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EVIDENCE FROM QUASI-RANDOMLY ASSIGNED SNAP CASEWORKERS

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

We are the first to document that Supplemental Nutrition Assistance Program (SNAP) caseworker behavior impacts program receipt, likely due to differing levels of helpfulness in navigating the complicated application process. We use the conditional random assignment of caseworkers as an instrument for SNAP receipt to assess the impact of SNAP on work decisions. Two-thirds of SNAP applicants do not work before applying and experience no change in work if granted SNAP. Those working beforehand decrease work temporarily. The canonical, static labor supply model cannot fully explain these results, but taking account of other reasons individuals do not work can.

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1 Introduction

Proponents of means-tested transfer programs argue they provide crucial resources to those in need, while critics claim they disincentivize work and lead to long-term "dependency" on government benefits. Recent policy discussions and proposals for the Supplemental Nutrition Assistance Program (SNAP) exemplify this debate. SNAP is a key part of the safety net in the United States providing debit cards to be used for food purchases. It expanded after welfare reform in 1996 to serve 37 million recipients monthly (pre-COVID) and it is now the "[o]nly truly universal means-tested transfer program in the USA" (Moffitt, 2002).

We propose and validate a new empirical strategy in the means-tested transfers literature that leverages variation in caseworkers' propensity to accept applicants as an instrument for SNAP receipt. With this design we provide important quasi-experimental evidence of the labor supply effects of the modern SNAP program. Static economic theory, where individuals trade off consumption and leisure, has strong predictions about the impact of means-tested transfers on labor supply–both the income and substitution effects imply a reduction in work (Hoynes and Schanzenbach, 2015). However, this discounts the possibility that SNAP improves health and human capital, relaxes the budget constraint to allow participants to purchase goods and services that complement work such as child care, and the possibility that SNAP increases job search effort and allows workers to be more selective in their job search, thus improving job quality in future periods.[1] Additionally, many SNAP recipients are in demographic groups that have low rates of work, regardless of whether they receive SNAP, for example because they are not of working-age or are disabled (CBPP, 2018).[2] In our sample, 66% of first-time SNAP applicants did not work *at all* in the year prior to applying. Past empirical research on labor supply has found mixed evidence on the impact of changing access to SNAP, and its predecessor Food Stamps. Some papers find reductions in work when access increases, whereas others find no effects (e.g., Hoynes and Schanzenbach, 2012; East, 2016; Homonoff and Somerville, 2021). Due to the lack of quasi-experimental variation in SNAP, these papers study very specific sub-populations or study the Food Stamp program in the 1960-70s.[3]

Our detailed administrative data from an anonymous single state allows us to ob-

[1] This is similar to predictions that Unemployment Insurance will subsidize job search and thus may increase job quality (Nekoei and Weber, 2017).

[2] Reasons these groups do not work at high rates are complex and include other policy decisions such as the provision of social security (Friedberg, 2000), labor market discrimination (Baldwin and Johnson, 1994), and physical and mental limitations preventing work. Investigating these reasons is beyond the scope of this paper.

[3] We describe this and a related literature on work requirements in section 2.3.

serve the caseworker assigned to each applicant and the earnings of all applicants, as well as whether they receive SNAP. Our approach is an examiner design using variation in caseworkers' propensity to accept a random SNAP application as an instrument for SNAP benefit receipt. Beyond acting as an instrument for benefit receipt, studying how caseworker discretion impacts SNAP participation is important and policy relevant. Caseworkers play a key role in the application process for SNAP. They are the first point-of-contact and the main resource for new applicants. Moreover, caseworkers conduct screening interviews and input all of the information in the computer system that ultimately determines an applicant's eligibility. Caseworkers must balance the goals of getting benefits to those who qualify and making correct decisions in a setting where their decisions are closely monitored.

Everyone in our sample has already chosen to apply for SNAP and has overcome the costs of initially doing so. However, the application process is complex and time consuming– about 40% of first-time applicants do not complete the process in our setting. Thus, caseworkers can be a key resource for applicants to help them complete their applications and submit requirement documentation by the deadline (Wu, 2021).[4] An important contribution of our work is documenting the presence of heterogeneity in caseworker behavior using data that allows us to observe individual caseworkers and showing this behavior has an impact on program receipt, which has not yet been done for any means-tested program in the United States.[5]

In our focal state, applicants are assigned to caseworkers in a conditionally random way. When an individual submits their application, they must conduct an interview within 30 days, which in our state happens almost exclusively over the phone. Based on demographic information submitted in the application, all applicants are assigned to an observable "track," which is a group of caseworkers that specialize in a certain type of application. We focus on the 65% of applications assigned to the General, Native American, and refugee tracks. Effectively this restricts the sample to working-age individuals, as elderly applicants have their own track, thus our estimates focus on the group more likely to change their labor supply in response to SNAP. Applicants can call in to the state-wide phone system anytime Monday-Friday during business hours to complete their interview and are assigned to the

[4]We shadowed a caseworker and witnessed firsthand the scope for individual caseworker behavior to influence program participation. For example, caseworkers can be less likely to follow up throughout the application process, which increases the likelihood that applicants fail to meet document submission deadlines and are automatically denied as a result.

[5]There is a literature on caseworker decisions in welfare-to-work and Unemployment Insurance programs, though this is focused on the decision of which type of program to place people in, rather than the decision of whether the person should receive *any* benefits (e.g., Bolhaar et al., 2020; Jonassen, 2013).

caseworker who happens to answer the phone.[6] We construct the Conditional Caseworker Approval Rate (CCAR) using the method proposed by Kolesár (2013); this allows us to measure the likelihood of each caseworker to accept a random application using a leave-one-out approach so that an applicant's own application decision is not included.

Conditional on application timing and assigned track, caseworkers should be as good as randomly assigned to applicants. We verify this empirically by showing that the CCAR (as well as caseworker's caseload and experience) is unrelated to applicant observable characteristics conditional on the fixed effects. Moreover, we document a strong first stage effect of the CCAR on SNAP receipt–a one standard deviation increase in the CCAR increases the likelihood of approval by 1.4 percentage points or 3% off the baseline approval rate of 51%. A significant increase persists for two years after the initial application indicating that those rejected do not simply reapply and receive benefits later. Investigating the mechanisms behind this effect, we find that caseworkers with higher CCARs are more likely to help applicants complete the application process. We also verify the CCAR satisfies the average monotonicity assumption laid out in Frandsen et al. (2023). Importantly for interpreting the results, applicants who are marginally receiving SNAP because of their assigned caseworker are similar to the full population of SNAP applicants in our state, so our results can be plausibly generalized to more than just SNAP applicants who are affected by their caseworker's CCAR.

We study individuals' first applications submitted between 2013-2017. We observe earnings from UI administrative data before and after an individual submits their SNAP application, regardless of whether they eventually receive SNAP. We leverage event-study-style designs to test whether the CCAR is systematically related to pre-application levels and trends in labor supply, and to examine dynamics in the labor supply effects after SNAP receipt, which has not been possible in prior research. We regress applicant's labor supply onto their assigned caseworker's CCAR for quarters surrounding their initial application up to one year prior and two years after. Our models include fixed effects for application timing and track, so we compare applicants exposed to the same pool of potential caseworkers. This research 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.

[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.

We find that on the full sample there is no labor supply response to the receipt of SNAP. However, this masks important heterogeneity when conditioning on whether the applicant worked in the year before applying for SNAP. Notably, only 21% of the sample had positive earnings every quarter of the year before applying for SNAP.[7] These applicants experience a large decrease in earnings if granted SNAP that lasts for only one quarter before rebounding and turning positive, though not statistically significant two years after initial application. To help understand this, we explore job quality proxied by median sector-level earnings, industry wage premium, and industry worker value-added. We find that those granted SNAP are suggestively more likely to work in a higher-paying sector and an industry with higher worker value-added than they did before applying for SNAP. These results are somewhat imprecise but are consistent with SNAP benefits allowing workers to search for a higher quality job. We then turn to the two-thirds of applicants who did not work at all during the year before applying. The size of this group is striking and indicates that many SNAP applicants are unlikely to work even in the absence of SNAP. This could be because of individual preferences about work, other policies that reduce the incentive to work, barriers to work such as discrimination, or a combination of these factors. Among this group of applicants, there is no meaningful change in labor supply for those who receive SNAP compared to those denied.

Our work contributes to several active literatures. First, many theoretical and empirical papers in Economics investigate the causes of incomplete take-up of transfer programs (e.g. Currie, 2006). Possible explanations for this include stigma, transaction costs and information costs. A related literature in Economics and other disciplines studies the role of administrative burden in determining program participation (e.g., Herd and Moynihan, 2019). Importantly, in our setting, all individuals have already submitted an initial application, so they have already learned about the program and think they may be eligible. We provide the first direct evidence of the role of caseworkers in determining participation in a means-tested transfer program in the U.S. Our findings suggest that caseworkers help applicants navigate the complex application process. This complements other work showing that connecting likely eligible nonparticipants to SNAP application assistance increases their likelihood of receiving SNAP benefits (Schanzenbach, 2009; Finkelstein and Notowidigdo, 2019).[8] Importantly, many applicants do not complete the application process and we show assistance matters even among those who already submitted their initial application for SNAP; so, interventions that encourage initial applications will only do so much to solve incomplete

[7] Results are similar if conditioning on shorter periods of work pre-application.

[8] Work 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).

take-up as long as the application process remains as complex as it currently is.

While policy-makers have the explicit goal of increasing take-up of means-tested transfers, including SNAP, economic theory is less clear about whether incomplete take-up is optimal. This is because the barriers that lead to incomplete take-up may help screen out the less needy. Our work suggests that reducing the cost of completing the SNAP application process by providing assistance increases participation. We find no consistent evidence that these marginal recipients are better or worse off than the average applicant or average beneficiary.[9] Past work has mixed findings on whether reducing application costs improves targeting. The closest paper to ours finds assistance and information interventions for likely-SNAP-eligible reduce targeting, but this is among a different population on a different margin–elderly SNAP non-participants enrolled in Medicaid who have not applied for SNAP benefits (Finkelstein and Notowidigdo, 2019). Our findings highlight the importance of understanding the impacts of other interventions within the same program to fully understand the targeting impacts of administrative burdens.[10]

Finally, we contribute to the sparse literature studying the effect of access to modern SNAP on labor supply decisions.[11] Our data allow us to study the dynamics in labor supply effects and to study heterogeneous effects by pre-application characteristics. This gives a more complete picture of the labor supply effects and their mechanisms than has been possible before. Our results show that the impact of SNAP on labor supply is more complicated than the static labor supply model predicts in two ways. First, prior scholars have suggested SNAP participants have low labor force attachment for reasons besides the incentives created by the SNAP benefit structure. Our data is uniquely suited to examine this and we are the first to show that SNAP applicants indeed have very low attachment to the labor force *before* applying. Moreover, we find those not working before applying do not change their labor supply whether or not they are granted SNAP benefits. This highlights the need to take other factors, beyond the benefit structure, into account when analyzing the expected labor supply impacts of transfer programs, and it highlights the value of administrative data like ours. Second, for those working before applying for SNAP,

[9] Note that in our setting, caseworker behavior does not influence the eligibility rules or eligibility verification steps directly.

[10] Improved targeting is not enough to say whether overall social welfare is improved from a given intervention (Finkelstein and Notowidigdo, 2019).

[11] Only one study that we are aware of examines this; Homonoff and Somerville (2021) use the timing of recertification interviews as an instrument for SNAP receipt–an interview date later in the month increases the likelihood of missing the interview and falling off the program–and they find imprecise null effects on labor supply and cannot rule out sizeable effects in either direction. Rather than studying the effect for those recertifying SNAP eligibility, we examine a different and important group–those applying for SNAP for the first time. Entering and exiting SNAP may not have symmetric effects on labor supply.

our results are suggestive that individual behavior is more consistent with a dynamic labor search model, where workers use SNAP benefits to increase their job search effort in order to get a higher quality job. This is important as it indicates that predictions from static economic theory will overstate the reduction in work due to SNAP. This also has important implications for the cost-benefit analysis of SNAP–the fiscal externalities due to reductions in work are likely lower than previously thought. This is because: 1) the majority of SNAP applicants do not work, regardless of whether they receive SNAP, 2) the small percentage that do reduce work in response to the receipt of SNAP do so only temporarily, with no evidence of longer-term reductions in work in favor of dependency on government benefits, and 3) if SNAP improves job quality in the longer-run this will increase tax revenue in the longer-run.

The rest of the paper proceeds as follows. Section 2 provides background on the SNAP program and related literature. 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 SNAP Background

2.1 SNAP

SNAP is a means-tested federal entitlement program, but states are responsible for determining eligibility and paying out benefits. Additionally, in the last several decades, states have been given more flexibility in setting their own eligibility rules and deciding what administrative procedures to use to screen applicants.

In general, to qualify for SNAP, applicants must have gross income below 130 percent of the federal poverty level and net income below 100 percent of the federal poverty level. Net income includes 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. The 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). The benefit formula also incentivizes having earned income over other sources of income through the earnings disregard when calculating net income.[12] This reduces the benefit reduction rate on earned income to

[12]States also have the option of implementing categorical eligibility rules when determining SNAP eligibility which smooths benefit fadeout as income increases and reduces labor supply disincentives. In our sample period our state of interest did not takeup this option.

24%. 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.

Since welfare reform in 1996, SNAP includes work requirements for Able Bodied Adults Without Dependents (ABAWDs). ABAWDs are ages 18-49, report having no disabilities and are not pregnant, and do not take care of any dependents (e.g., children, people with disabilities, or the elderly). ABAWDs are eligible for three months of SNAP benefits within a three-year period unless they work, volunteer, or participate in a qualified work program for at least 80 hours a month. However, there are exemptions to these requirements in times of economic hardship. In our state, 4% of all recipients are subject to ABAWD work requirements.

SNAP also includes General Work Requirements that focus more on tasks related to becoming employed. Non-exempt adult recipients who are aged 16-59 are considered "work registrants" and must satisfy General Work Requirements. Work registrants must register for work or participate in SNAP Employment and Training, not voluntarily quit or turn down a job offer, or not voluntarily reduce hours below 30 per week. Individuals can be exempt from these requirements by working at least 30 hours per week or having weekly earnings equivalent to 30 hours of minimum wage work, by meeting work requirements for another program, by taking care of children under 6 or an incapacitated person, by having a physical or mental disability, by participating in a drug or alcohol program, or by being enrolled in school or a training program.[13] Work registrants who fail to comply risk temporarily losing benefits; however, administrators in our focal state reported that work registrants are not commonly sanctioned because good cause exemptions are typically found. In our state 16% of all recipients are subject to these work requirements and not subject to additional ABAWD requirements.[14] In thinking about the LATE we estimate, caseworkers are told not to council applicants on how to meet work requirements, so caseworkers have little ability

[13]https://www.fns.usda.gov/snap/work-requirements

[14]A small set of work registrants must also adhere to mandatory Employment and Training (E&T) requirements. Mandatory E&T applies to work registrants who are younger than 47, are determined to be able to work, have no earned income, and are not exempt though other state-specific criteria. In our state, mandatory E&T requires SNAP recipients to complete online trainings, make 16 job contacts a month, and meet with their employment officer once a month. However, our state applies mandatory E&T to a very small group of work registrants (less than 300 a month) who could benefit from additional job training. Work requirements are communicated in several ways to new recipients. First, new recipients are verbally notified of the work requirements they must satisfy, along with the accompanying sanctions for non-compliance, during their initial interviews. Then in the first few weeks after approval, recipients receive a mailed notice that again outlines relevant work requirements and sanctions for non-compliance (see Appendix Figure A1 for an example).

to influence whether the applicant meets them.

Critics of these work requirements argue they are ineffective at incentivizing work[15] because many SNAP recipients are unlikely to work regardless of whether they receive SNAP since they are in demographic groups that have low labor force attachment generally (CBPP, 2018). Moreover, among non-disabled working-age adults who receive SNAP, many already work. Our sample is restricted to be mostly working-aged heads of household, so we are already looking at a sample more likely to work than all SNAP recipients. However, among this group, 20% are in a household with someone who has a disability (including the head themself), 58% have children and 32% have children of pre-school-age. Thus, some people in our sample may face limitations on work due to their demographics.

For those who qualify, SNAP benefit amounts are a function of the household size and net income. Within our sample of recipients, the average household receives \$237 in monthly benefits in 2012 dollars. These benefits are paid out automatically each month on electronic benefits transfer (EBT) cards, which can be used like a debit card for qualifying food purchases at SNAP-accepting retail stores.

2.2 SNAP Application Process and Caseworker Behavior

The application process for SNAP (and means-tested transfer programs in general) 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). The application process for SNAP is complicated and time consuming. Individuals must first submit an application and supporting documentation before completing a required screening interview and then providing any missing information.

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 A2. 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, 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 doc-

[15] We discuss the economics literature on this point below.

umentation on initial application submissions. Applicants have 30 days to submit all the necessary information or else 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.

Surveys 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.[16] 10% of applicants who did not complete the process said they dropped out because of some aspect of the process and another 46% cited that 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, caseworkers, 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 caseworkers' specialization. This ensures that caseworkers have the relevant skills, such as language skills or knowledge of special program rules to handle applications for specific subgroups. Each caseworker handles cases from all over the state and the caseworkers work together in call centers located around the state.[17]

The second useful institutional feature is that the mandatory interviews with caseworkers are also done almost exclusively over the phone and 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.

[16] 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).

[17] Prior to 2013, teams were also organized around physical locations and the applications were automatically sorted to the closest office. In 2013, the state moved to a state-wide model where caseworkers serviced applications from across the state. In 2000, only 1 state operated a state-wide call center for SNAP, but by 2016, 32 states were operating them.

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.[18] 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 to those who qualify and 2) they want to avoid errors in their decisions.[19] There are several layers of review of caseworker decisions in our setting. 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, though they can use the funds that would go towards the fine to improve their program operation instead. In our sample period, over-payment rates (Type II Errors — Kleven and Kopczuk, 2011) are between 3-6% on average across states. Our state of interest is not fined in our period and has relatively low error rates in general. In addition to this federal monitoring, our state has an Editing Team, which is not required by the federal government, but our state chose to run this program. The editors 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 our analysis) have a larger percent of the cases reviewed per month. Caseworkers who fall below a rate of 90% accuracy face consequences. These consequences vary and can include additional individual mentoring and coaching, a written warning, and 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

[18] At a minimum applicants must submit their name and address to begin an application.

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

of variation in caseworker behavior is how helpful they are at guiding applicants through the complicated application process. 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, 2021). Though, this change was also accompanied by increases in wait times and backlogs in processing applications, so the exact mechanism is unclear. Additionally, Finkelstein and Notowidigdo (2019) found that connecting likely SNAP-eligible nonparticipants to caseworker-like assistants significantly increased their program receipt.[20]

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.

2.3 SNAP and Labor Supply

The canonical static labor supply model where individuals trade off consumption and leisure predicts that access to SNAP will reduce work. This is due to both the income effect from the SNAP benefit amount, and the substitution effect from the benefit reduction rate. The benefit reduction rate lowers the effective wage by reducing benefits as they work more, so the price of leisure is lower. However, this model ignores several potentially important aspects of SNAP that may lead to a more complicated and ambiguous labor supply response. In particular, past work has shown that SNAP improves the health of children and adults (Almond et al., 2011; Bronchetti et al., 2018; East, 2020). Improved health of adults, and the children they care for, may lead to increased work. Additionally, SNAP benefits may allow individuals to search more intensively and selectively for a job while they receive SNAP and thus improve job quality for those who switch jobs. Finally, those who apply for SNAP may not work regardless of receipt of SNAP for a variety of reasons and our data support this since two thirds of applicants in our sample do not work prior to application.

Studying this effect empirically has been challenging because SNAP is a federal program without large variation in eligibility rules or benefit amounts. Existing quasi-experimental research on SNAP and labor supply must find creative ways of identifying the effects of

[20]There is 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 with local SNAP caseloads even though it is a federal program.

SNAP.[21] Hoynes and Schanzenbach (2012) take advantage of the rollout of the Food Stamp program–the precursor to SNAP–in the 1960-70s and find reductions in labor supply for single-female-headed households. East (2016) looks at changes in immigrants' Food Stamp eligibility across states and time in the 1990-2000s and similarly finds reductions in labor supply for low-educated heads of household.

Several other recent papers have used linked administrative data to study the more modern labor supply effects of specific SNAP policy rules on participants. Bitler et al. (2021) examine the intensive margin labor supply response to kinks in the SNAP benefit formula and find little evidence of SNAP participants adjusting work in response to the kinks. Two papers use a regression discontinuity in age to study the effect of the work requirements imposed on ABAWDs who are younger than age 50. The first links SNAP administrative data to the American Community Survey and finds that work requirements reduce SNAP participation and have no effect on labor supply (Stacy et al., 2018). The second paper uses similar data to ours in that it is administrative SNAP data for a single state (Virginia) linked to that state's UI earnings records (Gray et al., 2022). Among a cohort of ABAWD SNAP recipients who were exposed to the imposition of work requirements, there is a large reduction in SNAP participation, no effect on employment, and some evidence of longer run increases in wages for individuals who may already be near the earnings threshold to meet the work requirements.[22]

While these papers use high quality data to study the labor supply of SNAP participants, they focus on very specific policy rules within the SNAP program. We take a different approach and look at the effects of a policy-relevant instrument–caseworker approval rates. Given our empirical approach, we identify these effects on the marginal SNAP recipient, which, as we show below, is very similar to the average SNAP recipient. Additionally, our data allow us to estimate the dynamic effects of SNAP on labor supply and heterogeneous effects based on pre-application work, neither of which have been studied in the existing literature. This is important not only to understand the full effects of SNAP on labor supply, but to understand the mechanisms by which these effects operate.

[21] Earlier work used structural modelling to identify this effect as summarized by Moffitt (2002).

[22] Vericker et al. (2023) find similar results using a pre vs. post approach studying the reinstatement of work requirements on all participating ABAWDs, regardless of age, in 3 states with linked SNAP benefit and UI earnings data. Several other papers use survey data and have mixed findings of the effect of work requirements on labor supply though some find evidence of increases in work due to work requirements (Cuffey et al., 2022; Han, 2022; Harris, 2021).

3 Data

Our data come from a single state 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 any recertification information. For those who do not receive SNAP, we observe the reason for denial, which we use to understand the mechanisms through which caseworker behavior affects applicants.

Additionally, we have quarterly labor supply information from the state's Unemployment Insurance (UI) database matched to the heads of household in each SNAP case. The state only matched to the head of the household instead of every person in the household 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 Data (a nationally representative sample of SNAP recipients) only 2% of all SNAP recipients are in dual-income households and among a sample of income-eligible households in the Current Population Survey only 10% are dual-income households.[23] These UI records contain the earnings, industry, and number of jobs of each individual by quarter from 2011-2021. Importantly, we can observe these outcomes even for SNAP applicants who are denied. 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 a problem for our analysis. 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 likely eligible for SNAP are self-employed, and among those receiving SNAP only 1% are self-employed. We assume that individuals who are not observed in the UI data are not working and assign them a value of 0 for their earnings.

3.1 Caseworker and Decision Assignment

One complication in the data is that for a given applicant and application date, there can be multiple decisions made. For example, if an applicant fails to submit all the required materials within 30 days of the application date, they are automatically denied by the system. However, if they then submit their required materials within 60 days, the case would be

[23] Quality Control Data: https://snapqcdata.net/datafiles?page=0

reopened without needing to start a new application. 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.[24] We address these complications by keeping the final decision related to an initial application, but attribute this decision to the *first-assigned* caseworker. Results are nearly identical if we instead use the first decision on the application or include all decisions. We prefer to use the last decision since it reflects the final outcome.

Finally, 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 case has an accompanying recertification record corresponding with the given initial application date. We also consider cases approved if the case receives benefits during either the month of or the month after the initial application date. Otherwise, we consider the application denied.

3.2 Sample Construction

We construct a sample that allows us to cleanly identify the effects of caseworker subjectivity. To so do, we begin with a sample of 152,004 first-time applications that were submitted between 2013-2017. First applications are important in our context because this helps to isolate the initial impact of SNAP as it abstracts away from possible dynamic labor supply effects that could occur from prior SNAP receipt. 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 study this group, we look at each applicant's first observable application and remove any applicants for whom we observe SNAP receipt within two years prior to their first application.[25] We focus on applications beginning in 2013, so that our sample starts after the implementation of the state-wide call center model that generates the quasi-random caseworker assignment to applicants. We select 2017 to end our analysis sample in order to examine quarterly labor supply outcomes up to 2 years after application

[24] 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.

[25] We use a rolling two-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 not impacted if we instead condition on not having any SNAP receipt prior to the application.

and still exclude the COVID-19 pandemic. Next we limit the sample to applications handled within General, Refugee, and Native American tracks (106,630 observations). Assignment of caseworkers in these tracks is the most plausibly random given the many applicants and many caseworkers in each of these tracks. 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. Roughly 15% of all SNAP recipients are over age 64, so we are not able to speak to effects on this population.

We further limit this sample by dropping applications assigned to workers who handled relatively few cases that year—specifically, the bottom quartile of the distribution of the number of cases within the given track that year.[26] This sample restriction is important for two reasons. First, having enough decisions for each caseworkers helps ensure that there are enough observations to get an accurate estimate of worker decision making. Second, new caseworkers who are undergoing training are non-randomly given applications of varying complexity. These trainees also handle fewer applications than their colleagues. As a result, dropping observations handled by workers with a low number of cases ensures that the workers we analyze are fully trained and that their cases are randomly assigned, leaving us with 80,187 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. These restrictions leave us with our final regression sample of 68,901 application decisions.

3.3 Descriptive Statistics

We compare the demographics of our sample to those of a representative sample of working-age SNAP recipients from the SNAP Quality Control (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 main sample restrictions and look at all applicants and all recipients in our main analysis sample, respectively. Applicants and recipients in our sample are slightly younger with slightly smaller households than the full

[26]For the General track, the largest track in the system, the 25th percentile occurs at 98 cases a year. The cutoff for Native American and Refugee tracks fall at 39 and 47 cases in the given year, respectively. Results are robust to modest changes to these cutoffs.

sample of recipients in column 3. They are also slightly less racially diverse.[27] This is both because we restrict our sample to be first-time applicants and because we exclude many elderly and non-white applications by focusing only on three tracks. While these restrictions change the sample somewhat, we believe the trade-off is worthwhile in order to be able to cleanly identify the effect of caseworker behavior and SNAP initiation on labor supply.

Several other statistics are worth noting for interpreting the results in the next sections. First, only 51% of applicants receive benefits in the quarter of application. Even looking at the probability of receipt in the entire year after the application, this fraction remains very similar.[28] Additionally, only 28% of applicants are working in the quarter before application, and applicants have only \$1,325 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–\$4,446. To give a frame of reference for this, one person working full time at minimum wage for a full quarter would earn \$3,770. This is also similar to the quarterly household income that puts a household of two just at the poverty line – \$3,782 (in 2012).

Our sample of SNAP recipients (column 5) are less likely to be working pre-application (25%) and have lower earnings (\$1,113) compared to all applicants. Turning to the post-application labor supply (one quarter after application), we see that recipients are more likely to work than before application, but they have lower earnings compared to pre-application. These statistics are also similar to the full samples of beneficiaries in columns 1-3.

4 Empirical Methods

We take a different approach from the prior research that exploits changes in program rules and access that target specific subgroups, and, instead, we study the role of caseworkers in determining SNAP receipt. We then use caseworker propensity to accept as an instrument for SNAP receipt. Caseworkers should be randomly assigned to a given application in our sample, conditional on the timing and track of the application. This random assignment

[27]Our data have a lot of missing values for the race and ethnicity variables. We assume if the variable value is missing the individual is white given the demographics of our state, but these variables should be interpreted with some caution.

[28]This is slightly higher than the 44% acceptance rates in Los Angeles during this same time period (Giannella et al., 2023).

of caseworkers to applicants is critical to estimating the Conditional Caseworker Approval Rate (CCAR). If cases are randomly assigned to workers, then workers see the same mix of cases and their caseloads have the same baseline likelihood of being approved. As a result, differences in average caseworker approval rates must be driven by caseworker behavior. The CCAR quantifies and aggregates caseworker behaviors into a single measure.

We follow the newer examiner-effects literature to inform how we create the CCAR using the UJIVE approach ("unbiased jackknife instrumental variables estimator"). Bringing this examiner-effects methodology into the setting of the safety net to document heterogeneity in caseworker approval rates is an important contribution of our paper. Kolesár (2013) proposed the UJIVE and it has been used in other recent papers including Norris et al. (2021) and Agan et al. (2023).[29] 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 (respecitvely λ_a and ϕ_a), which determines the set of caseworkers the applicant may be assigned to and is the level of randomization.[30] Equation (2) adds caseworker fixed effects (ρ_c). We then calculate $\widehat{Approved}_i$ – 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 to whom they are assigned, netting out any heterogeneity due to application timing and assigned track, and the caseworker's decision on the focal application.[31]

There is considerable variation in the CCAR across caseworkers as shown in panel (a) of Figure 1. The standard deviation in our sample of the CCAR is 0.05. A 10 percentage point increase in CCAR is associated with a 15 percentage point increase in the likelihood

[29]This 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 of doing a better job of accounting for covariates and being relatively easy to compute (Norris et al., 2021). We have experimented with alternative estimators but they provide us with less precision, possibly because of the relatively small numbers of application decisions per caseworker in our setting.

[30]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.

[31]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. Approval includes additional information beyond whether actual benefits were received—see Section 3.2. It is possible to be approved, but still not appear in the benefits file as a beneficiary. The CCAR is very similar when using benefit receipt instead.

of approval (panel b). This is a 29% increase of the overall approval rate in our sample of 51% (Table 1), though this is not a causal estimate. As a test of the exogeneity of caseworker assignment and CCAR, we regress the assigned caseworker's caseload, months of experience, and CCAR onto baseline applicant demographics from the application and UI data—conditional on application timing and track fixed effects. We also 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 143. 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 0.34 to 1.44. This provides evidence that caseworker assignment and our instrument (CCAR), is indeed random, conditional on the fixed effects.

The empirical evidence from Table 2 supports the independence assumption that the CCAR is unrelated to determinants of labor supply. We demonstrate below that the CCAR is also strongly causally related to SNAP receipt (the first stage). 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), CCAR is a valid instrument for SNAP receipt. We address these assumptions in section 5.1 below.

Using the CCAR as an instrument for SNAP receipt, we estimate the following:

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

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

where y_i is the labor supply outcome of individual i. We instrument for the receipt of SNAP benefits in the quarter of application ($ReceiveSNAP_i$) with the caseworker's CCAR. We similarly include fixed effects for the application-date-by-track to ensure that we compare applicants who are exposed to the same set of potential caseworkers. This approach allows us to estimate the local average treatment effect (LATE) for SNAP applicants for whom caseworker assignment matters for SNAP receipt. Following best practices from recent design-based approaches to inference, we use heteroskedasticity-robust standard errors, but do not adjust for clustering (Abadie et al., 2022).

We estimate this model separately by quarter around the initial application in order to produce event-study-style plots including quarters from one year prior to two 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 across the CCAR values. If we see that these outcomes prior to application were unrelated to the CCAR of the assigned caseworker, this provides 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 whether the *levels* are different. So, this method tests a stronger assumption than if we were to run standard panel-style 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: 1) the applicant receives SNAP due to their caseworker's CCAR ("treated compliers") and 2) the applicant is denied due to their caseworker's CCAR ("untreated compliers"). To do this we use the method suggested by Frandsen et al. (2023). 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. Intuitively, this gives us the average outcome for those who receive benefits *because* of their caseworker's CCAR. 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.

5 Results

5.1 The Role of Caseworkers

We begin by studying the effect of the CCAR on the likelihood of an applicant receiving SNAP. Figure 2 shows that applicants do not receive SNAP prior to the application, which is mechanical since we keep first applications only. There is a large and significant effect of the CCAR on benefit receipt beginning in the quarter of application.[32] We report the coefficients and standard errors associated with this figure in the first column of Table 5. The coefficients indicate the effect of a unit increase in the CCAR, however the CCAR in our sample ranges from only -0.14 to 0.13. So, to interpret this coefficient, we scale it by a one standard deviation increase in the CCAR (0.05). A one standard deviation change increases the likelihood of benefit receipt in the quarter of application for the full sample by 1.4 percentage points, which is a 3% effect of the overall rate of acceptance of 51%. The F-statistic

[32]We aggregate the monthly benefit information to the quarterly level to match the UI data timing.

for the estimate on benefit receipt in the quarter of application is 54. 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 fact that the largest effect of the CCAR on benefit receipt occurs in the quarter of application motivates our choice to use this as the endogenous variable in Equation (3). 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 (Homonoff and Somerville, 2021).

We explore whether those who are denied SNAP because of their assigned caseworker re-apply after the initial quarter of application. Roughly 10% of applicants denied because of their assigned caseworker reapply in the quarter following the initial application and the likelihood of reapplication decreases in subsequent quarters (Appendix Figure A3).[33] Note, we cannot look at the effect of the CCAR on the likelihood of reapplying *among those denied* because this would condition on the endogeneous variable. Similarly, we do not look at recertification length as an outcome because this is only observed for those who receive SNAP.

To understand who is pushed into receiving SNAP because of their caseworker, we explore the characteristics of compliers following the method proposed by Frandsen et al. (2023). This method provides the average characteristics of those who receive SNAP because of the caseworker they are assigned (the compliers). We can then examine if the compliers are similar on these characteristics to all SNAP applicants and SNAP recipients. To do so, we estimate equation (3) instrumenting with the CCAR, but replace the labor supply outcome with various case characteristics interacted with an indicator for whether the case received SNAP during the quarter of application.[34] The first row in Table 3 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 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

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

[34]Specifically, we estimate: $y_i * ReceiveSNAP_i = \beta ReceiveSNAP_i + \theta_a + \zeta_i$ using $CCAR_i$ as an instrument for $ReceiveSNAP_i$. 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 the CCAR of their assigned caseworker.

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 the first five columns we explore measures of pre-application labor supply. The differences are mixed in the sense that compliers are underrepresented on some measures–less likely to work and have lower industry experience at baseline–and are over-represented on other measures–higher pre-application earnings. However, none of these differences are statistically significant from the full samples of applicants and beneficiaries. This suggests that caseworker assistance does little to change the targeting of SNAP based on pre-application labor supply.

Compliers are roughly 40 percent more likely to be female than all applicants and beneficiaries and this difference is statistically significant. Compliers are also a few years younger and less likely, though not significantly so, to be Black or Hispanic. This suggests that younger applicants and female applicants are more responsive to the assistance provided by caseworkers.[35]

Overall, we interpret this evidence as indicating that the applicant who marginally receives SNAP because of their assigned caseworker looks similar to the general population of applicants and beneficiaries. Given these findings, it is possible that the LATE we estimate could apply to the population of beneficiaries more generally.

5.1.1 Mechanisms Behind the Effect of the CCAR

To study the mechanisms through which the CCAR affects approval 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 customer, or those that failed to include all the required documentation. This is the most common reason for denial–78% of applicants who are denied are denied for this reason (our overall denial rate is 49%). In Table 4, we regress the CCAR on whether the given application was incomplete, conditional on application-date-by-track fixed effects. We find that a higher CCAR is negatively related to incomplete applications. A one standard deviation increase in the CCAR decreases the likelihood of being auto-denied by 1.3 percentage points, 3% of the sample mean. This suggests caseworkers with a higher CCAR are more helpful in ensuring the applicant submits all the necessary information. This is in contrast

[35] Another possibility is that caseworkers with higher CCARs are more likely to help female (or younger) applicants and caseworkers with lower CCARs are less likely to help female (or younger) applicants. We view that as very unlikely given the level of oversight in the caseworkers' work environment.

to the findings in Finkelstein and Notowidigdo (2019), who show that likely-SNAP-eligible individuals who are pushed onto applying are *more* likely to be rejected due to incomplete applications. The difference is likely because in our setting all individuals have taken the first step to apply for SNAP benefits, 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 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 (CCAR). Until recently, papers using "examiner fixed effect" 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 the validity of this assumption; 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) 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 (Frandsen et al., 2023). 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 and A5, 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.[36]

Finally, the exclusion restriction requires that caseworker strictness only impacts ap-

[36] 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.

plicant 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.[37]

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; so 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.[38] We construct a sample of working-aged heads of household who we observe transitioning from not receiving SNAP to receiving it during the survey. We focus on the first of these transitions observed for each individual.

In Appendix Figure A7, we plot the rates of SNAP receipt around SNAP spell initiation as well as the household-level receipt of other major programs. The horizontal axis indicates months from the start of first SNAP receipt where months are grouped into two-month bins. 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 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 a statistically significant 18 percentage points and WIC increases by a statistically significant

[37]Specifically, a point estimate and standard error of 0.002 and 0.006. Unfortunately, we do not currently have data on Medicaid enrollment.

[38]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.

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 across many subgroups including 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 measures of quarterly employment and earnings. As discussed in Section 4, in addition to estimating the IV model, we decompose the LATE into the potential outcomes under two alternative states of the world: 1) the applicant receives SNAP due to their caseworker's CCAR ("treated compliers") and 2) the applicant is denied due to their caseworker's CCAR ("untreated compliers") (Frandsen et al., 2023). This method is powerful because it allows us to visualize levels of the outcome in both states of the world for the compliers. Showing these potential outcomes before SNAP application allows us to check the validity of our empirical approach as the potential outcomes should be the same for the compliers in both states of the world prior to application. Note that these potential outcomes are not mechanically zero before application, we are simply plotting the average potential outcomes over time. After application, we can see the differential responses for compliers in the two states of the world—with and without SNAP.

We begin by analyzing labor supply for the full sample in Figure 3. Panel (a) shows the results for quarterly employment and panel (b) shows the results for quarterly earnings (inclusive of zeros). The orange and blue lines plot the potential outcomes for the treated compliers and untreated compliers, respectively. The black lines plot the coefficients from the IV model (i.e., the difference between the orange and blue lines) and the gray shaded regions display the 95% confidence intervals. The point estimates and standard errors corresponding to this figure are in Table 5. While these figures have a similar appearance to traditional event study panel designs, these plots simply include the results from separate cross-sectional regressions of Equation (3) by quarter over the event-time horizon.

We see no evidence of quantitatively large or statistically significant changes in labor supply for the full sample. And, importantly, the difference in the potential outcomes (i.e., the black line) before application is stable and close to zero, indicating that pre-application labor supply is unrelated to the assigned CCAR, providing support for our research design. Recall that the baseline rates of employment in our sample are very low–only 21% of applicants worked the entire year before applying for SNAP. Further, SNAP recipients who did not work before applying for SNAP may face other barriers and incentives that mean they will not change labor supply decisions (and start working) regardless of whether they receive SNAP. Therefore, it may be unsurprising we find little response in the full sample. To better understand this, we split the sample by whether the applicant was working the year prior to applying.[39]

In the left column of Figure 4, we examine the effects on labor supply for the 66% of the sample that did not work during all four quarters prior to SNAP application (also shown in Table 6).[40] Here, the levels and trends in potential outcomes are mechanically equal prior to application. After application, there is no significant change in employment in panel (a). Turning to earnings in panel (c), we find a quantitatively small and marginally statistically significant increase in earnings driven by an increase in the potential outcomes of treated compliers in the first year after application. The point estimate indicates an only $276 increase *per month* in labor income and is short-lived. Therefore, we are cautious to read much into this effect as it is economically very small in magnitude.

Next, we examine the effects for those working during all four quarters before application (21% of our sample) in the right column of Figure 4 and in Table 7. Because this group is working at baseline, they have revealed that they are capable of and have a preference for working in the absence of SNAP receipt. As a result, this group's labor supply decisions may be more sensitive to SNAP receipt. Again, the pre-application potential employment outcomes (panel (b)) are mechanically equal prior to application, but potential earnings outcomes in the pre-period (panel (d)) reinforce the validity of our empirical design. Given small sample sizes, we have wide confidence intervals on these estimates, but there is evidence of a significant, but temporary reduction in labor supply among those who received SNAP. This reduction could be some combination of intensive and extensive margin effects as our imprecise estimates in panel (b) prevent us from ruling out substantial negative extensive margin effects. One quarter after application, there is a significant reduction in earnings of $4,500. This is a large effect–about 75% off the baseline earnings in the pre-period. The reduction is driven by a larger decrease in the treated compliers' potential outcomes than untreated compliers. This reduction dissipates after the first year and interestingly, it ap-

[39]The results are very similar if we split the sample by whether the applicant worked in the quarter prior to applying.

[40]We do not know whether the applicant was searching for work or out of the labor force, we simply know they did not have any earnings in the UI earnings data.

pears that earnings and employment start to trend *positively* towards the end of our sample window, due to a differential increase for treated compliers compared to untreated compliers. We have estimated additional quarters more than two years after application and this pattern of positive and increasing earnings effects continues though is never statistically significant.

To better understand these effects on earnings among those working pre-application, we next investigate the impacts on the full distribution of earnings. We create variables that represent whether earnings are above a given cutoff, with cutoffs separated by $500 – this is the same as one minus the cumulative distribution function. We define the highest bin as the 95th percentile of the earnings distribution among those with earnings in the period before application. We run a regression for each event time and each earnings cutoff and plot these effects in Figure 5. The horizontal axis is event time and the vertical is the earnings value for the relevant cutoff. We shade estimates that are statistically significant in yellow. As expected, very few of the estimates for negative event time are significant. The reduction in earnings around application is also driven by reductions in earnings in the bottom part of the distribution–below $5,500. This is around the average earnings among this sample pre-application ($4,446). Interestingly, 6-8 quarters after application, there is evidence of a significant *increase* in earnings at higher parts of the earnings distribution–around $6,500-9,000–and some evidence of declines at lower parts of the distribution, though not always significantly so. This is consistent with workers shifting into higher paying jobs and/or working more hours in the longer-run.

We hypothesize this increase in earnings in the longer-run could be due to an increase in job quality if SNAP allows workers to search for a better job and we test this hypothesis next in Figure 6. We have limited measures of job quality in the data; for this exercise we use information about the worker's industry. If workers are switching industries, we may not see a large increase in earnings immediately if they begin in entry-level positions. However, if the new industry is higher quality, we expect the median wages in that industry to be larger than the industry in which they previously worked, as this indicates the potential for higher earnings in the future. We gather data from the BLS on median hourly wages by sector and year[41] and create a variable that equals one if the applicant worked in a sector for a given two quarters that paid a higher median wage than the sector they worked in before applying to SNAP. We collapse the data down to the half-year level to help with precision and smooth shorter-term shocks to the types of jobs worked. The variable will be zero if the person did not work or worked in a lower-paying sector. This is estimated only

[41] `https://www.bls.gov/oes/tables.htm`

on the subsample that worked before applying. SNAP recipients are less likely to become employed in higher-median-wage industries during the year following receipt. However, by the second year, we estimate that SNAP recipients are 19-37 percentage points more likely to be employed in a sector with median earnings above their baseline, though these estimates have wide confidence intervals. We find similar patterns if we look at other percentiles of the sectoral wage distribution.

Next, we use two measures of industry premiums at the 4-digit NAICS from Card et al. (2022). The first is the overall industry premium. Again, we create a variable that equals one if the premium is higher in each two quarters compared to the premium pre-application. We find little change in this outcome in panel (b) of Figure 6. Second, we look at a measure of the worker-value-added by industry–this is the average of the person-specific effects on wages aggregated to the industry level and can be thought of as measuring the types of workers in a given industry. We see suggestive evidence of an increase on this dimension in panel (c) and the magnitudes are large–after 1-3 half-years SNAP recipients are 33-47 percentage points more likely to work in this type of industry. This indicates SNAP recipients are moving to industries that pay higher wages because of the types of workers in that industry. Overall, we take this as suggestive evidence of increases in job quality in the longer-run for those working before applying to SNAP.

We show the robustness to our main sample restrictions of these baseline earnings results by pre-application work in Appendix Figure A8. The black estimates are the baseline specification as above (point estimates are the dots and the 95% confidence interval is plotted in the lines). The blue estimates restrict the sample to the General track only. The orange estimates do not trim extreme values of the CCAR. The green and gray estimates cutoff the sample of caseworkers at the 20th and 30th percentile of decisions per year, respectively. The results are very similar across all these changes with heavily overlapping confidence intervals, so none of these decisions drive our key findings.

5.2.1 Magnitudes

To compare these findings to the literature on SNAP and labor supply, we first note an important difference in who the compliers might be in our approach compared to the prior literature. Past papers generally estimate the effect of changes in SNAP access among a group likely to be affected by the changes, compared to groups who had no change in access and/or were unlikely to be affected by the changes. The compliers in a treatment where access to SNAP is generally made available is plausibly quite different than our setting where general access exists conditional on the household being eligible, but idiosyncratic caseworker qualities push households onto or off of the program among those who have opted into applying.

Another difference to note is that we find very different labor supply responses to receiving SNAP depending on whether the applicant worked before applying. Moreover, we see important dynamics in the labor supply responses that impact the interpretation of our estimates. Given data limitations, this type of heterogeneous response by pre-application work and over time has not been studied in the prior literature.

Nevertheless, we describe the findings of the papers closest to ours. First, studying the precursor to SNAP, Hoynes and Schanzenbach (2012) find an intent to treat effect of Food Stamp access on annual earnings for those most likely impacted by Food Stamps of 32%. The implied treatment on the treated effect is thus 58%. Note, however, that this estimate from Hoynes and Schanzenbach (2012) is on the full sample and incorporates the long and short-run effects. In our analysis, we find a negative effect of slightly larger magnitude, but only among those working prior to applying and it is short-lived.

Second, East (2016) estimates effects of a similar magnitude on 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%. Again, though, these are for the full sample and include both the long and short-run responses.

Finally, several papers find null effects. Bitler et al. (2021) find negligible intensive-margin labor supply responses among SNAP participants to kinks in the budget constraint created by various SNAP policy rules. Homonoff and Somerville (2021) find imprecise null effects on labor supply for those more likely to miss recertification and stop receiving SNAP.

5.2.2 Further Heterogeneity

We explore other dimensions of heterogeneity including the age and sex of the household head, household composition, and pre-application income to poverty ratio of the applicant. For these splits we condition on whether the applicant worked prior to applying as this is an important determinant of the labor supply effects. We also focus just on the effects in the quarter after application as a more parsimonious way of summarizing the results.

Figure 7 plots the effects on earnings and the first row displays the main estimates for the full unemployed group (left panel) and the full employed group (right panel). First, we split this sample by whether the applicant may be subject to ABAWD-specific work requirements. We can only proxy for this by looking at those who are ages 18-49 and without children in the household, given limited information in the data at the application stage (including no information about disability status). There is a suggestively larger positive effect on earnings for those possibly subject to ABAWD requirements and not working prior to applying, but the estimates are not significantly different from each other or from zero. There is also no evidence of a differential effect among those who worked prior to applying (right panel). Given this, we are cautious about drawing strong conclusions about the impact of work requirements, but they do not seem to be driving any of the main results.

The other demographic splits reveal very homogeneous responses. The point estimates differ some across subsamples but the general pattern is very consistent–those not working beforehand do not change work behavior and those working beforehand reduce work. Interestingly, our results are not driven solely by those with children, suggesting that barriers to childcare is not the only reason for SNAP applicants to not work prior to applying. Finally, we note the results are similar for single-adult compared to multiple-adult households, which is an important check since we can only observe the labor supply behavior of heads of household.

5.2.3 Discussion

Our main results are largely inconsistent with the canonical static labor supply model where an individual trades off consumption and leisure in a single period. We argue that our results can be understood by considering other factors that go into the decision to work and by viewing our results through a dynamic labor search model where individuals conduct costly job search activity to find a job and optimize their decisions over multiple periods. We have discussed the first phenomenon above and shown this is key to understanding the effect of SNAP on labor supply, so here we focus on comparing the static and dynamic search models, conditional on an individual being able to work.

The static model predicts a reduction in work due to both the income and the substitution effects whenever an individual receives SNAP. It does not allow for workers to choose to invest in job search activities or human capital building activities. On the other hand, a dynamic search model accounts for the fact that workers may take time out of work to look for a higher quality job or invest in human capital, which causes them to reduce their labor supply in one period in exchange for higher job quality in future periods. The results we find for those working before receiving SNAP are suggestively more consistent with the dynamic search model predictions. This is because these workers only temporarily decrease their work and there is suggestive evidence of higher earnings and job quality in the longer-run. This is important not only from a theoretical perspective, but for the welfare analysis of SNAP. If workers are using SNAP to subsidize job search and get higher quality jobs in the future, this changes the fiscal externalities of SNAP because the government can expect increases in tax revenue in the longer-run due to higher earnings of these workers. Unfortunately, our data does not allow for precise identification of these future increases in earnings, and this should be a subject for future work.

6 Conclusion

This paper examines the effect of SNAP on labor supply decisions. To identify this, we propose and validate a new and policy-relevant instrument for SNAP receipt–the likelihood of caseworkers to accept a randomly assigned application. We are the first to bring the examiner fixed effects design to the setting of means-tested transfer programs in the United States. We show that caseworker behavior matters for determining whether SNAP applicants receive benefits. We provide evidence that this operates through caseworkers helping applicants navigate the complex application process.

Turning to the labor supply results, the richness of data allow us to uncover important heterogeneity in the response based on pre-application work history. Notably only 21% of our sample worked the year leading up to their SNAP application. We find that those working before SNAP reduce their earnings temporarily in response to SNAP receipt, and there is suggestive evidence these recipients experience improvements in job quality in the longer-run. On the other hand, for the two-thirds of applicants not working in the year before applying for SNAP, the receipt of SNAP has no impact on their labor supply decisions.

Recently, lawmakers have raised concerns about work disincentives from SNAP and other means-tested transfer programs; work requirements were expanded under the Trump administration and are again being debated as part of the 2023 Farm Bill and the 2023 debt ceiling negotiations. Our findings inform this debate; we find no evidence that receiving these benefits led 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 for other reasons and may help individuals find a higher quality job and raise their earnings in the longer-run.

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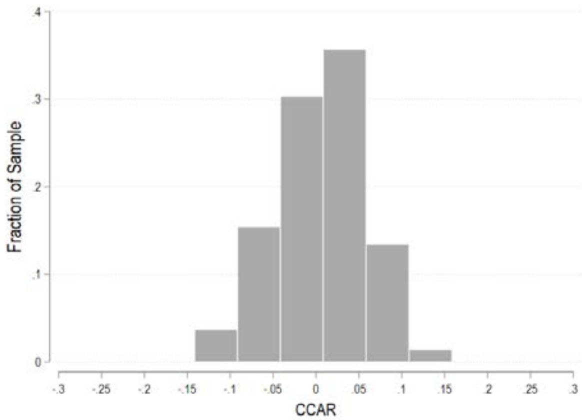
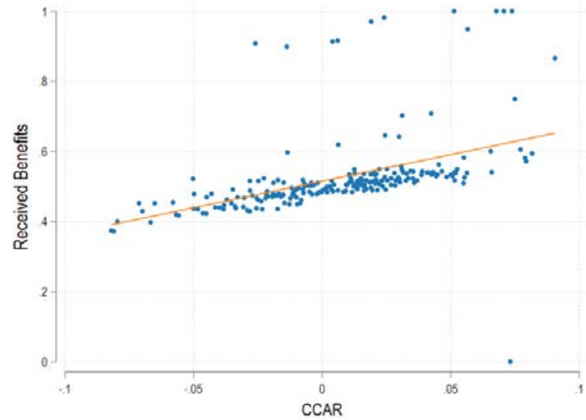
Figure 1: Distribution of the CCAR and Relationship with Benefit Receipt



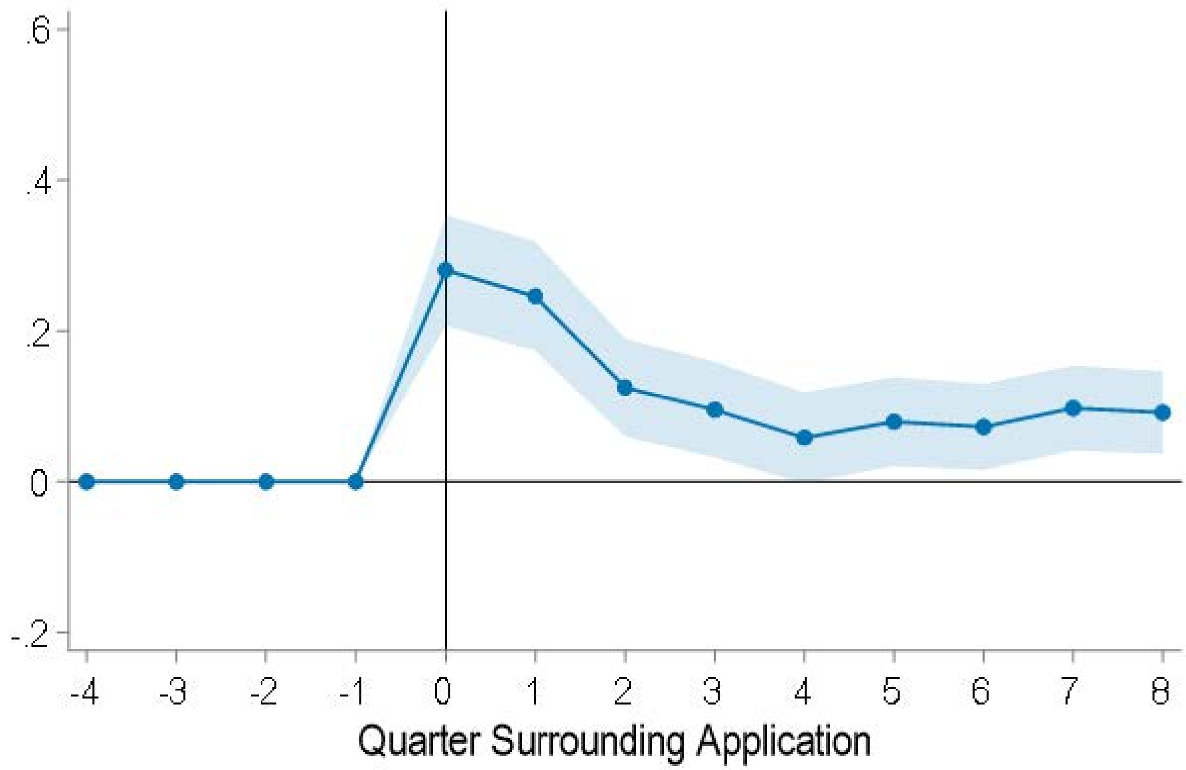
(a) Histogram of CCAR



(b) Scatterplot of CCAR and Benefit Receipt

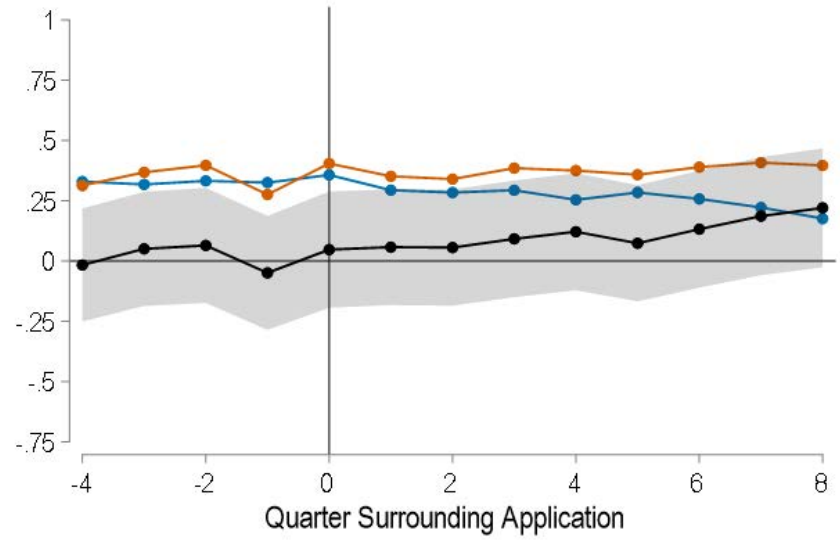
Notes: Panel (a) plots the histogram of our calculated CCAR for the main sample. Panel (b) is at the caseworker level and plots the relationship between the caseworker-level CCAR and the SNAP acceptance rate of applicants for each caseworker. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only.

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

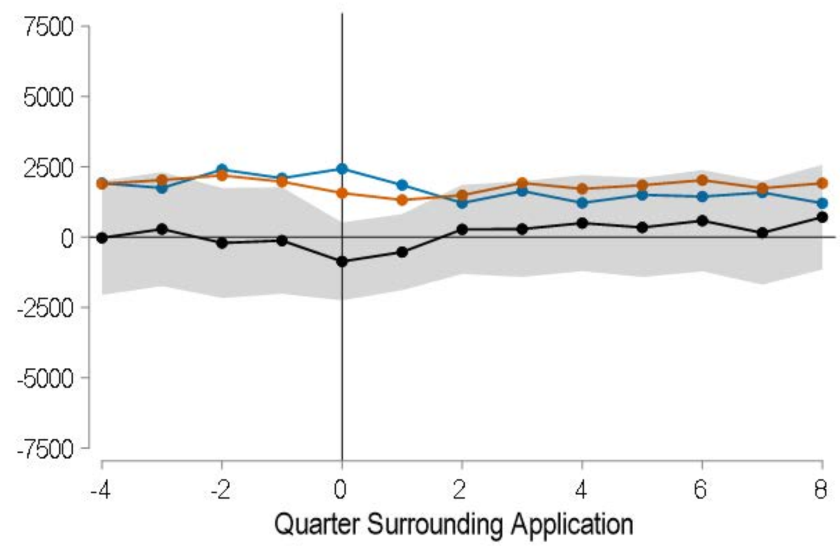


Notes: The figure presents estimates of the effect of the CCAR on benefit receipt from separate regressions using (4) for the quarters surrounding a case's first SNAP application. The 95% confidence intervals are shown in the shaded regions. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only.

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



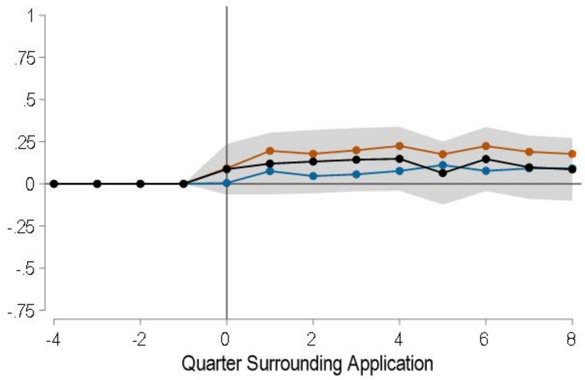
(a) Employment



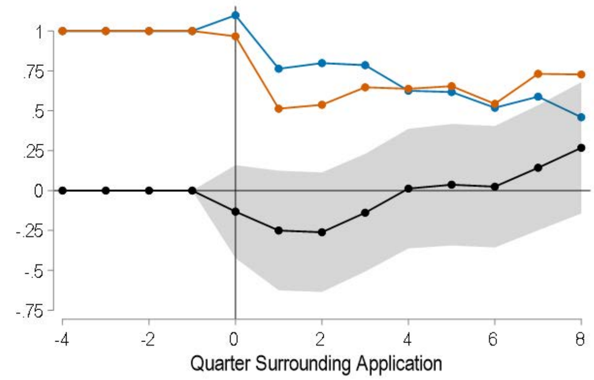
(b) Earnings

Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. The black solid line plots the coefficients from the IV model and the gray shaded region displays the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (those who did not receive SNAP because of their caseworker's CCAR) and the orange line plots the potential outcomes for the treated compliers (those who did receive SNAP because of their caseworker's CCAR).

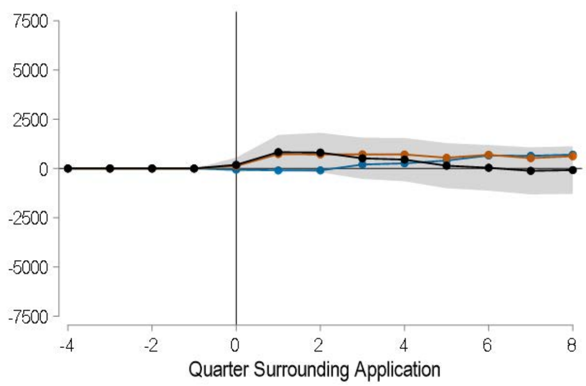
Figure 4: IV Estimates of the Effect of SNAP on Quarterly Labor Supply



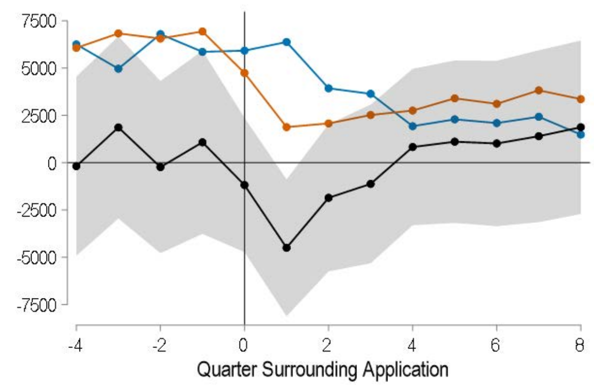
(a) Employment
Unemployed Across Baseline (66%)



(b) Employment
Employed Across Baseline (21%)



(c) Earnings
Unemployed Across Baseline (66%)

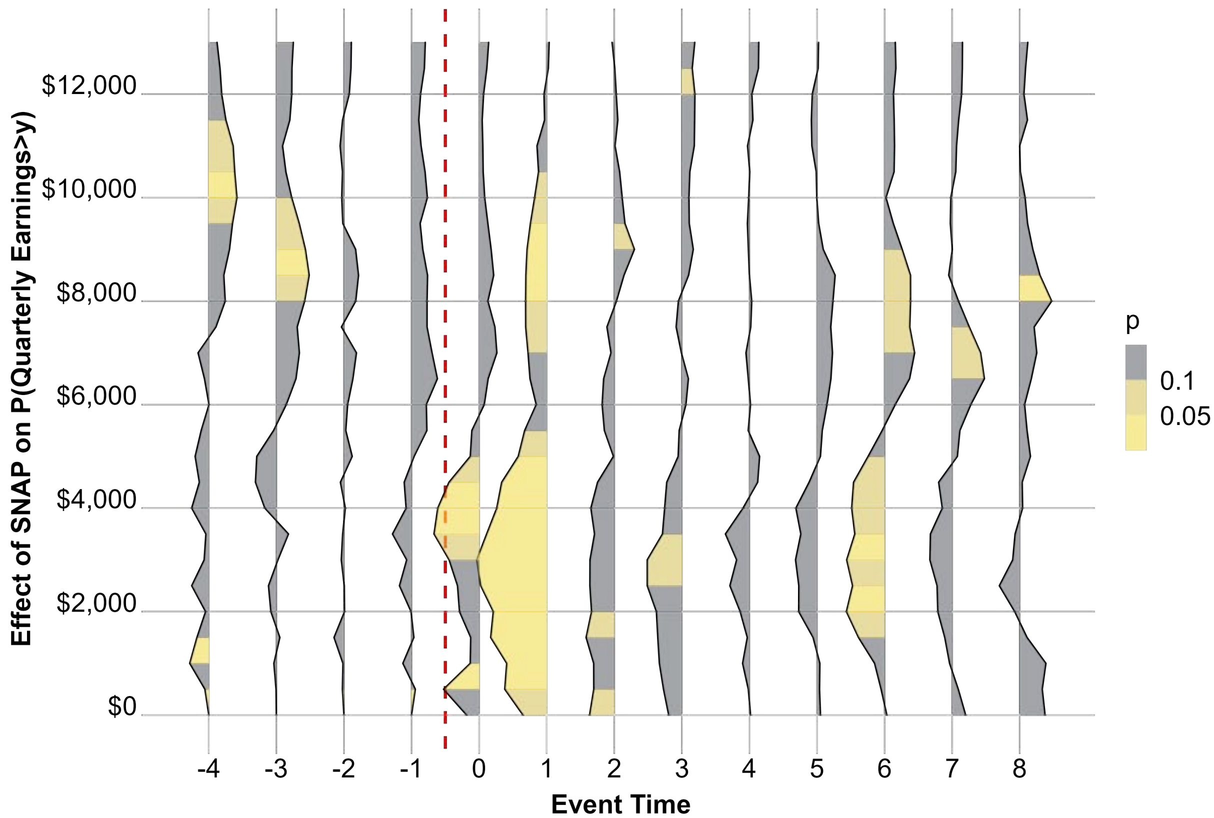


(d) Earnings
Employed Across Baseline (21%)

Untreated Compliers   Treated Compliers   IV Estimate

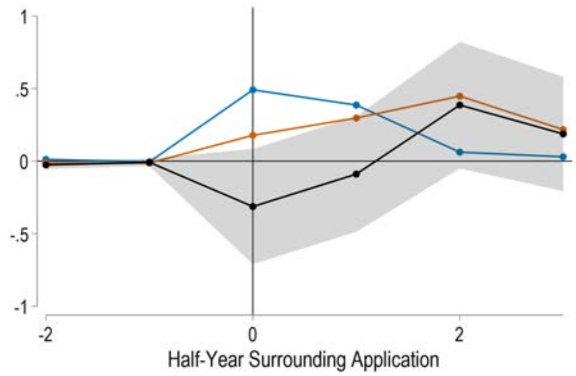
Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. The black solid line plots the coefficients from the IV model and the gray shaded region displays the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (those who did not receive SNAP because of their caseworker's CCAR) and the orange line plots the potential outcomes for the treated compliers (those who did receive SNAP because of their caseworker's CCAR).

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

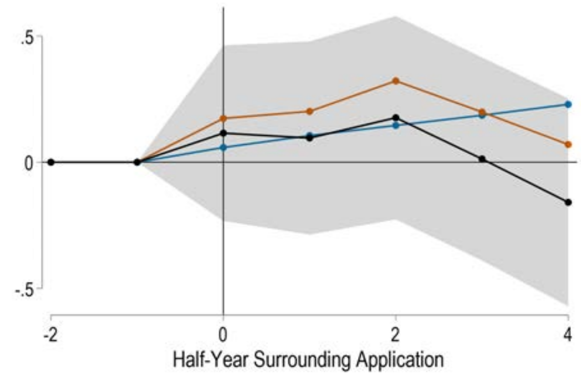


Notes: This figure compiles results from running our main IV specification from (3) instrumenting with the CCAR. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. The outcome variables are indicator variables for having quarterly earnings greater than the given y-axis threshold in the given event-time quarter. Negative point estimates are reflected by a shaded area to the left of the given event-time vertical line—positive estimates are to the right. Standard errors are clustered at the applicant level. The yellow coloring denotes the *p*-value of the given point estimate.

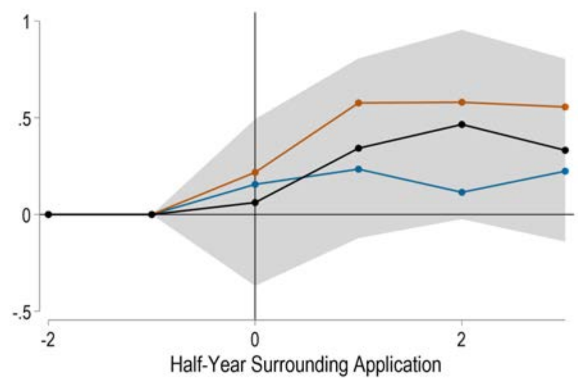
Figure 6: Impact of SNAP on Industry Outcomes for Applicants Employed Across Baseline



(a) Median Sector Earnings > Sector Earnings Pre-Application



(b) Industry Premium > Industry Premium Pre-Application

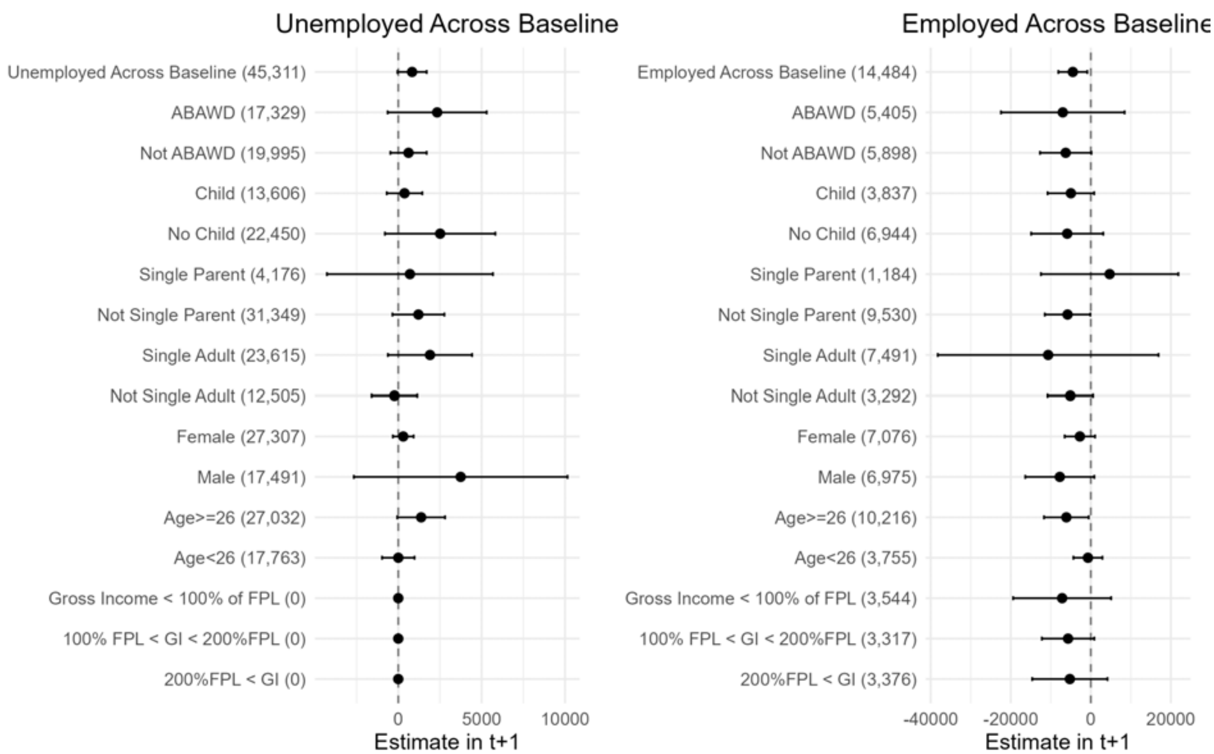


(c) Industry Worker Premium > Industry Worker Premium Pre-Application

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 first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. The black solid line plots the coefficients from the IV model and the gray shaded region displays the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (those who did not receive SNAP because of their caseworker's CCAR) and the orange line plots the potential outcomes for the treated compliers (those who did receive SNAP because of their caseworker's CCAR).

Figure 7: Heterogeneous Effects at $t+1$ for Real Quarterly Earnings



Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. The black solid line plots the coefficients from the IV model and the bars display the 95% confidence intervals on those coefficients.

Table 1: Summary Statistics

	All SNAP Recipients (QC Data)		Our Sample		
	National	Our State	All Recipients	1st Time Applicants	1st Time Recipients
Quarterly Receipt of Benefits	1	1	1	0.51	1
Quarterly Benefit Amount Received	-	-	734.04	-	712.17
Female	0.70	0.71	0.70	0.56	0.55
Age	39.87	37.85	38.27	33.49	34.69
Hispanic	-	-	0.15	0.08	0.08
Black	0.26	0.03	0.05	0.02	0.03
Pacific Islander	0.00	0.01	0.02	0.01	0.01
Asian	0.02	0.01	0.02	0.01	0.01
Number of Kids	1.03	1.38	1.35	0.69	0.73
Number of People in Hhold	2.19	2.35	2.61	1.85	1.89
Real Earnings in -1 (2012$)	-	-	-	1325.47	1113.77
Percent Employed in -1	-	-	-	0.28	0.25
Real Earnings in $+1$ (2012$)	748.67	881.63	806.36	1073.98	891.93
Percent Employed in $+1$	0.25	0.28	0.26	0.29	0.27

Notes: The first two columns use data from the SNAP Quality Control Data Set. The third-fifth columns present summary statistics from our state of interest using our administrative data. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. 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, and quarterly wage information during all periods of SNAP receipt in the Quality Control 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 (0.28, 0.45)	0.011	1.247	-0.026	-0.000
	(0.011)	(1.014)	(0.349)	(0.001)
Real Earnings t−1 (1,482, 3,524)	-0.000***	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Number of Jobs t−1 (0.36, 0.64)	-0.046***	-0.703	-0.095	-0.000
	(0.007)	(0.614)	(0.211)	(0.001)
Industry Experience t−1 (2.33, 5.57)	-0.000	-0.084*	0.010	-0.000
	(0.000)	(0.044)	(0.015)	(0.000)
Arc Percent t−1 (0.21, 0.49)	0.028***	-0.071	-0.060	0.000
	(0.004)	(0.398)	(0.137)	(0.000)
Works in NAICS 45 t−1 (0.04, 0.19)	0.011	-0.199	-0.297	0.001
	(0.011)	(0.992)	(0.341)	(0.001)
Works in NAICS 72 t−1 (0.04, 0.20)	0.003	-0.477	0.660**	0.000
	(0.011)	(0.947)	(0.326)	(0.001)
Works in NAICS 56 t−1 (0.06, 0.24)	0.034***	0.485	0.235	-0.001
	(0.009)	(0.831)	(0.286)	(0.001)
Female (0.56, 0.50)	-0.028***	-1.009***	-0.045	0.000
	(0.004)	(0.347)	(0.120)	(0.000)
Over 65 Head (0.03, 0.17)	-0.355***	0.402	-0.111	-0.001
	(0.013)	(1.181)	(0.407)	(0.001)
Age (33.49, 13.87)	0.005***	0.014	0.000	-0.000
	(0.000)	(0.014)	(0.005)	(0.000)
Black/Hispanic (0.10, 0.30)	0.034***	0.797	0.083	0.000
	(0.007)	(0.582)	(0.200)	(0.001)
Spanish-Speaking (0.01, 0.11)	-0.124***	-0.375	-0.003	-0.001
	(0.018)	(1.613)	(0.556)	(0.002)
Mean Y	0.51	235.26	41.13	0.00
F	143.14	1.44	0.62	0.34
N	68901	68828	68901	68901

Notes: This table regresses the pre-application characteristics of applicants 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 the CCAR of the applicants' assigned caseworker (column 4). We include the most common sectors of employment and the arc percent of earnings pre-application. We include application-date-by-track fixed effects. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. Standard errors clustered at the applicant level. * p<0.10, ** p<0.05, *** p<0.01

Table 3: 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	0.28	1854.53	0.42	2.10	0.22	0.77	29.17	0.06
Full-sample average char	0.28	1481.73	0.36	2.33	0.21	0.56	33.49	0.10
Beneficiary average char	0.25	1217.15	0.31	2.09	0.21	0.55	34.69	0.10
Complier-weighted char	0.97	1.25	1.17	0.90	1.07	1.36**	0.87	0.62
relative to overall	(0.29)	(0.39)	(0.32)	(0.44)	(0.46)	(0.18)	(0.08)	(0.59)
Complier-weighted char	1.09	1.52	1.34	1.00	1.07	1.40**	0.84**	0.59
relative to beneficiaries	(0.33)	(0.48)	(0.37)	(0.49)	(0.46)	(0.18)	(0.08)	(0.57)
Observations	68,901	68,901	68,901	68,901	68,901	68,901	68,901	68,901

Notes: Row 1 presents the results of our main IV specification from (3) instrumenting with the CCAR, but using as the outcomes 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 first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. Standard errors clustered at the applicant level. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Relationship of CCAR with Incomplete Application

	Incomplete Application
Caseworker CCAR	-0.259***
	(0.036)
Mean Y	0.38
N	68901

Notes: This table regresses the primary denial reason on the CCAR of the applicants' assigned caseworker. We include application-date-by-track fixed effects. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. Standard errors clustered at the applicant level. * p<0.10, ** p<0.05, *** p<0.01

Table 5: 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 8	0.086***	0.214*	0.182**	0.396***	713	1,204*	1,917***
	(0.028)	(0.128)	(0.090)	(0.089)	(951)	(716)	(618)
Event Time 7	0.090***	0.179	0.231***	0.410***	151	1,588**	1,739***
	(0.029)	(0.127)	(0.089)	(0.089)	(939)	(705)	(620)
Event Time 6	0.066**	0.125	0.264***	0.389***	583	1,441**	2,024***
	(0.029)	(0.126)	(0.089)	(0.089)	(920)	(693)	(600)
Event Time 5	0.073***	0.069	0.292***	0.361***	343	1,503**	1,846***
	(0.030)	(0.125)	(0.089)	(0.088)	(900)	(679)	(587)
Event Time 4	0.051*	0.122	0.263***	0.385***	499	1,217*	1,716***
	(0.031)	(0.126)	(0.089)	(0.089)	(872)	(663)	(560)
Event Time 3	0.093***	0.096	0.297***	0.392***	286	1,639***	1,925***
	(0.033)	(0.126)	(0.089)	(0.089)	(869)	(673)	(551)
Event Time 2	0.117***	0.067	0.287***	0.354***	275	1,215*	1,489***
	(0.033)	(0.126)	(0.089)	(0.088)	(809)	(628)	(507)
Event Time 1	0.237***	0.073	0.293***	0.366***	-532	1,852***	1,320***
	(0.037)	(0.125)	(0.089)	(0.088)	(692)	(554)	(424)
Event Time 0	0.277***	0.058	0.362***	0.420***	-865	2,431***	1,565***
	(0.038)	(0.126)	(0.091)	(0.088)	(707)	(607)	(403)
Event Time -1	0	-0.043	0.338***	0.294***	-123	2,094***	1,971***
		(0.123)	(0.090)	(0.085)	(966)	(765)	(597)
Event Time -2	0	0.073	0.339***	0.412***	-207	2,400***	2,193***
		(0.124)	(0.089)	(0.087)	(999)	(772)	(646)
Event Time -3	0	0.060	0.321***	0.382***	285	1,746**	2,031***
		(0.124)	(0.089)	(0.086)	(1,034)	(785)	(672)
Event Time -4	0	-0.009	0.339***	0.330***	-27	1,920***	1,894***
		(0.122)	(0.088)	(0.085)	(1,037)	(792)	(671)
First Stage F $(t = 0)$	54.30						
Outcome Mean $(t = -1)$	0	0.284			1,482		
Observations	68,901	68,901	68,901	68,901	68,901	68,901	68,901

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 (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 first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 6: First Stage and 2SLS Estimates: Unemployed across Baseline

	First Stage	*Quarterly Employment*			*Real Quarterly Earnings*		
	Estimate	Estimate	Untreated Complier	Treated Complier	Estimate	Untreated Complier	Treated Complier
Event Time 8	0.091***	0.032	0.095	0.127	-82.226	704*	622
	(0.035)	(0.100)	(0.062)	(0.079)	(616.607)	(380)	(490)
Event Time 7	0.082**	0.050	0.096	0.147*	-120.743	643*	523
	(0.036)	(0.100)	(0.062)	(0.080)	(610.927)	(373)	(485)
Event Time 6	0.055	0.108	0.075	0.183**	35.444	659*	695
	(0.037)	(0.102)	(0.062)	(0.081)	(590.878)	(372)	(465)
Event Time 5	0.080**	0.028	0.110*	0.139*	140.376	404	544
	(0.038)	(0.101)	(0.062)	(0.080)	(585.369)	(359)	(463)
Event Time 4	0.070*	0.113	0.074	0.188**	449.552	261	711
	(0.039)	(0.101)	(0.061)	(0.082)	(561.140)	(347)	(441)
Event Time 3	0.122***	0.119	0.051	0.170**	516.754	201	717*
	(0.041)	(0.101)	(0.060)	(0.081)	(534.502)	(324)	(425)
Event Time 2	0.105***	0.123	0.038	0.161**	808.845	-97.033	712*
	(0.042)	(0.101)	(0.060)	(0.081)	(511.501)	(309.063)	(400)
Event Time 1	0.207***	0.114	0.067	0.181**	828.783*	-98.275	731**
	(0.046)	(0.099)	(0.059)	(0.079)	(446.692)	(267.385)	(347)
Event Time 0	0.261***	0.081	-0.005	0.076	184.692	-60.738	124
	(0.047)	(0.081)	(0.052)	(0.062)	(187.142)	(130.650)	(133)
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 $(t = 0)$	31.45						
Outcome Mean $(t = -1)$	0	0.000			0.000		
Observations	45,311	45,311	45,311	45,311	45,311	45,311	45,311

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 (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 first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

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 8	0.116*	0.266	0.463***	0.729***	1,874	1,486	3,359***
	(0.062)	(0.224)	(0.168)	(0.144)	(2,338)	(1,955)	(1,372)
Event Time 7	0.130**	0.143	0.590***	0.733***	1,399	2,425	3,824***
	(0.062)	(0.213)	(0.156)	(0.144)	(2,317)	(1,864)	(1,398)
Event Time 6	0.117*	0.021	0.508***	0.529***	1,015	2,095	3,110**
	(0.064)	(0.207)	(0.161)	(0.149)	(2,231)	(1,834)	(1,358)
Event Time 5	0.061	0.056	0.619***	0.676***	1,109	2,290	3,399***
	(0.066)	(0.208)	(0.151)	(0.143)	(2,191)	(1,773)	(1,322)
Event Time 4	0.047	0.042	0.602***	0.644***	827	1,928	2,755**
	(0.067)	(0.205)	(0.151)	(0.142)	(2,108)	(1,754)	(1,277)
Event Time 3	0.070	-0.126	0.771***	0.645***	-1,122	3,641**	2,519**
	(0.071)	(0.200)	(0.143)	(0.141)	(2,139)	(1,758)	(1,269)
Event Time 2	0.143**	-0.274	0.798***	0.524***	-1,857	3,932***	2,075*
	(0.073)	(0.204)	(0.141)	(0.149)	(1,984)	(1,614)	(1,190)
Event Time 1	0.281***	-0.282	0.783***	0.502***	-4,499***	6,379***	1,880*
	(0.083)	(0.205)	(0.140)	(0.151)	(1,844)	(1,505)	(985)
Event Time 0	0.368***	-0.167	1.132***	0.965***	-1,182	5,923***	4,741***
	(0.086)	(0.159)	(0.118)	(0.122)	(1,811)	(1,551)	(1,109)
Event Time -1	0	0	1	1	1,078	5,858***	6,936***
					(2,469)	(1,995)	(1,441)
Event Time -2	0	0	1	1	-235	6,796***	6,561***
					(2,322)	(1,832)	(1,441)
Event Time -3	0	0	1	1	1,864	4,968***	6,832***
					(2,456)	(1,943)	(1,480)
Event Time -4	0	0	1	1	-185	6,252***	6,067***
					(2,415)	(1,932)	(1,457)
First Stage F $(t = 0)$	18.31						
Outcome Mean $(t = -1)$	0	1.000			5,921		
Observations	14,484	14,484	14,484	14,484	14,484	14,484	14,484

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 (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 first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Figure A1: SNAP Consolidated Work Requirements Notice

Date Mailed: 04-03-2023 [redacted]

[redacted]

SNAP Work Requirement

Dear [redacted]

This letter is to tell you about work rules for the Supplemental Nutrition Assistance Program (SNAP). **If you dont follow these rules, your SNAP benefits may decrease or end.**

What do you need to do?

You **must** participate in Employment and Training. This program can make it easier for you to find or keep a job.

You **must** follow these rules to keep your SNAP benefits:

- Register for work online at [redacted].
- Complete the online evaluation.
- Attend a virtual SNAP E&T Workshop. You will receive another letter with your appointment date and time.
- Meet with your assigned Employment Counselor each month.
- Complete the required workshops.
- Complete 48 job contacts.

Does everyone need to follow these Employment and Training Rules?

No, only certain people do. You may not have to follow these rules if you:

- Are between the ages of 47 and 60,
- Are currently employed,
- Are temporarily laid off from your work,
- Live more than 35 miles from a job search and training center,
- Do not have access to public or private transportation to work,
- Do not have child care for your children ages 12 and under,
- Are a domestic violence survivor,
- Are on probation or parole,
- Have limited English speaking skills,
- Have applied for Supplemental Security Income (SSI),
- Are pregnant,
- Are homeless,
- Are participating with a Vocational Rehabilitation program,

Toll free: [redacted] Toll free FAX: [redacted]
Phone Number: [redacted] FAX: [redacted]

- Are participating in a Title V program, such as Older American Programs, Easter Seals, and the Forestry Program, or
- Are participating in a Choose to Work, Americorps, and Americorps VISTA program.

Call us at [redacted] **or** [redacted] **as soon as possible** if you think one of these might describe you. If we find that it does, you will not need to follow the Employment and Training Rules.

What if you have costs from doing the program?

You may be eligible to receive a $50 payment for each month you complete the required Employment and Training activities. You may be eligible to receive up to three months of these payments. Your participation will be verified before we pay you. You may not receive all three payments if you do not participate each month.

If your cost is greater than $50 per month to participate in the required Employment and Training activities, you may not be required to participate. If your cost to participate is greater than $50 per month, please **call us at** [redacted] **or** [redacted] **as soon as possible.**

What happens if you do not follow these Employment and Training Rules?

If you do not follow these rules without a good reason, **you may lose your SNAP benefits.**

What if you have a good reason for not following these Employment and Training Rules?

Call us at [redacted] **or** [redacted] **as soon as possible** if you think you have a good reason, or your program is not a good fit for you. You should also tell your employment counselor or eligibility worker. Good reasons for not following these rules include issues you cant control such as getting sick or not having childcare for a child younger than age 12.

If we find that you have a good reason from not following the Employment and Training rules, there will be no change to your SNAP benefits

How long will you lose SNAP benefits if you dont follow these Employment and Training Rules?

- The first time you do not follow these rules and you dont have a good reason, you can't get SNAP benefits for 1 month.
- The second time you do not follow these rules, you can't get SNAP benefits for 3 months.
- The third time, you can't get SNAP benefits for 6 months.

And you must meet these Job Search and Training Rules before you can get SNAP benefits again.

<0554>

Toll free: [redacted] Toll free FAX: [redacted]
Phone Number: [redacted] FAX: [redacted]

Notes: Example of a "Consolidated Work Requirements Notice" sent out by the state we study. These are mailed to new beneficiaries in the first couple of weeks following a successful application.

Figure A2: 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
			$	
			$	
			$	
			$	

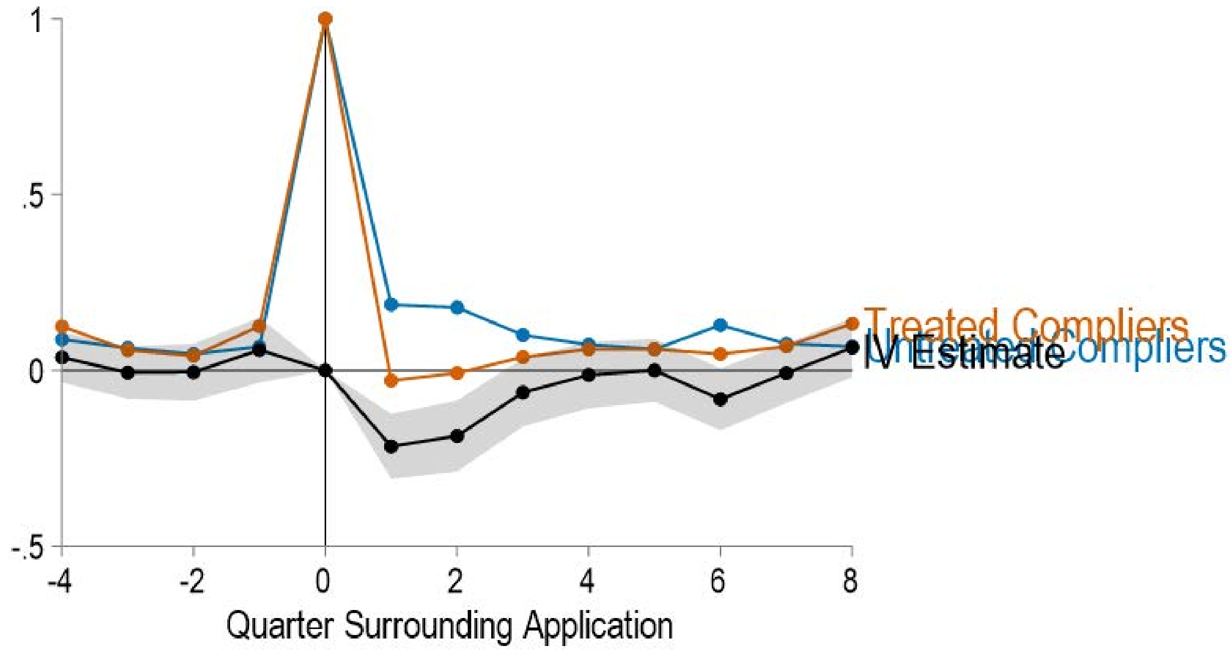
25. Does anyone in your household have any vehicles? .... ☐ Yes ☐ No
If yes, complete all columns. Some examples of vehicles are cars, trucks, boats or water craft, motorcycles, snowmobiles, motor homes, ATV's, etc.

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:

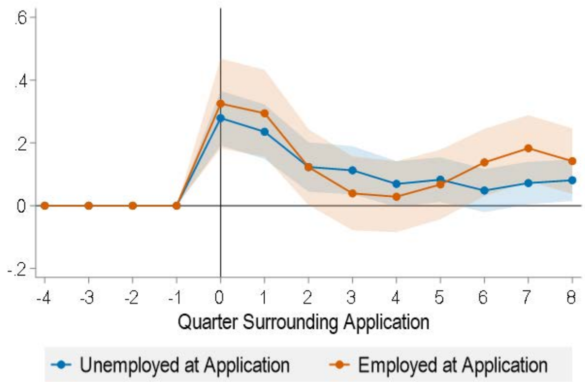
	Type	Who Owns This?	Fair Market Value	Amount Owed	Date Acquired
☐	Home		$	$	
☐	Other property (ex: Land, rental home, vacation home/time share, mineral/other rights, etc.): ____________		$	$	
☐	Trailers		$	$	
☐	Other (ex: equipment/tools, machinery, livestock, etc.): ____________		$	$	

Figure A3: IV Estimates of the Effect of SNAP on Reapplication for SNAP

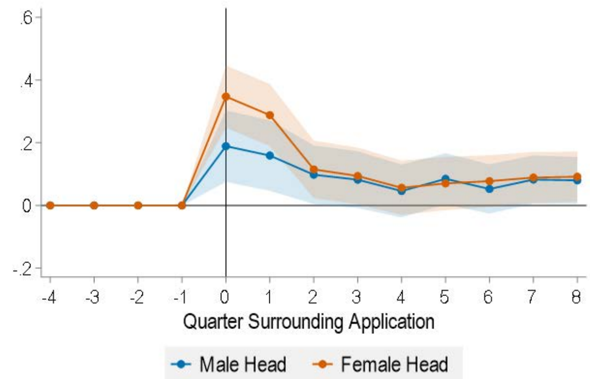


Notes: This figures shows the results from the IV model in equation (3) instrumenting with the CCAR. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. The black solid line plots the coefficients from the IV model and the gray shaded region displays the 95% confidence intervals on those coefficients. The blue line plots the potential outcomes for untreated compliers (those who did not receive SNAP because of their caseworker's CCAR) and the orange line plots the potential outcomes for the treated compliers (those who did receive SNAP because of their caseworker's CCAR). Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only.

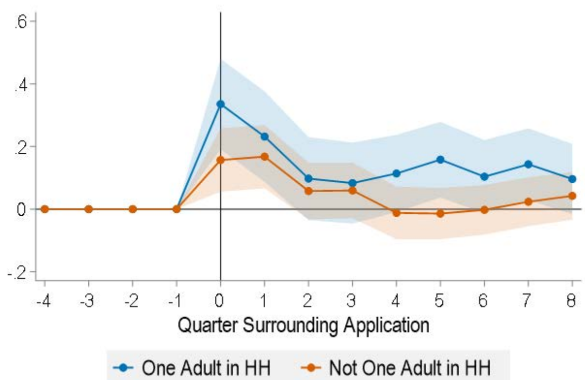
Figure A4: First Stage Estimates for Subgroups



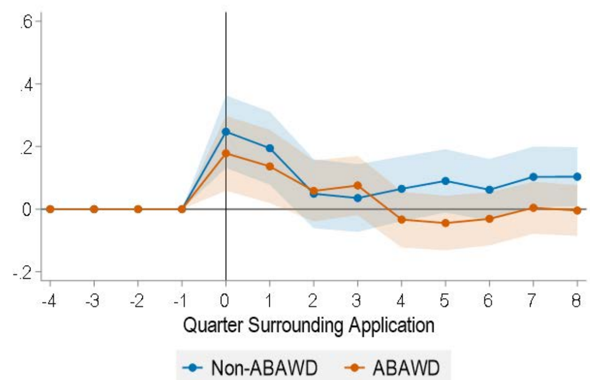
(a) Employment at Baseline



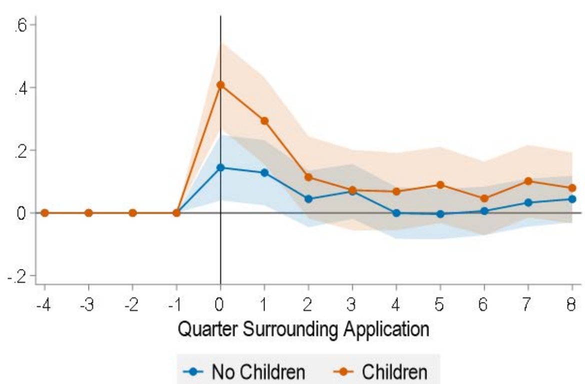
(b) Gender



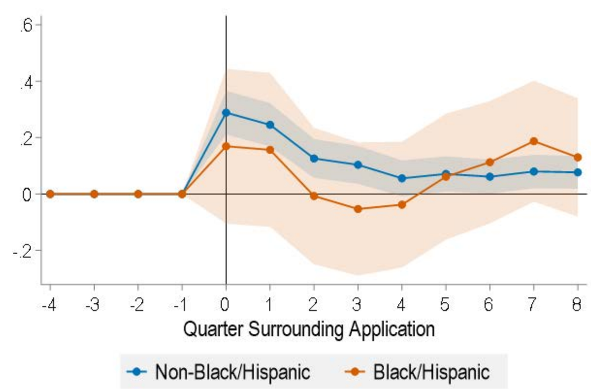
(c) Single Adult



(d) ABAWD



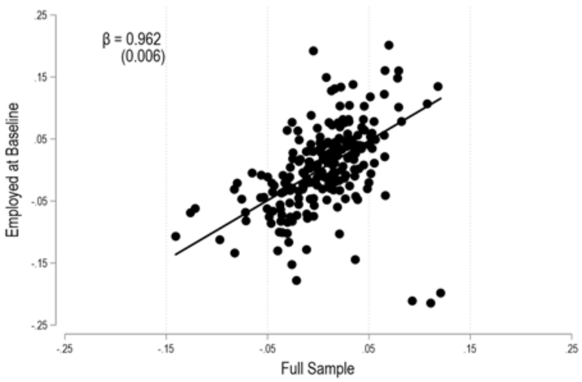
(e) Child Case



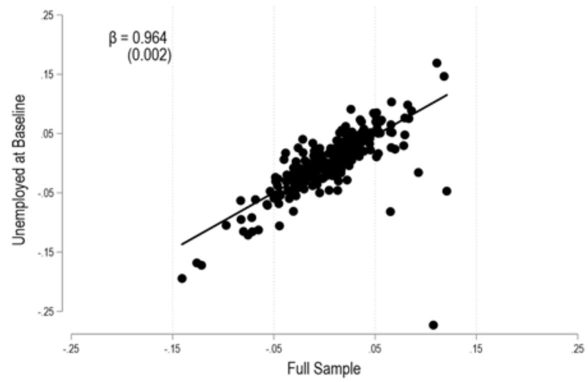
(f) Race/Ethnicity

Notes: The figures present estimates of the effect of the CCAR on benefit receipt from separate regressions using (4) for the quarters surrounding an application for the given subgroups. The 95% confidence intervals are shown in the shaded regions. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only.

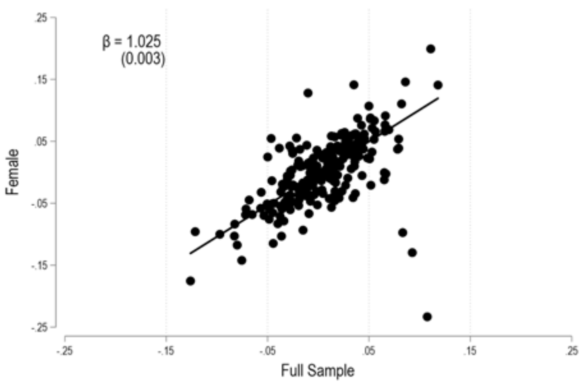
Figure A5: Group-Specific CCAR vs General CCAR



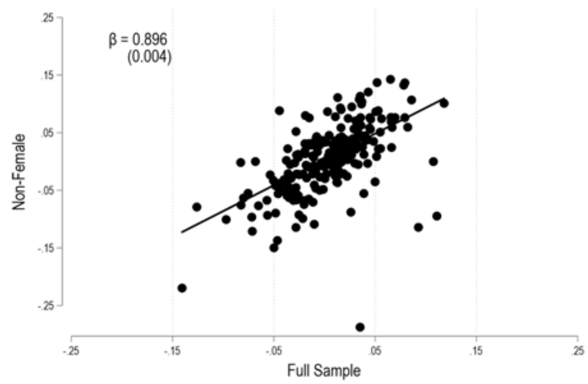
(a) Employed at Baseline



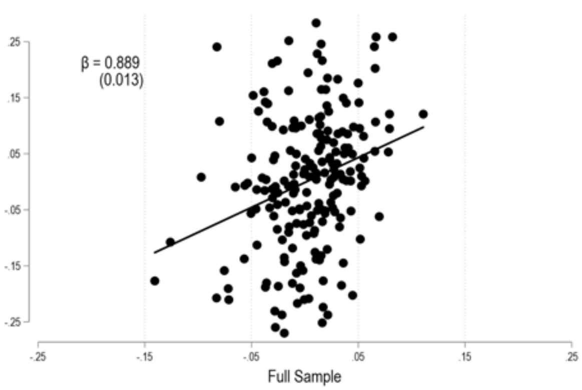
(b) Unemployed at Baseline



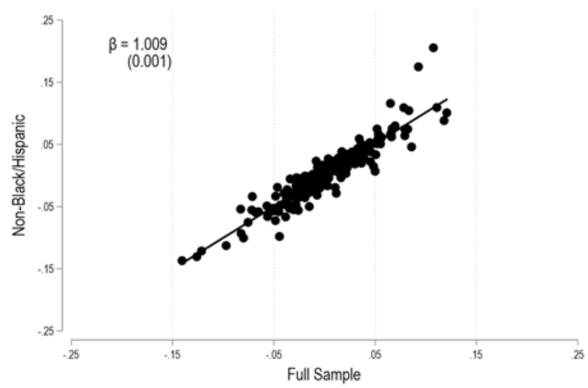
(c) Female



(d) Non-Female

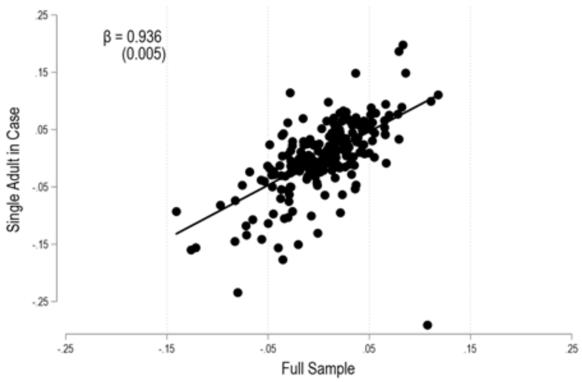


(e) Black/Hispanic

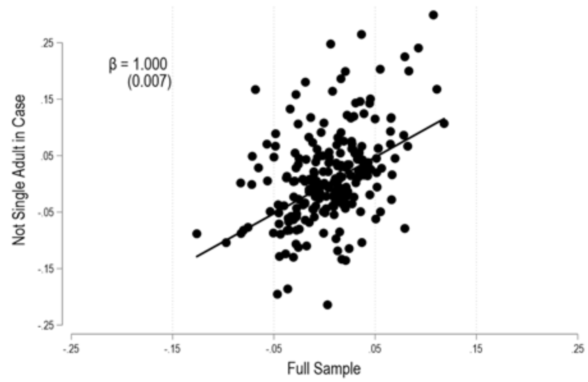


(f) Non-Black/Hispanic

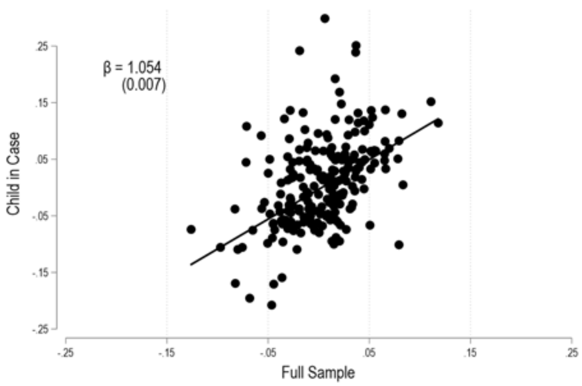
(Continued on next page)



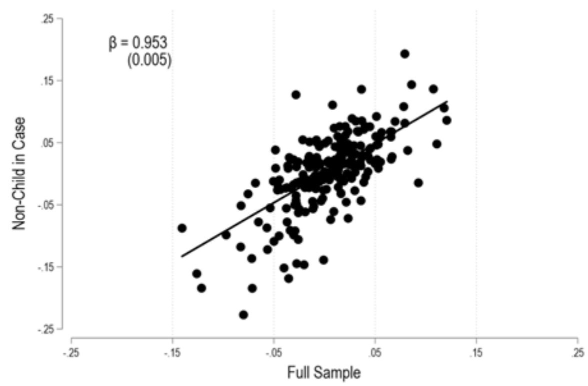
(g) One Adult in Case



(h) Non-One Adult in Case



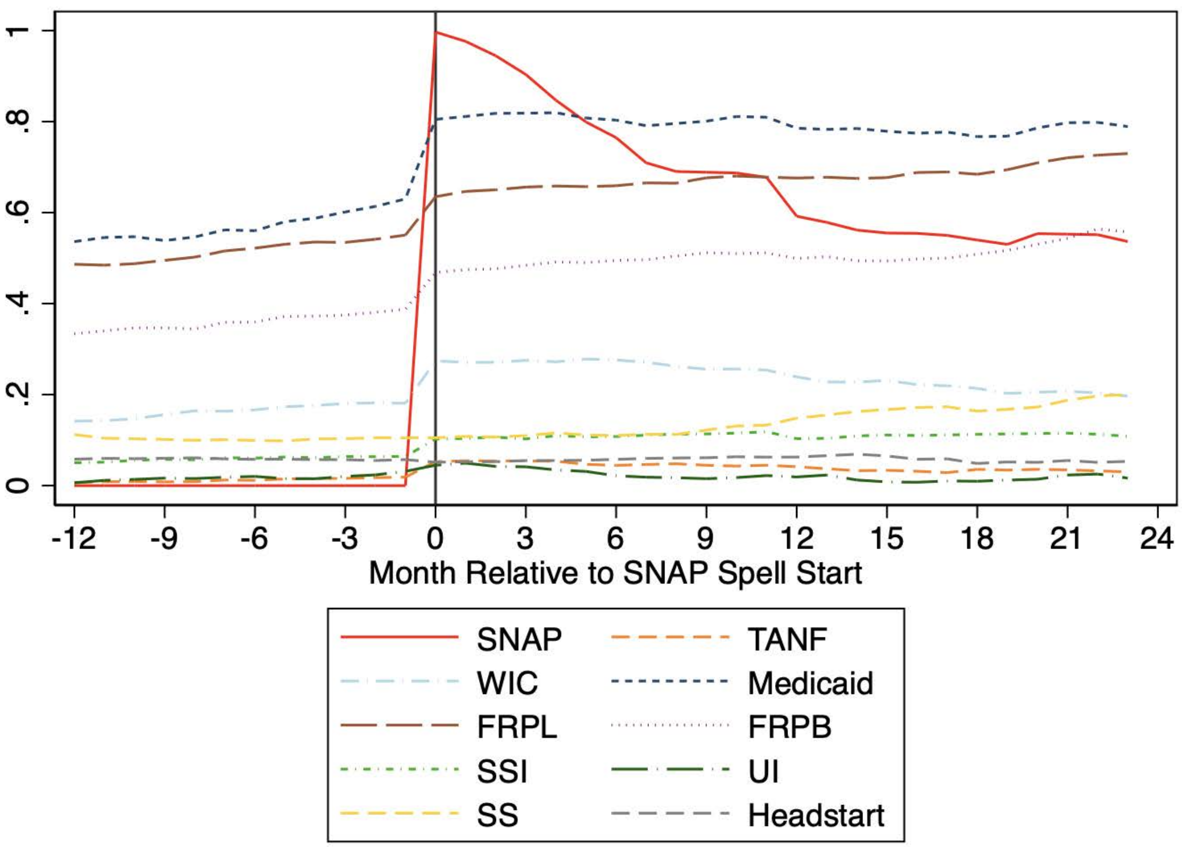
(i) Child in Case



(j) Childless Case

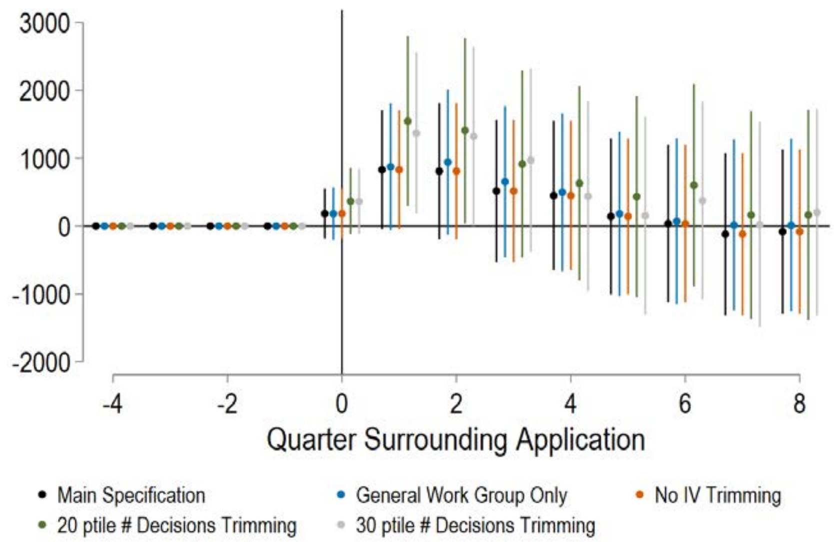
Notes: Each figure plots the CCAR for the specified subgroup (vertical axis) against the full-sample CCAR (horizontal axis). OLS estimates of the relationship between the two are displayed in the figure. Our sample includes first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. Code adapted from Dobbie et al. (2018).

Figure A7: Cross-Program Participation Around First SNAP Spell

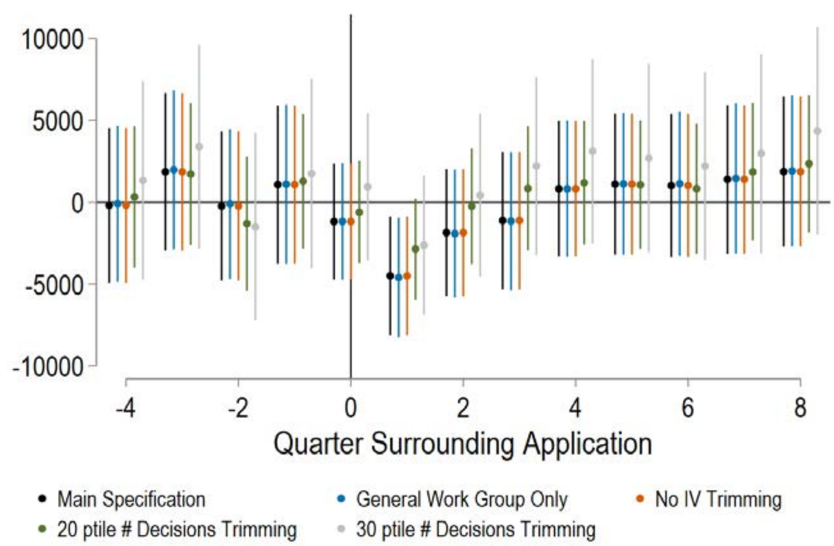


Notes: This figure plots the average household-level program receipt among heads of household in the 2014 Survey of Income and Program Participation. We focus on heads who are ages 18-64 and who we observe transitioning from not receiving SNAP to receiving SNAP for the first time in the survey period.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

Figure A8: Specification Sensitivity Checks for Real Quarterly Earnings



(a) Unemployed 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 first applications between 2013-2017 who apply in the General, Refugee, or Native American tracks. We exclude applicants assigned to caseworkers who handled fewer than the 25th percentile of applications that year as well as caseworkers who have extreme CCAR values. We restrict to a balanced sample only. "General Work Group Only" drop applications from specialized caseworker tracks. "No IV Trimming" includes applications that were assigned to caseworkers with extreme 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.