NBER WORKING PAPER SERIES

THE EFFECT OF MEANS-TESTED TRANSFERS ON WORK: EVIDENCE FROM QUASI-RANDOMLY ASSIGNED SNAP CASEWORKERS

Jason B. Cook
Chloe N. East

Working Paper 31307
http://www.nber.org/papers/w31307

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2023, Revised May 2025

We thank Elizabeth Ananat, Marianne Bitler, Manasi Deshpande, Brigham Frandsen, Amy Finkelstein, Matthew Freedman, Peter Ganong, Nathan Hendren, Hilary Hoynes, Peter Hull, Tatiana Homonoff, Emily Leslie, Adam Looney, Michelle Marcus, Katherine Meckel, Bruce Meyer, Sarah Miller, Matthew Notowidigdo, Analisa Packham, Marianne Page, Abi Peralta, Bruce Sacerdote, Maya Rossin-Slater, Diane Schanzenbach, Jesse Shapiro, Jesse Rothstein, Barton Willage, and Jim Ziliak for helpful comments as well as seminar participants at the NBER Public Economics Program, CESifo Labor Economics Program, University of Kentucky, BYU, Georgetown University, George Washington University, American University, Southern Methodist University, University of Delaware, University of Kansas, George Mason University, University of Duisburg-Essen, University of Oklahoma, SUNY Binghamton, the University of Pennsylvania, the University of Chicago, University of Wisconsin, Ohio State University, University of Utah, University of California Davis, Norwegian School of Economics (NHH), and University of Colorado Denver. We thank Kristine Kohlmeier and Elizabeth Cox for helpful research assistance. We learned about the experiences of applying for means-tested benefits and working while receiving benefits from listening to The Uncertain Hour podcast and through in-depth conversations and shadowing program administrators. We are grateful to the Russell Sage Foundation for funding. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Effect of Means-Tested Transfers on Work: Evidence from Quasi-Randomly Assigned SNAP Caseworkers
Jason B. Cook and Chloe N. East
NBER Working Paper No. 31307
June 2023, Revised May 2025
JEL No. H51, H53, I38, J22

ABSTRACT

Canonical economic theory clearly predicts means-tested transfers will reduce work. We provide new evidence on this question in the context of the Supplemental Nutrition Assistance Program (SNAP)—the backbone of the U.S. safety net. It is challenging to identify the causal effects of SNAP, so we introduce an examiner design based on conditional random assignment of SNAP applicants to caseworkers using rich administrative data. We empirically establish that caseworkers help determine whether applicants receive benefits. We then use this as an instrument for SNAP receipt to produce new, comprehensive and generalizable evidence of the labor supply effects of modern SNAP. Our results shed light on why the canonical theory's predictions might not always bear out.

Jason B. Cook
University of Utah
jason.cook@eccles.utah.edu

Chloe N. East
University of Colorado Denver
Department of Economics
and NBER
chloe.east@ucdenver.edu

1 Introduction

The canonical, static labor supply model, in which individuals trade off consumption and leisure, predicts that access to means-tested transfers, where benefits are reduced as income increases, will disincentivize work. In the United States, the means-tested program that has become the backbone of the safety net is the Supplemental Nutrition Assistance Program (SNAP). SNAP serves 41 million recipients monthly and is the only nearly universal means-tested transfer program (Moffitt, 2002). Proponents of SNAP argue it provides crucial resources to those in need, while critics claim it reduces work and leads to long-term "dependency" on government benefits. However, there are no estimates of the causal effect of modern SNAP on labor supply outcomes for a large and generalizable group of recipients.

We bring new, rich administrative data to bear on this question and provide the most comprehensive evaluation of the labor supply effects of modern SNAP to date. In particular, we use newly-linked SNAP and earnings administrative data from a mountain-plains state (hereafter "the mountain-plains state"). Our main sample is working-age SNAP applicants between 2011-2016. A perennial challenge in the SNAP literature has been the lack of quasi-experimental variation in SNAP; this meant past studies often exploit very particular policy changes to identify effects, at the cost of reduced external validity. To causally identify the effects of SNAP receipt, we bring the examiner design (aka "judge fixed effects") to the SNAP setting by taking advantage of variation in caseworker behavior and conditional random assignment of caseworkers to applicants.[1] Specifically, we use the application approval rate of the assigned caseworker as an instrument for SNAP receipt.

We first examine trends in labor supply for those accepted and denied SNAP. Crucially, we see applicants' earnings whether or not they receive SNAP, allowing us to observe work behavior *prior* to SNAP application, which was not possible in earlier studies. We find that earnings trend slightly down in the quarter immediately before SNAP application, indicating deterioration of labor supply even before applying for SNAP. The downward trends are slightly steeper for those accepted compared to those denied, motivating our instrumental variables approach.

Additionally, we use this data to document that only 18% of SNAP applicants were strongly attached to the labor market before applying for SNAP (measured as having quarterly earnings above full-time, minimum-wage earnings). This is consistent with past research

[1] This design has been used in other contexts where quasi-experimental variation is hard to find, such as the criminal legal system, Disability Insurance receipt, and foster care placement (e.g. Dobbie et al., 2018; Norris et al., 2021; Agan et al., 2023; Maestas et al., 2013; Autor et al., 2019; Doyle Jr, 2007).

that has found barriers to work in low-income populations, including transportation costs, dependent care costs (Keith-Jennings and Chaudhry, 2018), discrimination (Turner et al., 1991; Lang and Spitzer, 2020), and volatile low-wage labor markets (Butcher and Schanzenbach, 2018; Bauer et al., 2025). Thus, the canonical labor supply model may not fully reflect the choices available for most SNAP recipients and this informs our analysis.

Our main results use our instrumental variables approach and focus on the group of SNAP applicants who were strongly attached to the labor market before applying and thus might plausibly decrease their labor supply in response to SNAP receipt. We estimate important dynamics in the effects of SNAP on employment and earnings over time. In the quarter of SNAP application, there is a significant reduction in earnings of $1,418, and this is driven by a decrease in the intensive margin of work, rather than the extensive margin. In subsequent quarters, the effect fades to zero and by the third year after SNAP receipt, both earnings and employment are significantly higher among those granted SNAP than those denied.

To understand these longer-run positive effects, we take advantage of the fact that our data and approach allow us to show that applicants experience a decline in earnings around the time of application, *regardless of whether they receive SNAP*. This is consistent with prior descriptive evidence that showed disruptive events, such as job loss or other decreases in earnings, are very common right before SNAP receipt (Leftin et al., 2014). Given this evidence, the results are consistent with a model where applicants treat SNAP as a source of insurance in the face of a negative shock. So, it may be more appropriate to think of the effects of SNAP on labor supply for this group in a dynamic labor search model, as is common in the Unemployment Insurance literature (Chetty et al., 2017; Nekoei and Weber, 2017). In this kind of model, SNAP allows recipients to maintain higher consumption in the face of negative shocks, which leads to better outcomes in the longer-run. These consumption smoothing effects may be very important, given that SNAP recipients have very little private savings and are unlikely to face perfect credit and insurance markets (Cox et al., 2024).

We also look at the effects of SNAP receipt on those with little-to-no labor market attachment prior to applying for SNAP. For this group, we find relatively precise null effects. We can rule out changes in quarterly earnings of larger than a $268 increase, or a $300 decrease, in the quarter of application. These findings are consistent with this group facing barriers to work, which are generally ignored in the canonical, static labor supply model.

Our study makes four contributions: 1) we provide new estimates of the effect of access to modern SNAP on labor supply, 2) we use high quality administrative data to accurately measure earnings changes over time, 3) we propose and validate the use of an instrumental variable strategy exploiting caseworker behavior, and 4) we are the first to quantitatively evaluate the role of caseworker behavior in U.S. means-tested transfer programs on a wide scale. We discuss each of these contributions in more detail below.

The most widely cited study of the impact of SNAP on labor supply is Hoynes and Schanzenbach (2012), who study the rollout of the precursor to SNAP—Food Stamps—in the 1960-70s. While this research is invaluable, there have been important changes to the program, labor market, and household structure since then. For example, Food Stamps no longer has a purchase requirement, which meant recipients had to buy Food Stamps with cash, and, women's labor supply elasticities are much lower today. Research in more modern time periods is unable to study the causal effects of access to SNAP on a generalizable group. Instead, it looks only at certain subgroups, such as non-citizens, or at specific policy margins, such as kinks in the budget constraint and work requirements (East, 2016; Bitler et al., 2021; Stacy et al., 2018; Gray et al., 2022; Cuffey et al., 2022; Vericker et al., 2023). Additionally, the richness of our data allows us to precisely identify the effect of SNAP receipt, instead of approximating who might receive SNAP using demographic characteristics, as is common in the existing literature.

Our second contribution is the use of administrative data to precisely observe labor market outcomes. Much of the past research relied on self-reported earnings and employment information, whereas our data come from Unemployment Insurance (UI) earnings records, which do not suffer from issues of misreporting. We are also able to identify dynamic effects of SNAP receipt on individuals' labor market outcomes up to three years after SNAP receipt, which was not possible in the past due to data limitations. Even with this rich administrative earnings data, a potential concern is that it might miss work not covered by the UI system, such as gig work. To address this concern, we verify that the earnings we observe in the UI records are very similar to earnings reported on SNAP forms and verified by SNAP caseworkers, and, in our time period, we show very few potential SNAP recipients had non-UI-covered earnings.

Our next contribution is that we propose and validate a new empirical strategy in the SNAP literature; we exploit variation in caseworker behavior, along with conditional random assignment of SNAP applicants to caseworkers, to identify casual effects.[2] Formally,

[2] Other papers have used safety net caseworker assignment to look at placement into different types of benefits among recipients (e.g., Bolhaar et al., 2020; Jonassen, 2013; Autor and Houseman, 2010; Cohen, 2024).

we construct the Conditional Caseworker Approval Rate (CCAR) following the examiner design approach in Kolesár (2013), and we verify conditional random assignment by showing that the CCAR is unrelated to applicant observable characteristics, including pre-application labor supply, conditional on fixed effects. Then, we document a strong effect of the CCAR on SNAP receipt—a one standard deviation increase in the CCAR increases the likelihood of SNAP receipt in the quarter of application by 2 percentage points, which is a 4% effect of the overall rate of acceptance of 49%. Importantly, we also show the local average treatment effect generated from this approach can be plausibly generalized to all SNAP applicants.[3] Finally, we verify the CCAR satisfies the average monotonicity assumption needed to use it as an instrument for SNAP receipt.

Beyond allowing us to look at how SNAP affects labor supply, studying how caseworkers impact take-up of SNAP is important in its own right. Our findings add new evidence to the literature investigating causes of incomplete take-up of transfer programs and the ability of programs to target the neediest recipients (Nichols and Zeckhauser, 1982; Currie, 2006; Herd and Moynihan, 2019; Finkelstein and Notowidigdo, 2019). We not only show that caseworkers matter, but we document empirically that the mechanism driving variation in caseworkers' approval rates is differences in caseworker helpfulness in completing the application, rather than differences in the type or number of applicants a caseworker approves.

While our data is limited to a single state, several pieces of evidence point to external validity. First, we use data from the SNAP Quality Control system to show that on most dimensions, including employment rates and earnings, SNAP recipients in the mountain-plains state are similar to SNAP recipients in the whole country. The main exception to this is the mountain-plains state is less racially diverse, but we see no evidence of heterogeneous effects by race. Second, our analysis sample is observationally similar to all applicants in the mountain-plains state (data on SNAP applicants at the national level are not available). Importantly, this includes being similar in both the levels and trends in labor supply around SNAP application. Finally, the compliers in our empirical approach have similar observable characteristics to the full analysis sample.

The rest of the paper proceeds as follows. Section 2 provides background on SNAP policy and our setting. Section 3 describes our data. Section 4 explores trends in labor supply around SNAP application. Section 5 presents the results on the role of caseworkers and Section 6 examines the impact of SNAP on labor supply using the IV approach.

[3]To do this, we compare the observable characteristics of the compliers to the observables of the full sample of SNAP applicants and SNAP beneficiaries using the method proposed by Frandsen et al. (2023).

2 Policy Background

2.1 SNAP and Labor Supply

SNAP (formerly the Food Stamps Program) is a means-tested federal entitlement program, though states are responsible for determining eligibility and paying out benefits. In general, to qualify for SNAP, applicants must have gross income below 130 percent of the federal poverty level and net income after deductions below 100 percent of the federal poverty level. Households with zero and near-zero income receive maximum SNAP benefits, which are a function of household size. As a household's income increases, benefits are decreased by the benefit reduction rate.[4] Benefits are paid out automatically each month on electronic benefits transfer (EBT) cards, which are used like a debit card for qualifying food purchases at SNAP-accepting stores. Within our sample of recipients, the average monthly benefit is $226 in 2012 dollars.

We present a simple conceptual framework in Figure 1 to understand the predicted effects of SNAP labor supply using the canonical theory. This figure plots the budget constraint without SNAP as the solid black line, and with SNAP as the dotted black line. The slope of the budget constraint without SNAP is equivalent to the market wage. Point A represents someone who spends all their time working, and point E represents someone who does not work (we assume no unearned income besides potential SNAP benefits). Turning to the budget constraint with SNAP, segment CD represents the benefit guarantee, which is the benefit amount received by those who have zero income. D is slightly to the left of point E to take account of the fact that applying for SNAP and proving eligibility takes time. By providing this benefit guarantee for low-income households, the canonical labor supply model predicts a decrease in labor supply due to the income effect. Between points B and C on the SNAP budget constraint, individuals work more and receive more earnings, but the slope is shallower than the market wage due to the SNAP benefit reduction rate. The model predicts this will cause a decrease in labor supply due to the substitution effect. Finally, at point B on the SNAP budget constraint, individuals earn income at the 130% FPL level and are no longer eligible for SNAP. Between points B and A the SNAP budget constraint then follows the no-SNAP budget constraint.

Since the Personal Responsibility and Work Opportunity Reconciliation Act of 1996,

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

SNAP includes work requirements for able-bodied adults without dependents (ABAWDs). Generally, ABAWDs are between the ages of 18-49, report having no disabilities, are not pregnant, and do not take care of any dependents (e.g., children, people with disabilities, or the elderly). In the mountain-plains state, 4% of all recipients are subject to ABAWD work requirements.[5] Unfortunately, we cannot precisely identify who is subject to work requirements at the time of application in our sample. However, previous research using high-quality administrative data has found that these work requirements do not affect work (Stacy et al., 2018; Gray et al., 2022; Cook and East, 2024).

To understand what we expect to see in terms of labor supply responses, and whether the canonical model discussed above applies in our setting, it is useful to know the characteristics of SNAP recipients and the labor market they work in. First, many SNAP recipients are in demographic groups that generally have low labor force attachment, making them unlikely to work regardless of whether they receive SNAP (Keith-Jennings and Chaudhry, 2018). Children and the elderly make up about half of SNAP recipients. Even among working-aged SNAP recipients in the SNAP Quality Control Data, 61% have children and 25% have children of pre-school-age, and 22% are flagged as living in a household with someone who has a disability. However, at least some of these disabled households will be excluded based on our sample restrictions.[6]

In surveys of households in low-income areas, working-aged adults reported that childcare and transportation present the largest barriers to work, following lack of labor demand and job mismatch (Edmiston, 2019). Households in these low-income areas are less likely to own a vehicle and are also less able to work close to home compared to households in high-income areas. Similarly, households in low-income areas spend a much larger fraction of their income on childcare and are more likely to be headed by a single female, making child care responsibilities even more salient for work decisions. Finally, a study of mothers receiving welfare in the 1990s (similar to the SNAP population) found that half had no vehicle or driver's license, half reported depression, Post-Traumatic Stress Disorder, or anxiety (which

[5]SNAP also includes General Work Requirements that focus more on tasks related to becoming employed. In the mountain-plains state, 16% of all recipients are subject to these work requirements and not subject to additional ABAWD requirements. Additionally, it is worth noting the historical context of work requirements. These requirements were often selectively placed on Black recipients and were sometimes motivated by the idea that Black people were less likely to work than white people (Minoff, 2020), when in fact the desire to work more has been consistently higher among Black Americans than white Americans (Nunn et al., 2019). Finally, our state operates a mandatory Employment and Training program that applies to a small fraction of working-aged participants.

[6]Our sample is restricted to be mostly working-aged heads of household, who are not flagged as having a disability at the time of application (this does not mean they do not have a disability, only that they did not submit sufficient proof of disability at the time of application).

do not qualify as a disability under SNAP), 14% commonly were discriminated against and, finally, 14% experienced severe domestic violence in the past year (Danziger et al., 2000). The incidence of all of these barriers to work was much higher among this disadvantaged sample compared to all women in the U.S. Further, we find 46% of working-aged adults who are income-eligible for SNAP report the reason they do not currently work is caregiving responsibilities; another 27% report disability, and 22% report that their schooling limits their ability to work.[7]

The labor market that potential SNAP recipients work in also present challenges to finding and maintaining stable work. Butcher and Schanzenbach (2018) document that the most common occupations among SNAP recipients not only pay less than middle-class occupations, but are more volatile. In particular, workers in these occupations (whether or not they receive SNAP) have a 1 percentage point higher unemployment rate and face a 5 percentage point higher job displacement rate than workers in middle-class occupations. Furthermore, Bauer et al. (2025) document that low-wage workers are much more likely to report that their employer requires them to work volatile weekly schedules and fewer hours than they would like, compared to higher-wage workers. In section 6 below we revisit these facts and discuss how they motivate considering a dynamic job search model with credit constraints where some people face barriers to work.

2.2 SNAP Application Process and Caseworker Behavior

The application process must balance the goals of providing support for qualifying individuals and screening out ineligible individuals. In the U.S., the burden of proving eligibility is generally placed on the applicants (Herd and Moynihan, 2019). Applying for SNAP is complicated and time consuming. Individuals must first submit an application and supporting documentation before completing a required screening interview and then any missing information must be provided.[8] Two-thirds of SNAP administrative costs—which is about 5% of total SNAP spending—are spent on caseworkers and case management.

SNAP applications require information on household composition, income sources, and financial and property assets. An example of the application form is included in Appendix Figure A1. These applications can be submitted online, in person, or via mail, but in the mountain-plains state almost all are submitted online. Some of the fields on the application

[7] Authors' calculation from the Current Population Survey.

[8] 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.

form are verified automatically against administrative records (e.g. earnings are verified against UI earnings data and vehicle ownership is verified against DMV records for asset tests). However, applicants must provide supporting documentation for many other components of their application, such as rent or mortgage payments, letters from their employers, bank statements, utility bills, child or elder care bills, and child support payments. It is common for applicants to not fill in all fields on the application form and/or to not submit all the required supporting documentation with initial application submissions. Applicants have 30 days to submit all necessary information, or their application is automatically denied. However, they have 60 days after the initial submission to go back and finish the application process without having to start from the beginning with a new application. Additionally, after individuals submit their initial application, they must complete a mandatory interview within 30 days to have a caseworker verify their information. During this interview, caseworkers can collect any missing information from the initial application. Caseworkers can also choose to do extra follow-up work with applicants, for example, notify them via email if a form or supporting document is missing.

A USDA-commissioned survey of applicants confirms 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.[9] 10% of applicants who did not complete the process said they dropped out because of some aspect of the process and another 46% cited they thought they were ineligible, possibly because of information they received during the process. This study also found that applicants were more likely to complete their application if they were at an office with a more "pro-participation" supervisor.

The institutional structure surrounding applications and case management in the mountain-plains state provides an ideal setting to explore the impact of caseworkers on SNAP receipt. First, case management is almost exclusively handled over the phone through a statewide system. Caseworkers are organized within tracks based on their specialization to ensure that caseworkers have the relevant skills, such as language or knowledge of special program rules to handle applications. In our analysis, we focus only on the 63% of applications in the General track, which handles the majority of applications, where assignment to caseworkers is the most random. We show below that the demographics and labor supply patterns of

[9] 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).

those in the General track are very similar to those of the overall sample of SNAP applicants, so this is unlikely to impact the generalizability of our results. And, in the General track, caseworkers have a median of 32 months of work experience, compared to 34 months for the broader sample. Each caseworker works in one of multiple call centers located around the state and caseworkers handle cases from all over the state, rather than just those nearest to them.[10]

The second useful institutional feature is that the mandatory interviews with caseworkers are on demand from the perspective of the applicants. Unlike some states (see for example, Homonoff and Somerville, 2021), applicants in the mountain-plains state can call into the statewide phone system at any time Monday through Friday, 8am to 5pm to complete their interview. During the interview, caseworkers do not have a set script to follow and have flexibility in the type and number of questions that they ask. Interviews last about 20 minutes on average. Caseworkers then enter the information into a computer system and the software ultimately determines eligibility.

Third, and crucial to our empirical strategy, caseworkers 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. 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, caseworkers are effectively randomly assigned to applicants within the General track.

By and large, 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.[11] In our setting, the second factor is in part prompted by the several layers of review that exist to monitor caseworker decisions. First, the USDA has its Quality Control system that audits decisions of caseworkers in all states each year. To do this, they select a random sample of SNAP recipients and do a follow-up survey with them to decide if they are indeed eligible or not. States are then ranked based on the percentage of incorrect decisions, and states with lower rankings are fined. In our sample period, over-payment rates (Type II Errors, as defined by Kleven and Kopczuk, 2011) are 3-6% across states. The mountain-plains state is not fined in our sample period and has relatively low error rates in general. In addition to this federal monitoring, the mountain-plains state chose to have an Editing Team, which is not required

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

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

by the federal government, but many states choose to have. Editors from the Editing Team review the decisions of caseworkers every month by examining the case file information (they do not collect any additional information beyond what the caseworker initially collected). Newer caseworkers—who we exclude from this analysis—have more cases reviewed per month than seasoned caseworkers, who have about 10 cases reviewed per month. Caseworkers who fall below a rate of 90% accuracy are subject to consequences, including additional individual mentoring and coaching, a written warning, or further disciplinary action.

Given that caseworker decisions are closely monitored and that a computer decides eligibility, what are the mechanisms through which caseworker behavior can affect SNAP receipt? We hypothesize and provide supporting evidence below that the biggest source of variation in caseworker behavior is how helpful they are at guiding applicants through the complicated application process. This is also consistent with prior work that found when a state automated assistance for means-tested transfer applications, rather than having caseworkers assist, there was a reduction in means-tested transfer program receipt (Wu and Meyer, 2021). Though, this change was accompanied by increases in wait times and backlogs in processing applications, so the exact mechanism is unclear. Additionally, Finkelstein and Notowidigdo (2019) and Schanzenbach (2009) found that connecting likely SNAP-eligible nonparticipants to application assistance significantly increased their program receipt.[12]

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

3 Data

Our data come from a single state in the mountain-plains region, which remains unidentified for anonymity, and include all SNAP applicants. We observe basic demographic information of applicants 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

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

those who receive SNAP, we observe benefits paid over time. For those who do not receive SNAP, we observe the reason for denial.

Application information is linked to quarterly labor supply information from the state's Unemployment Insurance (UI) database. This type of linked data has been used in the past to evaluate the labor supply effects of other means-tested programs like Medicaid, public housing, and SNAP work requirements (Baicker et al., 2014; Chyn, 2018; Gray et al., 2022). The state only matched the head of the household for each application as a data security measure. However, 67% of all applicants in our sample are single-adult-headed households and we show the results are similar among this subsample. Moreover, in the mountain-plains state, only 2% of all SNAP recipients are in dual-income households in the SNAP Quality Control (QC) Data (a nationally representative sample of SNAP recipients), and among a sample of SNAP-income-eligible households in the Current Population Survey (CPS) only 10% are dual-income households (Flood et al., 2023; Cronquist et al., 2024). The UI records contain the earnings and industry for each individual and job by quarter. Importantly, we can observe these outcomes even for SNAP applicants who are denied, and, for all applicants, we observe these outcomes before SNAP application. A limitation of any study using UI earnings data to measure labor supply is that a small group of workers are excluded from the data because they work in jobs not covered by UI. We show in the Appendix that this is unlikely to impact our results, and below we confirm that the earnings measured in the UI data are very similar to total earnings that SNAP recipients report when they apply for SNAP. We describe further details of these data in Appendix A.

3.1 Sample Construction

To construct a sample that allows us to cleanly identify the effects of caseworker behavior on labor supply dynamics, we begin with the 405,475 applications that were submitted between 2011-2016 for which the applicant did not receive SNAP during the previous 6 months. This 6-month condition helps ensure that applicants have to submit a new application and thus are randomly assigned a new caseworker. Application data becomes available in 2011, and we limit to those who applied before 2017 so we can examine quarterly labor supply outcomes 3 years after application for all applicants while excluding the COVID-19 pandemic. Next, we limit the sample to applications handled within General tracks (246,558 observations). Assignment of caseworkers in these tracks is the most plausibly random given the plethora of applicants and caseworkers. Note, that we do not restrict our main analysis sample on age or disability explicitly, but because we drop tracks that handle applications for elderly and disabled applicants, this effectively restricts our sample to non-disabled working-aged applicants. The results are, however, nearly identical if we drop the few applicants in our sample outside working age.

We further drop applications assigned to workers who handled relatively few cases in a given year to ensure that there are enough observations to get an accurate estimate of caseworker decision making and also exclude new caseworkers who are given fewer and nonrandom sets of applications. Specifically, we drop the bottom 25% of the caseload distribution, which is 200 cases per year. This leaves us with 202,073 observations. Finally, we keep applications assigned to caseworkers with Conditional Caseworker Approval Rate (CCAR) values between the 5th and 95th percentiles of the CCAR distribution and to a balanced sample over time. These restrictions leave us with our final regression sample of 178,133 applications. We show below that this final analysis sample looks very similar to *all* working-age SNAP applicants in the mountain-plains state, and, we demonstrate robustness to these sample restriction decisions in the Appendix.

For a given applicant and application date, multiple caseworkers can work a single case and this happens to about 4% of applicants in our sample.[13] We address this complication by attributing SNAP receipt to the *first-assigned* caseworker. We define SNAP receipt using receipt in the month after the application. We do this because some applicants can be temporarily granted a few days of emergency SNAP prior to their intake interview and assignment to their caseworker. This prevents misclassifying these emergency allotments as a true SNAP approval.

3.2 Descriptive Statistics

To understand the external validity of our findings, we explore how working-age SNAP recipients in the mountain-plains state differ from working-age SNAP recipients in the whole country using the SNAP Quality Control (QC) Data in the first two columns of Table 1. On most dimensions, the mountain-plains state is similar to the national sample, except there are fewer recipients who identify as Black. And, importantly, the rate of employment and quarterly earnings are similar in the mountain-plains state and the national sample.

In column (3) of this table, we show equivalent statistics for all working-age SNAP

[13]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.

recipients using the mountain-plains data and this group is very similar to the sample from the QC data (column (2)) as expected. Especially important is that the likelihood of working and the value of real earnings among SNAP recipients is very similar in the QC data (measured as reported to the SNAP office) and our data (measured with the UI data). This supports the idea that the UI earnings data do a good job of fully capturing employment and earnings among our relevant population.

In the fourth column, we include all working-age *applicants* using the mountain-plains data. Applicants are similar to recipients (comparing columns (3) and (4)), though not identical; in particular, applicants are slightly younger, are slightly less racially diverse, and have somewhat smaller households. These differences are the result of two things: 1) not all applicants receive benefits, and 2) beneficiaries who receive SNAP for longer are weighted more heavily in column (3), and may be different than those who receive SNAP for shorter periods of time.

Finally, columns (5)-(7) implement our sample restrictions and include applicants, recipients, and those denied in our main analysis sample, respectively. Our analysis sample of applicants is very similar to the full sample of all working-aged SNAP applicants in the mountain-plains state, suggesting our results can be plausibly generalized. Additionally, we show in Appendix Figure A2 that the labor supply trends, both before and after SNAP application, of applicants in our analysis sample are almost identical to the labor supply trends of all, new applicants in the mountain plains state in our sample period.

Several other statistics are worth noting for the interpretation of results in the following sections. First, only 49% of new applicants receive benefits in the month of application. The probability of receipt in the entire year after the application is very similar. This is similar to the 44% acceptance rates found in Los Angeles during this same time period (Giannella et al., 2023). Additionally, only 36% of applicants in our sample are working in the quarter before application, and applicants have only $1,506 in real quarterly earnings (2012$s) before application on average. Even among those working, earnings are relatively low before application—only $4,964 quarterly. To give a frame of reference for this, one person working full time at minimum wage for a full quarter would earn $3,770, which is almost the same as the quarterly household income that puts a household of two just at the poverty line – $3,782 in 2012.

We compare this to a sample of working-age adults who are income-eligible for SNAP in our state in the CPS and find, in 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. This highlights a strength of our data—because our sample is *applicants* to SNAP, those that receive and are denied SNAP are more similar than if we compared outcomes within a sample that is all *income-eligible* for SNAP but some receive SNAP and some do not.

Finally, comparing SNAP recipients (column (6)) to those denied (column (7)), recipients are slightly less likely to be working pre-application (34% compared to 39%) and have lower earnings ($1,020 compared to $1,394). On the demographic variables, those granted and denied SNAP are similar. However, since we see that there are some differences in observables between those granted and those denied, we might worry there are differences in unobservables, and this motivates our IV approach.

4 Descriptive Results

Our data allow us to observe labor supply before SNAP application, so we begin by comparing trends in labor supply before *and* after application for those granted and denied SNAP benefits. While we saw evidence of differences in pre-period levels between those granted and denied SNAP above, of more concern for estimating the casual impact of SNAP using a panel design is whether there were differential pre-trends in labor supply. Figure 2 plots the labor supply outcomes across each quarter relative to SNAP application date. Those granted SNAP in the quarter of application have outcomes plotted in solid black lines, and those denied SNAP have outcomes plotted in dashed blue lines. These figures control only for application-date fixed effects.

For the full sample, in panels (a) and (b), the pre-trends are different across those granted and denied SNAP, especially in the quarters right before application. This motivates our use of the IV approach below. Additionally, there is some evidence of an "Ashenfelter dip" pattern, where employment and earnings decline right before SNAP application, and this decrease is larger for those eventually granted SNAP. Finally, for *both* those denied and granted SNAP there is a drop in earnings after SNAP receipt. This provides further suggestive evidence that some external event–such as a job loss–is occurring that causes households to experience a drop in earnings and to apply for SNAP.

Next, we split the sample by the quarterly earnings in the quarter before SNAP application. We create two groups—those with earnings above and below $3,770. We chose this earnings cutoff because it is the earnings of a full-time minimum wage worker over the entire quarter, but the results are robust to other cutoffs. We consider those with earnings above $3,770 to be "attached" to the labor market, and those with earnings below this cutoff to be partially attached or not attached.

The drop in earnings in the quarter after SNAP application that we saw for the full sample is driven by those who were attached to the labor market prior to applying for SNAP (panels (c) and (d)). On the other hand, those with less or no labor market attachment actually see an increase in employment and earnings after SNAP application, regardless of whether they received SNAP (panels (e) and (f)). These very different pre- and post-application labor supply outcomes across the two subgroups motivate us to split our main IV analysis below into these same groups.

5 The Role of Caseworkers

5.1 Estimating Caseworker Behavior

We build on the descriptive analysis above by using the assigned caseworker's application acceptance rate as an instrument for SNAP receipt. Caseworkers are randomly assigned to applicants in our sample, conditional on the timing of the application. Because of this, caseworkers' applicants have the same baseline likelihood of being approved, so differences in average caseworker approval rates must be driven by caseworker behavior. The Conditional Caseworker Approval Rate (CCAR) quantifies and aggregates caseworker behaviors that impact application acceptance.

We follow the newer examiner-effects literature to create the CCAR using the UJIVE approach ("unbiased jackknife instrumental variables estimator"). Kolesár (2013) proposed the UJIVE and it has been used in other recent papers including Norris et al. (2021) and Agan et al. (2023). It is also recommended as a best practice by Chyn et al. (2024).[14] Bringing this examiner-effects methodology into the setting of safety net program receipt to demonstrate the importance of caseworkers is an important contribution of our paper. To implement this, we estimate the two regression equations below for each observation in the data, omitting the focal application i in each iteration. Specifically, we estimate:

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

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

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

where $ReceiveSNAP_{-i}$ indicates whether applications, besides the focal application i, are observed receiving SNAP during the month after application. In each equation, we include a set of application-date fixed effects (λ_a and ϕ_a, respectively), which determines the set of caseworkers the applicant may be assigned to and is the level of randomization. Note, we do not observe the exact date that each applicant calls to conduct their interview, which is the true level of randomization, so we use the application start date to proxy for this. Equation (2) adds caseworker fixed effects (ρ_c). We then calculate the $CCAR_i$—the predicted approval likelihood for applicant i—by subtracting the predicted value of Equation (1) from the predicted value from Equation (2). In practice, we implement this procedure using the `manyiv` Stata package to calculate the UJIVE. Intuitively, this gives us each applicant's predicted likelihood of approval based solely on the caseworker they are assigned, netting out any heterogeneity due to application timing, and the caseworker's decision on the focal application. Thus, our instrument for SNAP receipt is unique to each application, though for simplicity we still sometimes refer to it as "the CCAR" or "caseworker's CCAR".

There is considerable variation in the CCAR as shown in panel (a) of Figure 3. The standard deviation in our sample of the CCAR is 0.03. We collapse the data to the caseworker level and show that a 10 percentage point increase in the average CCAR across caseworkers is associated with a 23 percentage point increase in their approval rate (panel (b)). This is a 47% increase of the overall approval rate in our sample of 49% (Table 1). We demonstrate below that the CCAR is strongly *causally* related to SNAP receipt (the first stage).

As a test of the exogeniety of caseworker assignment, we regress the assigned caseworker's caseload, months of experience, and applicant-specific CCAR onto baseline applicant demographics and pre-application labor supply—conditional on application-timing fixed effects. We contrast this with the relationship between whether an application is receives benefits 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 receipt, with an F-statistic of 82. 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 0.68 to 1.46. This provides evidence that caseworker assignment is indeed random, conditional on the fixed effects, supporting the independence assumption that the CCAR is unrelated to determinants of labor supply.

5.2 The Effect of Caseworkers on SNAP Receipt

Table 3 examines the effect of the CCAR on receipt of SNAP for the full sample (column 1) and broken out by baseline labor market attachment (columns 2 and 3) by quarter following the focal application. In the quarter of application, there is a large and statistically significant effect of the CCAR on benefit receipt. The coefficients indicate the effect of a unit increase in the CCAR, however the CCAR in our sample ranges from -0.07 to 0.06. So, to interpret this coefficient, we scale it by a one standard deviation increase in the CCAR (0.03). A one standard deviation change increases the likelihood of receiving SNAP in the quarter of application for the full sample by 2.6 percentage points, which is a 5% effect of the overall rate of acceptance of 49%. The F-statistic for the estimate on benefit receipt in the quarter of application is 124. 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 effects of the CCAR fade out in subsequent quarters, as shown in the rows of the table. The decline in the effect of the CCAR over time could be due to those denied reapplying and being accepted for SNAP, or those who initially receive SNAP not receiving it for very long. We investigate this and show re-approval rates are relatively low, so the latter mechanism drives these dynamic effects. These results are discussed in more detail in Appendix B.

Turning to heterogeneity by baseline labor market attachment, we see that among those who are attached to the labor market prior to applying, the effect of the CCAR on SNAP receipt fades out after 2 quarters. This is intuitive since most SNAP recipients face a 6-month recertification cycle and recertification is a common time for households to stop receiving SNAP, either because they became ineligible or because of the administrative costs imposed on participants to recertify eligibility (Unrath, 2024; Homonoff and Somerville, 2021).

We see a similar pattern for the group of applicants who were less attached to the labor market at baseline. The effect of the CCAR on SNAP receipt shrinks after two quarters, but does remain positive and at least marginally significant for several quarters after that. Among both groups, it appears that SNAP receipt is often limited to a single benefit cycle, which is consistent with many applicants using SNAP to weather a short-term shock and not remaining on the program indefinitely.

5.2.1 Mechanisms Behind the Effect of Caseworkers

We hypothesize that the main way caseworkers can impact applicants' outcomes is through assistance during the application process. To test this, we examine the relationship between the CCAR and the likelihood an applicant does not complete their application. Incomplete applications are those that are auto-denied for administrative reasons, withdrawn by the applicant, or those that failed to include all the required documentation. An incomplete application is the most common reason for denial–74% of applicants who are denied are denied for this reason. In Table 4, we regress onto the CCAR whether the given application was incomplete, conditional on application date fixed effects.

We find that, for the full sample in the first column, a one standard deviation increase in the CCAR decreases the likelihood of having an incomplete application by 1.3 percentage points, 4% of the sample mean. The results are similar across the subsamples in columns (2) and (3). This suggests that caseworkers with a higher CCAR are more helpful in ensuring that the applicant submits all the necessary information and completes the application process. This is in contrast to the findings in Finkelstein and Notowidigdo (2019), who show that likely-SNAP-eligible individuals who are pushed to apply are *more* likely to be rejected due to incomplete applications. However, their study is on a different population and on a different margin—elderly SNAP non-participants enrolled in Medicaid who have not applied for SNAP benefits. The difference is likely because in our setting all individuals have taken the first step to apply, whereas in their setting people are marginally pushed to apply and may be less likely to follow through with their application as a result.

We also explore what observable characteristics of caseworkers are correlated with their average CCAR in Appendix Table A3. While we do not see caseworker demographics, we do know information about their workload, how long they have worked as a caseworker, the team of other caseworkers they work with, and their manager. These teams are not always located in the same geographic place, but meet together and message each other virtually with regularity. In panel (a), we show the relationship between the CCAR and the caseworker characteristics listed in the column, conditional on application-date fixed effects. In panel (b), we report the adjusted R-squared from regressions where the CCAR is the dependent variable and the variables listed in the columns are the independent variables, along with application-date fixed effects. Panel (a), column (1), suggests that caseworkers with a higher CCAR also have a higher monthly caseload. This is somewhat mechanical since caseworkers that have a higher CCAR will have more cases granted SNAP and those cases stay assigned to that caseworker when they receive SNAP. The second column indicates that caseworkers who have been at the job longer have a lower CCAR. Though both these relationships are quantitatively small. Panel (b) indicates that the caseworker's team has the most explanatory power. This suggests peers or managers may impact the CCAR, which future research could investigate.

6 Instrumental Variables Approach

In order to identify the effect of SNAP on labor supply using our instrumental variables approach, we estimate the following:

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

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

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

We include fixed effects for the application date (θ_a and μ_a) to ensure that we compare applicants who are exposed to the same set of potential caseworkers. We include a vector of baseline controls X to improve statistical precision.[15] Results in the Appendix however confirm that estimates are stable to the exclusion of these controls. This design estimates the Local Average Treatment Effect (LATE) among the set of compliers, i.e., the SNAP applicants who are accepted, compared to those who are denied, because of the caseworker they are assigned.[16] Following best practices from recent design-based approaches to inference, we use heteroskedasticity-robust standard errors, but do not adjust for clustering because each applicant is randomly assigned to their own caseworker (Abadie et al., 2022; Chyn et al., 2024).

In our main analysis, we estimate this model by quarter-after-SNAP application. Following Frandsen et al. (2023), we also decompose the LATE into the potential outcomes under two alternative states of the world: 1) compliers receive SNAP due to their caseworker's CCAR ("treated compliers"), and 2) compliers are denied due to their caseworker's CCAR ("untreated compliers"). This is useful because it allows us to visualize

[15] The vector X includes the following head-of-household information: gender, race/ethnicity, citizenship, age, and Spanish speaking status. It also includes baseline labor supply information for each of the four quarters preceding the initial SNAP application, including: quarterly employment, earnings, indicators for quarterly earnings within $\$1 - \$1999$, arc percent of earnings, and industry experience.

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

levels of the outcomes of interest in both states of the world for the compliers before and after application. Specifically, we run the following regression to recover the outcomes for treated compliers: $y_i * ReceiveSNAP_i = \beta ReceiveSNAP_i + \theta_a + \zeta_i$ (5), using $CCAR_i$ as an instrument for the endogenous right-hand-side variable–$ReceiveSNAP_i$. Similarly, we run the following regression to recover the outcomes for *untreated* compliers: $y_i * (1 - ReceiveSNAP_i) = \beta(1 - ReceiveSNAP_i) + \theta_a + \zeta_i$ (6), using $CCAR_i$ as an instrument for the endogenous right-hand-side variable–$(1 - ReceiveSNAP_i)$. Intuitively, this gives us the average outcome if all marginal applicants either received or were denied benefits because of their caseworker's CCAR.

6.1 Validity of the CCAR as an Instrument for SNAP Receipt

6.1.1 Monotonicity

A key assumption underlying our research design is monotonicity of the instrument. Until recently, papers using examiner designs often invoked the strong assumption of pairwise monotonicity in order to ensure that IV estimates were properly-weighted aggregates of complier treatment effects. Intuitively, the assumption requires that if a caseworker with a higher CCAR is assigned to an application, this caseworker will be more likely to accept that application than a caseworker with a lower CCAR, regardless of case characteristics. A growing literature has emphasized the importance of this assumption and suggested tests that researchers can use to support its validity; Frandsen et al. (2023) propose a joint test for violations of either exclusion or pairwise monotonicity assumptions. In our empirical design, we reject the null hypothesis that both conditions are satisfied. Fortunately, Frandsen et al. (2023) also show that under a relaxed "average monotonicity" assumption, IV still estimates a convex combination of treatment effects. Average monotonicity requires that for each individual, the covariances between that individual's caseworker-specific treatment status and caseworker overall CCAR are positive. Two testable implications of this assumption are: 1) the first stage estimates for all sub-samples should yield positive estimates, and, 2) there should be a positive relationship between the CCAR for the full sample and the CCAR for various subgroups. In Appendix Table A4 and Appendix Figure A3, 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.[17]

[17] Other prominent papers fail pairwise 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

6.1.2 Exclusion Restriction

The exclusion restriction requires that caseworkers only impact applicant outcomes through the proposed causal channel: whether the applicant is approved for SNAP. In the state we study, caseworkers have a limited scope for affecting applicants outside of the SNAP application process. Caseworkers interact with applicants during a short mandatory phone interview, the purpose of which is to simply verify the information on the application form. Caseworkers can also message applicants via the online system or use snail-mail correspondence to notify applicants if there are any issues with their application. The mountain-plains state 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. Caseworkers are instructed to focus on the given application and not to direct applicants to other sources of government support or provide any sort of labor market advice or resources. If the applicant did decide to apply to other programs, they would need to start a brand new application for the given program, which would be handled by a different caseworker from the other tracks. Indeed, when we regress whether the applicant receives TANF onto the CCAR, we estimate a precise zero. Unfortunately, we do not have data on Medicaid enrollment and thus cannot directly test for effects on Medicaid. However, we also investigated changes in cross-program participation at initial SNAP receipt using the Survey of Income and Program Participation in Appendix C, the results of which also suggest that other programs are unlikely to drive our main results.

6.1.3 Targeting Effects of Caseworkers

To understand who is pushed into receiving SNAP because of their caseworker, we explore the characteristics of compliers following the method outlined in Frandsen et al. (2023). While the IV estimates are internally valid regardless of impacts on targeting, this analysis helps us interpret the LATE we estimate with the IV approach. To do so, we estimate Equation (5) instrumenting with the CCAR, but replace the labor supply outcome with various applicant characteristics interacted with an indicator for whether the applicant received SNAP during the month after application.[18] The first row in Table 5 shows the characteristics of the compliers calculated using this method. The second and third rows show the average of the

argue that the "pro-participation" attitude of the caseworker is the primary determinant of the CCAR and provide evidence to support this above.

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

same characteristics for the full analysis sample of applicants and the sample of applicants who receive benefits in the month after application, respectively. The fourth and fifth rows provide the ratio of the complier characteristics to the full sample characteristics to test if the compliers differ significantly from all applicants and beneficiaries, respectively. The statistical test shows whether this ratio is significantly different from one.

In general, compliers seem very similar to a full sample of beneficiaries in terms of attachment to the labor market. Compliers are also a similar age and gender and more likely, though not significantly so, to be Black or Hispanic. We also explore whether compliers are those closer to the income-eligibility cutoff; we use the observed earnings in the UI data relative to the eligibility threshold based on their household size. This is a coarse measure of eligibility, but we see no statistically-significant evidence that compliers are closer to the eligibility margin.[19]

Overall, there is no conclusive evidence that caseworkers affect targeting. Given these findings, it is plausible that the LATE we estimate can apply to the population of beneficiaries more generally. It is also informative to compare our findings to that of Finkelstein and Notowidigdo (2019), who show assistance and informational interventions for likely-SNAP-eligible populations that push more people to apply for SNAP reduce targeting. In contrast, everyone in our sample has already chosen to apply for SNAP and overcome the initial costs of doing so. The evidence indicates that caseworkers with higher CCARs are providing help to all applicants they interact with, regardless of the applicants' characteristics, which is why they have little impact on targeting. Our findings highlight the importance of studying the impacts of different interventions within the same program to fully understand the targeting impacts of changing administrative burdens.

6.2 Labor Supply Results

In the descriptive analysis above, we documented that there are differential pre- and post-trends in labor supply outcomes based on pre-SNAP-application labor market attachment. Given this, we continue to split the sample into two groups based pre-SNAP-application labor market attachment: 1) those with attachment prior to SNAP application, meaning they earned at least full-time minimum wages in the quarter before SNAP application ($3,770), and, 2) those with partial or no labor market attachment prior to SNAP application, who earned less than $3,770 in the quarter before application.

[19]Targeting results are similar when we split the sample by baseline labor market attachment. Results available upon request.

We begin by focusing on those who were more attached to the labor market at baseline, because this is the group that likely faces fewer barriers to work and might plausibly respond to the receipt of SNAP as predicted by canonical economic theory. Figure 4 shows the IV results on this group by quarter after SNAP application. The left-hand side panels show the point estimates and standard errors from the IV model, which are also reported in Table 6. These estimates correspond to taking the difference between the potential outcomes, which are shown in the right hand side panels. These potential outcomes help us to understand mechanisms, as we discuss below.

Focusing first on quarterly earnings in panel (a), we see that SNAP significantly reduces earnings in the quarter of SNAP application. In this quarter, earnings are reduced by $1,418, which is 27% relative to the baseline mean of $5,294 (reported in Table 6). This effect shrinks and becomes statistically insignificant after the quarter of application. Finally, beginning in the ninth quarter after SNAP application, there is a statistically significant *positive* effect of SNAP on earnings. SNAP increases earnings in the third year after application by $3,454-4,694, or 65-89% relative the baseline mean.

Quarterly employment, shown in panel (c), follows a similar pattern as earnings, except there is no significant decrease in the quarter of application, suggesting changes in earnings in that quarter occur on the intensive rather than extensive margin. Looking at the results for the earnings bins in the quarter of application confirms this. There is a significant shift from earning more than $2,000 per quarter (panel (g)), to less than $2,000 per quarter, but still having positive earnings (panel (e)), in the quarter of application. We chose this $2,000 cutoff because it is about the earnings of someone working part-time at minimum wage an entire quarter, but results are similar if we vary this cutoff slightly. Thus, those who receive SNAP still continue to work in the quarter of application, but work less intensely than those denied SNAP. Unfortunately, we do not observe hours or months worked, so we cannot directly test changes on these margins of labor supply.

After the quarter of application, and through the eighth quarter after application, the effects on employment and both earnings bins are close to zero and insignificant. Then, beginning in the ninth quarter, the effects on employment and earnings above $2,000 become positive and significant. In the third year after SNAP application, employment is 0.55-0.77 percentage points higher for those who received SNAP. Taken together, this suggests that the positive longer-run effects on average earnings in panel (a) are driven by an increase in the likelihood of working at all, and earning more than $2,000 per quarter, instead of a shift from earning less than $2,000 to more than $2,000.

To better understand the mechanisms, we turn to the potential outcome results on the right-hand side of the figure. We decompose the LATE from the IV model into the potential outcomes at the quarterly level for the compliers under two alternative states of the world: 1) "treated compliers" and 2) "untreated compliers". In both states of the world, there is a large drop in earnings and a smaller drop in employment in the quarter of application, relative to the baseline mean shown in the panels. This again suggests that SNAP applicants experience a shock that negatively impacts their labor supply *whether or not they receive SNAP* (as in Figure 2). Further, in the state of the world where compliers are all denied SNAP, shown in blue, they experience a sharp downward trajectory in their earnings and employment in the longer-run. Three years after application, the likelihood of quarterly employment in this state of the world is less than 25%, which is striking given that this entire group worked in the quarter before applying for SNAP. Average earnings also fall to close to zero after three years in this state of the world. On the other hand, in the state of the world where compliers receive SNAP, shown in orange, earnings and employment rebounds to roughly the pre-application baseline mean after three years.

Looking at the results for the earnings bins, the dynamic pattern for earning more than $2,000 per quarter is the same as for employment and earnings. The likelihood of earning less than $2,000 per quarter increases in the quarter of application, but then decreases, and, in subsequent quarters the potential outcomes in both states of the world are very similar.

This overall pattern of a short-run decrease followed by a positive rebound is consistent with the receipt of SNAP helping people who work buffer against a negative shock. In the short-run, when people are experiencing the shock most acutely, those who receive SNAP reduce labor supply along the intensive margin. But, in the longer-run, the receipt of SNAP helps individuals weather the shock and recover labor supply back to pre-application levels. This finding is very consistent with a dynamic job search model.

How exactly might these effects operate? While we cannot test these mechanisms directly, we can learn from the past literature. First, SNAP could help people who lost a job to search for a higher-quality job, as has been found in the Unemployment Insurance context (e.g. Nekoei and Weber, 2017). Second, SNAP could allow recipients to pay for goods and services necessary to prevent cascading events creating a downward trajectory, such as an eviction, which causes a reduction in employment and earnings (Collinson et al., 2022). This consumption-smoothing benefit is likely important among this population because they live hand to mouth and are credit constrained. Only 62% of SNAP recipients have bank accounts before receiving SNAP, and, among those with accounts, the median balance is only $389

(2012 dollars).[20] Moreover, 17% of SNAP recipients paid their rent late, 11% paid utility bills late, many reported having to decide between spending money on food or on rent and utilities, and are in danger of eviction (Propel, 2023). Among SNAP recipients with children, the majority have expenses that exceed their income in a given month and they report SNAP benefits help alleviate this deficit, though not entirely.

6.2.1 Results for Those Less Attached to the Labor Market

Next, we turn to the group of SNAP applicants partially attached, or not attached at all, to the labor market in the quarter before applying for SNAP. This group likely faces some barriers to work and may have a harder time finding a job, so the predictions of the canonical labor supply model may not apply to them. We show the results for this group in Table 7 and Appendix Figure A5. Overall, there is little evidence of significant changes to this group's labor market outcomes in either the short or longer-run. We can rule out changes in earnings of larger than a $268 increase in quarterly earnings and a $300 decrease, in the quarter of application.

The null results are consistent with the hypothesis that these SNAP applicants face barriers to work regardless of whether they receive SNAP benefits. In a different context, Gray et al. (2022) and Cook and East (2024) argue these barriers largely explain the lack of effects of SNAP work requirements on labor supply.

6.2.2 Discussion

The prior literature on the impact of SNAP on labor supply studying plausibly generalizable samples suggests negative-to-null effects of SNAP on labor supply, though with wide confidence intervals. The closest paper to ours studies the effects of the rollout of Food Stamps in the 1960-70s (Hoynes and Schanzenbach, 2012). They analyze the effects on a variety of potentially affected subgroups and mostly find negative, but insignificant, effects. Focusing on the outcome most similar to what we can measure—annual earnings—Hoynes and Schanzenbach estimate a 95% confidence interval that ranges from a decrease in earnings of $2,510 to an increase of $1,990 for all households where the head of household has a high school education or less. For female-headed households, the 95% confidence interval includes a decrease in earnings of $4,361 to an increase of $1,830. Finally, for non-white female-headed households, the effect is statistically significant and the 95% confidence intervals include a decrease in earnings of $6,834 to a decrease of $698.

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

In comparison, our estimates generally allow for stronger conclusions based on the precision we have. We are also able to make several other contributions beyond this past study. First, we can identify who actually receives SNAP, instead of the previous estimates that are among subgroups likely impacted by SNAP based on observable demographic characteristics. Second, our panel data allow us to study dynamic effects within individuals that receive SNAP, compared to those who are denied. This reveals important heterogeneity in the effects over time and sheds light on what model of labor supply might be most appropriate in this setting. Finally, we analyze a more modern time period which expands our present understanding of SNAP's labor effects, as many things have changed since the 1960-70s; for instance, reductions in women's labor supply elasticities over time make women less responsive to transfer programs (Bishop et al., 2009; Kumar and Liang, 2014).

6.2.3 Specification Checks

We test the sensitivity of the IV results to our sample construction and model choices in Appendix Tables A5 and A6. The estimates in panel (a) use the baseline specification. Panel (b) removes all demographic and baseline labor supply controls that we included to enhance statistical precision. In our baseline sample, we omit applications with a CCAR below the 1st and above the 99th percentiles. We show the robustness to further restricting extreme CCAR values in panels (c) and (d), respectively. Additionally, in our baseline sample we drop applications assigned to caseworkers with a number of decisions below the 25th percentile. The estimates in panels (e) and (f) show results with alternative cutoffs—the 10th percentile and 30th percentile, respectively. The results are very similar across all these choices, demonstrating none of these decisions drive our findings.

Next, in Appendix Table A7, we reproduce our main results and then limit the sample to only households with one working-aged adult to ensure our results are similar for households where we observe all potential earners in the earnings data. The results are nearly the same for this subgroup as for the full sample, so having the UI earnings information only for heads of households is not important for the broader conclusions.

6.2.4 Welfare Effects

We quantify the results in a social welfare framework using the Marginal Value of Public Funds (MVPF) approach in Hendren and Sprung-Keyser (2020). Specifically, we calculate the MVPF of the CCAR being one standard deviation higher.[21] The MVPF is the ratio of

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

benefits to net government costs of the policy change, defined as:

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

The numerator is the willingness to pay to get SNAP benefits for SNAP applicants, which we assume to be equivalent to the change in the benefit amount paid due to a one standard deviation increase in the CCAR. The denominator is the direct cost of operating the program (C) for marginal recipients, including benefits paid out, administrative costs, and any fiscal externalities (FE) due to changes in behavior for marginal recipients. We focus on fiscal externalities due to changes in labor supply here. However, the fiscal externalities of a program like SNAP are complex and include effects beyond just the labor supply response of adult recipients.

We estimate an MVPF of SNAP due to a one standard deviation increase in caseworker CCAR of 3.3, indicating the value to beneficiaries is larger than the net cost to the government.[22] In fact, we find that the effect of SNAP on government revenue due only to changes in labor supply over three years is *positive* because the longer-run positive effects outweigh the short-term negative ones.

Prior estimates of the MVPF of increasing access to SNAP range from 0.89 to 56.25. However, it is important to note that the study that produces estimates close to 1 are unable to examine any benefits to SNAP recipients beyond the direct value of the transfers themselves (Finkelstein and Notowidigdo, 2019), whereas the study that produces a higher MVPF is able to use rich data to quantify many other benefits and the fact that enough time has passed to observe longer-run effects (Bailey et al., 2020). This highlights a perennial challenge with analyzing the costs and benefits of safety net programs–the costs are often borne out in the short-run and are relatively easy to quantify, whereas many of the benefits, including improvements in health and labor market outcomes (Bailey et al., 2020) and reductions in crime (Barr and Smith, 2023), only appear much later and are harder to measure.

7 Conclusion

This paper examines the effect of SNAP on labor supply decisions using an examiner design. We are the first to bring this design to the setting of means-tested transfer program receipt in the United States. We show that caseworker behavior matters for determining

[22] Appendix D provides the details of the MVPF calculation.

whether SNAP applicants receive benefits and provide evidence that this operates through caseworkers helping applicants navigate the complex application process.

We also provide generalizable estimates of the effect of access to modern SNAP on labor supply using high-quality administrative data. Among the minority of our sample that worked in the year leading up to their SNAP application, SNAP appears to act as insurance against negative shocks and reduces earnings temporarily, but increases earnings and the likelihood of work in the longer-run. Among the majority not working the year before applying for SNAP, the receipt of SNAP has no impact on their labor supply decisions. We posit these applicants likely face other, larger barriers to work that dominate any potential effect of SNAP.

While our analysis is for a single state, we show a variety of evidence that suggests our results are generalizable. First, our analysis sample looks very similar to the full sample of working-aged applicants in the mountain-plains state, on both demographics as well as levels and trends in labor supply. Second, the labor supply of SNAP recipients in our state is very similar to the labor supply of a national sample of SNAP recipients. And, finally, we show the compliers in our IV approach are similar to the full sample of SNAP applicants.

Recently, lawmakers have raised concerns about work disincentives from SNAP and other means-tested transfer programs; work requirements were expanded under the first Trump administration, changed as a result of the 2023 debt ceiling negotiations, and are again being suggested in policy debates including in Project 2025 (Bakst, 2024). Our findings inform this debate; we find no evidence that receiving SNAP leads to long-term reductions in labor supply or dependency on government benefits. If anything, our results suggest the opposite—SNAP provides support for those who are unable to work and provides important insurance for workers experiencing a negative shock.

References

Abadie, A., S. Athey, G. W. Imbens, and J. M. Wooldridge (2022). When should you adjust standard errors for clustering? *The Quarterly Journal of Economics 138*(1), 1–35. Publisher: Oxford Academic.

Abraham, K. G., B. Hershbein, S. N. Houseman, and B. Truesdale (2023). The Independent Contractor Workforce: New Evidence on Its Size and Composition and Ways to Improve Its Measurement in Household Surveys. Technical report, National Bureau of Economic Research.

Agan, A., J. L. Doleac, and A. Harvey (2023, January). Misdemeanor Prosecution. *The Quarterly Journal of Economics*, qjad005.

Autor, D., A. Kostøl, M. Mogstad, and B. Setzler (2019). Disability benefits, consumption insurance, and household labor supply. *American Economic Review 109*(7), 2613–2654. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Autor, D. H. and S. N. Houseman (2010). Do temporary-help jobs improve labor market outcomes for low-skilled workers? Evidence from "Work First". *American economic journal: applied economics 2*(3), 96–128. Publisher: American Economic Association.

Baicker, K., A. Finkelstein, J. Song, and S. Taubman (2014). The impact of Medicaid on labor market activity and program participation: evidence from the Oregon Health Insurance Experiment. *American Economic Review 104*(5), 322–328. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Bailey, M. J., H. W. Hoynes, M. Rossin-Slater, and R. Walker (2020). Is the social safety net a long-term investment? Large-scale evidence from the food stamps program. Technical report, National Bureau of Economic Research.

Bakst, D. (2024). Project 2025. Accessed: 2025-1-8.

Barr, A. and A. A. Smith (2023). Fighting Crime in the Cradle The Effects of Early Childhood Access to Nutritional Assistance. *Journal of Human Resources 58*(1), 43–73. Publisher: University of Wisconsin Press.

Bartlett, S., N. Burstein, W. Hamilton, R. Kling, and M. Andrews (2004). Food Stamp Program Access Study: Final Report. *Electronic Publications from the Food Assistance & Nutrition Research Program E-FAN-03-013-3*, 1–40.

Bauer, L., C. N. East, and O. Howard (2025). Low-income workers experience the most earnings and work hours instability - The Hamilton Project.

Bhargava, S. and D. Manoli (2015, November). Psychological Frictions and the Incomplete Take-Up of Social Benefits: Evidence from an IRS Field Experiment. *American Economic Review 105*(11), 3489–3529.

Bishop, K., B. Heim, and K. Mihaly (2009, October). Single Women's Labor Supply Elasticities: Trends and Policy Implications. *ILR Review 63*(1), 146–168.

Bitler, M., J. Cook, and J. Rothbaum (2021). Working for Your Bread: The Labor Supply Effects of SNAP. *AEA Papers and Proceedings 111*, 496–500.

Bolhaar, J., N. Ketel, and B. van der Klaauw (2020). Caseworker's discretion and the effectiveness of welfare-to-work programs. *Journal of Public Economics 183*, 1–19.

Bullinger, L. R. and T. Gurley-Calvez (2016). WIC participation and maternal behavior: breastfeeding and work leave. *Contemporary Economic Policy 34*(1), 158–172. Publisher: Wiley Online Library.

Butcher, K. F. and D. W. Schanzenbach (2018). Most Workers in Low-Wage Labor Market Work Substantial Hours, in Volatile Jobs. *Center for Budget and Policy Priotities*.

Chan, D. C., M. Gentzkow, and C. Yu (2022). Selection with variation in diagnostic skill: Evidence from radiologists. *The Quarterly Journal of Economics 137*(2), 729–783. Publisher: Oxford University Press.

Chetty, R., J. N. Friedman, E. Saez, N. Turner, and D. Yagan (2017). Mobility report cards: The role of colleges in intergenerational mobility. *American Economic Review 107*(10), 1–44.

Chyn, E. (2018). Moved to opportunity: The long-run effects of public housing demolition on children. *American Economic Review 108*(10), 3028–3056. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Chyn, E., B. Frandsen, and E. C. Leslie (2024, April). Examiner and Judge Designs in Economics: A Practitioner's Guide.

Cohen, E. (2024). Housing the Homeless: The Effect of Placing Single Adults Experiencing Homelessness in Housing Programs on Future Homelessness and Socioeconomic Outcomes. *American Economic Journal: Applied Economics 16*(2), 130–175. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203-2425.

Collinson, R., J. E. Humphries, N. S. Mader, D. K. Reed, D. I. Tannenbaum, and W. Van Dijk (2022). Eviction and poverty in American cities. Technical report, National Bureau of Economic Research.

Cook, J. B. and C. N. East (2024, May). The Disenrollment and Labor Supply Effects of SNAP Work Requirements.

Cox, E., C. East, and I. Pula (2024). Beyond hunger: The role of SNAP in alleviating financial strain for low-income households.

Cronquist, K., S. Lauffer, C. Tadler, S. Hong, A. Vigil, E. Wilcox-Cook, K. Farson Gray, and B. Miller (2024). SNAP Quality Control Data.

Cuffey, J., T. K. M. Beatty, and E. Mykerezi (2022, January). Work Effort and Work Requirements for Food Assistance among U.S. Adults. *American Journal of Agricultural Economics 104*(1), 294–317.

Currie, J. (2006). The Take Up of Social Benefits. In *Poverty, the Distribution of Income, and Public Policy*, pp. 80–148.

Danziger, S., M. Corcoran, S. Danziger, C. Heflin, A. Kalil, J. Levine, D. Rosen, K. Seefeldt, K. Siefert, and R. Tolman (2000). Barriers to the employment of welfare recipients. *Prosperity for all? The economic boom and African Americans*, 245–78. Publisher: New York: Russell Sage Foundation.

Deshpande, M. and Y. Li (2019). Who Is Screened Out? Application Costs and the Targeting of Disability Programs. *American Economic Journal: Economic Policy 11*(4), 213–248. Number: 4.

Dobbie, W., J. Goldin, and C. S. Yang (2018). The effects of pre-trial detention on conviction, future crime, and employment: Evidence from randomly assigned judges. *American Economic Review 108*(2), 201–240. Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Doyle Jr, J. J. (2007). Child protection and child outcomes: Measuring the effects of foster care. *American Economic Review 97*(5), 1583–1610. Publisher: American Economic Association.

East, C. N. (2016). The Labor Supply Response to Food Stamp Access. Technical report.

Edmiston, K. D. (2019). Why Aren't More People Working in Low-and Moderate-Income Areas? *Economic Review-Federal Reserve Bank of Kansas City 104*(4), 41–0_3. Publisher: Federal Reserve Bank of Kansas City.

Finkelstein, A. and M. J. Notowidigdo (2019). Take-Up and Targeting: Experimental Evidence from SNAP. *The Quarterly Journal of Economics 134*(3), 1505–1556. Number: 3.

Flood, S., M. King, R. Rodgers, S. Ruggles, R. Warren, D. Backman, A. Chen, G. Cooper, S. Richards, M. Schouweiler, and M. Westberry (2023). IPUMS CPS: Version 11.0.

Frandsen, B., L. Lefgren, and E. Leslie (2023). Judging Judge Fixed Effects. *American Economic Review 113*(1), 253–277.

Garthwaite, C., T. Gross, and M. J. Notowidigdo (2014). Public health insurance, labor supply, and employment lock. *The Quarterly Journal of Economics 129*(2), 653–696. Publisher: MIT Press.

Giannella, E., T. Homonoff, G. Rino, and J. Somerville (2023). Administrative Burden and Procedural Denials: Experimental Evidence from SNAP.

Gray, C., A. Leive, E. Prager, K. Pukelis, and M. Zaki (2022). Employed in a SNAP? The Impact of Work Requirements on Program Participation and Labor Supply. *American Economic Journal: Economic Policy*.

Han, J. (2022, January). The impact of SNAP work requirements on labor supply. *Labour Economics 74*, 102089.

Hendren, N. and B. Sprung-Keyser (2020). A unified welfare analysis of government policies. *The Quarterly Journal of Economics 135*(3), 1209–1318. Publisher: Oxford University Press.

Herd, P. and D. Moynihan (2019). *Administrative burden: Policymaking by other means.* Russel Sage Foundation.

Homonoff, T. and J. Somerville (2021). Program recertification costs: Evidence from snap. *American Economic Journal: Economic Policy 13*(4), 271–298.

Hoynes, H. W. and D. W. Schanzenbach (2012). Work incentives and the Food Stamp Program. *Journal of Public Economics 96*(1-2), 151–162. Number: 1-2 Publisher: Elsevier B.V.

Jonassen, A. B. (2013). Disincentive Effects of a Generous Social Assistance Scheme. *Working Paper*.

Kaestner, R., B. Garrett, J. Chen, A. Gangopadhyaya, and C. Fleming (2017). Effects of ACA Medicaid expansions on health insurance coverage and labor supply. *Journal of Policy Analysis and Management 36*(3), 608–642. Publisher: Wiley Online Library.

Keith-Jennings, B. and R. Chaudhry (2018). Most Working-Age SNAP Participants Work, But Often in Unstable Jobs | Center on Budget and Policy Priorities.

Kleven, H. J. and W. Kopczuk (2011). Transfer program complexity and the take-up of social benefits. *American Economic Journal: Economic Policy 3*(1), 54–90. Number: 1.

Kogan, V. (2017). Administrative centralization and bureaucratic responsiveness: Evidence from the food stamp program. *Journal of Public Administration Research and Theory 27*(4), 629–646. Number: 4.

Kolesár, M. (2013). Estimation in an Instrumental Variables Model with Treatment Effect Heterogeneity.

Kumar, A. and C.-Y. Liang (2014). Declining Female Labor Supply Elasticities in the U.S. and Implications for Tax Policy: Evidence from Panel Data. *Proceedings. Annual Conference on Taxation and Minutes of the Annual Meeting of the National Tax Association 107*, 1–38. Publisher: National Tax Association.

Lang, K. and A. K.-L. Spitzer (2020). Race Discrimination: An Economic Perspective. *Journal of Economic Perspectives 34*(2), 68–89.

Leftin, J., N. Wemmerus, J. Mabli, T. Godfrey, and S. Tordella (2014). Dynamics of Supplemental Nutrition Assistance Program Participation from 2008 to 2012.

Maestas, N., K. J. Mullen, and A. Strand (2013). Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt. *American Economic Review 103*(5), 1797–1829.

Maneely, J. and C. Roth-Eisenberg (2020, August). Streamlining SNAP for the Gig Economy.

Meyer, B. D., N. Mittag, and R. M. Goerge (2022). Errors in survey reporting and imputation and their effects on estimates of food stamp program participation. *Journal of Human Resources 57*(5), 1605–1644. Publisher: University of Wisconsin Press.

Meyer, B. D., W. K. Mok, and J. X. Sullivan (2009). The under-reporting of transfers in household surveys: its nature and consequences. Technical report, National Bureau of Economic Research.

Meyers, M. K. and V. L. Nielsen (2012). Street-level bureaucrats and the implementation of public policy. *The SAGE handbook of public administration*, 305–318. Publisher: Sage Los Angeles, CA.

Minoff, E. (2020). The Racist Roots of Work Requirements. *Center for the Study of Social Policy*.

Moffitt, R. a. (2002). Welfare programs and labor supply. *Handbook of Public Economics 4*, 2393–2430. ISBN: 9780444823151.

Moynihan, D., E. Giannella, P. Herd, and J. Sutherland (2022). Matching to categories: Learning and compliance costs in administrative processes. *Journal of Public Administration Research and Theory 32*(4), 750–764. Publisher: Oxford University Press US.

Nekoei, A. and A. Weber (2017). Does Extending Unemployment Benefits Improve Job Quality? *American Economic Review 107*(2), 527–561.

Nichols, A. L. and R. J. Zeckhauser (1982). Targeting transfers through restrictions on recipients. *The American Economic Review 72*(2), 372–377. Publisher: JSTOR.

Norris, S., M. Pecenco, and J. Weaver (2021). The Effects of Parental and Sibling Incarceration: Evidence from Ohio. *The American Economic Review 111*(9), 39. Number: 9.

Nunn, R., J. Parsons, and J. Shambaugh (2019). Race and underemployment in the US labor market.

Propel (2023). June 2023 Pulse Survey.

Rossin-Slater, M. (2013). WIC in your neighborhood: New evidence on the impacts of geographic access to clinics. *Journal of Public Economics 102*, 51–69.

Schanzenbach, D. W. (2009). Experimental estimates of the barriers to Food Stamp enrollment. *Institute for Research on Poverty, University of Wisconsin-Madison* (1367). Number: 1367.

Stacy, B., E. Scherpf, and Y. Jo (2018). The Impact of SNAP Work Requirements.

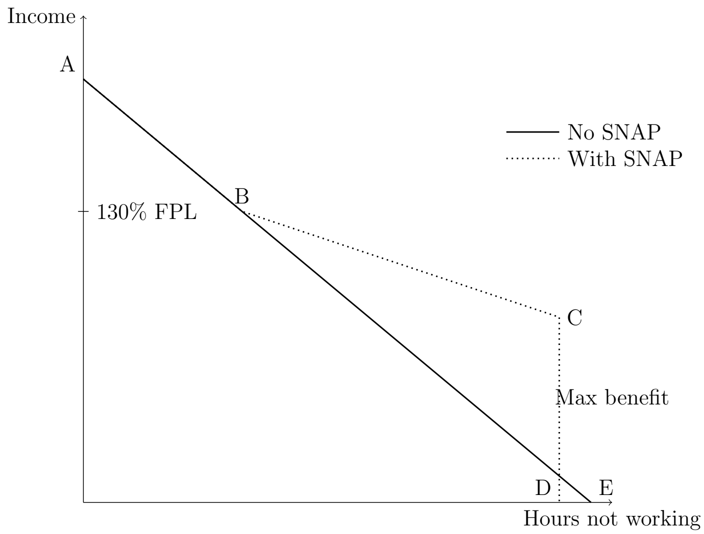
Turner, M. A., M. Fix, and R. J. Struyk (1991). *Opportunities denied, opportunities diminished: Racial discrimination in hiring.* The Urban Insitute.

Unrath, M. (2024). Targeting, Screening, and Retention: Evidence from California's Food Stamps Program. *California Policy Lab Working Paper 1*(1), 1–56. Number: 1.

Vericker, T., L. Wheaton, K. Baier, and J. Gasper (2023, April). The Impact of ABAWD Time Limit Reinstatement on SNAP Participation and Employment. *Journal of Nutrition Education and Behavior 55*(4), 285–296.

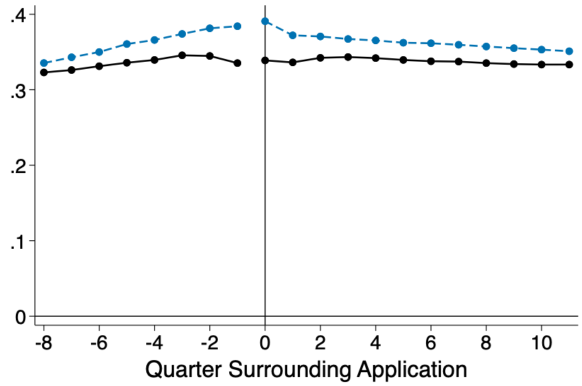
Wu, D. and B. Meyer (2021). Certification and Recertification in Welfare Programs: What Happens When Automation Goes Wrong?

Figure 1: SNAP Budget Constraint

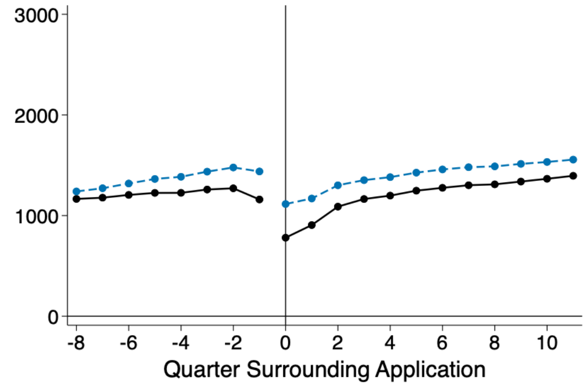


Notes: The solid line plots the budget constraint when there are no SNAP benefits, and the dotted line represents the budget constraint when receiving SNAP benefits. Line AB is the same in both SNAP and no SNAP cases and has a slope equivalent the market wage. The line CD represents the benefit guarantee, which increases income among those not working. In the segment BC the slope is shallower than that of AB, representing the benefit reduction rate. Finally, with no SNAP benefits, the slope of BE remains equivalent to the market wage.

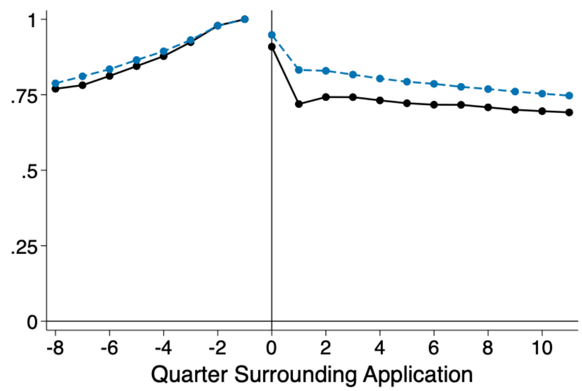
Figure 2: Differences in Quarterly Labor Supply by SNAP Receipt at $t = 0$ (Full Sample)



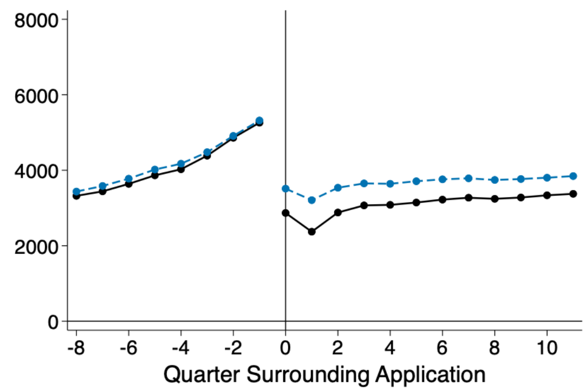
(a) Employment



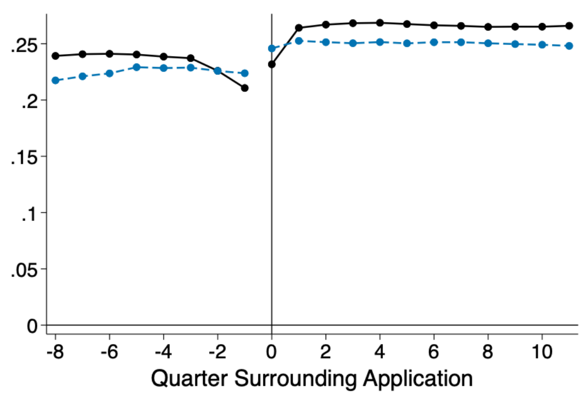
(b) Earnings



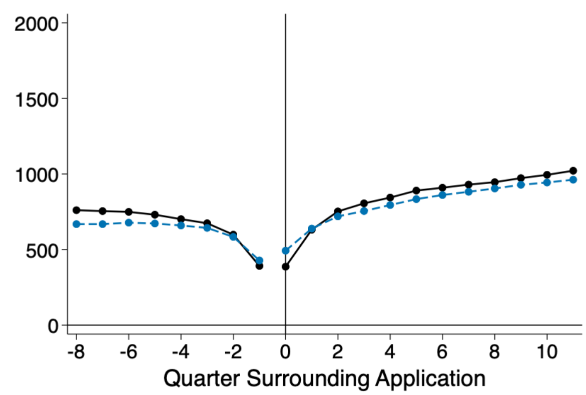
(c) Employment, Attached to Labor Market



(d) Earnings, Attached to Labor Market



(e) Employment, Partially-Attached/Unattached to Labor Market

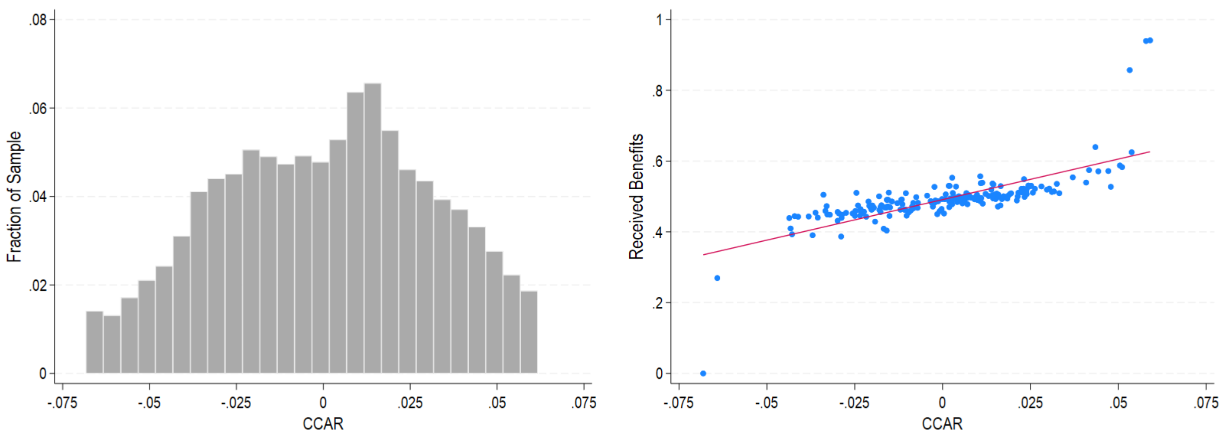


(f) Earnings, Partially-Attached/Unattached to Labor Market

Received SNAP   Denied SNAP

Notes: These figures show the results from running separate regressions for the given event time of the outcome. We regress the given outcome in the given period on whether the applicant received SNAP during period 0 along with application date fixed effects. The blue dashed line is the coefficient on the constant from those regressions and the black solid line is the coefficient on the SNAP indicator added to the constant coefficient. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

Figure 3: Distribution of the CCAR and its Relationship with Benefit Receipt

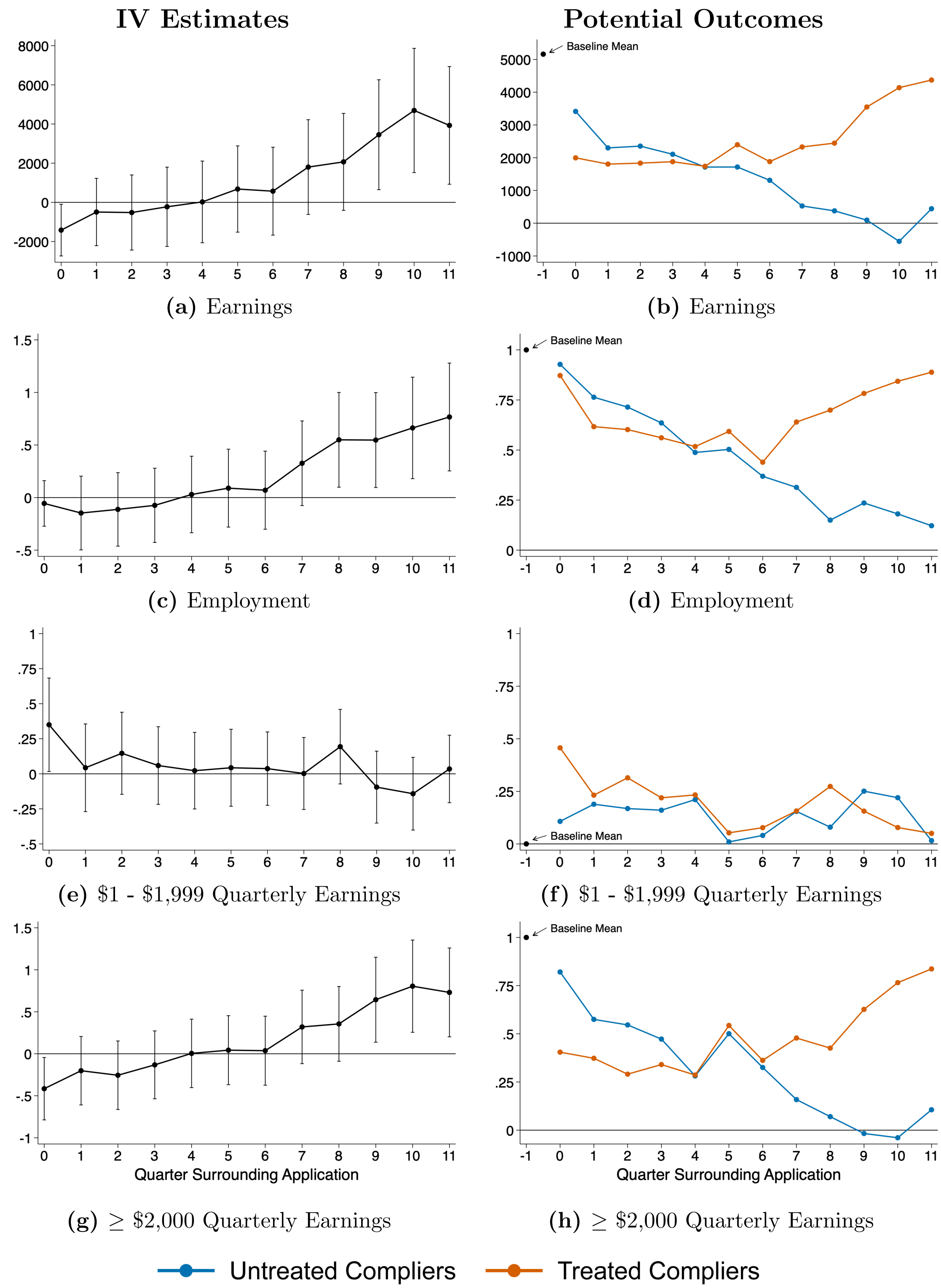


(a) Histogram of CCAR

(b) Scatterplot of CCAR and Benefit Receipt

Notes: Panel (a) plots the histogram of our calculated CCAR for the main sample. Panel (b) is at the caseworker level and plots the relationship between the caseworker-level average CCAR and the SNAP acceptance rate of applicants for each caseworker. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.

Figure 4: IV Estimates and Potential Outcomes for those Attached to Labor Market at Baseline



(a) Earnings

(b) Earnings

(c) Employment

(d) Employment

(e) \$1 - \$1,999 Quarterly Earnings

(f) \$1 - \$1,999 Quarterly Earnings

(g) $\geq$ \$2,000 Quarterly Earnings

(h) $\geq$ \$2,000 Quarterly Earnings

Notes: These figures depict the potential outcomes in the state of the world that complier applicants are either approved or denied SNAP due to their caseworker's CCAR. Section 6 details the method. We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. The blue line plots the potential outcomes for untreated compliers (i.e., the state of the world where compliers are denied SNAP) and the orange line plots the potential outcomes for treated compliers (i.e., the state of the world where compliers receive SNAP).

Table 1: Summary Statistics

	All SNAP Recipients (QC Data)		Mountain Plains Administrative Data				
					Analysis Sample		
	National	Our State	All Recipients	All Applicants	New Applicants	New Recipients	New Denials
Monthly Receipt of Benefits	1.000	1.000	1.000	0.525	0.487	1.000	0.000
Female	0.697	0.694	0.691	0.634	0.596	0.579	0.613
Age	39.384	37.635	37.517	33.185	32.991	34.209	31.836
Hispanic	-	-	0.110	0.123	0.084	0.085	0.084
Black	0.259	0.029	0.039	0.020	0.020	0.021	0.018
Pacific Islander	0.004	0.015	0.014	0.011	0.012	0.012	0.013
Asian	0.015	0.013	0.019	0.010	0.010	0.009	0.010
Any Kids Under Age 5	0.250	0.332	-	-	-	-	-
Number of Kids	1.024	1.377	1.372	0.907	0.827	0.844	0.811
Number of People in Hhold	2.342	2.710	2.671	2.285	2.021	2.009	2.033
Any Member w Disability	0.220	0.196	-	-	-	-	-
Real Earnings Before Application (2012$)	-	-	-	1544.686	1505.627	1325.179	1676.650
Percent Employed Before Application	-	-	-	0.364	0.360	0.335	0.385
Real Earnings After Application (2012$)	751.244	909.104	780.754	1238.181	1212.017	1020.144	1393.868
Percent Employed After Application	0.248	0.278	0.258	0.365	0.355	0.336	0.372

Notes: The first two columns use data from the SNAP Quality Control Data for years 2011-2016. Columns (3)-(7) present summary statistics from the mountain plains state using our administrative data. Columns (5)-(7) are for only those in our main analysis sample. 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 for all recipients, all applicants, new applicants, new recipients, and new denials in our data. Because the SNAP QC Data is a random cross-section, we only include real earnings after application in columns (1) and (2). In the Quality Control data, and the mountain-plains data that is not our analysis sample, the head of household must be aged 18 - 64. We use the weights provided by the Quality Control Data and report statistics from 2011-2016.

Table 2: Balance Test

Covariate (Mean, Std. Dev.)	Received Benefits	Monthly Caseworker Caseload	# Months of Caseworker Experience	CCAR
Employment t−1 (0.36, 0.48)	-0.050***	-1.220*	0.074	0.001
	(0.008)	(0.698)	(0.213)	(0.000)
Real Earnings t−1 (1,508, 2,658)	-0.000***	0.000	-0.000	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Quarterly Earnings $1 - $1999 (0.09, 0.29)	0.016**	0.698	0.105	-0.001*
	(0.007)	(0.578)	(0.176)	(0.000)
Yr-Qtr Experience For Highest-Paid 6-Digit NAICS (2.24, 4.57)	-0.001	-0.077	0.018	-0.000
	(0.001)	(0.057)	(0.021)	(0.000)
Industry Experience t−1 (2.39, 4.87)	0.002***	0.011	0.010	0.000
	(0.001)	(0.051)	(0.019)	(0.000)
Arc Percent t−1 (0.25, 0.52)	0.006*	-0.079	-0.090	-0.000
	(0.003)	(0.259)	(0.078)	(0.000)
Female (0.60, 0.49)	-0.024***	-0.637***	-0.025	-0.000
	(0.003)	(0.219)	(0.067)	(0.000)
Hispanic (0.08, 0.28)	0.024***	0.790**	0.056	0.000
	(0.005)	(0.380)	(0.126)	(0.000)
Black (0.02, 0.14)	0.023***	-1.159	-0.297	0.000
	(0.009)	(0.762)	(0.247)	(0.001)
Pacific Islander (0.01, 0.11)	-0.025**	1.273	-0.270	0.000
	(0.011)	(0.883)	(0.337)	(0.001)
Asian (0.01, 0.10)	-0.044***	-0.046	0.032	-0.000
	(0.012)	(1.010)	(0.342)	(0.001)
Other Race (0.49, 0.50)	-0.022***	0.044	-0.030	-0.000
	(0.002)	(0.218)	(0.067)	(0.000)
Citizen (0.97, 0.17)	0.078***	-0.146	-0.157	0.000
	(0.008)	(0.636)	(0.205)	(0.000)
Age (32.99, 12.50)	0.005***	0.006	0.001	0.000
	(0.000)	(0.010)	(0.003)	(0.000)
Over 65 Head (0.02, 0.13)	-0.346***	0.445	-0.560**	-0.001
	(0.010)	(0.903)	(0.271)	(0.001)
Spanish-Speaking (0.01, 0.09)	-0.055***	0.452	-0.213	-0.001
	(0.013)	(1.159)	(0.342)	(0.001)
Labor Supply Outcomes ($t-2$ to $t-4$)	X	X	X	X
Mean Y	0.49	244.98	34.29	0.00
F	81.74	1.46	1.03	0.68
N	178,133	177,977	178,133	178,133

Notes: This table regresses benefit receipt (column (1)), the monthly caseload of the assigned caseworker (column (2)), the months of experience of the assigned caseworker (column (3)), and the CCAR (column (4)) onto the pre-application characteristics of the head of household. "Labor Supply Outcomes ($t-2$ to $t-4$)" includes quarterly employment, earnings, indicators for quarterly earnings within $1 − $1999, arc percent of earnings, and industry experience 2–4 quarters prior to application. We include application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application.
* p<0.10, ** p<0.05, *** p<0.01

Table 3: First-Stage Estimates of CCAR on SNAP Receipt – Baseline-Labor-Market-Attachment Subgroups

Quarter	Full Sample	Attached	Partially-Attached/ Unattached
0	0.894***	0.785***	0.917***
	(0.036)	(0.104)	(0.039)
1	0.604***	0.416***	0.639***
	(0.062)	(0.172)	(0.067)
2	0.224***	0.252	0.214***
	(0.075)	(0.190)	(0.083)
3	0.129*	-0.005	0.146*
	(0.076)	(0.198)	(0.083)
4	0.116	-0.032	0.139*
	(0.075)	(0.193)	(0.082)
5	0.192***	0.147	0.196***
	(0.074)	(0.185)	(0.081)
6	0.154**	0.118	0.156*
	(0.073)	(0.181)	(0.080)
7	0.105	0.175	0.080
	(0.072)	(0.178)	(0.079)
8	0.116	0.186	0.091
	(0.071)	(0.179)	(0.078)
9	0.153**	0.267	0.127*
	(0.070)	(0.180)	(0.077)
10	0.113	0.137	0.099
	(0.069)	(0.173)	(0.076)
11	0.099	0.009	0.104
	(0.069)	(0.171)	(0.076)
N	178,133	32,556	145,551

Notes: We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. "Attached" cases are those with quarterly earnings during the quarter prior to their SNAP application above the full-time minimum wage equivalent. "Partially-attached/unattached" cases have baseline quarterly earnings below this threshold, inclusive of zero earnings. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 4: Relationship of the CCAR with Incomplete Application

	Full Sample	Attached	Partially-Attached/ Unattached
Caseworker CCAR	-0.424***	-0.358***	-0.439***
	(0.036)	(0.088)	(0.040)
Mean Y	0.35	0.38	0.35
N	178,133	32,556	145,551

Notes: We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. "Attached" cases are those with quarterly earnings during the quarter prior to their SNAP application above the full-time minimum wage equivalent. "Partially-attached/unattached" cases have baseline quarterly earnings below this threshold, inclusive of zero earnings. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5: Complier Characteristics (Full Sample)

	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	Within $250 of GI Limit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Complier-weighted char	0.39	1309.58	0.50	2.30	0.29	0.61	32.96	0.16	0.24
Full-sample average char	0.36	1508.17	0.46	2.39	0.25	0.60	32.99	0.10	0.18
Beneficiary average char	0.33	1327.57	0.42	2.22	0.26	0.58	34.21	0.11	0.18
Complier-weighted char	1.07	0.87	1.10	0.96	1.13	1.03	1.00	1.56	1.29
relative to overall	(0.15)	(0.19)	(0.17)	(0.23)	(0.24)	(0.10)	(0.04)	(0.35)	(0.41)
Complier-weighted char	1.15	0.99	1.20	1.04	1.13	1.06	0.96	1.53	1.34
relative to beneficiaries	(0.16)	(0.22)	(0.19)	(0.25)	(0.24)	(0.10)	(0.04)	(0.34)	(0.42)
Observations	178,133	178,133	178,133	178,133	178,133	178,133	178,133	178,133	178,133

Notes: Row 1 presents the results of our main IV specification from Equation (3) instrumenting with the CCAR, where the outcome variable is the given column characteristic interacted with an 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 applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 6: IV Estimates of SNAP Receipt on Labor Supply for those Attached to Labor Market at Baseline

Quarter	Earnings	Employed	$\mathbb{1}(< \$2,000)$	$\mathbb{1}(\geq \$2,000)$
0	-1,418**	-0.056	0.350**	-0.416**
	(673)	(0.111)	(0.170)	(0.190)
1	-494	-0.147	0.043	-0.202
	(877)	(0.179)	(0.160)	(0.208)
2	-520	-0.112	0.146	-0.256
	(978)	(0.178)	(0.149)	(0.208)
3	-227	-0.074	0.059	-0.132
	(1,032)	(0.180)	(0.141)	(0.206)
4	23	0.029	0.022	0.005
	(1,064)	(0.186)	(0.139)	(0.208)
5	680	0.090	0.043	0.043
	(1,123)	(0.189)	(0.140)	(0.210)
6	571	0.070	0.037	0.037
	(1,144)	(0.190)	(0.134)	(0.210)
7	1,802	0.327	0.002	0.320
	(1,235)	(0.205)	(0.131)	(0.223)
8	2,067	0.549***	0.194	0.356
	(1,263)	(0.230)	(0.136)	(0.227)
9	3,454***	0.547***	-0.095	0.644***
	(1,432)	(0.230)	(0.131)	(0.258)
10	4,694***	0.662***	-0.142	0.805***
	(1,618)	(0.246)	(0.132)	(0.280)
11	3,930***	0.767***	0.035	0.731***
	(1,533)	(0.262)	(0.123)	(0.270)
Baseline Y	5,294	1.000	0.000	1.000

Notes: N=32,556. We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table 7: IV Estimates of SNAP Receipt on Labor Supply for those Partially-Attached/Unattached to Labor Market at Baseline

Quarter	Earnings	Employed	$\mathbb{1}(< \$2,000)$	$\mathbb{1}(\geq \$2,000)$
0	-16	0.007	0.016	-0.012
	(145)	(0.056)	(0.057)	(0.046)
1	29	0.009	-0.012	0.020
	(216)	(0.065)	(0.055)	(0.058)
2	129	0.035	-0.007	0.038
	(251)	(0.066)	(0.052)	(0.061)
3	149	0.004	-0.054	0.057
	(267)	(0.067)	(0.051)	(0.062)
4	60	0.010	-0.003	0.011
	(280)	(0.067)	(0.050)	(0.063)
5	12	-0.053	-0.024	-0.030
	(293)	(0.068)	(0.048)	(0.064)
6	-170	-0.057	-0.005	-0.055
	(302)	(0.068)	(0.048)	(0.064)
7	-19	-0.126*	-0.113**	-0.013
	(308)	(0.070)	(0.049)	(0.064)
8	151	-0.045	-0.038	-0.007
	(314)	(0.069)	(0.047)	(0.064)
9	71	-0.116*	-0.102**	-0.014
	(321)	(0.070)	(0.047)	(0.065)
10	-124	-0.031	0.028	-0.056
	(328)	(0.069)	(0.045)	(0.066)
11	-207	-0.028	0.048	-0.074
	(337)	(0.069)	(0.044)	(0.066)
Baseline Y	410	0.217	0.112	0.105

Notes: N=145,551. We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

A Further Data Details

The UI earnings records only include workers living in the mountain-plains state, but we estimate that 97% of households with SNAP-eligible income don't move across states in a given year in the Current Population Survey, so out-of-state migration is unlikely to be an issue. Additionally, as with all studies that use this type of administrative earnings data, we do not observe workers who are self-employed, federal employees, or independent contractors. While some states further exclude agricultural workers, domestic workers, and workers without sufficient wages and credit weeks from the UI administrative earnings data, the mountain-plains state *includes* these workers in their data. Further, using the Current Population Survey, we tabulate that only 6% of heads of household who are income-eligible for SNAP are self-employed, and among those receiving SNAP, in the QC data, only 1% are self-employed.[23] In other research that allows for this test using this same data set, we show that the effects of SNAP Work Requirements on labor supply are the same whether we use earnings reported to the SNAP office that will include self-employed earnings, or earnings in the UI earnings records (Cook and East, 2024). 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. We do not know whether the applicant was searching for work or was out of the labor force.

B Dynamics of SNAP Receipt

The decrease in the magnitude of the impact of the CCAR over time is consistent with two hypotheses: 1) SNAP benefit spells are on average shorter than three years, so the effect of the CCAR fades out as people stop receiving benefits, or 2) denied applicants re-apply and receive benefits later.

To understand which mechanism is more important, we explore whether those who are denied SNAP because of their assigned caseworker re-apply after the initial quarter of application. Specifically, we estimate a reduced form regression where the outcome is application to SNAP in Appendix Table A1. Since everyone in our sample applies for SNAP in period zero, this is a test for subsequent re-applications. A one standard deviation increase in the CCAR leads to a 1 percentage point (0.32×0.03) lower rate of reapplication. So, reapplication and re-timing of benefit receipt is likely not a primary driver of the dynamics

[23]Gig work has become increasingly important since the end of our sample period, especially in and after the COVID-19 pandemic (Maneely and Roth-Eisenberg, 2020). Gig work is also poorly measured in many data sets (Abraham et al., 2023). Additionally, the complex nature of the application process may be particularly costly for those with self-employment income (Moynihan et al., 2022). Future work with more recent data that measures self-employment would help to shed light on this issue.

we observe. Note, we cannot look at the effect of the CCAR on the likelihood of reapplying *among those denied* because this would condition on the endogenous variable. Similarly, we do not look at recertification length as an outcome because this is only observed for those who receive SNAP.

Next, we compare the dynamics of benefit receipt for marginal recipients in our analysis from Table 3, to the dynamics of benefit receipt for all applicants and those who receive benefits in the quarter of application, regardless of whether they receive benefits due to their assigned caseworker, in Appendix Table A2. The levels in these comparison groups will be different than those in Table 3, because they are conditional means rather than the effect of the CCAR, but we are interested in whether the *dynamics* in benefit receipt are the same. We find the pattern of benefit receipt over time is nearly identical across these groups. Many recipients—whether they are pushed to receive SNAP because of their caseworker or not—stop receiving SNAP by the second quarter, and there are only about 25% of applicants that continue to receive SNAP during the third year after initial receipt. This pattern is consistent with prior evidence that the median length of SNAP participation among new entrants is about 12 months, with 26% exiting after 4 months (Leftin et al., 2014). This also suggests the reason the effects of the CCAR fade out over time is that recipients reduce their SNAP participation over time.

C Cross-Program Participation

Other data sources point to a high degree of cross-program participation among SNAP recipients. However, of greatest concern is that *changes* in program participation occur at the same time; that when individuals begin to receive SNAP, they also start receiving benefits from other programs. If this were the case, our IV estimates might be the effects of multiple programs and not just SNAP. We use the Survey of Income and Program Participation (SIPP) to investigate this directly. The SIPP is a panel study that asks individuals about their demographics and receipt of many safety net and social insurance programs.[24] In Appendix Figure A4, we plot the rates of safety net program receipt around SNAP spell initiation. It is clear that households that start receiving SNAP are already receiving benefits from other programs—most commonly Medicaid (short dashed blue line), Free and Reduced Price Lunch (long dashed maroon line) and Free and Reduced Price Breakfast (dotted purple line). Notably, the *change* in program receipt of these other programs in the period the

[24]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.

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 18 percentage points and WIC increases by 7 percentage points. To understand if changes in these other programs impact labor supply decisions we turn to the prior literature. Recent work finds mixed evidence of whether Medicaid impacts adult labor supply decisions, with some finding it reduces labor supply and some finding no effects (Baicker et al., 2014; Garthwaite et al., 2014; Kaestner et al., 2017). 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), though this is a very small fraction of our sample.

D Details of MVPF Calculation

To calculate the change in SNAP benefit amount (WTP) due to a one standard deviation increase in the CCAR, we calculate a version of the model in Table 3 over the entire three year period following application. Total additional benefits received are \$683, so a one standard deviation increase in the CCAR increases benefit amount over three years by \$20 (683×0.03).

Using statistics from the USDA, the administrative costs of operating SNAP are \$261 per year and case in 2012\$s.[25] We assume the administrative costs include the costs of certifying and recertifying SNAP recipients. This likely overstates the costs somewhat because part of the initial certification costs have already been paid by the time the caseworker interacts with each application. Our first stage effects on SNAP receipt indicate a total increase of 2.657 quarters of benefit receipt over three years, or 0.08 quarters per one standard deviation in the CCAR (2.657×0.03). Thus, administrative costs increase by \$5 for a one standard deviation increase in the CCAR ((\$261/4)$\times$0.08). Total direct costs are thus $20 + 5 = 25$ for both the increase in benefits paid out and administrative costs.

Finally, turning to fiscal externalities, we take the IV cumulative three-year estimate on quarterly earnings as the outcome variable. The total change in earnings for the full sample over three years is an increase of \$1,596. So, a one standard deviation increase in the CCAR increases earnings by \$47.88 over the following three years.

We then calculate the tax rate on earnings for this group. The average working SNAP recipient is a single adult earning \$23,104 in the year before applying for SNAP (from Table

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

1). Applying the 2012 tax rules, the standard deduction is $5,950, so taxable income is $17,154. Head of households are taxed 10% on the first $12,400 of income and then 15% on the remaining $4,754. Additionally, they are subject to a payroll tax of 4.2% and the SNAP benefit amount is reduced by 24% as earnings increase. Thus, the average tax rate for this group is $24 + 4.2 + (10 * (12,400/17,154) + (15 * 4,754/17,154) = 40\%$. Multiplying the change in earnings due to a one standard deviation increase in the CCAR by this tax rate, the increase in government revenue is $19.

Combining all these estimates, the MVPF is 3.3 $(20/(25 - 19))$.

Figure A1: SNAP Application Form

HOUSEHOLD AND GENERAL INFORMATION

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

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

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

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

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

Name: ____________________ Type of Deduction: ____________ Deduction amount: $ ________

Name: ____________________ Type of Deduction: ____________ Deduction amount: $ ________

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

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

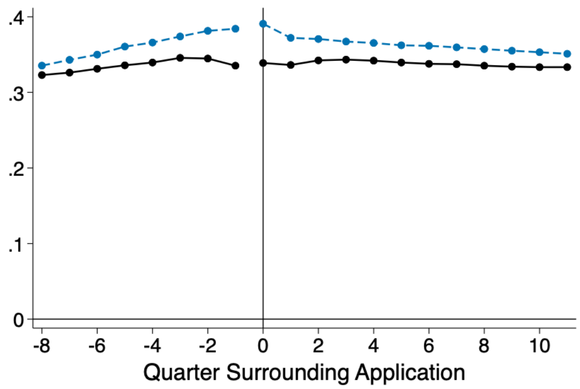
25. Does anyone in your household have any vehicles? .... ☐ Yes ☐ No
If yes, complete all columns. Some examples of vehicles are cars, trucks, boats or water craft, motorcycles, snowmobiles, motor homes, ATV's, etc.

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

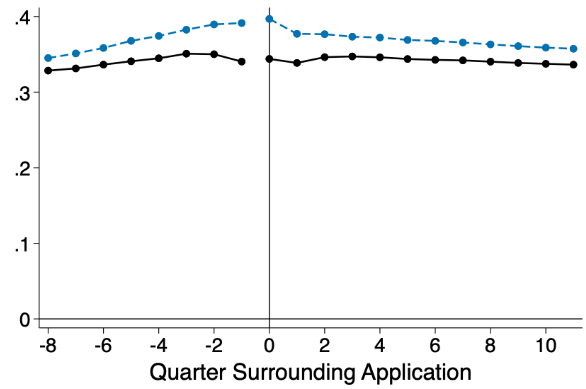
26. Does anyone in your household have any of the following property assets? .... ☐ Yes ☐ No
If yes, complete all columns:

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

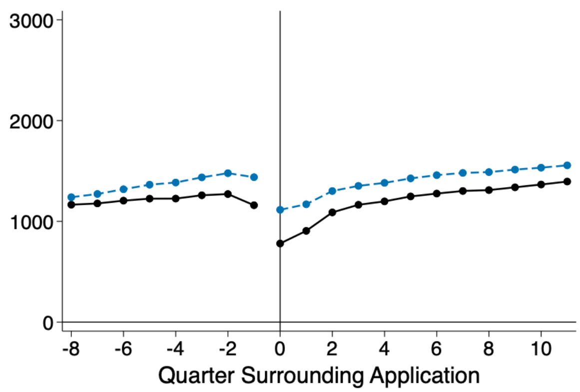
Figure A2: Differences in Quarterly Labor Supply by SNAP Receipt at $t = 0$ – Analysis Sample and Broader Sample Comparisons



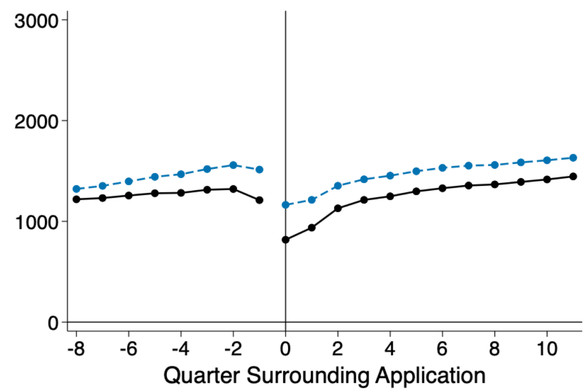
(a) Employment–Analysis Sample



(b) Employment–Broader Sample



(c) Earnings–Analysis Sample

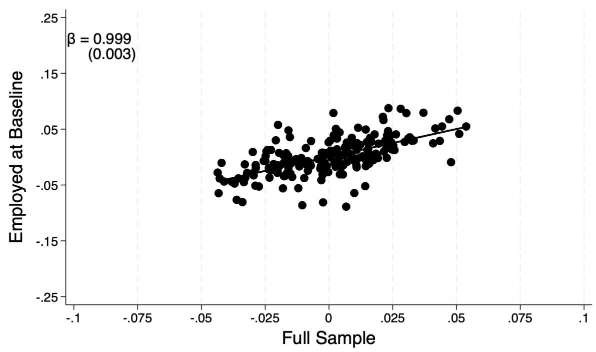


(d) Earnings–Broader Sample

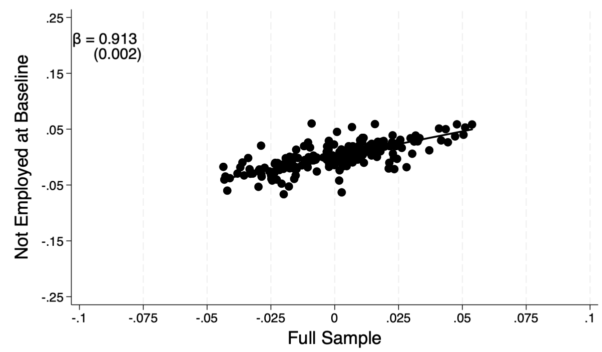
Received SNAP Denied SNAP

Notes: These figures show the results from running separate regressions for the given event time of the outcome. We regress the given outcome in the given period on whether the applicant received SNAP during period 0 along with application date fixed effects. The blue dashed line is the coefficient on the constant from those regressions and the black solid line is the coefficient on the SNAP indicator added to the constant coefficient. The left column presents results from the main analysis sample, while the right column presents results from the broader sample, described in detail in the text.

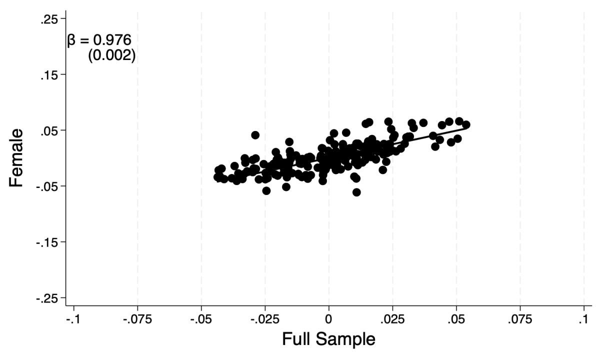
Figure A3: Group-Specific CCAR vs General CCAR



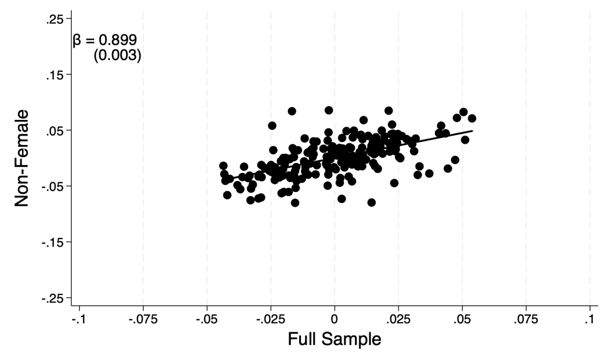
(a) Employed at Baseline



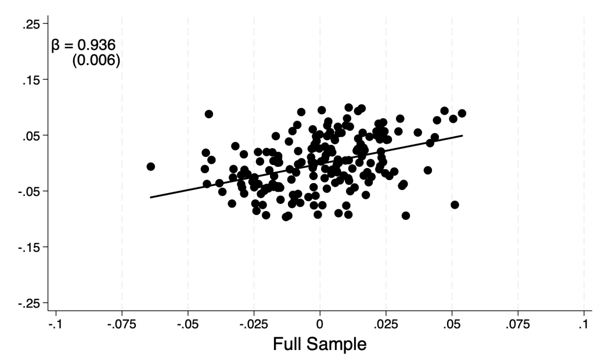
(b) Not Employed at Baseline



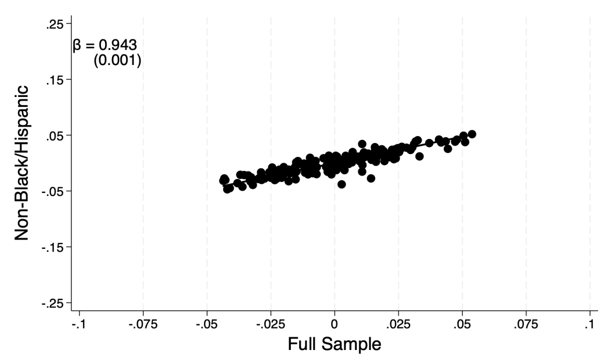
(c) Female



(d) Non-Female

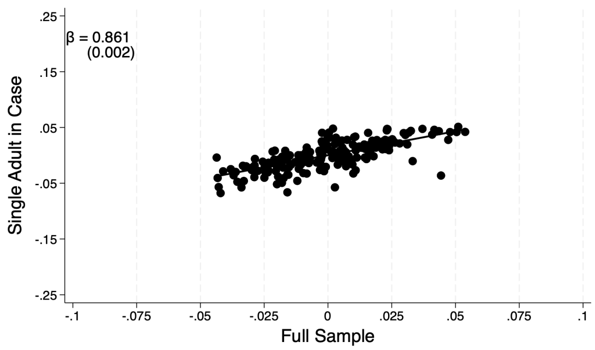


(e) Black/Hispanic

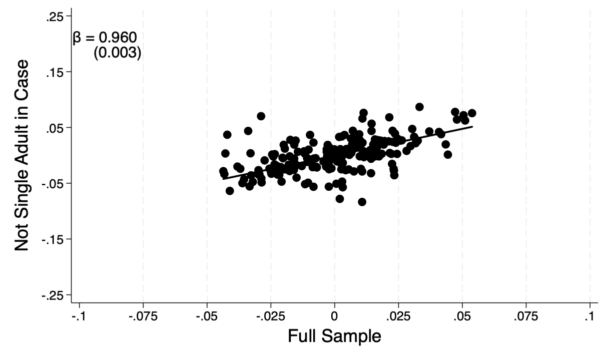


(f) Non-Black/Hispanic

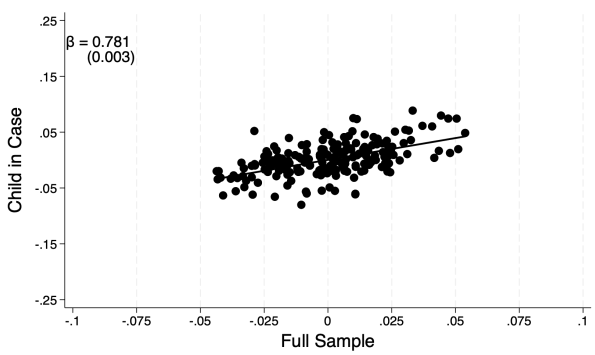
(Continued on next page)



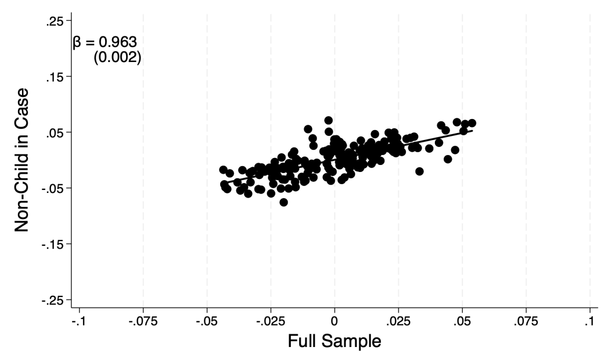
(d) One Adult in Case



(e) Non-One Adult in Case



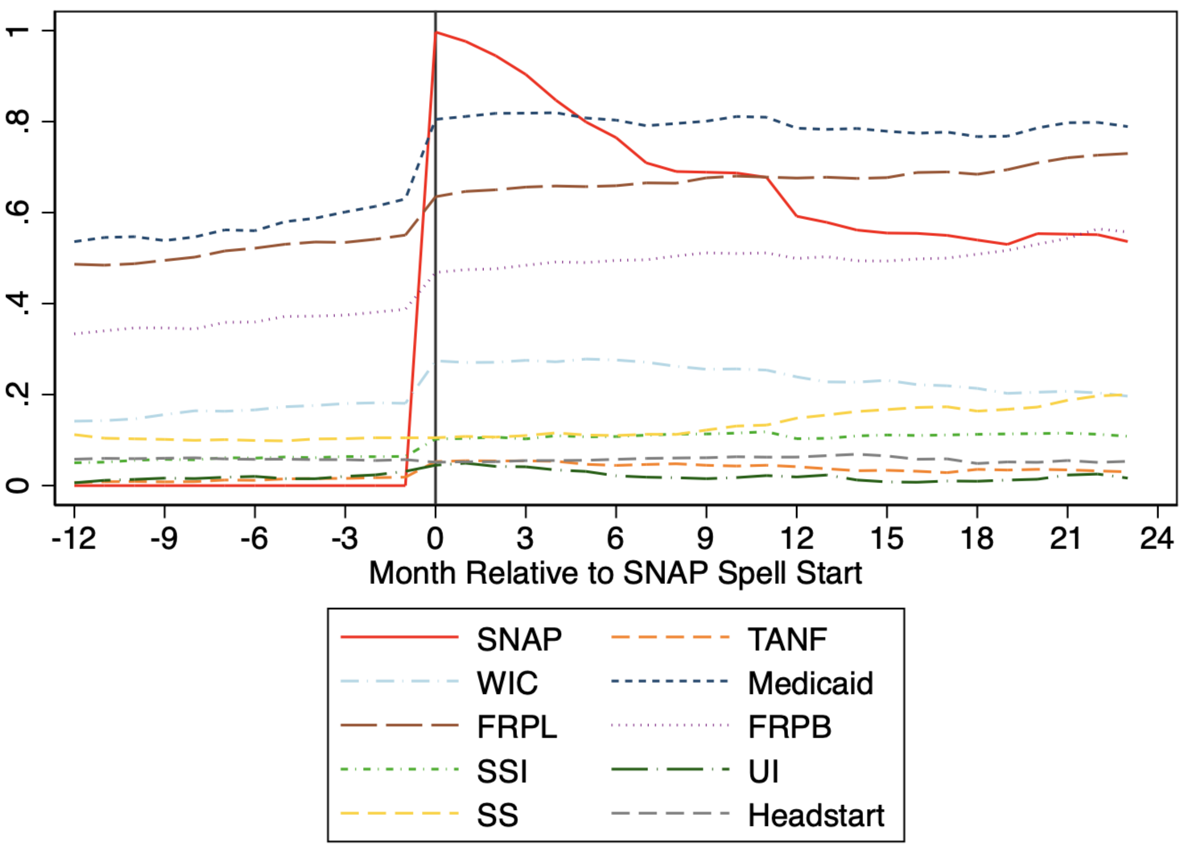
(f) Child in Case



(g) Childless Case

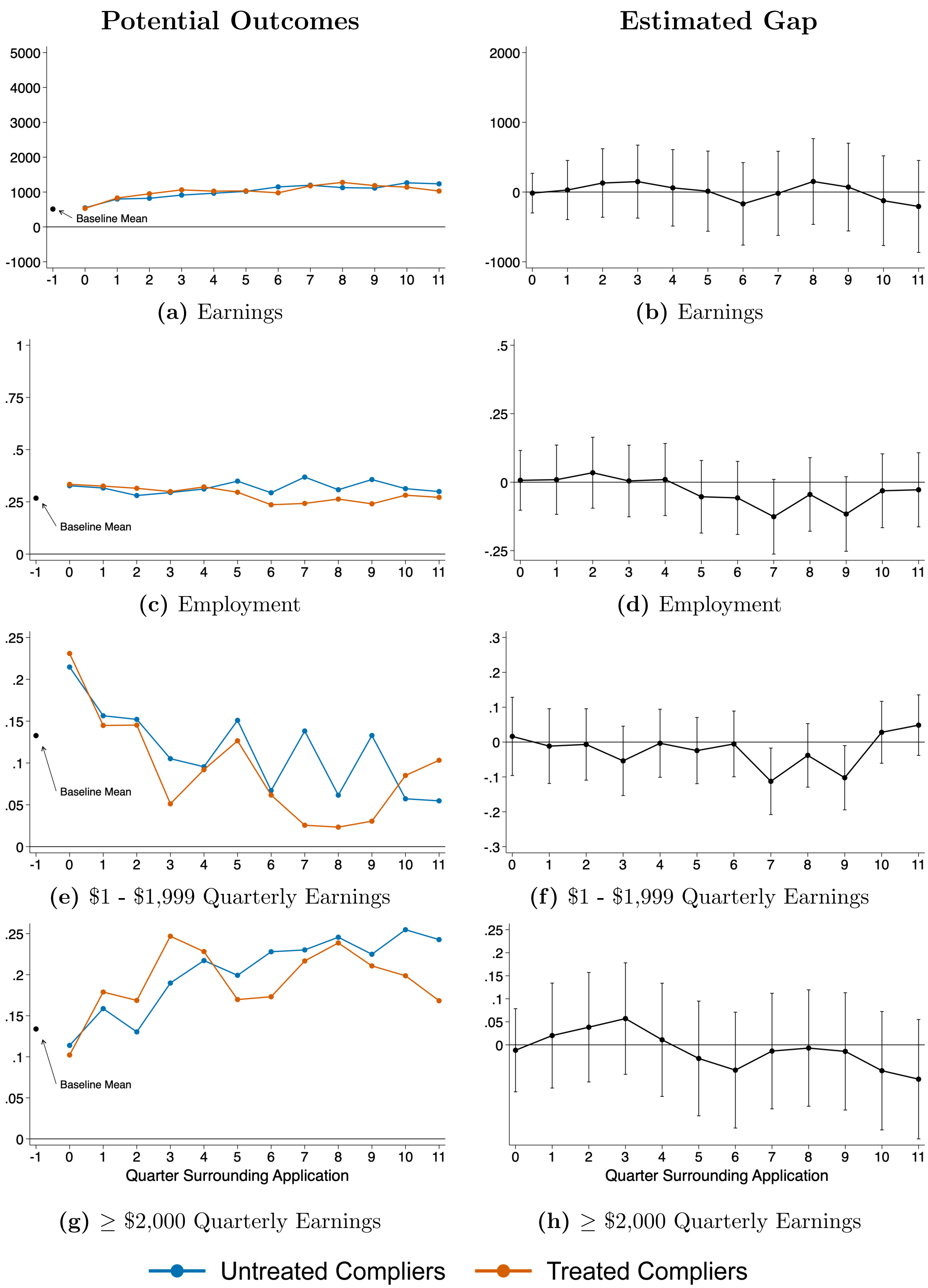
Notes: Each figure plots the CCAR for the specified subgroup (vertical axis) against the full-sample CCAR (horizontal axis). OLS estimates of the relationship between the two are displayed in the figure. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. Code adapted from Dobbie et al. (2018).

Figure A4: Cross-Program Participation Around First SNAP Spell



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

Figure A5: Decomposing IV Estimates into Potential Outcomes for those Partially-Attached/Unattached to Labor Market at Baseline



(a) Earnings

(b) Earnings

(c) Employment

(d) Employment

(e) \$1 - \$1,999 Quarterly Earnings

(f) \$1 - \$1,999 Quarterly Earnings

(g) $\geq$ \$2,000 Quarterly Earnings

(h) $\geq$ \$2,000 Quarterly Earnings

Notes: These figures depict the potential outcomes in the state of the world that complier applicants are either approved or denied SNAP due to their caseworker's CCAR. Section 6 details the method. We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. The blue line plots the potential outcomes for untreated compliers (i.e., the state of the world where compliers are denied SNAP) and the orange line plots the potential outcomes for treated compliers (i.e., the state of the world where compliers receive SNAP).

Table A1: Estimates of CCAR on Quarterly SNAP (Re)Applications – Baseline-Labor-Market-Attachment Subgroups

Quarter	Full Sample	Attached	Partially-Attached/ Unattached
1	-0.320***	-0.232*	-0.334***
	(0.051)	(0.135)	(0.055)
2	-0.141***	-0.053	-0.168***
	(0.052)	(0.141)	(0.057)
3	-0.058	-0.085	-0.059
	(0.051)	(0.139)	(0.055)
4	-0.075	-0.271*	-0.037
	(0.051)	(0.146)	(0.055)
5	-0.027	0.063	-0.037
	(0.047)	(0.129)	(0.051)
6	-0.091*	-0.218	-0.075
	(0.047)	(0.133)	(0.050)
7	0.034	-0.003	0.029
	(0.045)	(0.119)	(0.048)
8	0.018	0.156	-0.009
	(0.044)	(0.122)	(0.048)
9	0.038	-0.016	0.046
	(0.042)	(0.111)	(0.046)
10	0.004	-0.098	0.022
	(0.041)	(0.110)	(0.045)
11	0.124***	0.117	0.121***
	(0.041)	(0.108)	(0.045)
N	178,133	32,556	145,551

Notes: We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table A2: Average Benefit Receipt for All Applicants and Initial Recipients – Baseline-Labor-Market-Attachment Subgroups

	All Applicants			Recipients at $t+1$		
Quarter	Full Sample	Attached	Partially-Attached/ Unattached	Full Sample	Attached	Partially-Attached/ Unattached
0	0.535***	0.478***	0.548***	1.000***	1.000***	1.000***
	(0.001)	(0.003)	(0.001)	(0.000)	(0.000)	(0.000)
1	0.474***	0.407***	0.489***	0.827***	0.783***	0.835***
	(0.001)	(0.003)	(0.001)	(0.001)	(0.004)	(0.001)
2	0.317***	0.285***	0.324***	0.466***	0.438***	0.471***
	(0.001)	(0.003)	(0.001)	(0.002)	(0.004)	(0.002)
3	0.308***	0.274***	0.316***	0.446***	0.408***	0.453***
	(0.001)	(0.003)	(0.001)	(0.002)	(0.004)	(0.002)
4	0.275***	0.253***	0.280***	0.375***	0.354***	0.379***
	(0.001)	(0.003)	(0.001)	(0.002)	(0.004)	(0.002)
5	0.264***	0.240***	0.269***	0.356***	0.333***	0.361***
	(0.001)	(0.003)	(0.001)	(0.002)	(0.004)	(0.002)
6	0.246***	0.223***	0.252***	0.323***	0.296***	0.328***
	(0.001)	(0.003)	(0.001)	(0.002)	(0.004)	(0.002)
7	0.238***	0.213***	0.243***	0.310***	0.280***	0.315***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)
8	0.227***	0.206***	0.232***	0.290***	0.267***	0.295***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)
9	0.221***	0.199***	0.225***	0.281***	0.259***	0.285***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)
10	0.211***	0.188***	0.216***	0.264***	0.238***	0.269***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)
11	0.206***	0.180***	0.211***	0.257***	0.227***	0.262***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.004)	(0.002)
N	178,133	32,556	145,551	86,674	13,545	72,987

Notes: We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. * p<0.10, ** p<0.05, *** p<0.01

Table A3: Explanatory Power of Caseworker Demographics and Caseworker Teams for the CCAR

	Monthly Caseworker Caseload	# Months of Caseworker Experience	Team FE	Combined
Panel a) Correlation Between CCAR and Column Outcome				
CCAR	263***	-13***		
	(3)	(1)		
Mean Y	245	34		
Panel b) Variation of CCAR Explainied by Column Outcome				
Adjusted R^2	0.031	-0.004	0.075	0.094

Notes: Panel (a) shows the results from regressing the given caseworker characteristics in the column header onto the CCAR and application-date fixed effects. Panel (b) provides the Adjusted R^2 from regressing the CCAR onto the given caseworker characteristic listed in the column header. The "Combined" column regresses the CCAR on to all the caseworker characteristics from columns (1)-(3).

Table A4: Effect of CCAR on SNAP Receipt – Various Sub-Groups

	Quarter of App.		Q2		Q5		Q9		Q11	
a) Any Benefit Receipt										
Attached	0.785***	(0.104)	0.252	(0.190)	0.147	(0.185)	0.267	(0.180)	0.009	(0.171)
Partially-Attached/Unattached	0.917***	(0.039)	0.214***	(0.083)	0.196***	(0.081)	0.127*	(0.077)	0.104	(0.076)
Female Head	0.919***	(0.046)	0.301***	(0.097)	0.189*	(0.098)	0.198**	(0.095)	0.089	(0.093)
Male Head	0.823***	(0.062)	0.093	(0.123)	0.226*	(0.116)	0.087	(0.107)	0.119	(0.103)
One Adult in HH	0.908***	(0.054)	0.239**	(0.115)	0.160	(0.113)	0.295***	(0.110)	0.185*	(0.106)
Not One Adult in HH	0.857***	(0.062)	0.143	(0.132)	0.252**	(0.128)	0.185	(0.121)	0.063	(0.118)
ABAWD	0.868***	(0.053)	0.109	(0.107)	0.010	(0.104)	0.149	(0.098)	0.153	(0.097)
Non-ABAWD	0.905***	(0.051)	0.289***	(0.107)	0.275***	(0.106)	0.134	(0.101)	0.038	(0.099)
Children	0.909***	(0.076)	0.329**	(0.162)	0.230	(0.163)	0.295*	(0.158)	-0.011	(0.153)
No Children	0.875***	(0.048)	0.114	(0.099)	0.140	(0.095)	0.195**	(0.091)	0.203**	(0.090)
Black/Hispanic	0.869***	(0.104)	0.175	(0.212)	0.262	(0.212)	0.215	(0.203)	0.096	(0.201)
Non-Black/Hispanic	0.899***	(0.039)	0.224***	(0.082)	0.195***	(0.080)	0.144*	(0.076)	0.099	(0.074)
b) Total Real Benefit Amount										
Attached	388***	(171)	388***	(171)	117	(167)	123	(157)	-51	(152)
Partially-Attached/Unattached	632***	(75)	632***	(75)	183***	(75)	134*	(68)	24	(66)
Female Head	643***	(96)	643***	(96)	195**	(97)	230***	(92)	19	(89)
Male Head	531***	(89)	531***	(89)	157*	(84)	-14	(73)	2	(68)
One Adult in HH	585***	(95)	585***	(95)	173*	(95)	175**	(89)	35	(85)
Not One Adult in HH	754***	(139)	754***	(139)	256*	(139)	241*	(127)	52	(121)
ABAWD	478***	(77)	478***	(77)	37	(79)	51	(76)	77	(73)
Non-ABAWD	670***	(104)	670***	(104)	229**	(103)	175*	(94)	-44	(92)
Children	849***	(181)	849***	(181)	254	(182)	289*	(167)	-181	(168)
No Children	495***	(68)	495***	(68)	109	(68)	106	(65)	133**	(63)
Black/Hispanic	520***	(186)	520***	(186)	233	(195)	84	(183)	16	(182)
Non-Black/Hispanic	597***	(74)	597***	(74)	173***	(73)	143**	(68)	24	(65)

Notes: This tables shows the results from the first stage of the IV model from Equation (4) for the subgroups listed in the row headers. Outcomes include an indicator that equals one if the applicant receives SNAP during any of the months during the window of time specified in the column header (panel (a)) or the total real SNAP benefit dollars received over the given period (panel (b)).

Table A5: Specification Sensitivity Checks for Sample that is Attached to the Labor Market at Baseline

	Quarter of App.		Q2		Q5		Q9		Q11	
a) Main Specification (N=32,556)										
Earnings	-1,418**	(673)	-520	(978)	680	(1,123)	3,454***	(1,432)	3,930***	(1,533)
Employment	-0.056	(0.111)	-0.112	(0.178)	0.090	(0.189)	0.547***	(0.230)	0.767***	(0.262)
\$1 - \$1,999 Quarterly Earnings	0.350**	(0.170)	0.146	(0.149)	0.043	(0.140)	-0.095	(0.131)	0.035	(0.123)
≥ \$2,000 Quarterly Earnings	-0.416**	(0.190)	-0.256	(0.208)	0.043	(0.210)	0.644***	(0.258)	0.731***	(0.270)
b) No Demog./Labor Supply Controls (N=32,556)										
Earnings	-1,657***	(693)	-642	(967)	493	(1,113)	3,211**	(1,404)	3,555***	(1,488)
Employment	-0.062	(0.110)	-0.104	(0.175)	0.102	(0.187)	0.528**	(0.227)	0.732***	(0.256)
\$1 - \$1,999 Quarterly Earnings	0.376**	(0.169)	0.155	(0.147)	0.070	(0.138)	-0.090	(0.128)	0.043	(0.120)
≥ \$2,000 Quarterly Earnings	-0.448***	(0.190)	-0.256	(0.204)	0.028	(0.207)	0.619***	(0.253)	0.687***	(0.263)
c) 5th/90th ptile IV Trimming (N=34,028)										
Earnings	-1,176**	(584)	-813	(869)	165	(981)	1,929*	(1,131)	1,656	(1,157)
Employment	-0.063	(0.098)	-0.056	(0.158)	0.093	(0.168)	0.386**	(0.190)	0.490***	(0.202)
\$1 - \$1,999 Quarterly Earnings	0.253*	(0.146)	0.224	(0.138)	0.092	(0.125)	-0.040	(0.115)	0.112	(0.111)
≥ \$2,000 Quarterly Earnings	-0.328**	(0.164)	-0.276	(0.186)	-0.000	(0.186)	0.428**	(0.208)	0.377*	(0.206)
d) 1st/95th ptile IV Trimming (N=34,047)										
Earnings	-1,296***	(544)	524	(830)	995	(939)	2,681***	(1,105)	2,847***	(1,150)
Employment	-0.085	(0.090)	-0.024	(0.148)	0.144	(0.158)	0.465***	(0.183)	0.607***	(0.197)
\$1 - \$1,999 Quarterly Earnings	0.234*	(0.135)	0.057	(0.122)	-0.007	(0.116)	-0.034	(0.105)	0.044	(0.101)
≥ \$2,000 Quarterly Earnings	-0.329**	(0.151)	-0.072	(0.170)	0.148	(0.175)	0.501***	(0.198)	0.562***	(0.203)
e) 10 ptile # Decisions Trimming (N=35,807)										
Earnings	-1,395**	(643)	-591	(937)	-143	(1,054)	1,951	(1,223)	2,000	(1,273)
Employment	-0.059	(0.106)	-0.228	(0.173)	0.031	(0.180)	0.438**	(0.210)	0.604***	(0.230)
\$1 - \$1,999 Quarterly Earnings	0.323**	(0.161)	0.040	(0.141)	0.113	(0.135)	-0.017	(0.122)	0.124	(0.120)
≥ \$2,000 Quarterly Earnings	-0.389**	(0.180)	-0.257	(0.199)	-0.085	(0.199)	0.456**	(0.227)	0.479**	(0.229)
f) 30 ptile # Decisions Trimming (N=27,791)										
Earnings	-1,906**	(848)	-1,410	(1,178)	-1,412	(1,323)	2,782*	(1,605)	3,639**	(1,770)
Employment	-0.109	(0.134)	-0.193	(0.215)	-0.166	(0.223)	0.368	(0.251)	0.648**	(0.290)
\$1 - \$1,999 Quarterly Earnings	0.467**	(0.216)	0.234	(0.182)	0.181	(0.171)	-0.058	(0.154)	-0.024	(0.149)
≥ \$2,000 Quarterly Earnings	-0.587***	(0.245)	-0.423*	(0.257)	-0.344	(0.257)	0.428	(0.278)	0.669**	(0.310)

Notes: This table shows the results from the IV model in Equation (3) instrumenting with the CCAR. The "Main Specification" uses our primary sample and controls. "No Demog./Labor Supply Controls" drop the baseline demographic and labor supply controls. "XX/YY ptile IV Trimming" includes applications that were assigned CCAR values within the XXth to YYth percentile. "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.

Table A6: Specification Sensitivity Checks for the Sample that is Partially/Unattached to the Labor Market at Baseline

	Quarter of App.		Q2		Q5		Q9		Q11	
a) Main Specification (N=145,551)										
Earnings	-16	(145)	129	(251)	12	(293)	71	(321)	-207	(337)
Employment	0.007	(0.056)	0.035	(0.066)	-0.053	(0.068)	-0.116*	(0.070)	-0.028	(0.069)
\$1 - \$1,999 Quarterly Earnings	0.016	(0.057)	-0.007	(0.052)	-0.024	(0.048)	-0.102**	(0.047)	0.048	(0.044)
$\geq$ \$2,000 Quarterly Earnings	-0.012	(0.046)	0.038	(0.061)	-0.030	(0.064)	-0.014	(0.065)	-0.074	(0.066)
b) No Demog./Labor Supply Controls (N=145,551)										
Earnings	172	(192)	333	(287)	220	(332)	275	(364)	4	(376)
Employment	0.064	(0.080)	0.086	(0.082)	-0.002	(0.081)	-0.067	(0.082)	0.017	(0.082)
\$1 - \$1,999 Quarterly Earnings	0.021	(0.064)	-0.004	(0.055)	-0.020	(0.050)	-0.096**	(0.048)	0.051	(0.045)
$\geq$ \$2,000 Quarterly Earnings	0.041	(0.056)	0.087	(0.068)	0.016	(0.071)	0.029	(0.073)	-0.032	(0.074)
c) 5th/90th ptile IV Trimming (N=152,001)										
Earnings	-80	(140)	-111	(243)	-143	(284)	-169	(310)	-398	(327)
Employment	-0.007	(0.054)	-0.044	(0.064)	-0.061	(0.065)	-0.125*	(0.067)	-0.076	(0.067)
\$1 - \$1,999 Quarterly Earnings	0.011	(0.056)	-0.015	(0.050)	-0.000	(0.047)	-0.047	(0.045)	0.037	(0.043)
$\geq$ \$2,000 Quarterly Earnings	-0.020	(0.045)	-0.031	(0.059)	-0.063	(0.062)	-0.079	(0.063)	-0.111*	(0.064)
d) 1st/95th ptile IV Trimming (N=151,999)										
Earnings	6	(130)	71	(225)	-4	(263)	-12	(288)	-45	(300)
Employment	0.019	(0.050)	0.010	(0.059)	-0.052	(0.060)	-0.090	(0.062)	0.006	(0.061)
\$1 - \$1,999 Quarterly Earnings	0.033	(0.051)	-0.023	(0.047)	-0.025	(0.043)	-0.068	(0.041)	0.050	(0.040)
$\geq$ \$2,000 Quarterly Earnings	-0.017	(0.041)	0.029	(0.054)	-0.029	(0.057)	-0.022	(0.058)	-0.042	(0.059)
e) 10 ptile # Decisions Trimming (N=159,810)										
Earnings	10	(149)	163	(260)	145	(303)	299	(333)	135	(348)
Employment	0.059	(0.058)	0.082	(0.068)	0.024	(0.070)	-0.037	(0.071)	0.074	(0.071)
\$1 - \$1,999 Quarterly Earnings	0.079	(0.059)	0.021	(0.054)	0.029	(0.050)	-0.084*	(0.048)	0.078*	(0.046)
$\geq$ \$2,000 Quarterly Earnings	-0.022	(0.047)	0.058	(0.063)	-0.006	(0.065)	0.046	(0.067)	-0.003	(0.068)
f) 30 ptile # Decisions Trimming (N=124,572)										
Earnings	162	(191)	378	(334)	256	(387)	330	(425)	-273	(443)
Employment	0.045	(0.074)	0.107	(0.088)	-0.011	(0.089)	-0.066	(0.091)	-0.051	(0.091)
\$1 - \$1,999 Quarterly Earnings	-0.001	(0.076)	0.018	(0.069)	-0.020	(0.064)	-0.100	(0.062)	0.054	(0.059)
$\geq$ \$2,000 Quarterly Earnings	0.042	(0.061)	0.085	(0.081)	0.006	(0.084)	0.034	(0.086)	-0.102	(0.088)

Notes: This table shows the results from the IV model in Equation (3) instrumenting with the CCAR. The "Main Specification" uses our primary sample and controls. "No Demog./Labor Supply Controls" drop the baseline demographic and labor supply controls. "XX/YY ptile IV Trimming" includes applications that were assigned CCAR values within the XXth to YYth percentile. "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.

Table A7: IV Estimates of Effect of SNAP Receipt on Employment and Earnings– Single Adult Households

	Quarter of App.		Q2		Q5		Q9		Q11	
Attached to Labor Market										
a) Full Sample (N=$32,556$)										
Earnings	-1,418**	(673)	-520	(978)	680	(1,123)	3,454***	(1,432)	3,930***	(1,533)
Employment	-0.056	(0.111)	-0.112	(0.178)	0.090	(0.189)	0.547***	(0.230)	0.767***	(0.262)
\$1 - \$1,999 Quarterly Earnings	0.350**	(0.170)	0.146	(0.149)	0.043	(0.140)	-0.095	(0.131)	0.035	(0.123)
$\geq$ \$2,000 Quarterly Earnings	-0.416**	(0.190)	-0.256	(0.208)	0.043	(0.210)	0.644***	(0.258)	0.731***	(0.270)
b) Single-Adult Sample (N=$18,973$)										
Earnings	-2,262*	(1,232)	985	(1,740)	2,461	(2,124)	4,636*	(2,674)	7,407**	(3,480)
Employment	-0.208	(0.198)	0.264	(0.325)	0.405	(0.356)	0.749*	(0.434)	1.049**	(0.516)
\$1 - \$1,999 Quarterly Earnings	0.464	(0.305)	0.338	(0.279)	0.266	(0.255)	-0.096	(0.224)	-0.298	(0.236)
$\geq$ \$2,000 Quarterly Earnings	-0.680*	(0.359)	-0.070	(0.350)	0.144	(0.360)	0.849*	(0.482)	1.348**	(0.619)
Partially/Unattached to Labor Market										
c) Full Sample (N=$145,551$)										
Earnings	-16	(145)	129	(251)	12	(293)	71	(321)	-207	(337)
Employment	0.007	(0.056)	0.035	(0.066)	-0.053	(0.068)	-0.116*	(0.070)	-0.028	(0.069)
\$1 - \$1,999 Quarterly Earnings	0.016	(0.057)	-0.007	(0.052)	-0.024	(0.048)	-0.102**	(0.047)	0.048	(0.044)
$\geq$ \$2,000 Quarterly Earnings	-0.012	(0.046)	0.038	(0.061)	-0.030	(0.064)	-0.014	(0.065)	-0.074	(0.066)
d) Single-Adult Sample (N=$81,873$)										
Earnings	10	(236)	182	(405)	95	(470)	238	(513)	272	(533)
Employment	-0.009	(0.090)	0.064	(0.106)	-0.048	(0.108)	-0.108	(0.111)	0.078	(0.110)
\$1 - \$1,999 Quarterly Earnings	-0.012	(0.093)	-0.017	(0.084)	-0.032	(0.077)	-0.092	(0.075)	0.100	(0.071)
$\geq$ \$2,000 Quarterly Earnings	0.002	(0.075)	0.077	(0.098)	-0.017	(0.102)	-0.016	(0.103)	-0.019	(0.104)

Notes: This table shows the results from the IV model in Equation (3) instrumenting with the CCAR separately for the analysis-sample households and among single-adult households. We include the baseline employment and demographic controls specified in Equation (3) as well as application-date fixed effects. Our sample includes applicants between 2011-2016 who apply in the General track and have not received SNAP within the last 6 months. We exclude applicants assigned to caseworkers who handled fewer than 200 applications that year as well as applicants who have extreme CCAR values. We further restrict to applications for whom we observe outcomes for at least one year prior and three years following the given SNAP application. "Attached" cases are those with quarterly earnings during the quarter prior to their SNAP application above the full-time minimum wage equivalent. "Partially-attached/unattached" cases have baseline quarterly earnings below this threshold, inclusive of zero earnings. * p<0.10, ** p<0.05, *** p<0.01