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Jason Cook
Chloe N. East

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740 15th St. NW 8th Floor
Washington DC 20005
202-545-6002

The Effect of Means-Tested Transfers on Work: Evidence from Quasi-Randomly Assigned SNAP Caseworkers*

Jason Cook, University of Utah, IZA, and CESifo
Chloe N. East, University of Colorado Denver, IZA, and NBER

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Abstract

We provide a comprehensive evaluation of the dynamic labor supply effects of the Supplemental Nutrition Assistance Program (SNAP) for a representative population using novel administrative data and an examiner design. We find no effects of SNAP receipt on the full sample of working-aged SNAP applicants. This is because the majority of working-aged applicants do not work before applying and experience no change in work if granted SNAP, consistent with this group facing barriers to work. The minority who work before applying appear to treat SNAP as insurance against negative shocks; they decrease work temporarily but work more in the longer-run.

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1 Introduction

The Supplemental Nutrition Assistance Program (SNAP) is a key part of the safety net in the United States. It serves 41 million recipients monthly and is the only universal means-tested transfer program (Moffitt, 2002). Proponents of SNAP argue it provides crucial resources to those in need, while critics claim it disincentivizes work and leads to long-term "dependency" on government benefits. The canonical static labor supply model where individuals trade off consumption and leisure predicts that access to SNAP will reduce work (Hoynes and Schanzenbach, 2015) and past empirical research suggests SNAP has at most a small negative labor supply effect. However, due to the lack of exploitable variation in SNAP, these papers either study the program in the 1960-70s (Hoynes and Schanzenbach, 2012), or study the effects on very specific sub-populations–immigrants, who represent 12% of SNAP recipients (East, 2016), or those subject to work requirements, who represent 5% of SNAP recipients (e.g. Stacy et al., 2018; Gray et al., 2022).

In this paper, we provide a comprehensive evaluation of the dynamic effects of the modern SNAP program for a large, generalizable group using an examiner design. This design has been used in other contexts where quasi-experimental variation is hard to find, such as the criminal justice system, Disability Insurance receipt, and foster care placement (e.g. Dobbie et al., 2018; Norris et al., 2021; Agan et al., 2023; Maestas et al., 2013; Autor et al., 2019; Doyle Jr, 2007) and we are the first to bring it to the literature on means-tested transfer program receipt in the U.S.[1] Specifically, we take advantage of quasi-random assignment of new SNAP applicants to caseworkers and variation in caseworkers' application acceptance rate. Beyond allowing us to look at how SNAP affects labor supply, studying how caseworkers impact take-up of SNAP is important in its own right and contributes to the literature investigating the causes of incomplete take-up of transfer programs and the ability of programs to target the neediest recipients (Nichols and Zeckhauser, 1982; Currie, 2006; Herd and Moynihan, 2019; Finkelstein and Notowidigdo, 2019).

Our main sample is new SNAP applicants, most of whom are working-aged, between 2012-2016. We link this sample to Unemployment Insurance (UI) earnings records, and, importantly, we see applicants' earnings whether or not they receive SNAP. Moreover, this data allows us to observe work behavior *prior* to SNAP application, which was not possible in earlier studies. With this longitudinal data, we estimate event-study-style models up to one year before application–to verify the random assignment of caseworkers–and three years

[1] Other papers have used safety net caseworker assignment to look at placement into different types of benefits among recipients, but not benefit receipt (e.g., Bolhaar et al., 2020; Jonassen, 2013; Autor and Houseman, 2010).

after application–to study dynamic effects. Crucially for thinking about labor supply effects, we also document that only 25% of SNAP working-aged applicants had strong attachment to the labor market *prior* to applying, and we split the analyses by pre-application labor market attachment.

Caseworkers are randomly assigned to applicants conditional on the timing and observable characteristics of the application. Following the examiner design approach in Kolesár (2013), we construct the Conditional Caseworker Approval Rate (CCAR), which measures the likelihood of each caseworker to accept a random application. We verify conditional random assignment by showing that the CCAR (as well as caseworker's caseload and experience) is unrelated to applicant observable characteristics conditional on fixed effects. Then, we document a strong effect of the CCAR on SNAP receipt–a one standard deviation increase in the CCAR increases the likelihood of approval at application by 1 percentage point, 2% off the baseline approval rate of 52%. This is an increase in the likelihood of ever receiving SNAP and not a shift in timing of receipt. Importantly, applicants who marginally receive SNAP because of their caseworker are similar to all SNAP applicants in our state, so caseworkers do not impact program targeting and the results can be plausibly generalized.

Investigating mechanisms, we provide context for why caseworkers are unlikely to directly affect applicants' take-up of other programs or labor supply. Additionally, caseworkers are closely monitored and have no discretion over program eligibility rules, so helping applicants navigate the complex application process is likely the main way caseworkers influence applicants' outcomes.[2] In fact, 37% of new applicants do not complete the application process and this is the most common reason for application denial. We find that a one standard deviation increase in the CCAR increases the likelihood of an applicant completing the process by 3%. Finally, we verify the CCAR satisfies the average monotonicity assumption needed to use it as an instrument for SNAP receipt (Frandsen et al., 2023)–that for each applicant there is a positive correlation between their potential treatment status and caseworkers' CCAR across all caseworkers.

Unlike the prior literature that focused on specialized subgroups or historical events, we find there is no labor supply response to SNAP on the full sample of working-aged adults. We take advantage of the richness of our data to show that this is driven by the fact that most SNAP applicants do not work even before applying for SNAP, which suggests other barriers to entering the labor market. These barriers are likely at least in part demographic–among working-aged SNAP recipients, many are disabled, are caring for children and other

[2]We shadowed a caseworker and witnessed firsthand the scope for individual caseworker behavior to influence program participation.

dependents (Keith-Jennings and Chaudhry, 2018), or face discrimination in the labor market due to their race, ethnicity, or sex (Lang and Spitzer, 2020).

When we focus on the 25% of the sample with strong labor market attachment pre-application, we do find a brief dip in employment and earnings followed by a rebound. Three years after application, those granted SNAP have higher rates of employment and higher earnings than they would have had if they had been denied. Our data and approach allow us to show that applicants who were working before applying experience a negative shock around the time of application regardless of whether they receive SNAP.[3] Thus, the income (liquidity) effect–that allows recipients to maintain higher consumption and reduces the pressure to, for example, find a new job after layoff–likely plays an important role in the effect of SNAP on labor supply, in addition to any substitution effects (Chetty, 2008). This is particularly important for these households, who have very little private savings and are unlikely to face perfect credit and insurance markets.

Taken together, our analyses give a much more complete picture of the labor supply effects and their mechanisms than has been possible before. Additionally, our results shed light on what labor supply model best explains individuals' response to SNAP. Counter to the conventional wisdom, we find that a model with labor market frictions and credit constraints best fits our results. These models are common in the literature on Unemployment Insurance (e.g. Nekoei and Weber, 2017), but have been mostly absent from the discussion of means-tested transfer programs. From a policy perspective, our estimates suggest no net negative impact of SNAP on government revenues due to changes in labor supply, which has important implications for cost-benefit analyses of SNAP.

The rest of the paper proceeds as follows. Section 2 provides background on SNAP policy and our setting. Section 3 describes our data. Section 4 details our empirical strategy and section 5 presents the results on the role of caseworkers and the impact of SNAP on labor supply.

2 Policy Background

2.1 SNAP and Labor Supply

SNAP (formerly "Food Stamps") is a means-tested federal entitlement program, and states are responsible for determining eligibility and paying out benefits. In general, to qualify for SNAP, applicants must have gross income below 130 percent of the federal poverty level

[3]This is consistent with prior evidence on disruptive events prior to SNAP receipt (Leftin et al., 2014).

and net income after deductions below 100 percent of the federal poverty level. Households with zero and near zero income receive maximum SNAP benefits, which are a function of household size. By providing this benefit guarantee for low-income households, the canonical labor supply model predicts a decrease in labor supply due to the income effect. As a household's income increases, benefits are decreased by the benefit reduction rate.[4] This lowers the return to work for SNAP recipients, so the model predicts a decrease in labor supply due to the substitution effect. Benefits are paid out automatically each month on electronic benefits transfer (EBT) cards, which are used like a debit card for qualifying food purchases at SNAP-accepting stores. Within our sample of recipients, the average monthly benefit is $226 in 2012 dollars.

Since welfare reform in 1996, SNAP includes work requirements for able-bodied adults without dependents (ABAWDs). Generally, ABAWDs are between the ages of 18-49, report having no disabilities, are not pregnant, and do not take care of any dependents (e.g., children, people with disabilities, or the elderly). In our state, 4% of all recipients are subject to ABAWD work requirements.[5] Unfortunately, we cannot identify who is subject to work requirements at the time of application in our sample. However, previous research using high-quality administrative data has found that these work requirements do not affect work (Stacy et al., 2018; Gray et al., 2022). Caseworkers are trained not to counsel applicants on how to meet work requirements.

Many SNAP recipients are unlikely to work regardless of whether they receive SNAP because they are in demographic groups that have low labor force attachment generally (Keith-Jennings and Chaudhry, 2018). About half of SNAP recipients are children or elderly. Our sample is restricted to be mostly working-aged heads of household, however, among working-aged SNAP recipients, 20% are in a household with someone who has a disability (including the head themselves), 58% have children and 32% have children of pre-school-age.

[4]SNAP's benefit reduction rate is 30%; however, the actual benefit reduction rate as income increases varies by the types of deductions the household has and is very close to zero at low income levels (Bitler et al., 2021; Han, 2022). SNAP-allowable deductions include a 20 percent deduction for every dollar of earned income, as well as deductions for certain types of expenditures including costs for shelter, child care, and medical care. Households participating in multiple programs may have a more complicated benefit reduction rate. There are also asset tests and residency tests for non-citizens that vary by state and time.

[5]SNAP also includes General Work Requirements that focus more on tasks related to becoming employed. In our state, 16% of all recipients are subject to these work requirements and not subject to additional ABAWD requirements, but these requirements have little bite in practice. Additionally, it is worth noting the historical context of work requirements. These requirements were often selectively placed on Black recipients and were sometimes motivated by the idea that Black people were less likely to work than white people (Nunn et al., 2019), when in fact the desire to work more has been consistently higher among Black Americans than white Americans (Minoff, 2020).

2.2 SNAP Application Process and Caseworker Behavior

The application process must balance the goals of providing support for qualifying individuals and screening out ineligible individuals. In the U.S., the burden of proving eligibility is generally placed on the applicants (Herd and Moynihan, 2019). Applying for SNAP is complicated and time consuming. Individuals must first submit an application and supporting documentation before completing a required screening interview and then provide any missing information.[6] Two-thirds of SNAP administrative costs–which is about 5% of total SNAP spending–are spent on caseworkers and case management.

SNAP applications require information on household composition, many different income sources, and financial and property assets. An example of the application form is in Appendix Figure A1. These applications can be submitted online, in person, or via mail, but in our state almost all are submitted online. Some of the fields on the application form are verified automatically against administrative records (e.g. earnings are verified against UI earnings data and vehicle ownership is verified against DMV records for asset tests). However, applicants must provide supporting documentation for many other components of their application such as rent or mortgage payments, letters from their employers, bank statements, utility bills, child or elder care bills, and child support payments. It is common for applicants to not fill in all the fields on the application form and/or to not submit all the required supporting documentation on initial application submissions. Applicants have 30 days to submit all the necessary information or their application is automatically denied. However, they have 60 days after the initial submission to go back and finish the application process without having to start from the beginning with a new application. Additionally, after individuals submit their initial application, they must complete a mandatory interview within 30 days to have a caseworker verify their information. During this interview caseworkers can also collect any missing information from the initial application.

A USDA-commissioned survey of applicants confirm that the application process is complex and costly (Bartlett et al., 2004). In 2000, applicants spent an average of 3.9 hours in Food Stamp offices completing the application process. They took an average of 2.4 trips to the office as well as 1.2 trips to additional locations to acquire necessary documentation. 39% of working households said they had to miss work to complete the application.[7] 10% of

[6]Many other programs require similar interviews though some require in-person visits, e.g., the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), Temporary Assistance for Needy Families (TANF), and Social Security Disability Insurance (SSDI). Giannella et al. (2023) find that moving from pre-scheduled interviews to on-demand interviews increases participation in SNAP.

[7]Administrative changes to the program since 2000 have streamlined this process somewhat (e.g. creating online applications and replacing in-person interviews with over-the-phone ones).

applicants who did not complete the process said they dropped out because of some aspect of the process and another 46% cited they thought they were ineligible, possibly because of information they received during the process. This study also found that applicants were more likely to complete their application if they were at an office with a more "pro-participation" supervisor.

The institutional structure surrounding applications and case management in our state provides an ideal setting to explore the impact of caseworkers on SNAP receipt and applicants' subsequent outcomes. First, case management is nearly exclusively handled over the phone through a statewide system. Caseworkers are organized within tracks based on the their specialization to ensure that caseworkers have the relevant skills, such as language or knowledge of special program rules to handle applications. Each caseworker works in one of multiple call centers located around the state and caseworkers handle cases from all over the state, rather than just those nearest to them.[8]

The second useful institutional feature is that the mandatory interviews with caseworkers are on-demand from the perspective of the applicants. Unlike some states, applicants in our state can call into the statewide phone system at any time Monday through Friday 8am to 5pm to complete their interview. During the interview, caseworkers do not have a set script to follow and have flexibility in the type and number of questions that they ask. Interviews last about 20 minutes on average. Caseworkers then enter the information into a computer system and the software ultimately determines eligibility.

Third, and crucial to our empirical strategy, caseworkers within each track take calls in the order they are received, and the case is officially assigned to that worker when they take the call for the interview. When an applicant calls the phone system, they put in the applicant number assigned to them and are then routed to their appropriate track based on the information on their application, even if it is incomplete. For initial applications, the caseworker does not see any information about the case until they answer the phone and have no control over which cases they receive. So, conditional on the timing of application and the assigned track (both of which we observe), caseworkers are effectively randomly assigned to applicants.

In general, caseworkers are motivated by two factors: 1) they want to give benefits

[8]Prior to 2012, teams were also organized around physical locations and the applications were automatically sorted to the closest office. In 2012, the state moved to a state-wide model where caseworkers serviced applications from across the state. Nationwide in 2000, only 1 state operated a state-wide call center for SNAP, but by 2016, 32 states were operating them.

to those who qualify and 2) they want to avoid errors in their decisions.[9] In our setting, the second factor is in part motivated by the several layers of review that exist to monitor caseworker decisions. First, the USDA has its Quality Control system that audits decisions of caseworkers in all states each year. To do this, they select a random sample of SNAP recipients and do a follow-up survey with them to decide if they are indeed eligible or not. States are then ranked based on the percentage of incorrect decisions and states with lower rankings are fined. In our sample period, over-payment rates (Type II Errors — Kleven and Kopczuk, 2011) are 3-6% across states. Our state of interest is not fined in our sample period and has relatively low error rates in general. In addition to this federal monitoring, our state chose to have an Editing Team, which is not required by the federal government. Editors from the editing team review the decisions of caseworkers every month by examining the case file information (they do not collect any additional information beyond what the caseworker initially collected). Seasoned caseworkers have about 10 cases reviewed per month and newer caseworkers, who we exclude from the analysis, have more cases reviewed per month. Caseworkers who fall below a rate of 90% accuracy face consequences including additional individual mentoring and coaching, a written warning, or further disciplinary action.

Given that caseworker decisions are closely monitored and that a computer decides eligibility, what are the mechanisms through which caseworker behavior can affect SNAP receipt? We hypothesize, and provide supporting evidence below that the biggest source of variation in caseworker behavior is how helpful they are at guiding applicants through the complicated application process.[10] This is also consistent with prior work that found when a state automated assistance for means-tested transfer applications, rather than having caseworkers assist, there was a reduction in means-tested transfer program receipt (Wu and Meyer, 2021). Though, this change was accompanied by increases in wait times and backlogs in processing applications, so the exact mechanism is unclear. Additionally, Finkelstein and Notowidigdo (2019) and Schanzenbach (2009) found that connecting likely SNAP-eligible nonparticipants to application assistance significantly increased their program receipt.[11]

[9] In 2000, 80% of a national sample of supervisors had "pro-participation" attitudes (Bartlett et al., 2004).

[10] An alternative interpretation of this is how discouraging caseworkers are to applicants in the application process. We cannot distinguish between these two interpretations.

[11] Work in Economics on other programs shows that streamlining the application process increases take-up (e.g. Rossin-Slater, 2013; Bhargava and Manoli, 2015; Deshpande and Li, 2019). There is also a large literature in Public Administration that studies the determinants of decision-making for "street-level bureaucrats" including caseworkers in programs such as SNAP (Meyers and Nielsen, 2012). This research has suggested that several factors may play a role: 1) political control such as the goals of politicians, 2) organizational factors including the tasks assigned, resources available and oversight from managers, and 3) worker ideology and professional norms. The strong oversight of caseworker decisions in our context limits the potential discretion quite a bit relative to many of these studies. However, Kogan (2017) hypothesizes that caseworker behavior may be a reason that local public support for redistribution is positively correlated

We construct a one-dimension measure of the Conditional Caseworker Approval Rate (CCAR) discussed in more detail below, which captures all caseworker behavior that leads to applicants being more likely to receive SNAP when assigned to a particular caseworker.

3 Data

Our data come from a single state, which remains unidentified for anonymity, and include all SNAP applicants from 2011 through early 2022. For applicants, we observe basic demographic information along with application dates. Unique to our setting, we can also see the caseworker assigned to the application and the track in which the caseworker works. For those who receive SNAP, we observe benefit amounts and more detailed demographics as well as recertification information. For those who do not receive SNAP, we observe the reason for denial. We use all this information to code caseworker decisions, described in detail in Appendix A.

Application information is linked to quarterly labor supply information from the state's Unemployment Insurance (UI) database. The state only matched the head of the household for each application as a data security measure. However, 52% of all applicants in our sample are single-adult-headed households and results are similar among this subsample. Moreover, in the SNAP Quality Control (QC) Data (a nationally representative sample of SNAP recipients) only 2% of all SNAP recipients are in dual-income households and among a sample of SNAP-income-eligible households in the Current Population Survey (CPS) only 10% are dual-income households. The UI records contain the earnings and industry for each individual and job by quarter from 2011-2021. Importantly, we can observe these outcomes even for SNAP applicants who are denied, and, for all applicants, we observe outcomes before SNAP application. A limitation of these data is that we can only observe workers living in our state of interest, but we estimate that 97% of households with SNAP-eligible income don't move across states in a given year in the Current Population Survey, so out-of-state migration is unlikely to be an issue. Additionally, we do not observe workers who are self-employed, federal employees, or independent contractors. Using the Current Population Survey, we tabulate that only 6% of heads of household who are likely eligible for SNAP are self-employed, and among those receiving SNAP, in the QC data, only 1% are self-employed.[12] We assume that individuals who are not observed in the UI data are

with local SNAP caseloads even though it is a federal program.

[12] Gig work has become increasingly important especially in and after the COVID-19 pandemic (Maneely and Roth-Eisenberg, 2020). Gig work is also poorly measured in many data sets (Abraham et al., 2023). Additionally, the complex nature of the application process may be particularly costly for those with self-employment income (Moynihan et al., 2022). Future work with data that measures self-employment would

not working and assign them a value of 0 for their earnings. We do not know whether the applicant was searching for work or was out of the labor force.

3.1 Sample Construction

To construct a sample that allows us to cleanly identify the effects of caseworker behavior on labor supply dynamics, we begin with the 196,435 new applications that were submitted between 2012-2016. New applications are important in our context because it abstracts away from possible dynamic labor supply effects that could occur from prior SNAP receipt. Also, new applicants have less program knowledge and may be more reliant on the caseworker to navigate the application process, which affects the interpretation of our results. To identify this group, we remove any applicants for whom we observe SNAP receipt within one year prior to their first-observed application in our sample.[13] We focus on applications after the implementation of the state-wide call center model in 2011 that generates the quasi-random caseworker assignment to applicants. We limit to those who applied before 2017 so we can examine quarterly labor supply outcomes 3 years after application and exclude the COVID-19 pandemic. Next, we limit the sample to applications handled within General tracks (123,975 observations). Assignment of caseworkers in these tracks is the most plausibly random given the many applicants and many caseworkers. Note, that we do not restrict our main analysis sample on age explicitly, but because we drop tracks that handle applications for the elderly, this effectively restricts our sample to working-aged applicants and the results are nearly identical if we drop the few applicants in our sample outside working age.

We further drop applications assigned to workers who handled relatively few cases that year to ensure that there are enough observations to get an accurate estimate of caseworker decision making and also exclude new caseworkers who are given fewer and a nonrandom set of applications. Specifically, we drop the bottom 20% of the caseload distribution, which is 126 cases a year. This leaves us with 99,410 observations. Finally, we keep applications assigned to caseworkers with Conditional Caseworker Approval Rate (CCAR) values between the 1st and 99th percentiles of the CCAR distribution and to a balanced sample over time. These restrictions leave us with our final regression sample of 88,543 application decisions. We show robustness to these sample restrictions in the Appendix.

help to shed light on this issue.

[13]We use a rolling one-year period instead of all observable prior SNAP receipt so that observations at the beginning and end of our sample are treated similarly. Results are similar if we instead condition on not having any SNAP receipt prior to the application.

3.2 Descriptive Statistics

We compare demographics of our sample to those of a representative sample of working-age SNAP recipients from the QC Data in Table 1. Columns 1-2 show the characteristics for the national and state-specific samples using the QC data. On most dimensions our state is similar to the national sample, except our state is less racially diverse. In column 3, we show equivalent statistics for all working-age SNAP recipients using our data and this group is very similar to the sample from the QC data (column 2) as expected. Finally, columns 4-5 implement our sample restrictions and include applicants and recipients in our main analysis sample, respectively. Our sample is slightly younger with slightly smaller households than the full sample of recipients in column 3. This is largely because we restrict to new applicants, but we believe the trade-off in representativeness is worthwhile to be able to cleanly identify effects.

Several other statistics are worth noting for interpreting the results in the next sections. First, only 52% of new applicants receive benefits in the quarter of application. The probability of receipt in the entire year after the application is very similar. This is slightly higher, but similar to the 44% acceptance rates in Los Angeles during this same time period (Giannella et al., 2023). Additionally, only 32% of applicants are working in the quarter before application, and applicants have only $1,583 in real quarterly earnings (2012$s) before application on average. We compare this to a sample of all working-age adults with household income below 130% of the poverty line in our state in the CPS and find for that sample roughly 50% report working at all. So, SNAP applicants are less attached to the labor force than a sample of those likely income-eligible for SNAP. Even among those working, earnings are relatively low before application–$4,964 quarterly. To give a frame of reference for this, one person working full time at minimum wage for a full quarter would earn $3,770, which is almost the same as the quarterly household income that puts a household of two just at the poverty line – $3,782 (in 2012). Past work suggests these low earnings are due to the types of occupations SNAP-eligible adults work in, which pay low wages and are volatile (Butcher and Schanzenbach, 2018).

Our sample of SNAP recipients (column 5) are less likely to be working pre-application (30%) and have lower earnings ($1,372) compared to all applicants. Turning to the post-application labor supply (one quarter after application), we see that recipients are slightly more likely to work than before application, but they have lower earnings compared to pre-application. These labor supply statistics are also similar to the full samples of recipients in columns 1-3.

4 Empirical Methods

We take a different approach from the prior research that exploited changes in program access affecting specific subgroups or using historical events. Instead, we use the assigned caseworker's propensity to accept applications as an instrument for SNAP receipt. Caseworkers are randomly assigned to applicants in our sample, conditional on the timing and track of the application. Because of this, caseworkers' cases have the same baseline likelihood of being approved, so differences in average caseworker approval rates must be driven by caseworker behavior. The Conditional Caseworker Approval Rate (CCAR) quantifies and aggregates caseworker behaviors.

We follow the newer examiner-effects literature to create the CCAR using the UJIVE approach ("unbiased jackknife instrumental variables estimator"). Kolesár (2013) proposed the UJIVE and it has been used in other recent papers including Norris et al. (2021) and Agan et al. (2023).[14] Bringing this examiner-effects methodology into the setting of safety net program receipt is an important contribution of our paper. To implement this, we estimate two equations for each application i:

$$Approved_{-i} = \lambda_a + \epsilon_{-i} \tag{1}$$

$$Approved_{-i} = \phi_a + \rho_c + \nu_{-i} \tag{2}$$

where $Approved_{-i}$ indicates whether each application, besides the focal application i, is approved. In each equation, we include a set of application-date-by-track fixed effects (respectively λ_a and ϕ_a), which determines the set of caseworkers the applicant may be assigned to and is the level of randomization. Note, we do not observe the date that each applicant calls to conduct their interview, which is the true level of randomization, so we use the application start date to proxy for this. Equation (2) adds caseworker fixed effects (ρ_c). We then calculate $CCAR_i$ – the predicted approval likelihood for applicant i – by subtracting the predicted value of equation (1) from the predicted value from equation (2). Intuitively, this gives us each applicant's predicted likelihood of approval based solely on the caseworker they are assigned, netting out any heterogeneity due to application timing and assigned track, and the caseworker's decision on the focal application.[15] Thus, our instrument for SNAP

[14]The UJIVE approach is robust to weak-instrument issues caused by small numbers of observations per examiner, which may be important in our setting. It has other advantages in terms of better accounting for covariates and being relatively easy to compute (Norris et al., 2021). We have experimented with alternative estimators, which are highly correlated with our primary measure, but they provide us with less precision, likely because of the relatively small numbers of application decisions per caseworker.

[15]We use whether the application was *approved* in this calculation of the CCAR, which is slightly different from our measure of benefit receipt used for the IV analysis below. See Appendix A. The CCAR is very similar when using benefit receipt instead.

receipt is unique to each application, though for simplicity we still sometimes refer to it as "the CCAR" or "caseworker's CCAR".

There is considerable variation in the CCAR as shown in panel (a) of Figure 1. The standard deviation in our sample of the CCAR is 0.03. We collapse the data to the caseworker level and show that a 10 percentage point increase in the average CCAR across caseworkers is associated with a 14 percentage point increase in their approval rate (panel b). This is a 27% increase of the overall approval rate in our sample of 52% (Table 1). We demonstrate below that the CCAR is strongly *causally* related to SNAP receipt (the first stage). As a test of the exogeneity of caseworker assignment, we regress the assigned caseworker's caseload, months of experience, and applicant-specific CCAR onto baseline applicant demographics from the application and UI data—conditional on application timing and track fixed effects. We contrast this with the relationship between whether an application is approved and these applicant characteristics. In column (1) of Table 2, there is a strong relationship between the set of observable applicant characteristics and the likelihood of SNAP approval. The F-statistic on this model is 163. On the other hand, in columns (2)-(4), the caseworker characteristics and CCAR are largely unrelated to applicant characteristics and the F-statistics are very small– from 0.92 to 1.19. This provides evidence that caseworker assignment is indeed random, conditional on the fixed effects, supporting the independence assumption that the CCAR is unrelated to determinants of labor supply. Under additional assumptions of the exclusion restriction (caseworkers only affect outcomes through SNAP receipt) and monotonicity (that each applicants' SNAP receipt probability is increasing with CCAR), the CCAR is a valid instrument for SNAP receipt. We address these assumptions in section 5.1 below.

We estimate the following:

$$y_i = \beta ReceiveSNAP_i + \theta_a + \zeta_i \tag{3}$$

where y_i is the labor supply outcome of individual i. We instrument for the receipt of SNAP benefits in the quarter of application ($ReceiveSNAP_i$) in equation 3 with the caseworker-and-applicant-specific CCAR:

$$ReceiveSNAP_i = \alpha CCAR_i + \mu_a + \eta_i \tag{4}$$

We similarly include fixed effects for the application-date-by-track (θ_a and μ_a) to ensure that we compare applicants who are exposed to the same set of potential caseworkers. This design estimates the Local Average Treatment Effect (LATE) of SNAP applicants who are accepted, compared to those who are denied, because of the caseworker they are assigned.[16] Following best practices from recent design-based approaches to inference, we use heteroskedasticity-robust standard errors, but do not adjust for clustering because each applicant is randomly assigned to their own caseworker (Abadie et al., 2022).

We estimate this model separately by quarter around the initial application in order to produce event-study-style plots from one year prior to three years following the application. This approach provides an additional test of our empirical design not common in papers using examiner fixed effects designs–we can directly test the balance of the *outcomes* in the pre-application period. If we see that these outcomes prior to application were unrelated to the CCAR, this provides strong support for our research design. Running these regressions separately for each quarter means that we are testing not only if the *trends* in outcomes in the pre-period are related to the CCAR, but also whether the *levels* are different. Thus, this method tests a stronger assumption than standard panel event study regressions that only test for differential trends.

In addition to estimating the above IV model, we decompose the LATE into the potential outcomes under two alternative states of the world (Frandsen et al., 2023): 1) applicants receive SNAP due to their caseworker's CCAR ("treated compliers") and 2) applicants are denied due to their caseworker's CCAR ("untreated compliers"). This is useful because it allows us to visualize levels of the outcome of interest in both states of the world for the compliers before and after application. This method requires one to regress the outcome multiplied by the receipt of benefits on the receipt of benefits instrumented with the CCAR to recover the potential outcomes of the treated compliers. For the potential outcomes of untreated compliers, we regress the outcome multiplied by one minus receipt of benefits on one minus receipt of benefits instrumented with the CCAR. Intuitively, this gives us the average outcome if all marginal applicants received benefits, or were denied benefits, respectively, because of their caseworker's CCAR.

[16] Our estimates are a compliance-weighted average of treatment effects, so they are "local" to the affected population.

5 Results

5.1 The Role of Caseworkers

Figure 2 shows the effect of the CCAR on receipt of SNAP (panel (a)) and benefit amount received including zeros (panel (b)) by quarter surrounding application.[17] Applicants do not receive SNAP in the year prior to the focal application, which is mechanical since we keep new applications only. Beginning in the quarter of application, there is a large and statistically significant effect of the CCAR on benefit receipt. The coefficients indicate the effect of a unit increase in the CCAR, however the CCAR in our sample ranges from -0.12 to 0.11. So, to interpret this coefficient, we scale it by a one standard deviation increase in the CCAR (0.03). A one standard deviation change increases the likelihood of receiving SNAP in the quarter of application for the full sample by 1 percentage point, which is a 2% effect of the overall rate of acceptance of 52%. The F-statistic for the estimate on benefit receipt in the quarter of application is 86. There is a similar pattern on benefit amount received, and, in the quarter of application. A one standard deviation increase in the CCAR increases the amount received by about $7. To get a sense of the magnitude of this increase, informational interventions aimed at increasing SNAP enrollment among likely eligible elderly non-participants increased participation by 5 percentage points (Finkelstein and Notowidigdo, 2019); the same intervention when accompanied by application assistance increased participation by 12 percentage points.

The largest effect on SNAP receipt is in the quarter of application–which motivates our choice to use this as the endogenous variable in Equation (3)–and the effects fade out in subsequent quarters. The decline in the effect of the CCAR at and after 2 quarters could be because most SNAP recipients in this state have a 6-month recertification period and recertification is a common time for participants to stop receiving benefits due to administrative burden (Homonoff and Somerville, 2021). However, even three years after the application, there is still a statistically significant effect–a one standard deviation increase in the CCAR increases receipt 11 quarters later by 0.3 percentage points (Table 3 column 1).

The decrease in the magnitude of the impact of the CCAR over time is consistent with two hypotheses: 1) SNAP benefit spells are on average shorter than three years, so the effect of the CCAR fades out as people stop receiving benefits, or 2) denied applicants re-apply and receive benefits later. We explore whether those who are denied SNAP because of their assigned caseworker re-apply after the initial quarter of application. Overall, rates of

[17] We report the coefficients and standard errors associated with panel (a) of this figure in the first column of Table 3. We aggregate the monthly benefit information to the quarterly level to match the UI data timing.

reapplication are low among compliers, and we do not find significant differences in rates of re-application between the potential outcomes of treated compliers compared to untreated compliers in the quarters right after application (Appendix Figure A2).[18] So, we do not believe that reapplication and re-timing of benefit receipt due to the assigned caseworker's CCAR is driving the dynamics we observe. Note, we cannot look at the effect of the CCAR on the likelihood of reapplying *among those denied* because this would condition on the endogenous variable. Similarly, we do not look at recertification length as an outcome because this is only observed for those who receive SNAP.

Next, we compare the dynamics of benefit receipt for marginal recipients in our analysis (Figure 2) to the dynamics of benefit receipt for all applicants and those who receive benefits in the quarter of application, regardless of whether they receive benefits due to their assigned caseworker (Appendix Figure A3). The levels in these comparison groups will be different because they are unconditional means rather than the effect of the CCAR, but we are interested in whether the *dynamics* in benefit receipt are the same. We find the pattern of benefit receipt over time is nearly identical across these figures. Many recipients–whether they are pushed to receive SNAP because of their caseworker or not–stop receiving SNAP after quarter two, though there are about 20% of recipients that continue to receive SNAP for at least three years. This pattern is consistent with prior evidence that the median length of SNAP participation among new entrants is about 12 months, with 26% exiting after 4 months (Leftin et al., 2014). This also suggests the reason the effects of the CCAR fade out over time is that recipients reduce their SNAP participation over time. Additionally, if SNAP recipients alter their labor supply while receiving benefits, we expect the largest effects to be right around the time of application.

5.1.1 Mechanisms Behind the Effect of Caseworkers

We hypothesize that the main way caseworkers can impact applicants' outcomes is through assistance during the application process. To test this, we examine the relationship between the CCAR and the likelihood an applicant does not complete their application. Incomplete applications are those that are auto-denied for administrative reasons, withdrawn by the applicant, or those that failed to include all the required documentation. An incomplete application is the most common reason for denial–77% of applicants who are denied are denied for this reason (the overall denial rate is 48%). In Table 4, we regress onto the CCAR whether the given application was incomplete, conditional on application-date-by-track fixed effects. We find that a one standard deviation increase in the CCAR decreases

[18] We explain the layout of this figure below at the beginning of section 5.2.

the likelihood of having an incomplete application by 1 percentage point, 3% of the sample mean. This suggests that caseworkers with a higher CCAR are more helpful in ensuring the applicant submits all the necessary information and completes the application process. This is in contrast to the findings in Finkelstein and Notowidigdo (2019), who show that likely-SNAP-eligible individuals who are pushed to apply are *more* likely to be rejected due to incomplete applications. However, their study is on a different population and on a different margin–elderly SNAP non-participants enrolled in Medicaid who have not applied for SNAP benefits. The difference is likely because in our setting all individuals have taken the first step to apply, whereas in their setting people are marginally pushed to apply and may be less likely to follow through with their application as a result.

5.1.2 Targeting Effects of Caseworkers

To understand who is pushed into receiving SNAP because of their caseworker, we explore the characteristics of compliers following the method outlined in Frandsen et al. (2023). To do so, we estimate equation (3) instrumenting with the CCAR, but replace the labor supply outcome with various applicant characteristics interacted with an indicator for whether the applicant received SNAP during the quarter of application.[19] The first row in Table 5 shows the characteristics of the compliers calculated using this method. The second and third rows show the average of the same characteristics for the full analysis sample of applicants and the sample of applicants who receive benefits in the quarter of application, respectively. The fourth and fifth rows provides the ratio of the complier characteristics to the full sample characteristics to test if the compliers differ significantly from all applicants and beneficiaries, respectively. The statistical test is whether the ratio is significantly different from one. This helps us to understand the LATE we estimate as well as how caseworkers affect the pool of eventual SNAP recipients–whether they improve targeting or not.

In general, compliers seem slightly more attached to the labor market, however, only one of these differences is marginally statistically significantly different from the full samples of applicants and beneficiaries. Compliers are about 30 percent more likely to be female than all applicants and beneficiaries and this difference is statistically significant. This suggests that female applicants are more responsive to the assistance provided by caseworkers.[20]

[19] Specifically, we estimate: $y_i * ReceiveSNAP_i = \beta ReceiveSNAP_i + \theta_a + \zeta_i$ using $CCAR_i$ as an instrument for the endogenous right-hand-side variable–$ReceiveSNAP_i$. This gives the average of the characteristic y among those that receive SNAP and because we instrument for SNAP receipt, this is the characteristic among those who were pushed onto receiving SNAP because of their caseworker's CCAR.

[20] Another possibility is that caseworkers with higher CCARs are more likely to help female applicants. We view that as very unlikely given the level of oversight in the caseworkers' work environment and evidence for monotinicity provided below.

Compliers are also a few years younger and more likely, though not significantly so, to be Black or Hispanic.

There is no conclusive evidence that caseworkers affect targeting. On some dimensions, compliers seem more advantaged–more pre-application labor market attachment–but on others they seem less advantaged–more likely to have a female and non-white head of household. Given these findings, it is plausible that the LATE we estimate can apply to the population of beneficiaries more generally. It is also informative to compare our findings to that of Finkelstein and Notowidigdo (2019), who show assistance and informational interventions for likely-SNAP-eligible populations that push more people to apply for SNAP reduce targeting. In contrast, everyone in our sample has already chosen to apply for SNAP and overcome the initial costs of doing so. We believe caseworkers with higher CCARs are providing help to all applicants they interact with, regardless of the applicants' characteristics, which is why they have little impact on targeting. Our findings highlight the importance of studying the impacts of different interventions within the same program to fully understand the targeting impacts of administrative burdens.

5.1.3 Validity of CCAR as Instrument for SNAP Receipt

We have demonstrated a strong first stage of the effect of the CCAR on SNAP receipt. Next, we address additional assumptions needed to use the CCAR as an instrument.

A key assumption underlying our research design is monotonicity of the instrument. Until recently, papers using examiner designs often invoked the strong assumption of pairwise monotonicity in order to ensure that IV estimates are properly-weighted aggregates of complier treatment effects. Intuitively, the assumption requires that if a caseworker with a higher CCAR is assigned to an application, this caseworker will be more likely to accept that application than a caseworker with a lower CCAR, regardless of case characteristics. A growing literature has emphasized the importance of this assumption and suggested tests that researchers can use to support its validity; Frandsen et al. (2023) propose a joint test for violations of either exclusion or pairwise monotonicity assumptions. In our empirical design, we reject the null hypothesis that both conditions are satisfied. Fortunately, Frandsen et al. (2023) also show that under a relaxed "average monotonicity" assumption, IV still estimates a convex combination of treatment effects. Average monotonicity requires that for each individual, the covariances between that individual's caseworker-specific treatment status and caseworker overall CCAR are positive. Two testable implications of this assumption are: 1) the first stage estimates for all sub-samples should yield positive estimates and 2) there should be a positive relationship between the CCAR for the full sample and the CCAR for various subgroups. In Appendix Figures A4, A5, and A6, we show that our instrument passes both of these tests. Thus, the CCAR is plausibly a valid instrument for SNAP receipt under the weaker average monotonicity assumption.[21]

Finally, the exclusion restriction requires that caseworkers only impact applicant outcomes through the proposed causal channel: whether the applicant is approved for SNAP. In the state we study, caseworkers have a limited scope for affecting applicants outside of the SNAP determination. Caseworkers only interact with applicants during a short mandatory phone interview where the worker verifies all the application information. Caseworkers do not routinely direct applicants to other sources of government support or provide any sort of labor market advice or resources. The state we study administers joint applications for SNAP, Medicaid, and TANF. However, specialized teams focus on applicants jointly applying to multiple programs and the caseworkers we study mostly handle SNAP-only applications and thus have limited scope to impact participation in Medicaid and TANF. Indeed, when we regress whether the applicant receives TANF during the quarter after their SNAP application onto the CCAR, we estimate a precise zero. Unfortunately, we do not currently have data on Medicaid enrollment.

We note that other data sources point to a high degree of cross-program participation among SNAP recipients. However, of greatest concern is that *changes* in program participation occur at the same time; that when individuals begin to receive SNAP, they also start receiving benefits from other programs. If this were the case, our IV estimates might be the effects of multiple programs and not just SNAP. We use the Survey of Income and Program Participation (SIPP) to investigate this directly. The SIPP is a panel study that asks individuals about their demographics and receipt of many safety net and social insurance programs.[22] In Appendix Figure A7, we plot the rates of safety net program receipt around SNAP spell initiation. It is clear that households that start receiving SNAP are already receiving benefits from other programs–most commonly Medicaid (short dashed blue line), Free and Reduced Price Lunch (long dashed maroon line) and Breakfast (dotted purple line). Notably, the *change* in program receipt of these other programs in the period the household

[21] Other prominent papers fail pair-wise monotonicity and instead rely on average monotonicity like we do here (e.g., Norris et al., 2021). Recent research has pointed out that if there are multiple dimensions, such as skill and preferences, that both contribute to variation in actor's decision-making this can lead to a violation of the strict or average monotonicity assumptions (Chan et al., 2022). We do not observe false positives or false negatives, making it hard to use the suggested methods that explicitly test for this. However, we argue that the helpfulness of the caseworker is the primary determinant of the CCAR and provide evidence to support this above.

[22] One drawback of the SIPP is that, as with most major surveys, program receipt is under-reported. As a check, we have adjusted for this under-reporting as suggested by Meyer et al. (2022) and Meyer et al. (2009) and the results are very similar.

starts receiving SNAP is relatively small and much smaller than the change in receipt of SNAP. The programs with the most meaningful changes at SNAP initiation are Medicaid and WIC. Medicaid increases by 18 percentage points and WIC increases by 7 percentage points. To understand if changes in these other programs impact labor supply decisions we turn to the prior literature. Recent evidence indicates that Medicaid does not impact adult labor supply decisions (Baicker et al., 2014; Kaestner et al., 2017) so this is unlikely to drive our results. The literature on the impact of WIC on labor supply is very limited but does suggest that WIC may increase work leave among mothers with newborns (Bullinger and Gurley-Calvez, 2016). In heterogeneity analysis discussed below, we show the effects are similar among those without children, who are unlikely to receive WIC. So, we do not believe cross-program receipt drives our results.

5.2 IV Results on Labor Supply

We next analyze the effect of the CCAR on quarterly employment (panel (a)) and earnings including zeros (panel (b)) for the full sample in Figure 3. The black dots plot the coefficients from the IV model and the black spikes display the 95% confidence intervals (also reported in Table 3). As discussed, we decompose the LATE into the potential outcomes under two alternative states of the world: 1) applicants receive SNAP due to their caseworker's CCAR ("treated compliers", shown in orange) and 2) applicants are denied due to their caseworker's CCAR ("untreated compliers", shown in blue). This method is powerful because it allows us to visualize levels of the outcome of interest in both states of the world for the complier group.

We see no evidence of quantitatively meaningful or statistically significant changes in labor supply for the full sample. And, the difference in the potential outcomes (i.e., the black line, or comparing the blue and orange lines) before application is close to zero and stable, indicating that pre-application labor supply is unrelated to the assigned CCAR and providing support for our research design. Note, the potential outcomes are not mechanically zero before application because we are plotting the average potential outcomes over time. With the caveat of relatively large confidence intervals, this null finding is striking in that it is counter to predictions from the canonical labor supply model.

This result is less surprising when interpreted in light of the new evidence we are able to provide with our data that 61% of SNAP applicants did not work at all in the year before applying for SNAP. We show the effects on employment and earnings for this subgroup in panels (a) and (c) of Figure 4, respectively, and in Table 6. Here, the levels and trends in potential outcomes are all zero prior to application since these are non-workers. After application, there is no significant change in labor supply for this subsample. This null result is consistent with the hypothesis that many SNAP recipients face barriers to work for reasons besides the disincentive effects of SNAP.

A strength of our data is that we can examine the effects for those working during all four quarters before application (25% of our sample) who may be more sensitive to SNAP receipt. We do this in panels (b) and (d) of Figure 4 and in Table 7. For both employment and earnings, the results reveal an interesting dynamic pattern of effects. One quarter after application, there is a significant reduction in earnings of $2,758. This is a meaningful effect–about 48% off the baseline earnings in the pre-period–and is around the time the effects on SNAP receipt are the largest (Figure 2). The reduction in earnings could be a combination of intensive and extensive margin effects as there is a small but insignificant dip in employment in the quarter after application as well. To get a sense of this, we multiply the point estimate on employment (-0.117) by the average earnings among those working in the period before applying ($5,776). This implies a reduction in earnings of $676 due to just the change in work, suggesting some, but likely not all, of the reduction in earnings is due to a change in the extensive margin. However, the extensive margin is measured relatively coarsely in our data since it is at the quarterly level, a point we return to below when we analyze effects on the earnings distribution.

Examining the potential outcomes for this group sheds light on the mechanisms behind this short-run negative effect. Specifically, this effect could be driven by: 1) individuals who receive SNAP and then chose to reduce their labor supply because of the SNAP benefits, or 2) individuals who experienced a negative shock (e.g. a layoff) and then SNAP prolongs the time they remain not working or working at a lower level. The latter would suggest SNAP acts in a similar way as Unemployment Insurance, providing insurance to smooth consumption in the face of shocks. Our results are more in line with this latter interpretation. To see this, even in the state of the world where compliers are denied SNAP (the blue line in panels (b) and (d) of Figure 4), compliers experience a large decline in employment and earnings around the time of SNAP application–employment falls by roughly 30 percentage points and earnings decrease by about $600 from the period before to the period after application. The negative IV estimate of SNAP is driven by an *even larger* decline in employment and earnings in the state of the world where compliers are all accepted onto SNAP (the orange line). This is strong evidence that those who were working before applying for SNAP are experiencing a negative shock–either a direct labor demand shock such as a layoff, or another shock that affects labor supply such as having a child, separation or divorce, or a health change.[23]

Following this temporary dip in work, SNAP receipt causes both earnings and employment to rebound and actually trend *positively* three years after the initial SNAP application. Given large standard errors, we also estimate a difference-in-differences equivalent pooling observations in the third year after application together. We find a significant increase in employment in the third year of 34 percentage points and a marginally significant increase in earnings in the third year of $2,892 per quarter.

Again examining the potential outcomes sheds light on why SNAP recipients are doing better than those denied in the longer-run. In the state of the world where compliers are all denied SNAP, they experience a sharp downward trajectory in their earnings and employment in the longer-run. Three years after application, the likelihood of quarterly employment in this state of the world is only about one-third, which is striking given that by definition this entire group worked before applying for SNAP. Average earnings also fall by 60% after three years, relative to pre-application. On the other hand, in the state of the world where compliers receive SNAP, they experience an increase in earnings and employment relative to the quarter after application, so that three years after SNAP application, their outcomes are almost the same as pre-application. This is consistent with the receipt of SNAP helping people buffer against the negative shock they experienced at the time they applied for SNAP. For example, SNAP could help people who lost a job search for a higher quality job or allow recipients to pay for goods and services necessary to prevent further following a downward trajectory due to cascading events, such as eviction (Collinson et al., 2022).

This consumption-smoothing benefit is likely important for this population because they live hand to mouth and are credit constrained. Only 62% of SNAP recipients have bank accounts before receiving SNAP, and, among those with accounts, the median balance is only $389 (2012 dollars).[24] Moreover, 17% of SNAP recipients paid their rent late, 11% paid utility bills late, many reported having to decide between spending money on food or on rent and utilities, and are in danger of eviction (Propel, 2023). Among SNAP recipients with children, the majority have expenses that exceed their income in a given month and they report SNAP benefits help alleviate this deficit, though not entirely.

[23] This is consistent with descriptive evidence that among SNAP-receiving households who report financial hardship, this was generally precipitated by either: 1) abrupt decreases in their earnings due to a job loss, reduction in hours, childbirth or other household composition changes, or 2) increases in their expenditures due to things like a car breaking down or an unexpected health expense (Leftin et al., 2014).

[24] Authors' calculation with the Survey of Income and Program Participation.

5.2.1 Effects on Earnings Distribution for those Employed Across Baseline

To better understand the average effects, we next explore distributional effects across quarterly earnings *among those employed before applying for SNAP*. We discretize quarterly earnings into \$2,000 bins starting with \$1 to \$2,000, then \$2,001 to \$4,000, and so on, ending with any income over \$12,001. In panel (a) of Figure 5, we show the pdf of the distribution of earnings during the quarter before application. The highest density of earners for this sample (28%) occurs at the \$4,001-\$6,000 bin. In panel (b), we estimate the effect of SNAP on the likelihood that earnings are in each bin during the event times following an initial SNAP application. Each vertical line represents a given event time at or after application. Each horizontal bar on an event-time line displays the result of running our IV model with a binary variable for whether the applicant has earnings in the given bin during the given event time as the outcome. The horizontal height of the bars is the point estimate from that regression, with negative estimates extending to the left of the vertical line and positive estimates to the right. Statistical significance of the estimates is indicated by color–those significant at the 5 or 10 percent margin are red or pink, respectively, and insignificant estimates are gray.

In the quarter of and right after SNAP application, there is a significant decrease in earnings between \$4,001-\$6,000, which is around the average earnings among baseline-employed cases, shown in the baseline pdf in panel (a) of Figure 5. And, there is a significant increase in the likelihood of having earnings above \$0 but below \$2,000 per quarter. We consider this lowest bin to represent the earnings of someone who did not work full-time the entire quarter since this translates into at most a \$4 hourly wage for someone working full-time the entire quarter. This implies a decrease in hours worked per quarter and this is likely an important driver of the short-run decrease in average earnings in panel (d) of Figure 4.

Turning to the longer-run, the distributional results indicate SNAP increases the likelihood of having earnings both between \$1-2,000 (less than full-time work) and between \$8,001-10,000. We investigate if these effects are heterogeneous by baseline earnings in Appendix Figures A8, A9, and A10. The increase in the likelihood of earning between \$1-\$2,000 is driven by all groups regardless of baseline earnings. On the other hand, the increase in the likelihood of earning between \$8,001-\$10,000 appears to be driven by applicants with baseline earnings between \$4,000-\$8,000 and above \$8,000. These distributional analyses show that SNAP increases the likelihood of working at all in the longer-run among all applicants granted SNAP. And there is suggestive evidence to support the hypothesis that some workers who receive SNAP earn higher wages post-SNAP than they did pre-SNAP.

5.2.2 Specification Checks

We test the sensitivity of the results to our sample construction choices in Appendix Figures A11 and A12. The black estimates are the baseline specification as above (point estimates are the dots and the 95% confidence intervals are the lines). In our baseline sample we trim applications with a CCAR below the 1st and above the 99th percentiles. We show the robustness to further restricting extreme CCAR values, and not restricting the sample based on CCAR values, shown in blue and orange, respectively. Additionally, in our baseline sample we drop applications assigned to caseworkers with fewer than the 20th percentile number of decisions. The green and gray estimates show results with alternative cutoffs–the 10th percentile and 30th percentile, respectively. The results are very similar across all these choices and have overlapping confidence intervals, so none of these decisions drive our key findings.

Next, in Appendix Figure A13, we split the sample of those employed prior to applying by the presence of children to test whether our results are driven by other safety net programs that target households with children. The results are similar across both groups and, if anything, are slightly smaller for households with children. Finally, we split the sample of those employed prior to applying by the presence of multiple adults in the household to understand if we are missing important dual earner effects. Again, the results are similar across subgroups with overlapping confidence intervals.

5.2.3 Comparison to Prior Literature

The past literature studies specific subgroups or the effect of the program decades ago. Overall, existing work suggests modest negative-to-null effects of SNAP *access* on the labor supply of likely impacted groups. Studying Food Stamps in the 1960-70s–the precursor to SNAP–Hoynes and Schanzenbach (2012) find an intent-to-treat effect of Food Stamps on annual earnings for those most likely impacted of 32%. The implied treatment-on-the-treated effect is 58%. East (2016) estimates effects of a similar magnitude for immigrant households likely affected by the changes in non-citizen's eligibility for Food Stamps. The treatment-on-the-treated estimates imply a reduction in employment and hours among single females of 43% and 51%, respectively, and a reduction in hours among married men of 75%.

There are a handful of studies on the impact of SNAP *work requirements* for ABAWDs–who make up about 5% of all SNAP participants. These studies have mixed findings with some finding no effects of imposing work requirements on labor supply (Stacy et al., 2018; Vericker et al., 2023) and some finding small positive effects (Cuffey et al., 2022). Recent analysis by Gray et al. (2022) uses high-quality administrative data and a regression discontinuity design based on the maximum age of people subject to the requirement (49), and finds no effect on employment but a large negative effect on SNAP receipt. The authors hypothesize that the null effect on employment is potentially due to other barriers to work that SNAP recipients face, which is complementary to our findings from a broader group of SNAP applicants.

Finally, Bitler et al. (2021) study the effect of kinks in the budget constraint created by SNAP on labor supply. Among SNAP participants who might be affected, there is little evidence of a response.

In contrast to the existing literature, our paper studies the effect of modern SNAP *receipt* on a large, generalizable group. Additionally, the ability to observe heterogeneity in the effects over time and by applicant demographics allow us to shed more light on the full labor supply response and its mechanisms than has been possible before. Our results illuminate one potential reason why the prior literature has mixed findings, as those estimates often combine both the short-run and long-run response, and responses among those who do and do not face barriers to work, whereas we are able to separate out these differential effects.

5.2.4 Welfare Effects

We quantify the results in a social welfare framework using the Marginal Value of Public Funds (MVPF) approach in Hendren and Sprung-Keyser (2020). Specifically, we calculate the MVPF of the CCAR being one standard deviation higher.[25] The MVPF is the ratio of benefits to net government costs of the policy change, defined as:

$$MVPF = \frac{WTP}{C + FE} \tag{5}$$

The numerator is the willingness to pay to get SNAP benefits for SNAP applicants, which we assume to be equivalent to the change in the benefit amount paid due to a one standard deviation increase in the CCAR. The denominator is the direct cost of operating the program (C) for marginal recipients, including benefits paid out and administrative costs, as well as any fiscal externalities (FE) due to changes in behavior for marginal recipients. The fiscal

[25] We estimate the MVPF within the first three years of SNAP receipt, which assumes effects after three years on both SNAP participation and labor supply are zero.

externalities of a program like SNAP are complex and include effects beyond just the labor supply response of adult recipients that we identify here. We focus on fiscal externalities due to the changes in labor supply we estimate, with the caveat in mind that this will likely be an under-estimate of the MVPF.

We estimate an MVPF of SNAP due to a one standard deviation increase in caseworker CCAR of 1.2, indicating the value to beneficiaries is larger than the net cost to the government.[26] In fact, we find that the effect of SNAP on government revenue due only to changes in labor supply over three years is *positive* because the longer-run positive effects outweigh the short-term negative ones.

Prior estimates of the MVPF of increasing access to SNAP range from 0.89 to 56.25. However, it is important to note that the study that produces estimates around 1 are unable to examine any benefits to SNAP recipients beyond the direct value of the transfers themselves (Finkelstein and Notowidigdo, 2019), whereas the study that produces the much higher MVPF is able to quantify many other benefits because of the richness of the data and the fact that the policy changes analyzed happened many decades ago (Bailey et al., 2020). This highlights a perennial challenge with analyzing the costs and benefits of safety net programs–the costs are often borne out in the short-run, whereas many of the benefits, including improvements in health and labor market outcomes (Bailey et al., 2020) and reductions in crime (Barr and Smith, 2023), only appear much later.

6 Conclusion

This paper examines the effect of SNAP on labor supply decisions using an examiner design. We are the first to bring the examiner design to the setting of means-tested transfer program receipt in the United States. We show that caseworker behavior matters for determining whether SNAP applicants receive benefits and provide evidence that this operates through caseworkers helping applicants navigate the complex application process.

We find no effect of SNAP on labor supply for the full sample of working-aged applicants. The richness of data allow us to understand why our findings are counter to the canonical static labor supply model predictions. We document that most applicants do not work in the year before applying for SNAP, and the receipt of SNAP has no impact on their labor supply decisions. We argue these applicants likely face other, larger barriers to work that dominate any potential effect of SNAP. Among the 25% of our sample that worked in the year leading up to their SNAP application, SNAP appears to act as insurance against

[26] Appendix B provides the details of the MVPF calculation.

negative shocks and reduces earnings temporarily, but increases the likelihood of work in the longer-run.

Recently, lawmakers have raised concerns about work disincentives from SNAP and other means-tested transfer programs; work requirements were expanded under the Trump administration, changed as a result of the 2023 debt ceiling negotiations and are again being debated as part of the 2023 Farm Bill. Our findings inform this debate; we find no evidence that receiving SNAP leads to long-term reductions in labor supply or dependency on government benefits. If anything, our results suggest the opposite–SNAP provides support for those who are unable to work and provides important insurance for workers experiencing a negative shock.

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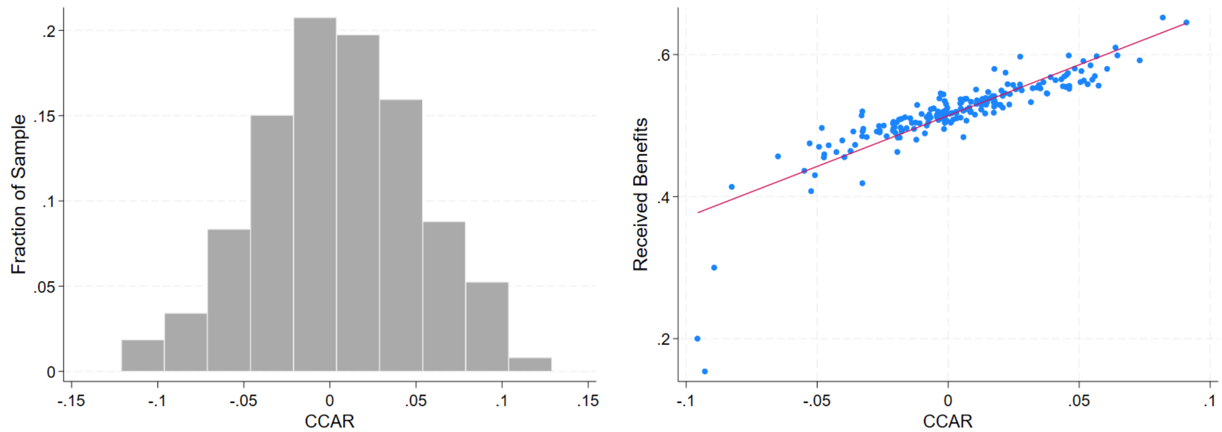
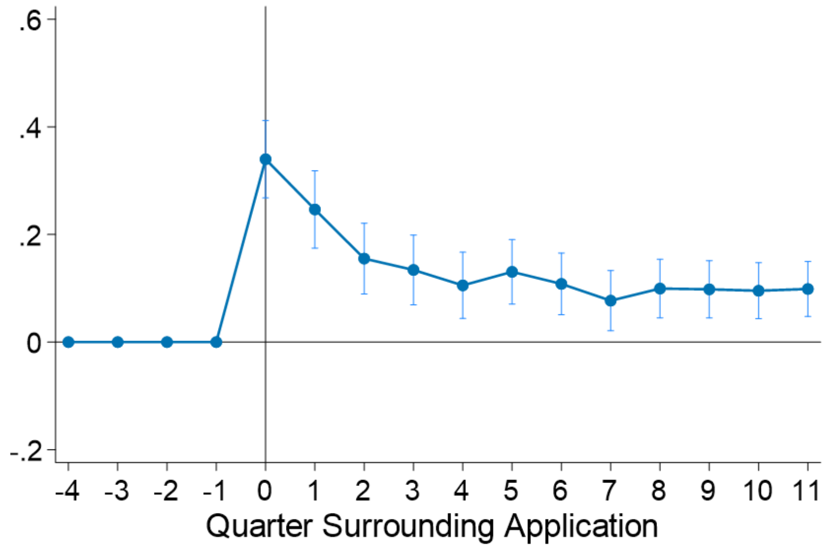
Figure 1: Distribution of the CCAR and its Relationship with Benefit Receipt



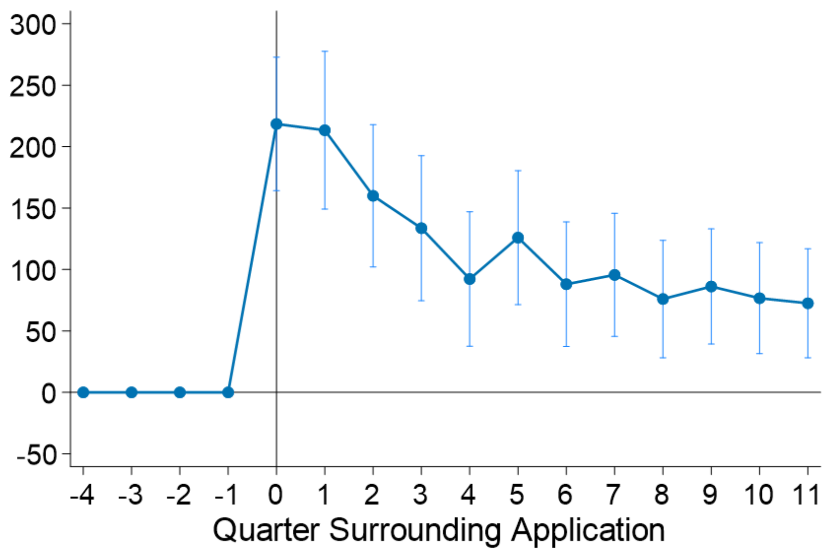
(a) Histogram of CCAR **(b)** Scatterplot of CCAR and Benefit Receipt

Notes: Panel (a) plots the histogram of our calculated CCAR for the main sample. Panel (b) is at the caseworker level and plots the relationship between the caseworker-level average CCAR and the SNAP acceptance rate of applicants for each caseworker. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

Figure 2: First Stage: Impact of the CCAR on Quarterly SNAP Benefit Receipt



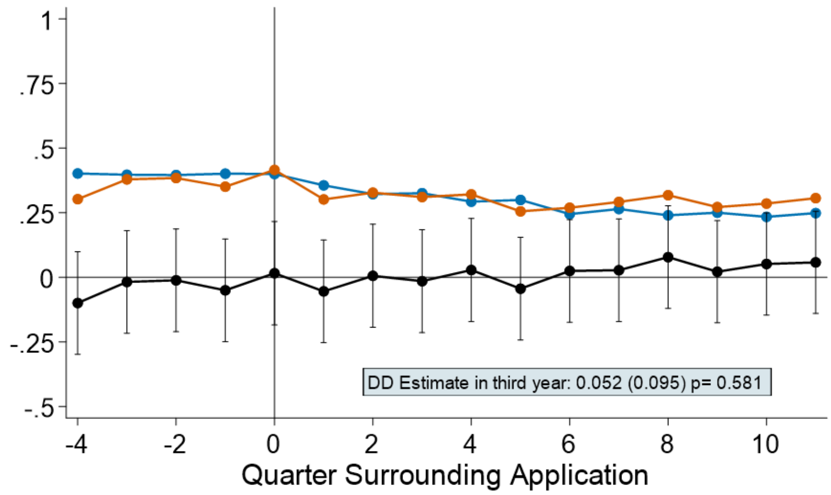
(a) Received Any SNAP



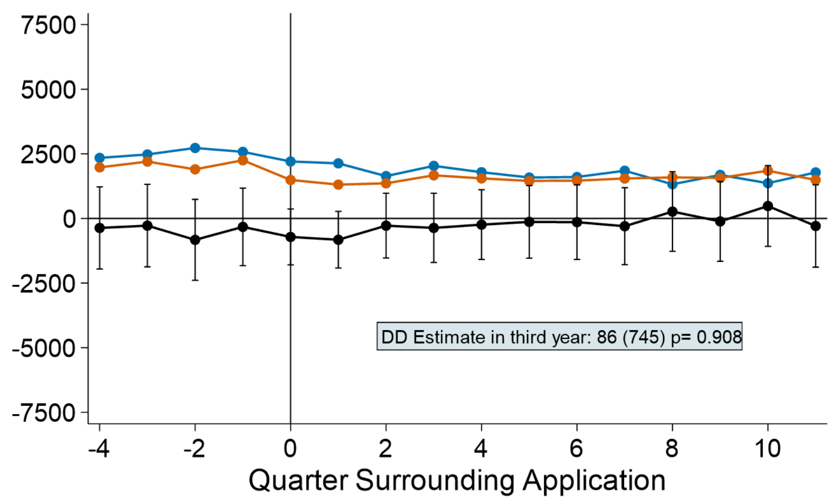
(b) Amount SNAP Received

Notes: The figure presents estimates of the effect of the CCAR on benefit receipt from separate regressions using equation (4) for the quarters surrounding a new SNAP application. The 95% confidence intervals are shown in the vertical lines. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

Figure 3: IV Estimates of the Effect of SNAP on Quarterly Labor Supply for Full Sample



(a) Employment

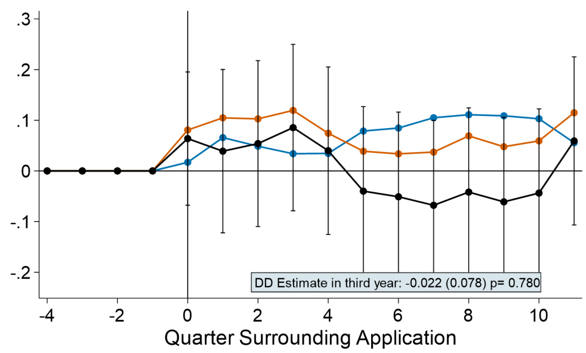


(b) Earnings

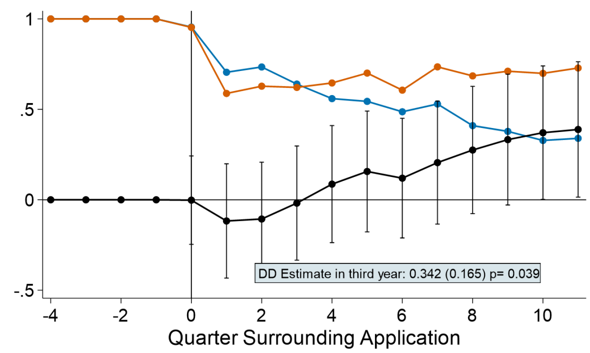
Untreated Compliers   Treated Compliers   IV Estimate

Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The black dots display the coefficients from the IV model and the black vertical lines display the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (i.e., the state of the world where compliers are denied SNAP) and the orange line plots the potential outcomes for treated compliers (i.e., the state of the world where compliers receive SNAP).

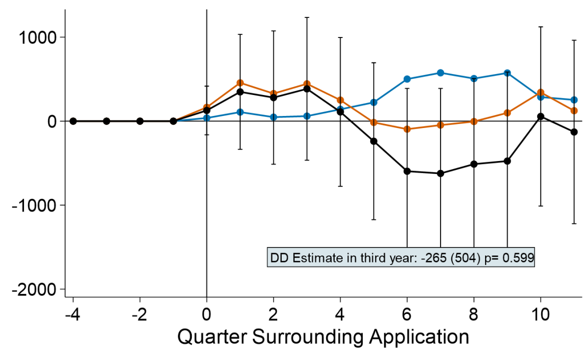
Figure 4: IV Estimates of the Effect of SNAP on Quarterly Labor Supply, by Pre-Application Work



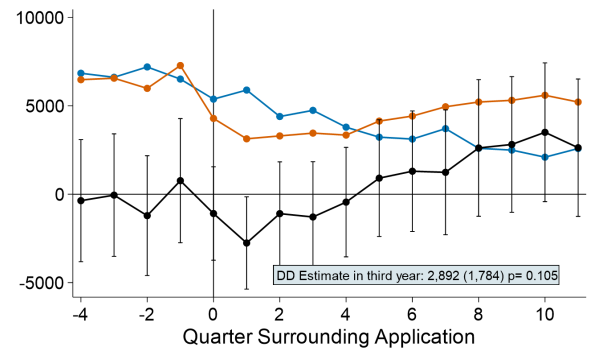
(a) Employment
Not Employed Across Baseline



(b) Employment
Employed Across Baseline



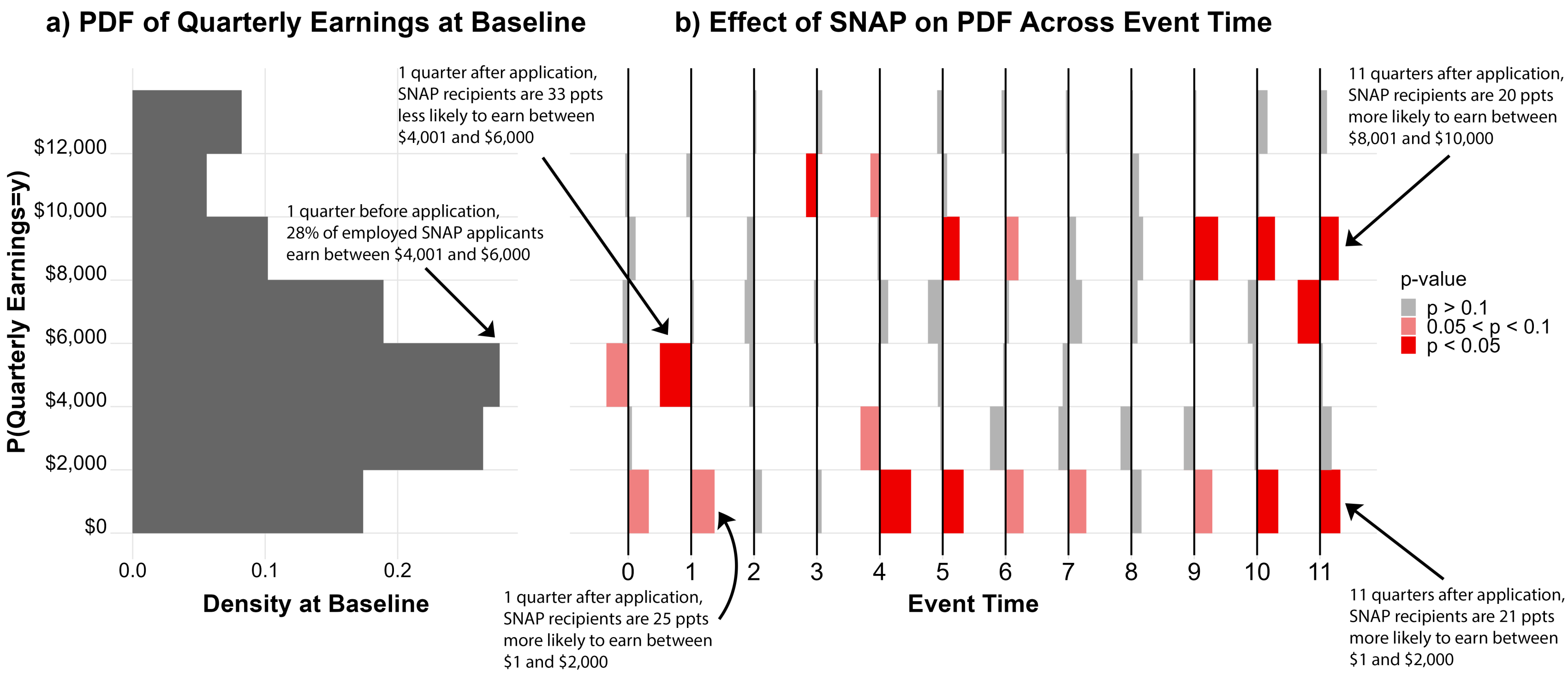
(c) Earnings
Not Employed Across Baseline



(d) Earnings
Employed Across Baseline

Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The black dots display the coefficients from the IV model and the black vertical lines display the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (i.e., the state of the world where compliers are denied SNAP) and the orange line plots the potential outcomes for treated compliers (i.e., the state of the world where compliers receive SNAP).

Figure 5: IV Estimates of the Effect of SNAP on Distribution of Quarterly Earnings (2012$), for Applicants Employed Across Baseline



Notes: Panel (a) of this figure displays the share of employed SNAP applicants with earnings in the given range during the quarter before application. Panel (b) compiles results from running our main IV specification from equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The outcome variables are indicator variables for having quarterly earnings within the given earnings bin (on the y-axis) in the given event-time quarter (on the x-axis). Negative point estimates are reflected by a shaded area to the left of the given event-time vertical line and positive estimates are to the right. The coloring denotes the p-value of the given point estimate.

Table 1: Summary Statistics

	All SNAP Recipients (QC Data)		Our Sample		
	National	Our State	All Recipients	New Applicants	New Recipients
Quarterly Receipt of Benefits	1	1	1	0.52	1
Female	0.70	0.71	0.70	0.55	0.54
Age	39.87	37.85	37.66	33.77	34.64
Hispanic	-	-	0.11	0.07	0.08
Black	0.26	0.03	0.04	0.02	0.02
Pacific Islander	0.00	0.01	0.01	0.01	0.01
Asian	0.02	0.01	0.02	0.01	0.01
Any Kids under Age 5	0.32	0.24	-	-	-
Number of Kids	1.03	1.38	1.44	0.70	0.71
Number of People in Hhold	2.19	2.35	2.75	1.87	1.86
Any Member w Disability	0.20	0.22	-	-	-
Real Earnings before Application (2012$)	-	-	-	1583.05	1372.57
Percent Employed before Application	-	-	-	0.32	0.30
Real Earnings after Application (2012$)	748.67	881.63	801.70	1222.10	1015.54
Percent Employed after Application	0.25	0.28	0.26	0.32	0.31

Notes: The first two columns use data from the SNAP Quality Control Data Set. Columns 3-5 present summary statistics from our state of interest using our administrative data. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. We present the demographics of the head of household only from both data sets. For pre-application labor supply information, we use 1 quarter *before* application in our data. For post-application labor supply information, we use 1 quarter *after* application in our data for new applicants and new recipients, and quarterly wage information during all periods of SNAP receipt in the Quality Control data and for all recipients in our data. In the Quality Control data the head of household must be aged 18 - 64. We use the weights provided by the Quality Control data.

Table 2: Balance Test

	Received Benefits	Montly Caseworker Caseload	# Months of Caseworker Experience	CCAR
Employment t−1 (0.32, 0.47)	0.016	-0.221	-0.094	-0.001
	(0.010)	(0.889)	(0.237)	(0.001)
Real Earnings t−1 (1,651, 3,509)	-0.000***	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Number of Jobs t−1 (0.40, 0.66)	-0.044***	-0.116	0.039	0.000
	(0.006)	(0.536)	(0.143)	(0.001)
Industry Experience t−1 (2.46, 5.35)	0.000	0.010	0.007	0.000
	(0.000)	(0.043)	(0.011)	(0.000)
Arc Percent t−1 (0.22, 0.49)	0.020***	-0.084	-0.033	-0.000
	(0.004)	(0.360)	(0.096)	(0.000)
Works in NAICS 45 Industry t−1 (0.04, 0.19)	0.032***	1.526*	-0.218	0.001
	(0.009)	(0.845)	(0.226)	(0.001)
Works in NAICS 72 Industry t−1 (0.05, 0.21)	0.012	-0.185	0.326	0.000
	(0.009)	(0.822)	(0.220)	(0.001)
Works in NAICS 56 Industry t−1 (0.07, 0.26)	0.042***	-0.190	0.282	-0.000
	(0.008)	(0.698)	(0.186)	(0.001)
Female (0.55, 0.50)	-0.024***	-0.788**	-0.054	0.000
	(0.003)	(0.312)	(0.083)	(0.000)
Over 65 Head (0.03, 0.16)	-0.390***	-0.209	-0.359	-0.001
	(0.012)	(1.110)	(0.297)	(0.001)
Age (33.86, 13.73)	0.005***	0.014	0.004	-0.000*
	(0.000)	(0.013)	(0.004)	(0.000)
Black/Hispanic (0.09, 0.29)	0.034***	0.309	-0.051	0.000
	(0.006)	(0.538)	(0.144)	(0.001)
Spanish-Speaking (0.01, 0.10)	-0.119***	1.360	-0.065	-0.002
	(0.016)	(1.487)	(0.397)	(0.002)
Mean Y	0.52	247.09	34.63	0.00
F	163.45	1.01	0.92	1.19
N	88543	88457	88543	88543

Notes: This table regresses the pre-application characteristics of the head of household on benefit receipt (column 1), the monthly caseload of their assigned caseworker (column 2) and the months of experience of their assigned caseworker (column 3) and their CCAR (column 4). We include application-date-by-track fixed effects. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table 3: First Stage and 2SLS Estimates: Full Sample

	First Stage	*Quarterly Employment*			*Real Quarterly Earnings*		
	Estimate	Estimate	Untreated Complier	Treated Complier	Estimate	Untreated Complier	Treated Complier
Event Time 11	0.099***	0.058	0.249***	0.306***	-291	1,782***	1,491***
	(0.026)	(0.101)	(0.070)	(0.072)	(813)	(595)	(555)
Event Time 10	0.095***	0.052	0.234***	0.285***	481	1,366**	1,847***
	(0.027)	(0.101)	(0.071)	(0.072)	(798)	(589)	(536)
Event Time 9	0.098***	0.022	0.250***	0.272***	-114	1,684***	1,571***
	(0.027)	(0.101)	(0.071)	(0.072)	(787)	(582)	(530)
Event Time 8	0.100***	0.078	0.240***	0.318***	267	1,324**	1,591***
	(0.028)	(0.101)	(0.071)	(0.072)	(785)	(598)	(509)
Event Time 7	0.077***	0.027	0.264***	0.292***	-300	1,847***	1,547***
	(0.029)	(0.101)	(0.071)	(0.073)	(759)	(567)	(505)
Event Time 6	0.108***	0.025	0.245***	0.269***	-142	1,605***	1,463***
	(0.029)	(0.101)	(0.071)	(0.073)	(737)	(550)	(490)
Event Time 5	0.131***	-0.044	0.299***	0.255***	-132	1,585***	1,453***
	(0.031)	(0.101)	(0.071)	(0.073)	(717)	(540)	(471)
Event Time 4	0.105***	0.028	0.293***	0.321***	-239	1,790***	1,552***
	(0.031)	(0.102)	(0.071)	(0.073)	(688)	(518)	(453)
Event Time 3	0.134***	-0.015	0.325***	0.310***	-365	2,034***	1,669***
	(0.033)	(0.102)	(0.071)	(0.073)	(683)	(522)	(443)
Event Time 2	0.155***	0.006	0.322***	0.328***	-278	1,638***	1,361***
	(0.034)	(0.102)	(0.071)	(0.073)	(638)	(484)	(416)
Event Time 1	0.247***	-0.054	0.356***	0.301***	-824	2,133***	1,309***
	(0.037)	(0.101)	(0.071)	(0.072)	(559)	(444)	(347)
Event Time 0	0.340***	0.016	0.400***	0.416***	-714	2,204***	1,490***
	(0.037)	(0.102)	(0.072)	(0.073)	(552)	(453)	(332)
Event Time -1	0	-0.050	0.401***	0.351***	-326	2,578***	2,252***
		(0.101)	(0.072)	(0.072)	(764)	(593)	(497)
Event Time -2	0	-0.012	0.396***	0.384***	-827	2,728***	1,901***
		(0.102)	(0.072)	(0.073)	(799)	(605)	(528)
Event Time -3	0	-0.018	0.397***	0.379***	-277	2,476***	2,200***
		(0.101)	(0.071)	(0.073)	(814)	(611)	(543)
Event Time -4	0	-0.099	0.402***	0.302***	-366	2,344***	1,977***
		(0.101)	(0.071)	(0.072)	(811)	(607)	(541)
First Stage F $(t = 0)$	85.56						
Outcome Mean $(t = -1)$	0	0.322			1,655		
Observations	88,543	88,543	88,543	88,543	88,543	88,543	88,543

Notes: Column 1 presents first-stage estimates from equation (4) estimated separately for the quarters surrounding application. The next two sets of columns present IV estimates from equation (3) instrumenting with the CCAR in the *Estimate* column, and our decomposition of this LATE into the potenital outcomes for untreated and treated compliers using the method proposed by Frandsen et al. (2023). Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Relationship of the CCAR with Incomplete Application

	Incomplete Application
Caseworker CCAR	-0.340***
	(0.035)
Mean Y	0.37
N	88543

Notes: We include application-date-by-track fixed effects. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Complier Characteristics

	Employed t−1	Earnings t−1	Number of Jobs t−1	Industry Experience (Quarters) t−1	Arc Percent t−1	Female	Age	Black or Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Complier-weighted char	0.35	2251.86	0.53	3.04	0.13	0.73	27.85	0.15
Full-sample average char	0.32	1654.96	0.40	2.45	0.22	0.55	33.77	0.09
Beneficiary average char	0.30	1418.81	0.37	2.27	0.22	0.54	34.64	0.09
Complier-weighted char relative to overall	1.09	1.36	1.31	1.24	0.59	1.33**	0.82***	1.68
	(0.22)	(0.30)	(0.25)	(0.33)	(0.37)	(0.15)	(0.06)	(0.51)
Complier-weighted char relative to beneficiaries	1.17	1.59*	1.44	1.34	0.57	1.37***	0.80***	1.63
	(0.24)	(0.35)	(0.27)	(0.36)	(0.36)	(0.15)	(0.06)	(0.49)

Notes: Row 1 presents the results of our main IV specification from equation (3) instrumenting with the CCAR, where the outcome variable is the given column characteristic interacted with a indicator equal to one if the case received SNAP during the quarter of application. This can be interpreted as the average value of the characteristic among compliers. Row 2 provides the average characteristics among the full regression sample (compliers, always-, and never-takers). Row 3 provides the average characteristics among the SNAP beneficiaries in the regression sample. Row 4 provides (Row 1)/(Row 2) and standard errors (calculated by the delta method) are in parentheses. Row 5 is a similar calculation but comparing compliers to the beneficiary average, i.e., (Row 1)/(Row 3). Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 6: First Stage and 2SLS Estimates: Not Employed across Baseline

	First Stage	*Quarterly Employment*			*Real Quarterly Earnings*		
	Estimate	Estimate	Untreated Complier	Treated Complier	Estimate	Untreated Complier	Treated Complier
Event Time 11	0.119***	0.059	0.056	0.115*	-128.869	253	124
	(0.034)	(0.085)	(0.051)	(0.067)	(557.018)	(344)	(440)
Event Time 10	0.110***	-0.044	0.103**	0.059	56.062	287	343
	(0.034)	(0.085)	(0.051)	(0.067)	(544.333)	(329)	(434)
Event Time 9	0.095***	-0.061	0.109**	0.048	-475.107	574*	98.431
	(0.035)	(0.085)	(0.051)	(0.068)	(540.807)	(325)	(427.168)
Event Time 8	0.109***	-0.042	0.111**	0.069	-511.597	507	-4.480
	(0.036)	(0.085)	(0.051)	(0.067)	(519.063)	(311)	(410.799)
Event Time 7	0.087***	-0.068	0.105**	0.037	-622.438	576*	-46.894
	(0.037)	(0.086)	(0.051)	(0.068)	(516.247)	(310)	(405.213)
Event Time 6	0.122***	-0.051	0.085*	0.034	-596.273	501*	-95.202
	(0.038)	(0.085)	(0.051)	(0.068)	(502.580)	(304)	(394.173)
Event Time 5	0.125***	-0.040	0.079	0.039	-238.861	224	-15.327
	(0.040)	(0.085)	(0.051)	(0.068)	(476.632)	(291)	(379.731)
Event Time 4	0.133***	0.040	0.035	0.074	110.101	141	251
	(0.041)	(0.084)	(0.050)	(0.068)	(452.222)	(275)	(361)
Event Time 3	0.167***	0.085	0.034	0.119*	384.406	60.121	445
	(0.043)	(0.084)	(0.049)	(0.068)	(433.885)	(257.388)	(349)
Event Time 2	0.158***	0.054	0.049	0.103	280.687	47.703	328
	(0.044)	(0.084)	(0.049)	(0.068)	(405.128)	(240.417)	(327)
Event Time 1	0.214***	0.039	0.066	0.105	348.523	108	456
	(0.047)	(0.082)	(0.048)	(0.067)	(349.057)	(206)	(281)
Event Time 0	0.320***	0.064	0.017	0.081	127.108	37.702	165
	(0.047)	(0.067)	(0.040)	(0.054)	(147.760)	(97.847)	(111)
Event Time -1	0	0	0	0	0	0	0
Event Time -2	0	0	0	0	0	0	0
Event Time -3	0	0	0	0	0	0	0
Event Time -4	0	0	0	0	0	0	0
First Stage F $(t = 0)$	45.75						
Outcome Mean $(t = -1)$	0	0.000			0.000		
Observations	54,218	54,218	54,218	54,218	54,218	54,218	54,218

Notes: Column 1 presents first-stage estimates from equation (4) estimated separately for the quarters surrounding application. The next two sets of columns present IV estimates from equation (3) instrumenting with the CCAR in the *Estimate* column, and our decomposition of this LATE into the potenital outcomes for untreated and treated compliers using the method proposed by Frandsen et al. (2023). Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table 7: First Stage and 2SLS Estimates: Employed across Baseline

	First Stage	*Quarterly Employment*			*Real Quarterly Earnings*		
	Estimate	Estimate	Untreated Complier	Treated Complier	Estimate	Untreated Complier	Treated Complier
Event Time 11	0.087*	0.389**	0.340***	0.728***	2,631	2,584*	5,215***
	(0.052)	(0.191)	(0.142)	(0.122)	(1,982)	(1,476)	(1,247)
Event Time 10	0.064	0.371**	0.328**	0.699***	3,503*	2,096	5,599***
	(0.053)	(0.188)	(0.143)	(0.121)	(2,002)	(1,494)	(1,206)
Event Time 9	0.121**	0.333*	0.378***	0.711***	2,816	2,497*	5,312***
	(0.055)	(0.184)	(0.138)	(0.120)	(1,958)	(1,461)	(1,210)
Event Time 8	0.105*	0.275	0.410***	0.685***	2,617	2,598*	5,215***
	(0.056)	(0.179)	(0.135)	(0.120)	(1,972)	(1,537)	(1,144)
Event Time 7	0.103*	0.206	0.530***	0.735***	1,238	3,709***	4,947***
	(0.057)	(0.173)	(0.124)	(0.119)	(1,802)	(1,373)	(1,129)
Event Time 6	0.128**	0.119	0.487***	0.606***	1,301	3,122***	4,423***
	(0.059)	(0.169)	(0.127)	(0.120)	(1,739)	(1,333)	(1,080)
Event Time 5	0.155***	0.156	0.544***	0.701***	910	3,227***	4,138***
	(0.062)	(0.170)	(0.122)	(0.119)	(1,685)	(1,305)	(1,039)
Event Time 4	0.110*	0.087	0.559***	0.646***	-446	3,792***	3,346***
	(0.064)	(0.165)	(0.120)	(0.118)	(1,580)	(1,226)	(1,000)
Event Time 3	0.135**	-0.018	0.640***	0.621***	-1,288	4,744***	3,456***
	(0.067)	(0.161)	(0.114)	(0.118)	(1,596)	(1,264)	(978)
Event Time 2	0.145**	-0.106	0.734***	0.628***	-1,097	4,396***	3,299***
	(0.068)	(0.160)	(0.109)	(0.119)	(1,494)	(1,166)	(938)
Event Time 1	0.235***	-0.117	0.705***	0.588***	-2,758**	5,893***	3,134***
	(0.075)	(0.161)	(0.110)	(0.122)	(1,331)	(1,117)	(785)
Event Time 0	0.406***	-0.002	0.955***	0.953***	-1,091	5,379***	4,288***
	(0.076)	(0.125)	(0.080)	(0.098)	(1,347)	(1,105)	(849)
Event Time -1	0	0	1	1	767	6,513***	7,280***
					(1,791)	(1,409)	(1,136)
Event Time -2	0	0	1	1	-1,212	7,199***	5,987***
					(1,729)	(1,349)	(1,090)
Event Time -3	0	0	1	1	-52	6,615***	6,563***
					(1,768)	(1,378)	(1,117)
Event Time -4	0	0	1	1	-364	6,843***	6,479***
					(1,762)	(1,375)	(1,121)
First Stage F $(t = 0)$	28.35						
Outcome Mean $(t = -1)$	0	1.000			5,776		
Observations	21,817	21,817	21,817	21,817	21,817	21,817	21,817

Notes: Column 1 presents first-stage estimates from equation (4) estimated separately for the quarters surrounding application. The next two sets of columns present IV estimates from equation (3) instrumenting with the CCAR in the *Estimate* column, and our decomposition of this LATE into the potenital outcomes for untreated and treated compliers using the method proposed by Frandsen et al. (2023). Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

A Caseworker and Decision Assignment

For a given applicant and application date, there can be multiple decisions made. For example, if an applicant is automatically denied because of lack of documentation but then reapplies within 60 days with the required documentation. In our analysis sample, roughly 10 percent of initial applications are associated with multiple decisions. Additionally, multiple caseworkers can work a single case and this happens to about 4% of applicants in our sample.[27] We address these complications by keeping the final decision related to an initial application, but attribute this decision to the *first-assigned* caseworker. We prefer to use the last decision since it reflects the final outcome. Results are nearly identical if we instead use the first decision on the application or include all decisions.

We use a combination of information to determine whether an application is ultimately approved, which is a key variable in calculating the CCAR. We define an application as being approved if the ending date for the case is later than the starting date, or if the applicant has an accompanying recertification record corresponding with the given initial application date. We also consider applications approved if the applicant receives benefits during either the month of or the month after the initial application date. Otherwise, we consider the application denied.

B Details of MVPF Calculation

To calculate the change in SNAP benefit amount (WTP) due to a one standard deviation increase in the CCAR, we aggregate the benefits received among marginal recipients for the three years after initial application from Figure 2. Total benefits received are $1438.52, so a one standard deviation increase in the CCAR increases benefit amount over three years by $42.

Using statistics from the USDA, the administrative costs of operating SNAP are $261 per year and case in 2012$s.[28] We assume the administrative costs include the costs of certifying and recertifying SNAP recipients. This likely overstates the costs somewhat because

[27] This occurs because cases are randomly reassigned due to regular equalizations of work load across caseworkers. Also, when an applicant calls in, the phone system makes *no* attempt to route their call to their original caseworker. As a result, if an applicant calls back after their interview and speaks with a new caseworker, the worker may opt to assign themselves the case. Caseworkers are trained to only assign themselves to the case if they made substantive changes to the case and are willing to take ownership. Caseworkers are often hesitant to do so because the caseworker who submits the case is the one who is penalized if errors are found–even if the errors originated from a previous caseworker.

[28] `https://fns-prod.azureedge.us/sites/default/files/media/file/SNAP-State-Variation-Admin-Costs-FullR` `pdf`

part of the initial certification costs have already been paid by the time the caseworker interacts with each application. Our first stage effects on SNAP receipt indicate a total increase of 1.69 quarters of benefit receipt, or 0.05 quarters per one standard deviation in the CCAR (1.69*0.03). Thus, administrative costs increase by $3 for a one standard deviation increase in the CCAR (($261/4)*0.05). Total direct costs are thus $42 + 3 = 45$ for both the increase in benefits paid out and administrative costs.

Finally, turning to fiscal externalities, we estimate the reduced form version of Table 7 with quarterly earnings as the outcome variable. The total change in earnings over three years is an increase of $3,383.46 because the longer-run positive effects dominate the short-run negative effects. We scale this by a one standard deviation in the CCAR and then by 0.25 since only 25% of our sample is employed before applying for SNAP and there is a null effect on earnings for the rest of the sample. So, a one standard deviation increase in the CCAR increases earnings by $25.37 over the following three years.

We then calculate the tax rate on earnings for this group. The average working SNAP recipient is a single adult earning $23,104 in the year before applying for SNAP (from Table 1). Applying the 2012 tax rules, the standard deduction is $5,950, so taxable income is $17,154. Head of households are taxed 10% on the first $12,400 of income and then 15% on the remaining $4,754. Additionally, they are subject to a payroll tax of 4.2% and the SNAP benefit amount is reduced by 24% as earnings increase. Thus, the average tax rate for this group is 24+4.2+(10*(12400/17154)+(15*4754/17154)=40%. Multiplying the change in earnings due to a one standard deviation increase in the CCAR by this tax rate, the increase in government revenue is $10.

Combining all these estimates, the MVPF is 1.2 $(42/(45 - 10))$.

Figure A1: SNAP Application Form

HOUSEHOLD AND GENERAL INFORMATION

4. List everyone who is living in your household and applying for benefits:

First and Last Name	Social Security #[1]	Birth Date	U.S. Citizen/ Eligible Non-Citizen Yes/No	Gender M / F	Relationship	[redacted] Resident Yes/No	Resident Since[2] (ex: 07/14/13)	Race[3, 6]	Ethnicity[4, 6]	Marital Status[5]
					Self					

22. Does anyone in your household receive any of the following types of income? .... ☐ Yes ☐ No
If yes, complete all columns:

	Type	Recipient's Name	Gross (before deductions) Amount Received	How Often Paid? (ex: weekly, monthly)	Date Income Started
☐	Social Security		$		
☐	SSI		$		
☐	Child Support received directly from parent or another state		$		
☐	Child Support received through ORS		$		
☐	Unemployment State:		$		
☐	Money received from family, friends or church From who?		$		
☐	Retirement		$		
☐	Pension		$		
☐	Alimony		$		
☐	Veteran's Benefits		$		
☐	Workers Compensation		$		
☐	Tribal Income		$		
☐	Lump Sum Payments		$		
☐	Other income (ex: Adoption, Mineral Rights, Rental, Royalty, Child and Adult Care Food Program payments etc.): ____________		$		

Other than taxes, are any deductions being withheld from anyone's income listed? .... ☐ Yes ☐ No
If yes, complete the following information:

Name: ______________________ Type of Deduction: ______________ Deduction amount: $ __________

Name: ______________________ Type of Deduction: ______________ Deduction amount: $ __________

24. Does anyone in your household have financial accounts? .... ☐ Yes ☐ No
If yes, list all accounts owned by you or anyone applying with you. Some examples of financial accounts are Checking, Savings, 401K*, IRA*, Annuities, Money Market, Stocks/Bonds/Mutual Funds, etc.
** Not Required for SNAP*

Type	Account Owner(s)	Bank Name	Account Balance	Date Opened
			$	
			$	
			$	
			$	

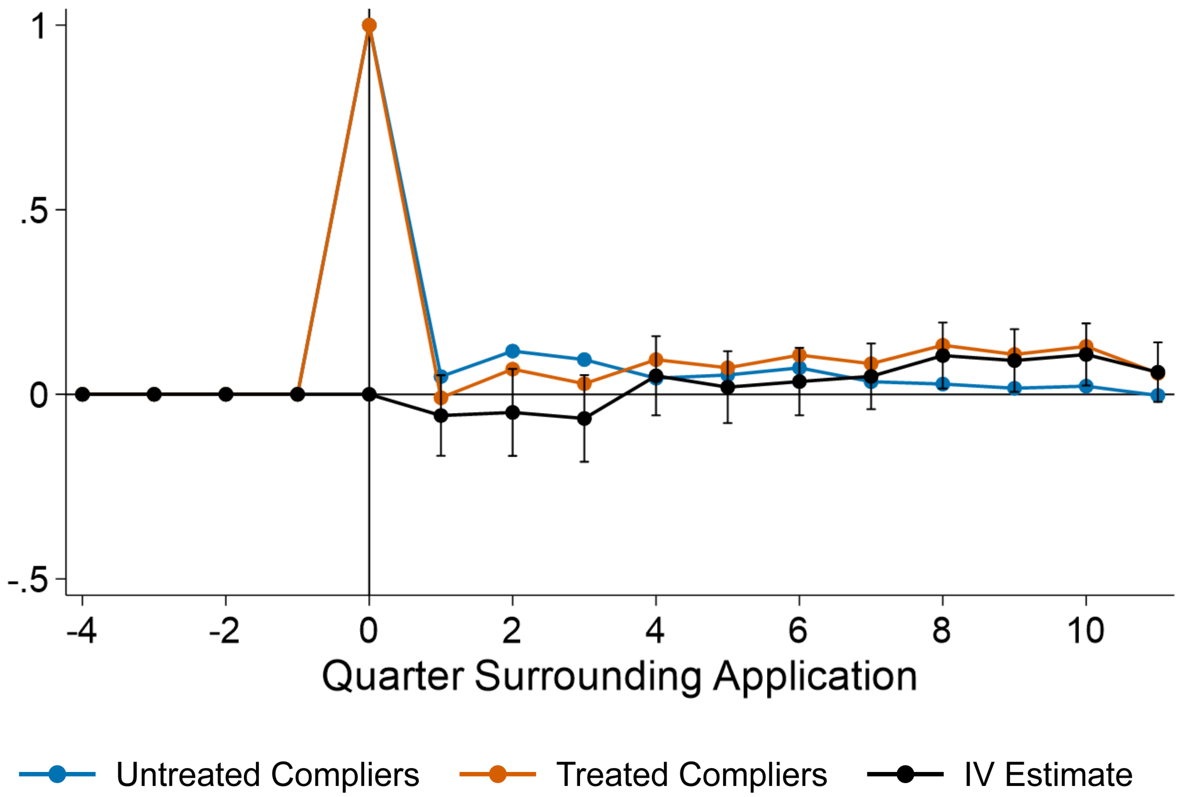
25. Does anyone in your household have any vehicles? .... ☐ Yes ☐ No
If yes, complete all columns. Some examples of vehicles are cars, trucks, boats or water craft, motorcycles, snowmobiles, motor homes, ATV's, etc.

Registered Owner(s)	Make	Model	Year	Licensed	State	Amount Owed	Vehicle Use	Date of Purchase
				☐ Yes ☐ No		$		
				☐ Yes ☐ No		$		
				☐ Yes ☐ No		$		

26. Does anyone in your household have any of the following property assets? .... ☐ Yes ☐ No
If yes, complete all columns:

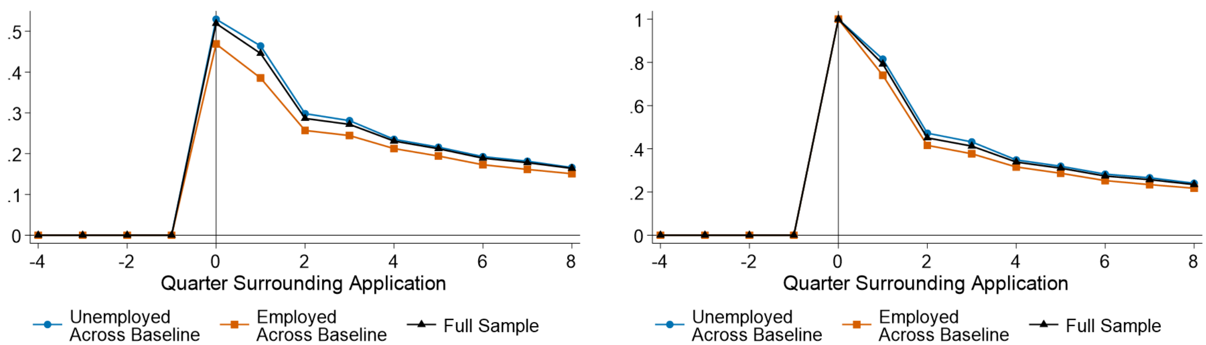
	Type	Who Owns This?	Fair Market Value	Amount Owed	Date Acquired
☐	Home		$	$	
☐	Other property (ex: Land, rental home, vacation home/time share, mineral/other rights, etc.): ____________		$	$	
☐	Trailers		$	$	
☐	Other (ex: equipment/tools, machinery, livestock, etc.): ____________		$	$	

Figure A2: IV Estimates of the Effect of SNAP on (Re)Application for SNAP



Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The black dots display the coefficients from the IV model and the black vertical lines display the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (i.e., the state of the world where compliers are denied SNAP) and the orange line plots the potential outcomes for treated compliers (i.e., the state of the world where compliers receive SNAP).

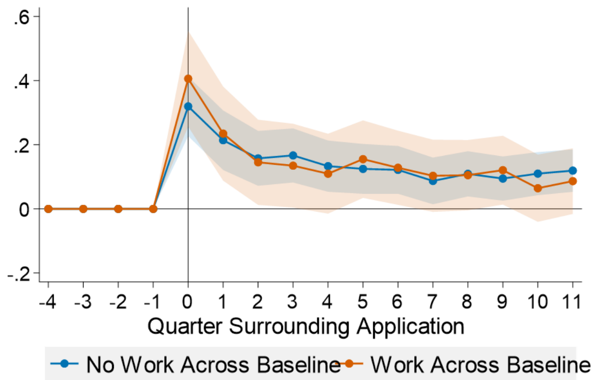
Figure A3: Likelihood of SNAP Receipt by Quarter



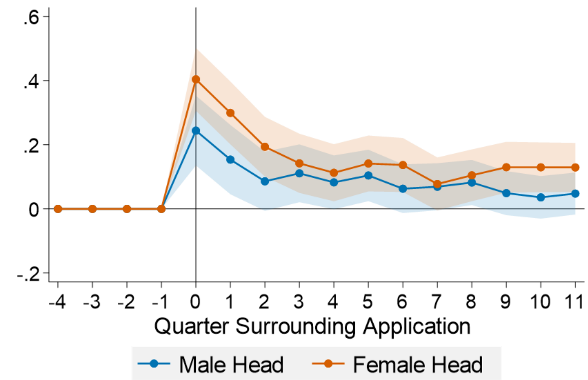
(a) All Applicants **(b)** Recipients in Quarter 0

Notes: The figures present the percent of the relevant sample that receives SNAP in a given quarter. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

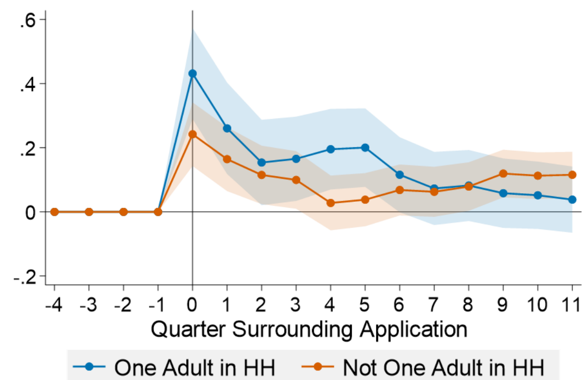
Figure A4: First Stage Estimates for Subgroups on Benefit Reciept



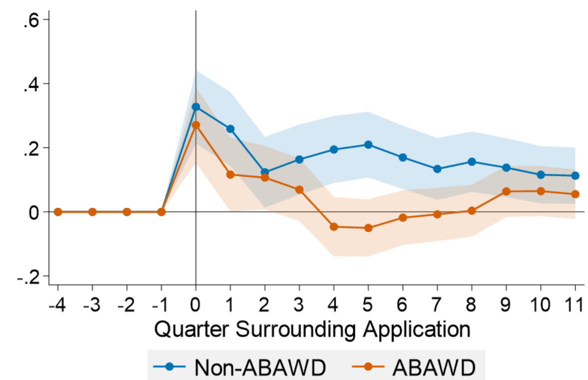
(a) Employment at Baseline



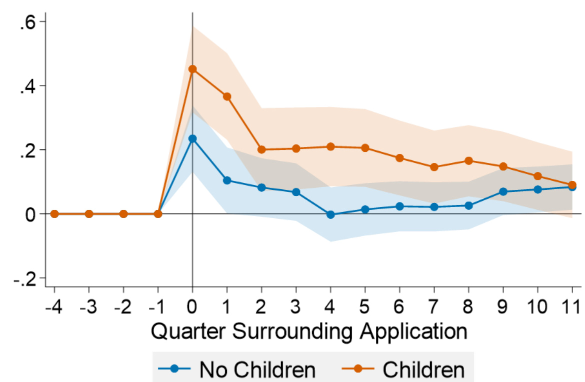
(b) Gender



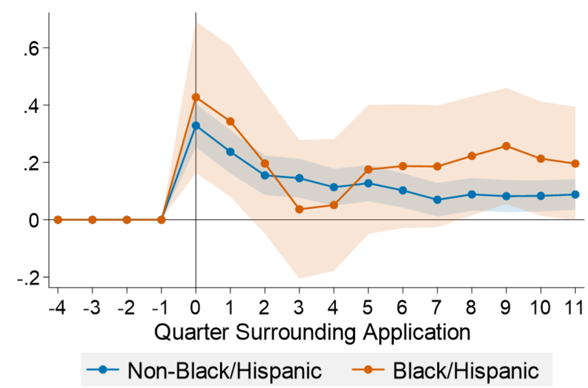
(c) Single Adult



(d) ABAWD



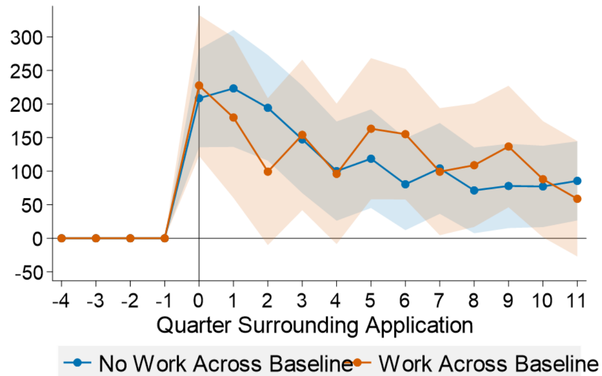
(e) Children



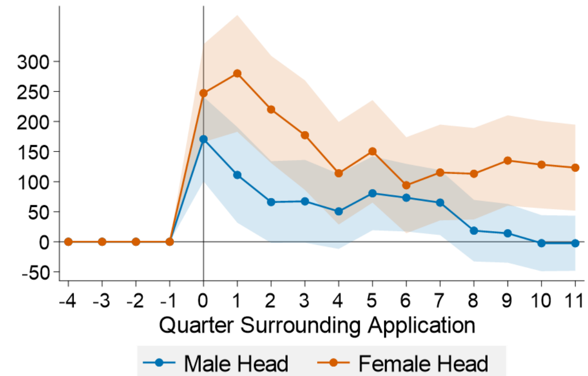
(f) Race/Ethnicity

Notes: The figures present estimates of the effect of the CCAR on benefit receipt from separate regressions using equation (4) for the quarters surrounding an application for the given subgroups. The 95% confidence intervals are shown in the shaded regions. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

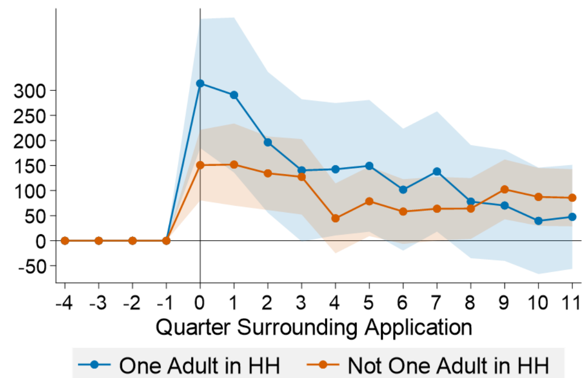
Figure A5: First Stage Estimates for Subgroups on Quarterly Real Issued Amount



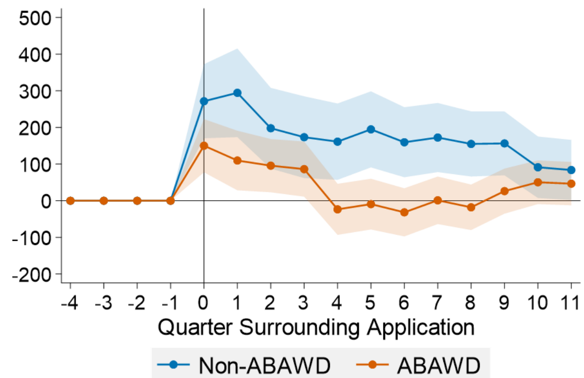
(a) Employment at Baseline



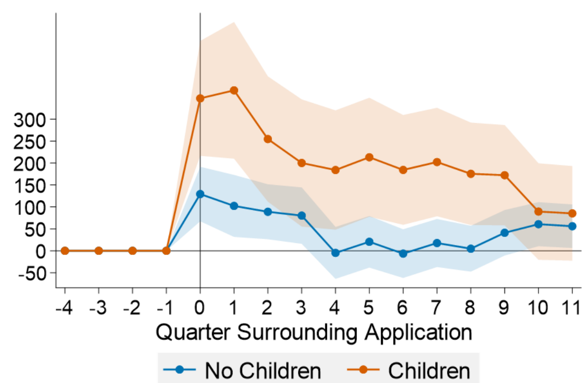
(b) Gender



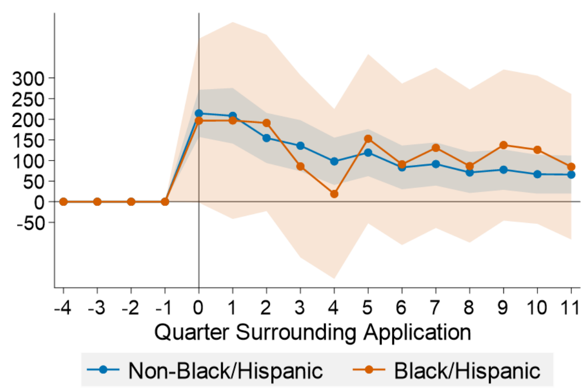
(c) Single Adult



(d) ABAWD



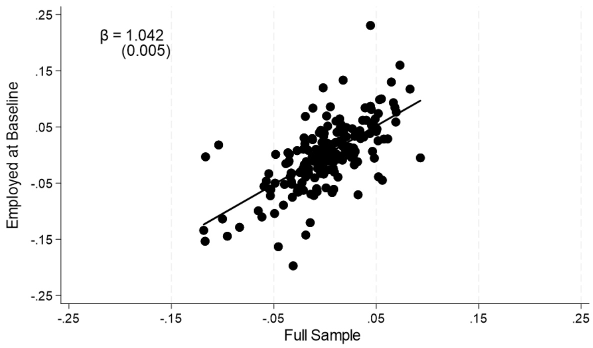
(e) Children



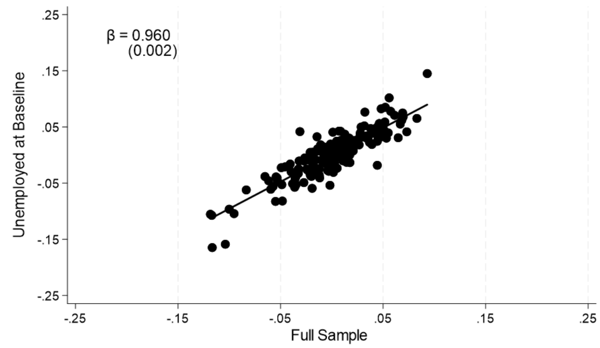
(f) Race/Ethnicity

Notes: The figures present estimates of the effect of the CCAR on benefit amount received including zeros from separate regressions using equation (4) for the quarters surrounding an application for the given subgroups. The 95% confidence intervals are shown in the shaded regions. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

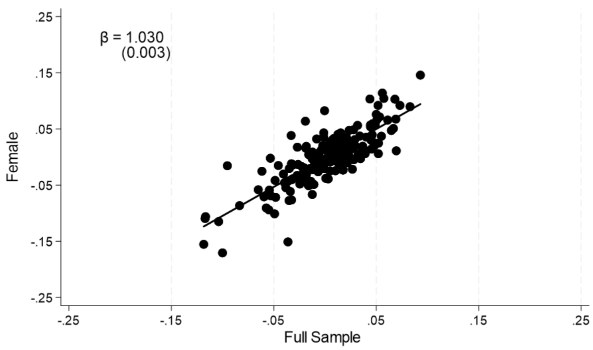
Figure A6: Group-Specific CCAR vs General CCAR



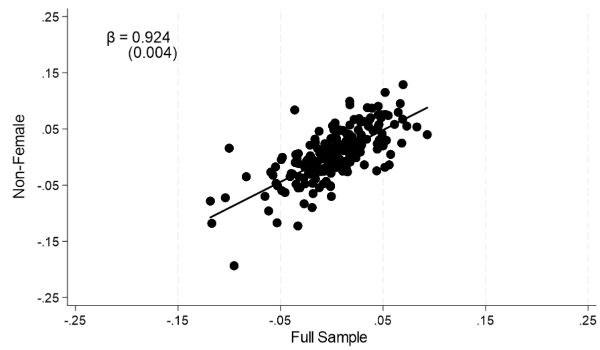
(a) Employed at Baseline



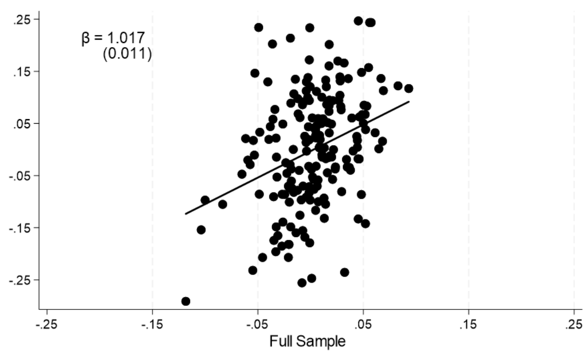
(b) Not Employed at Baseline



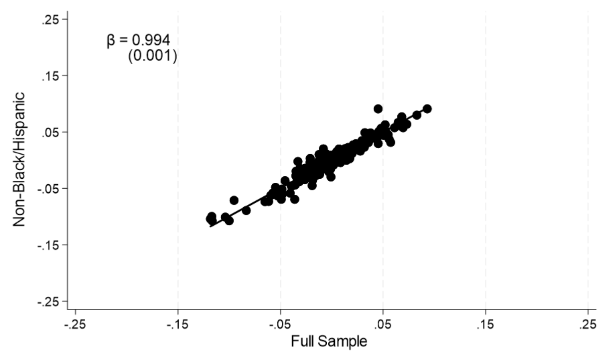
(c) Female



(d) Non-Female

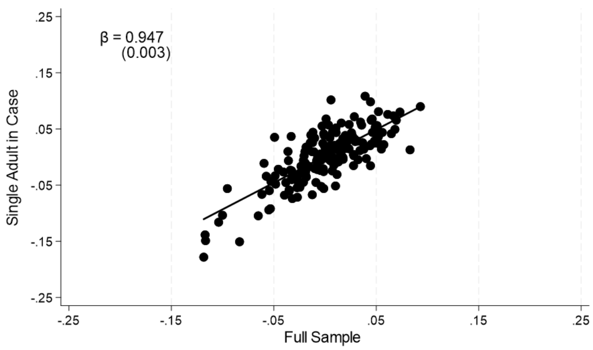


(e) Black/Hispanic

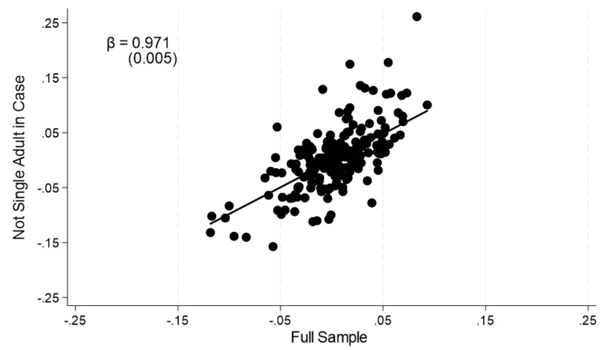


(f) Non-Black/Hispanic

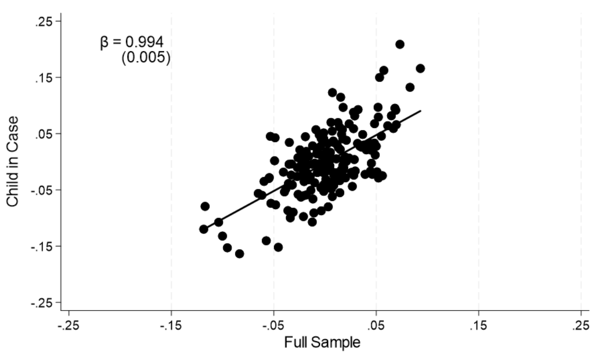
(Continued on next page)



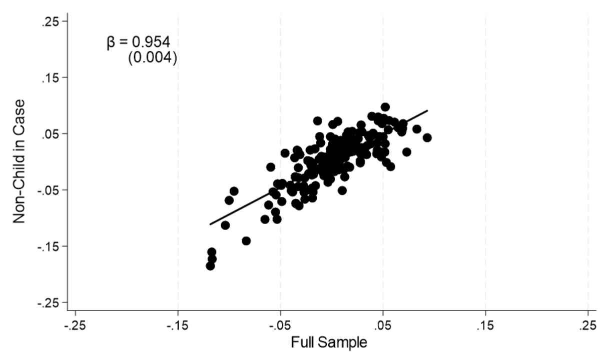
(g) One Adult in Case



(h) Non-One Adult in Case



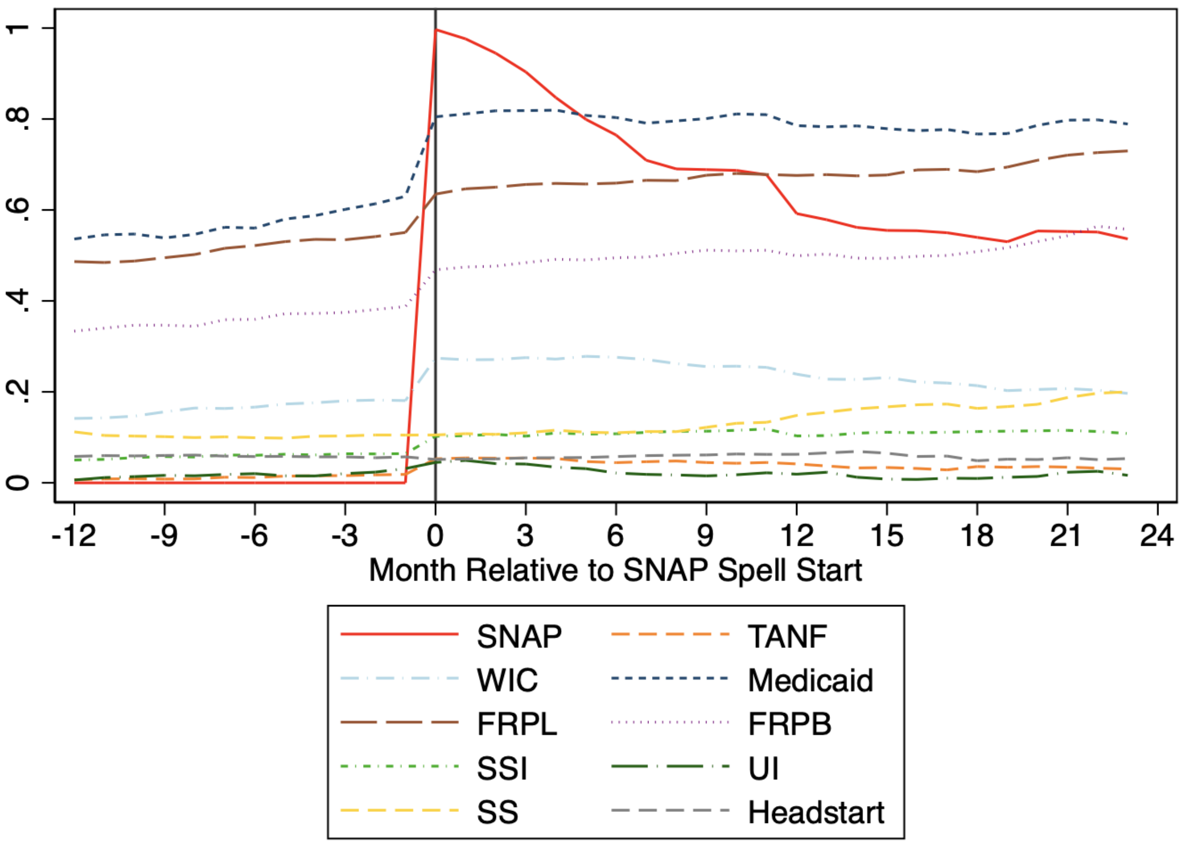
(i) Child in Case



(j) Childless Case

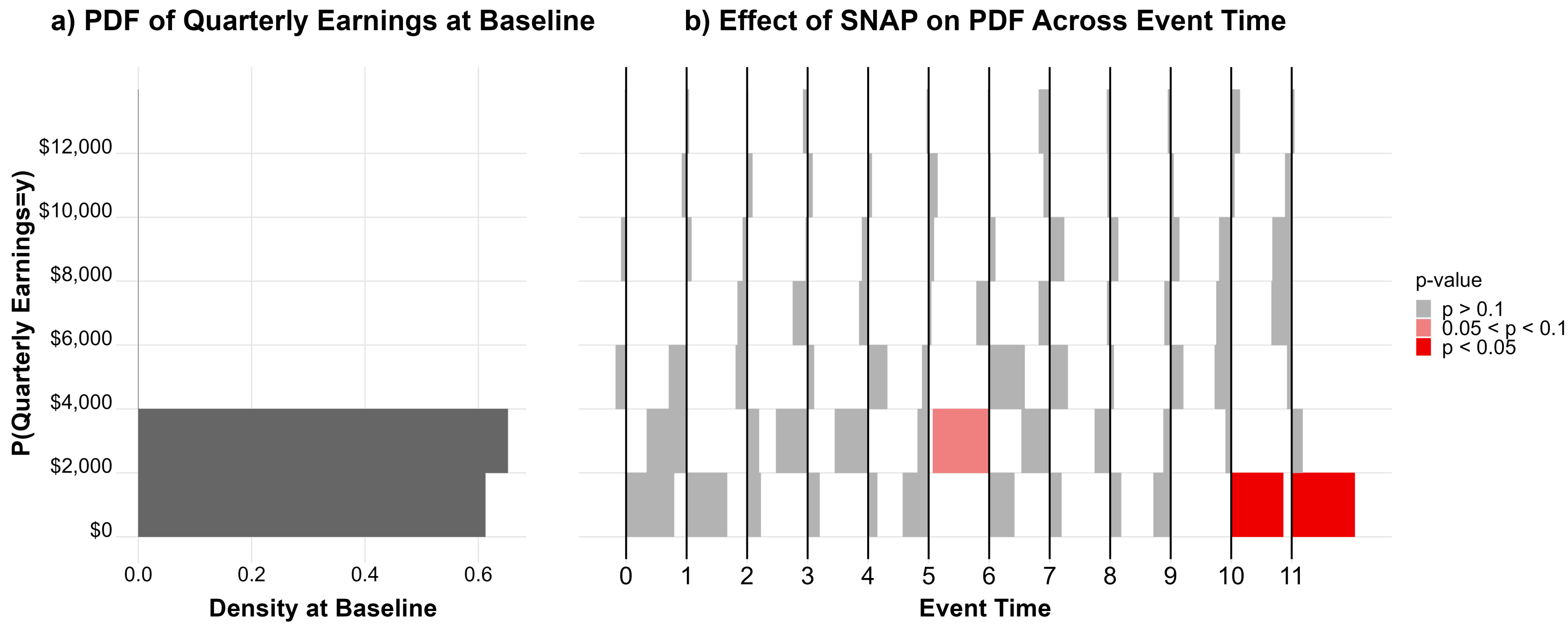
Notes: Each figure plots the CCAR for the specified subgroup (vertical axis) against the full-sample CCAR (horizontal axis). OLS estimates of the relationship between the two are displayed in the figure. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. Code adapted from Dobbie et al. (2018).

Figure A7: Cross-Program Participation Around First SNAP Spell



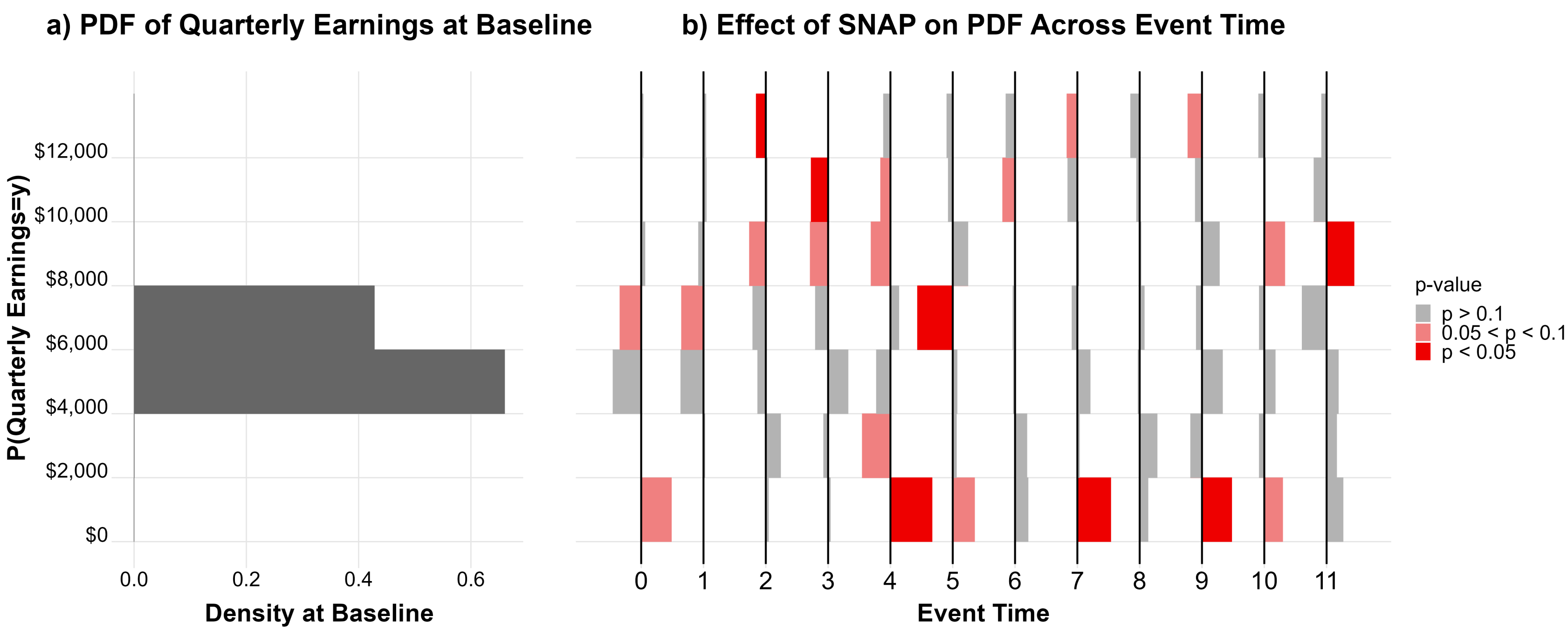
Notes: This figure plots the average household-level program receipt in the 2014 Survey of Income and Program Participation. We focus on households with heads who are ages 18-64 and who we observe transitioning from not receiving SNAP to receiving SNAP for the first time in the survey period. We weight observations using the SIPP-provided person weight in the month of SNAP participation initiation.

Figure A8: IV Estimates of the Effect of SNAP on Distribution of Quarterly Earnings (2012\$), for Applicants Employed Across Baseline with Baseline Earnings between $(\$0, \$4,000]$



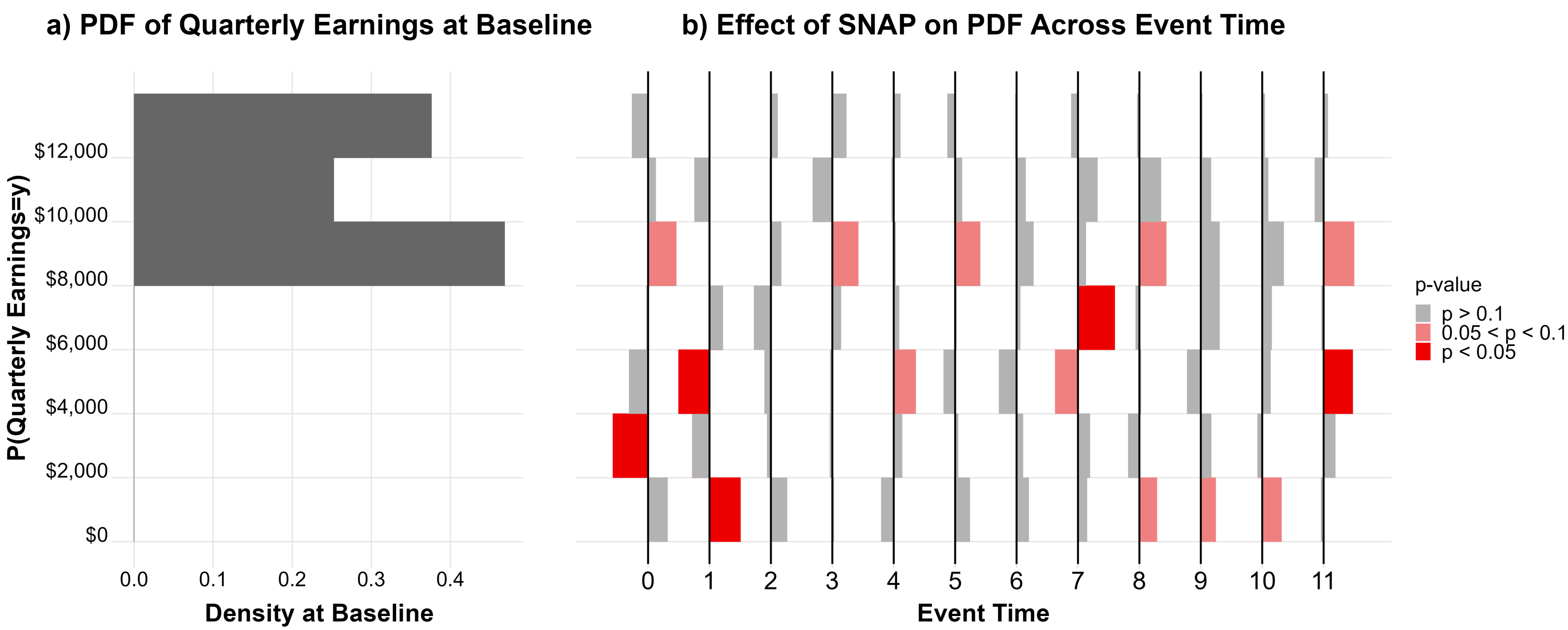
Notes: The left panel of this figure displays the share of employed SNAP applicants with earnings in the given range during the quarter before application. The right panel compiles results from running our main IV specification from equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The outcome variables are indicator variables for having quarterly earnings within the given earnings bin (on the y-axis) in the given event-time quarter (on the x-axis). Negative point estimates are reflected by a shaded area to the left of the given event-time vertical line and positive estimates are to the right. The coloring denotes the p-value of the given point estimate.

Figure A9: IV Estimates of the Effect of SNAP on Distribution of Quarterly Earnings (2012\$), for Applicants Employed Across Baseline with Baseline Earnings between $(\$4,000, \$8,000]$



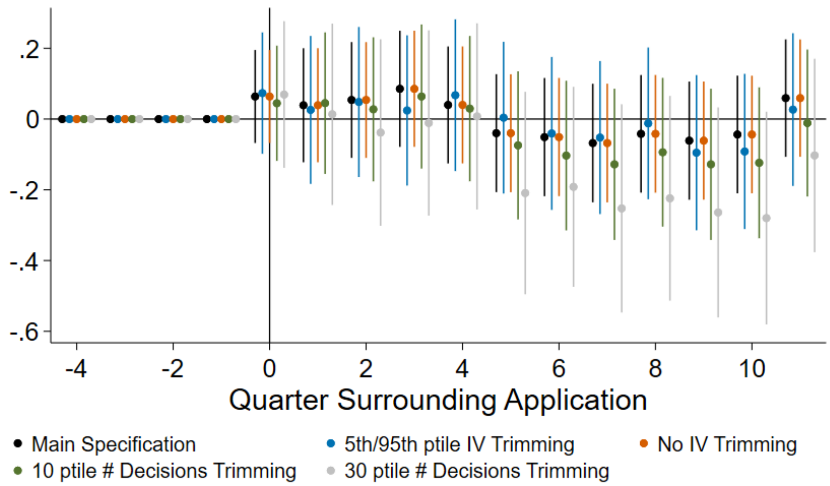
Notes: Panel (a) of this figure displays the share of employed SNAP applicants with earnings in the given range during the quarter before application. Panel (b) compiles results from running our main IV specification from equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The outcome variables are indicator variables for having quarterly earnings within the given earnings bin (on the y-axis) in the given event-time quarter (on the x-axis). Negative point estimates are reflected by a shaded area to the left of the given event-time vertical line and positive estimates are to the right. The coloring denotes the p-value of the given point estimate.

Figure A10: IV Estimates of the Effect of SNAP on Distribution of Quarterly Earnings (2012$), for Applicants Employed Across Baseline with Baseline Earnings above $8,000

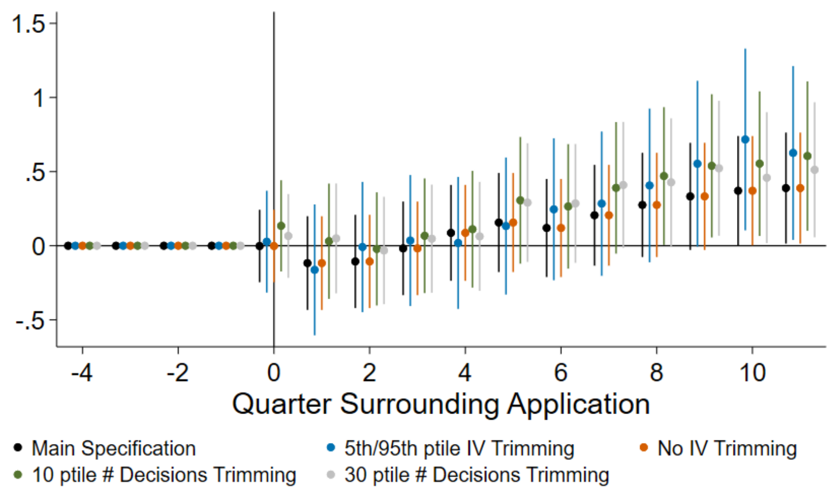


Notes: Panel (a) of this figure displays the share of employed SNAP applicants with earnings in the given range during the quarter before application. Panel (b) compiles results from running our main IV specification from equation (3) instrumenting with the CCAR. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. The outcome variables are indicator variables for having quarterly earnings within the given earnings bin (on the y-axis) in the given event-time quarter (on the x-axis). Negative point estimates are reflected by a shaded area to the left of the given event-time vertical line and positive estimates are to the right. The coloring denotes the p-value of the given point estimate.

Figure A11: Specification Sensitivity Checks for Employment



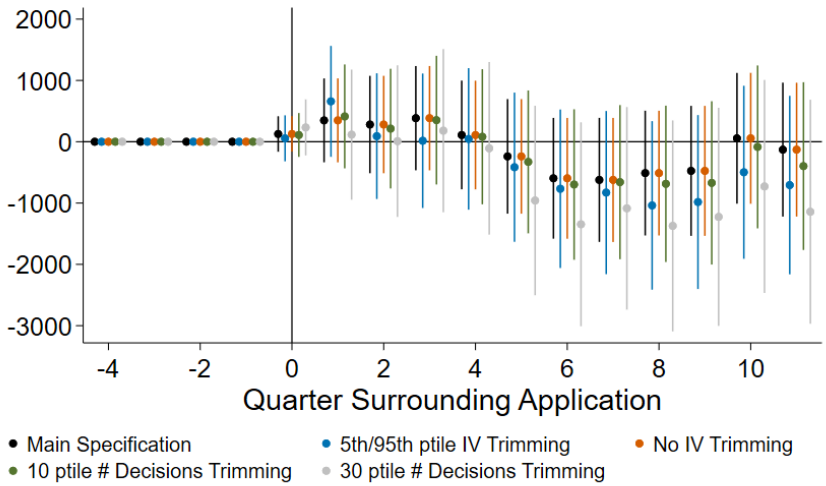
(a) Not Employed Across Baseline



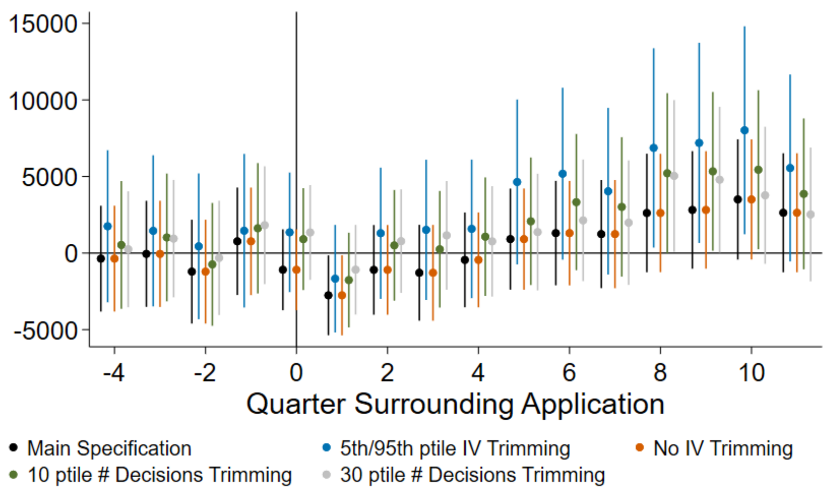
(b) Employed Across Baseline

Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. The "Main Specification" uses our primary sample. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. "5th/95th ptile IV Trimming" includes applications that were assigned CCAR values within the 5th to 95th percetile. "No IV Trimming" does not restrict the sample based on the CCAR values. "XX ptile # Decisions Trimming" changes the percentile cutoff for the minimum number of caseworker decisions per year in order for us to keep the caseworker and associated decisions in the sample.

Figure A12: Specification Sensitivity Checks for Real Quarterly Earnings



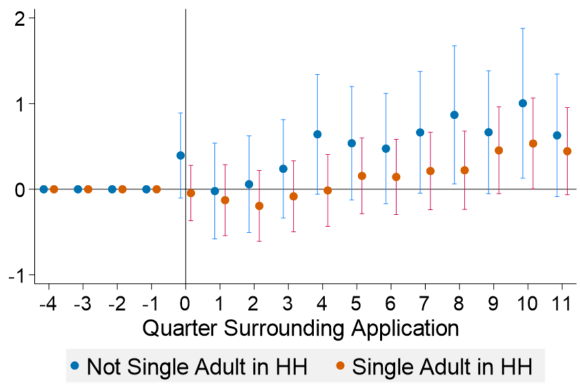
(a) Not Employed Across Baseline



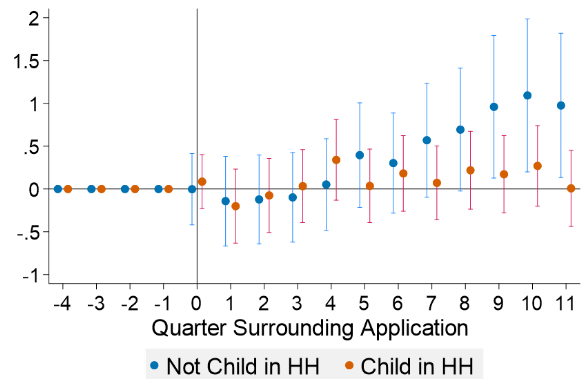
(b) Employed Across Baseline

Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. The "Main Specification" uses our primary sample. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. "5th/95th ptile IV Trimming" includes applications that were assigned CCAR values within the 5th to 95th percetile. "No IV Trimming" does not restrict the sample based on the CCAR values. "XX ptile # Decisions Trimming" changes the percentile cutoff for the minimum number of caseworker decisions per year in order for us to keep the caseworker and associated decisions in the sample.

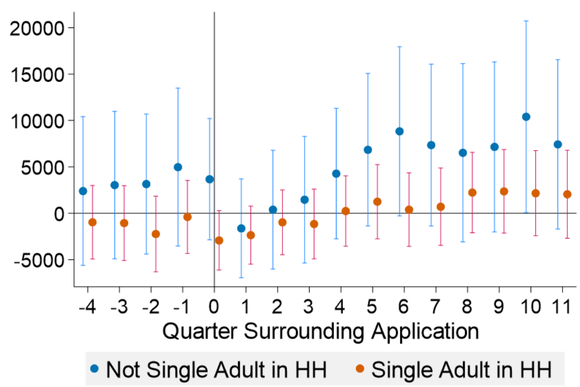
Figure A13: IV Estimates of SNAP on Employment and Earnings for Employed Across Baseline Subgroups



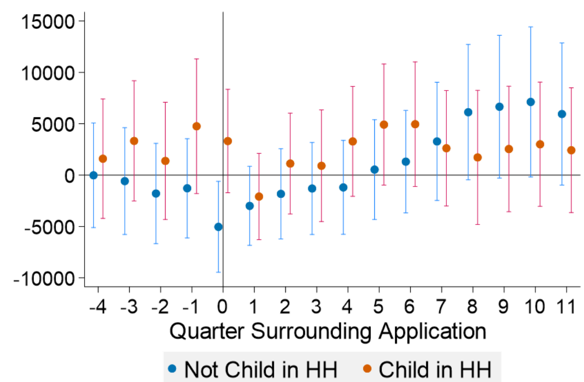
(a) Employment, by Number of Adults



(b) Employment, by Presence of Children



(c) Earnings, by Number of Adults



(d) Earnings, by Presence of Children

Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. The vertical lines display the 95% confidence intervals on the estimates. Our sample includes new applications between 2012-2016 who apply in General tracks. We exclude applicants assigned to caseworkers who handled fewer than the 20th percentile of applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.