the household was between 40 and 55 years of age in January 2011. The dependent variable is an indicator for whether the household is participating in SNAP or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11: Effects on Employment DRDID (Heads Aged 40-55)

	Employment Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	0.0134	0.0169**	0.017**
	(0.0112)	(0.0066)	(0.0077)
Outcome Mean 1 Qtr Prior	0.34	0.28	0.24
Observations	36,694	85,806	49,112
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. Households are limited to those without children, and where the head of the household was between 40 and 55 years of age in January 2011. The dependent variable is an indicator for whether the head of the household is employed or not. Estimates are from a doubly-robust difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 12: Effects on SNAP Participation TWFE

	SNAP Participation Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	-0.06668***	-0.0756***	-0.0712***
	(0.0049)	(0.0033)	(0.0047)
Outcome Mean 1 Qtr Prior	0.38	0.42	0.45
Observations	475,358	1,138,462	663,104
R^2	0.4	0.48	0.53
Within R^2	0.0019	0.0025	0.0022
Household fixed effects	✓	✓	✓
Month fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. The dependent variable is an indicator for whether the household is participating in SNAP or not. Estimates are from a two-way fixed-effects difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 13: Effects on Employment TWFE

	Employment Binary		
Sample	Feb 2011 - Feb 2012	Jan 2011 - Feb 2012	Jan 2011
ABAWD X Post	0.015***	0.02***	0.024***
	(0.0042)	(0.0027)	(0.0036)
Outcome Mean 1 Qtr Prior	0.33	0.28	0.24
Observations	255,962	613,018	357,056
R^2	0.642916	0.678514	0.703254
Within R^2	0.0001832	0.000372	0.000563
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different cohort as defined in the data section. The dependent variable is an indicator for whether the head of the household is employed or not. Estimates are from a two-way fixed-effects difference-in-differences procedure. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 14: Effects on Employment by Participant Category DRDID

	Employment Binary		
Sample	Induced-Leavers	Never-Leavers	Always-Leavers
ABAWD X Post	0.0378***	0.0535***	0.0075**
	(0.0072)	(0.0114)	(0.0038)
Outcome Mean 1 Qtr Prior	0.13	0.33	0.32
Observations	131,712	63,616	417,690
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different participant category as defined in section 7. The dependent variable is an indicator for whether the head of the household is employed or not. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 15: Effects on Earnings by Participant Category DRDID

	Log Earnings + 1		
Sample	Induced-Leavers	Never-Leavers	Always-Leavers
ABAWD X Post	0.3223***	0.4791***	0.0935***
	(0.0534)	(0.0901)	(0.0326)
Outcome Mean 1 Qtr Prior	263	983	1542
Observations	131,712	63,616	417,690
Household fixed effects	✓	✓	✓
Quarter fixed effects	✓	✓	✓

Each column uses a different participant category as defined in section 7. The dependent variable is the log transformed amount of quarterly earnings plus one. Standard errors are clustered at the household level. *Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 16: Distribution of Participant Categories

Sample	Short-Term Users	Long-Term Users
Proportion Always-Leavers	0.76	0.62
Proportion Induced-Leavers	0.17	0.25
Proportion Never Leavers	0.06	0.13

Each column shows the proportion of households which are sorted into each participant category following the LASSO algorithm explained in section 7, broken up by the February 2011 - February 2012 cohort (short-term users) and the January 2011 cohort (long-term users). Columns may not add up to 1 due to rounding.

12 Figures

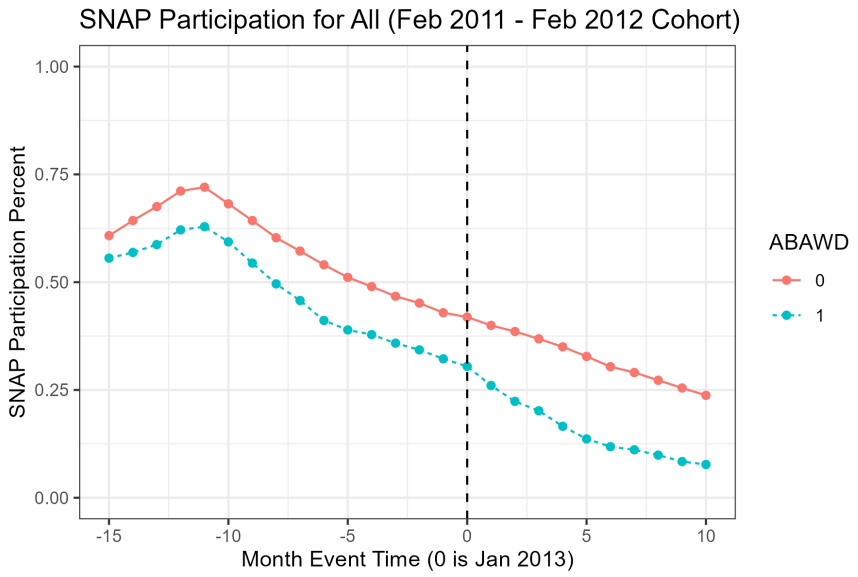


Figure 1: Participation in SNAP February 2011 - February 2012 Cohort

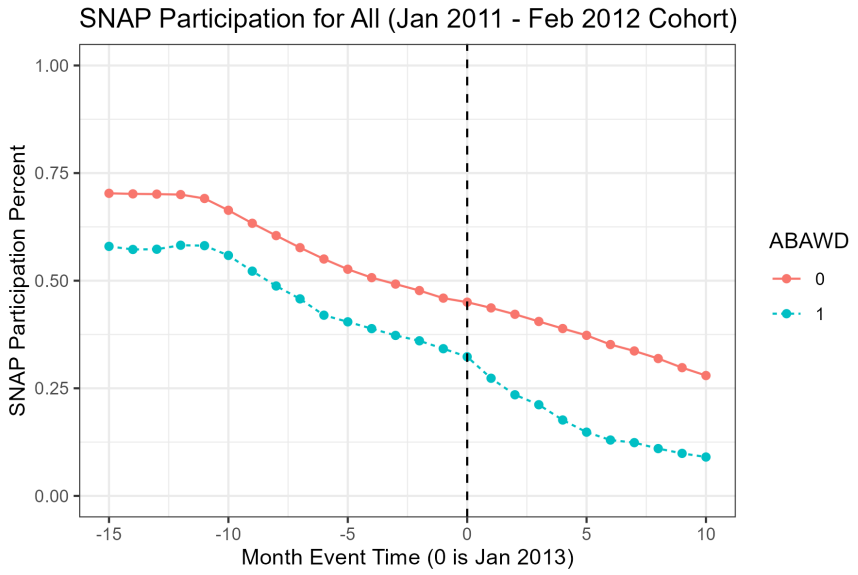


Figure 2: Participation in SNAP January 2011 - February 2012 Cohort

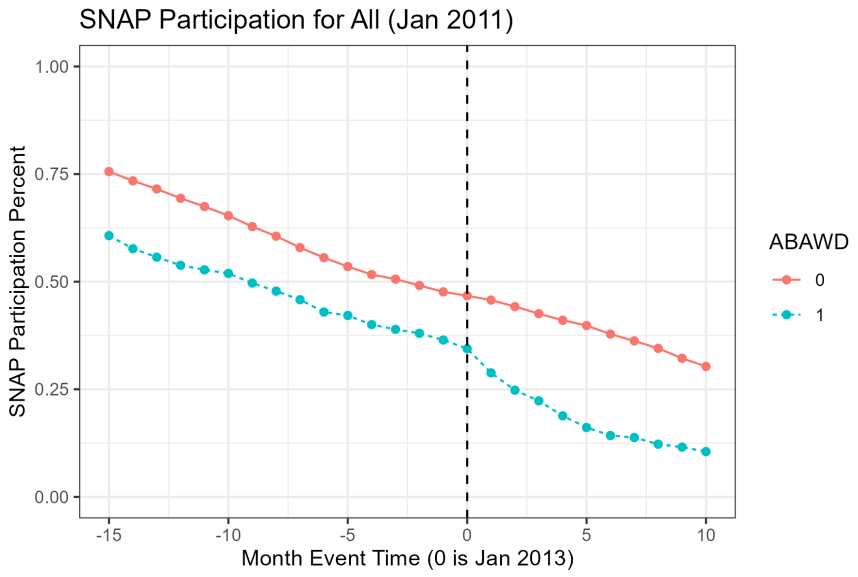


Figure 3: Participation in SNAP January 2011 Cohort

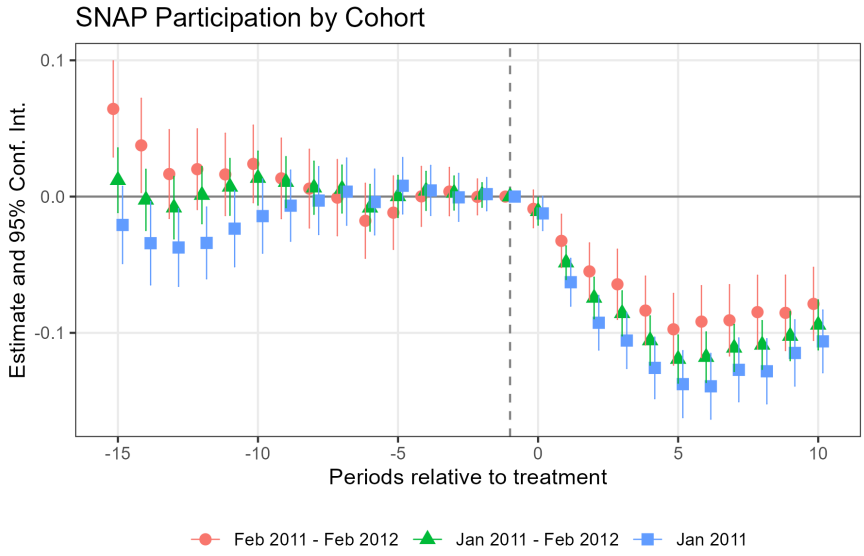


Figure 4

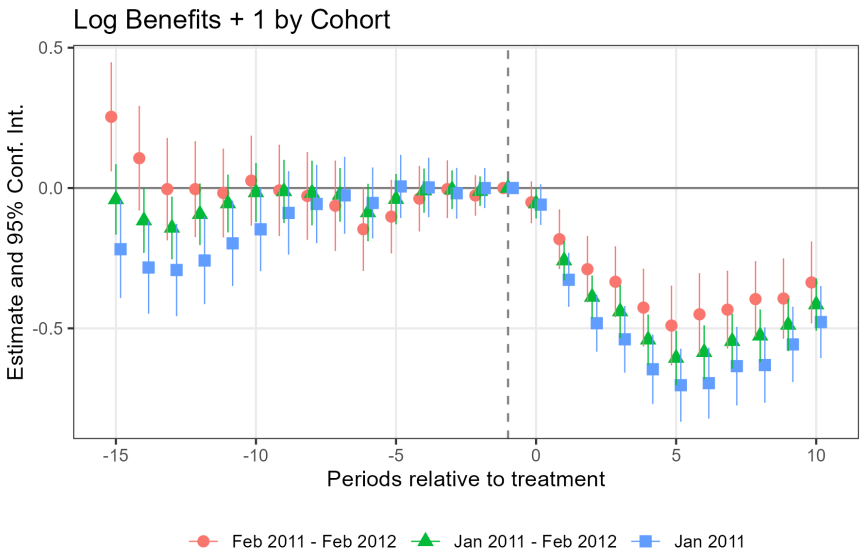


Figure 5

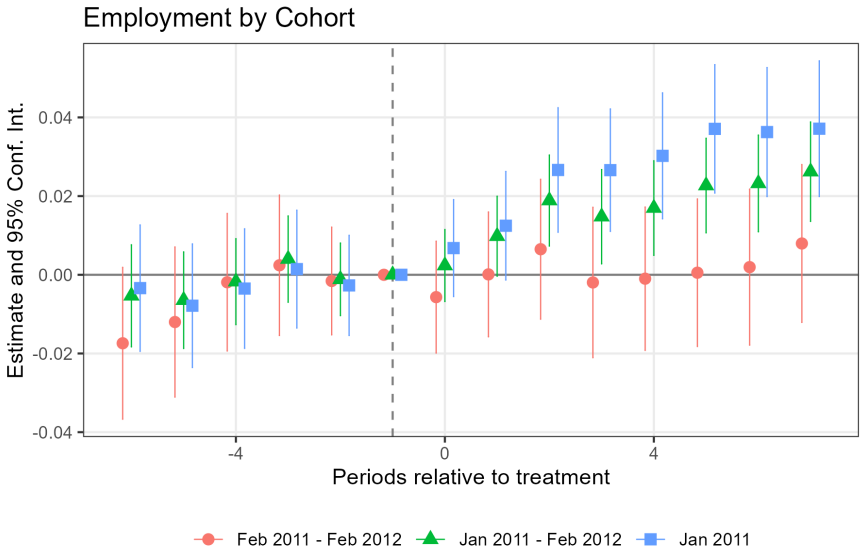


Figure 6

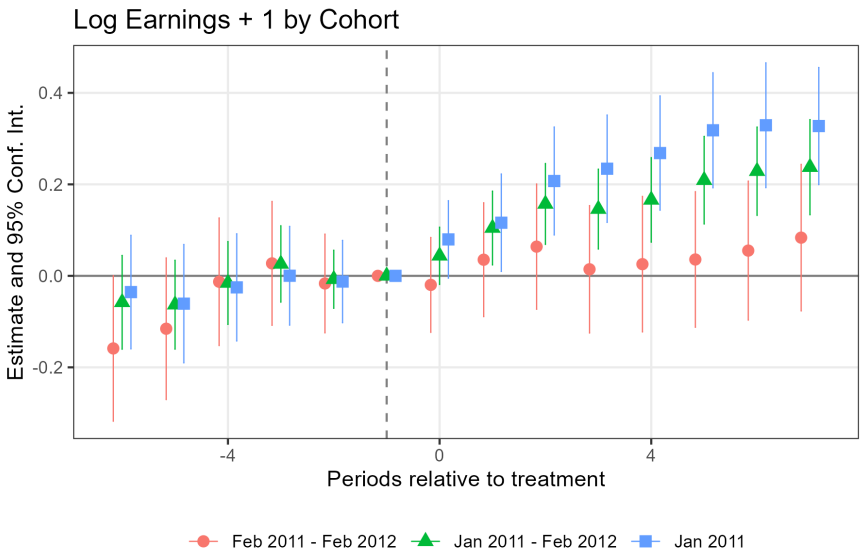


Figure 7

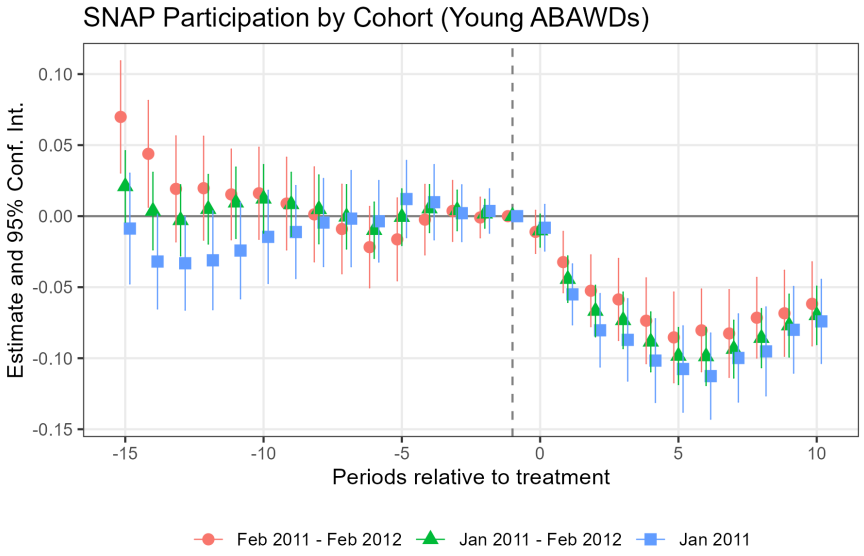


Figure 8

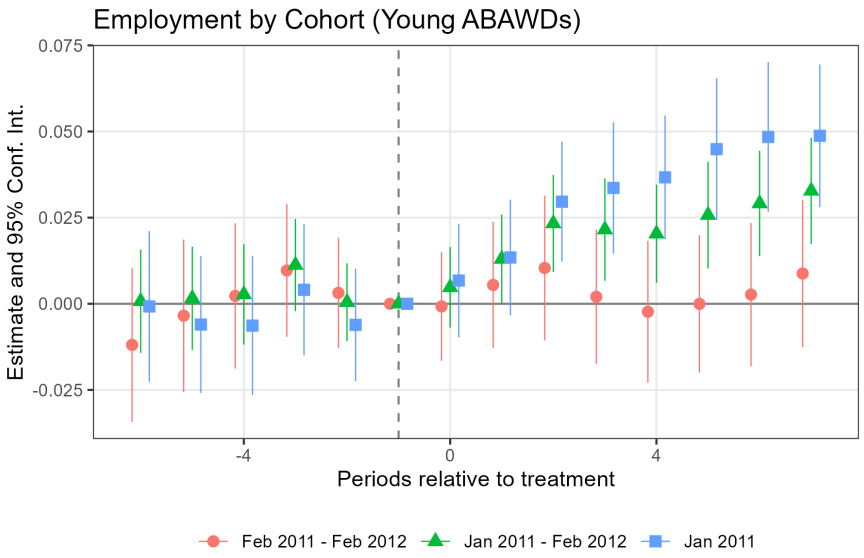


Figure 9

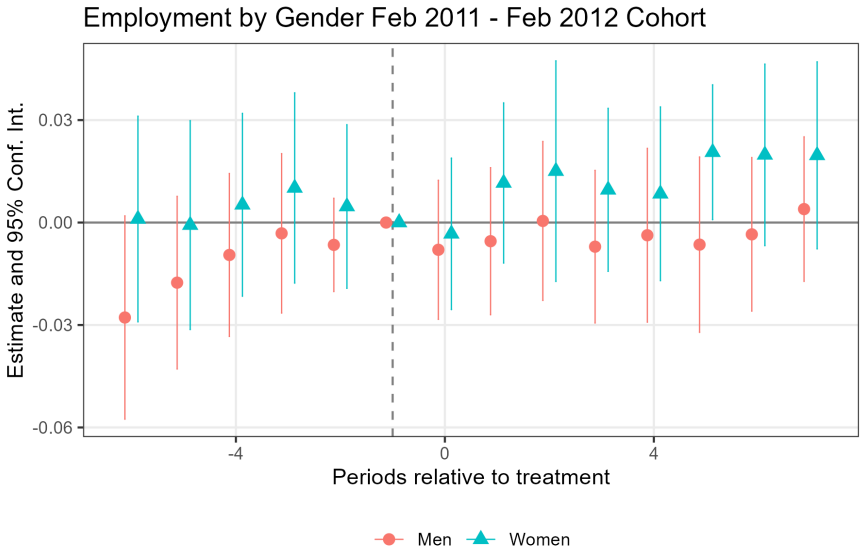


Figure 10

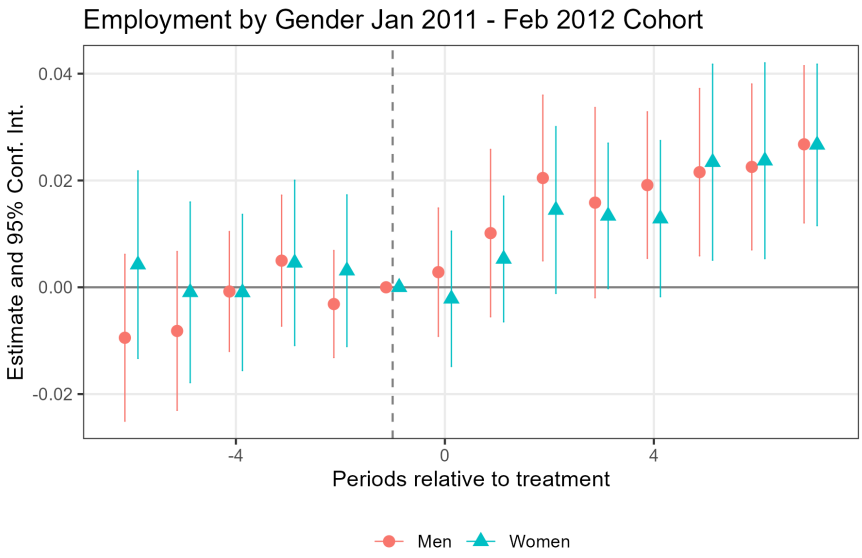


Figure 11

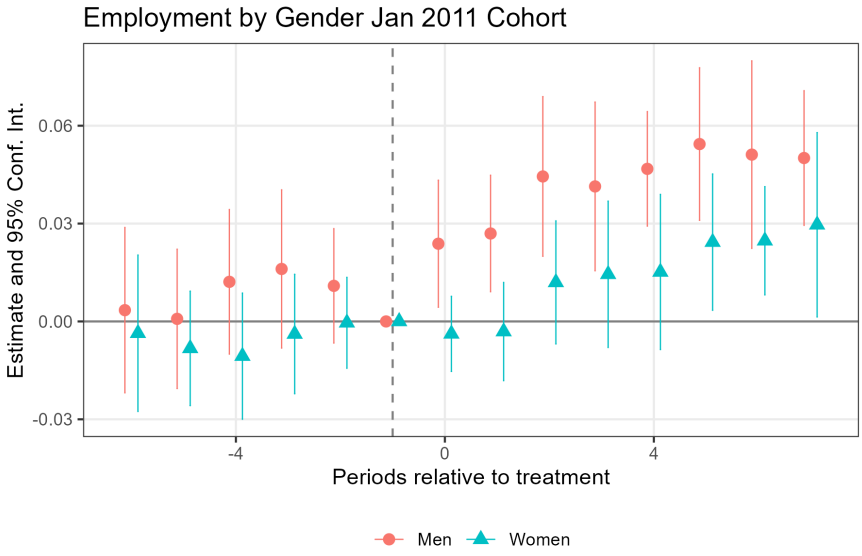


Figure 12

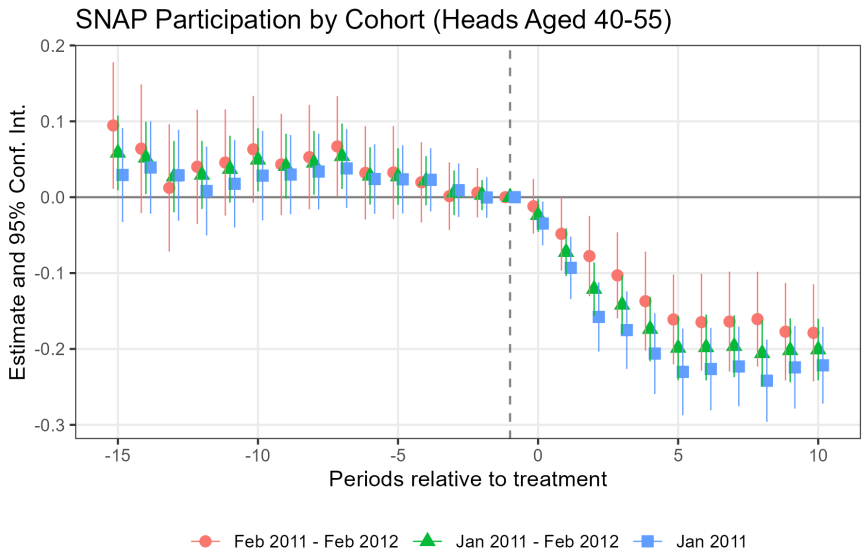


Figure 13

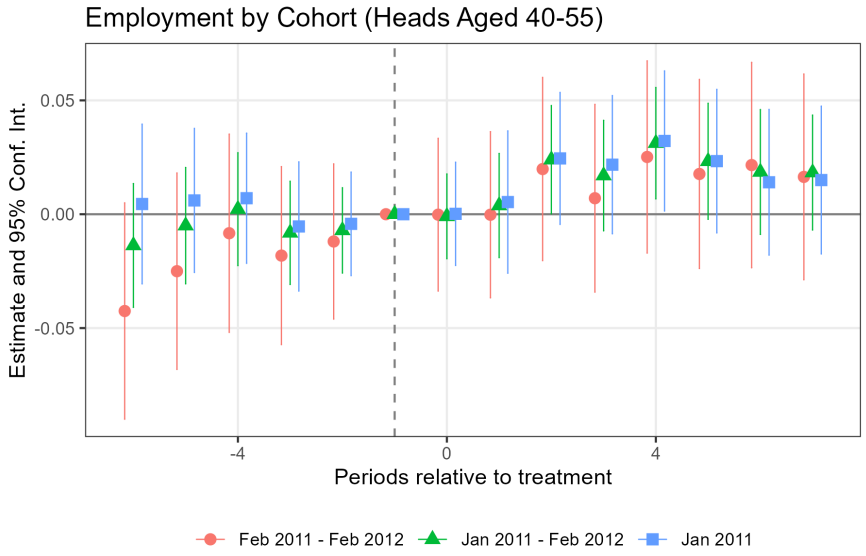


Figure 14

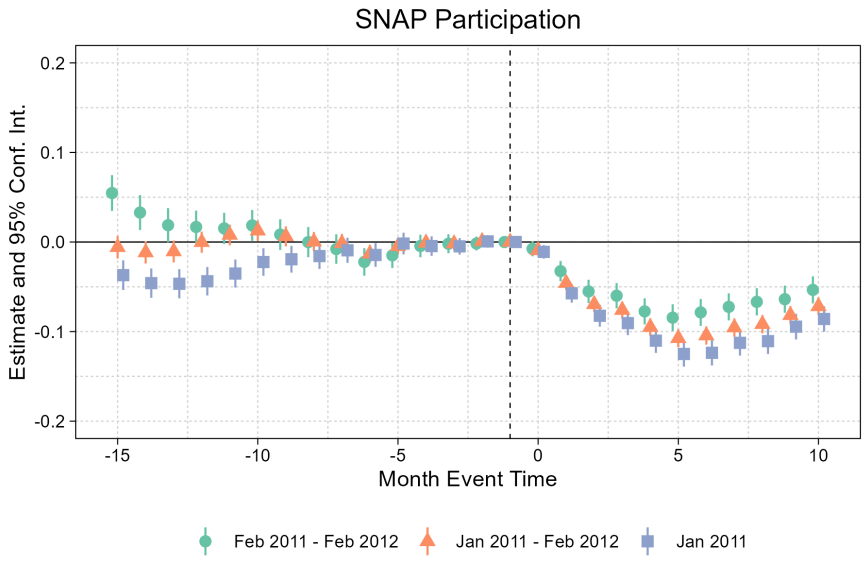


Figure 15: SNAP Participation Event Studies

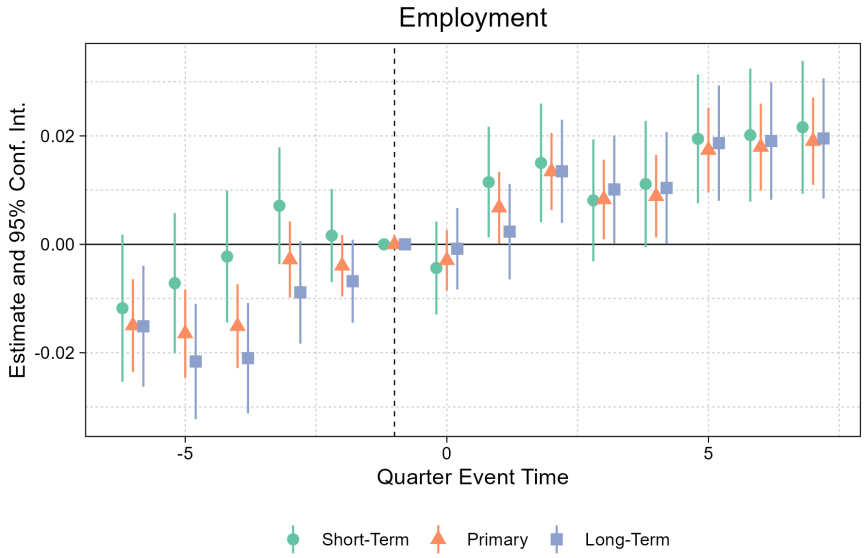


Figure 16: Employment Event Studies

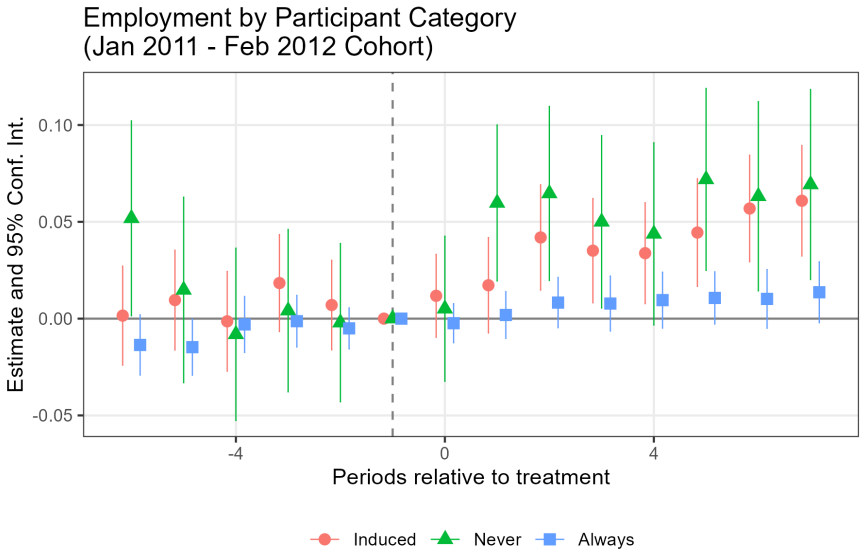


Figure 17: Employment by Participant Category Event Studies

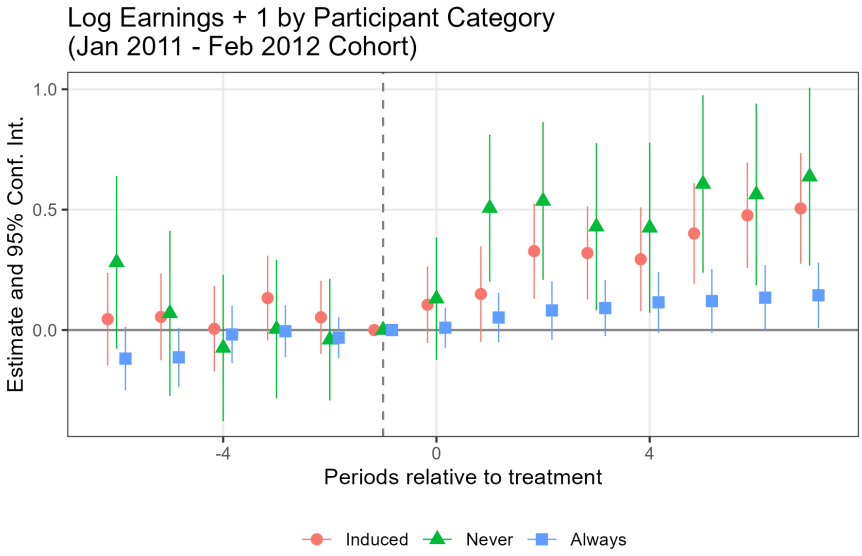


Figure 18: Earnings by Participant Category Event Studies

A Estimating SNAP Spell Lengths

Because my data start in January of 2011, I cannot observe how long participants who are observed on SNAP in this month have actually been participating in SNAP. To estimate when the long-term users began their SNAP spell, I estimate the survival rates of SNAP participation using the short-term users' participation history and use these rates to predict when long-term users began their SNAP spell. Formally, the stock of participants who I observe participating in SNAP in January 2011 can be written as:

$$Cohort_{Long-Term} = \sum_{k=0}^{\infty} f_{Jan2011-k} S(k) \tag{3}$$

where $Cohort_{Long-Term}$ is the number of households I observe participating in SNAP in January 2011, $f_{Jan2011-k}$ is the number of households that began participating in SNAP k months before January 2011, and $S(k)$ is the probability of remaining on SNAP for k months. $S(k)$ is estimated using the Kaplan-Meier estimator. Leaving SNAP is defined as the first time a participant exits the program for more than three months. This 3 month grace window is to allow for cases where households may exit SNAP inadvertently (for example, due to missing a recertification deadline) but intend to continue participating in SNAP.

The probability distribution of months that the long-term users start in can thus be expressed as:

$$P(K = k) = \frac{S(k)}{\sum_{j=0}^{K_m} S(j)} \tag{4}$$

where $P(K = k)$ is the probability that a household started participating in SNAP k months before January 2011, and K_m is the number of survival rates I estimate which is given by the number of months available from the short-term users. I do not use data from beyond when work requirements are reintroduced as survival rates likely differ beyond that point. This also means that rather than an infinite sum as shown in equation 3, the number of households in the long-term sample is assumed to be represented by a finite sum with K_m being the max-

imum value possible, meaning that all households in the long-term sample are assumed to have started participating in SNAP within K_m months of January 2011. This assumption will mechanically shorten the estimated SNAP spell length of these households as any households which began participating in SNAP before January 2011 - K_m would be at best predicted to have started in January 2011 - K_m. As such these estimated participation lengths should be seen as lower bounds.

Once the survival rates are calculated using the short-term sample, $P(K = k)$ is calculated and households in the long-term sample are assigned an imputed start month based on $P(K = k)$. Long-term users are assigned a starting month of K months before January 2011 where K is sampled from $P(K = k)$. A histogram of the imputed start months is shown in figure A.1.

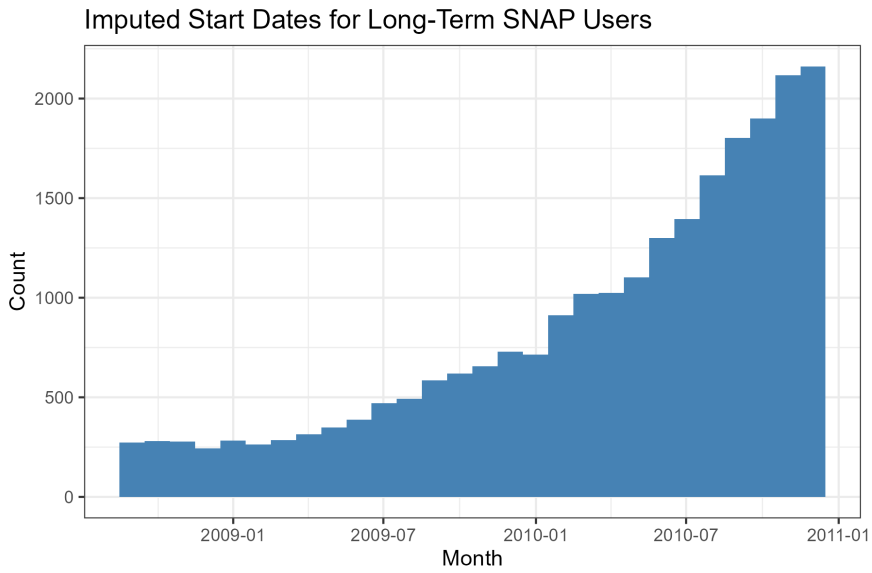


Figure A.1: Imputed Start Months for Long-Term SNAP Users

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