The Financial Consequences of Being Denied Benefit Access*

Tatiana Homonoff†, Min Lee‡, and Katherine Meckel§

September 16, 2025

Abstract

We examine the causal impact of being denied access to SNAP on financial health by linking administrative SNAP records to individual credit reports in two California counties. Using experimental and quasi-random variation in interview scheduling, we find that process-related denials increase credit card debt and delinquencies, and decrease credit scores. This suggests that households screened out of social insurance programs due to administrative barriers suffer tangible economic consequences, raising questions about the targeting benefits of these burdens and highlighting how administrative features of safety net programs influence not just participation, but also the broader financial well-being of low-income households.

JEL Codes: I38, H53, D14

Keywords: SNAP, administrative burden, benefit take-up and targeting, financial well-being

*PRELIMINARY—PLEASE DO NOT CITE OR DISTRIBUTE. For helpful comments and suggestions, we thank Francesca Costa, Jason Cook, Julie Cullen, Itzik Fadlon, Eric Giannella, and Jacob Goldin, and seminar participants at Better Government Lab. We thank Vince Dorie and Gwen Rino at Code for America, Peri Weisberg at San Francisco Human Service Agency, and Nikta Akhavan, Erika Brown, Huizhi Gong, Gary Gremaux, Jennifer Hogg, Sarah Hoover, Aparna Ramesh and Evan White at California Policy Lab for hosting, documenting, and facilitating access to the University of California Consumer Credit Panel and creating linkages to administrative SNAP data from Los Angeles and San Francisco. The authors gratefully acknowledge the support of the Russell Sage Foundation for this project, Grant ID Number G-2310-45127, under the program on Social, Political, and Economic Inequality.

†New York University and NBER

‡University of California, San Diego

§University of California, San Diego, CESifo and NBER

1 Introduction

A substantial body of evidence documents the financial precarity of low-income households in the United States (Board of Governors of the Federal Reserve System, 2024, Kramer-Mills et al., 2024, Cox et al., 2024, Brown and Braga, 2019). Even modest financial shocks, such as a traffic fine, can increase the likelihood of delinquency, add to collections debt, and lower credit scores (Mello, 2024), while more severe disruptions such as a hospital admission have been shown to cause persistent financial distress (Dobkin et al., 2018). Social insurance programs, like the Supplemental Nutrition Assistance Program (SNAP), are designed to help shield households from financial risk by providing resources during times of increased need, but take-up of these programs is far from complete (Ko and Moffitt, 2024). Recent research identifies administrative burdens as a major factor contributing to imperfect take-up (Currie, 2006, Herd and Moynihan, 2019). While several papers have studied the effects of various burdens on program take-up and their targeting properties, limited research exists on the downstream financial consequences of these ordeals for affected households.

Our paper addresses this open question by estimating the effects of barriers to SNAP participation on the financial well-being of those marginally denied. To do so, we link individual-level credit report data to administrative SNAP data from two interventions aimed at minimizing administrative burdens. Specifically, we exploit random variation in flexibility of the caseworker interview—a required component of the program's eligibility determination processes for both initial applications and annual recertifications—in two related experiments shown to increase SNAP approval rates in Los Angeles and recertification rates in San Francisco (Giannella et al., 2024, Homonoff and Somerville, 2021). Our novel data linkage between these experiments and the University of California Consumer Credit Panel (UC-CCP), a newly-available credit record database for the universe of California residents with a credit file, allows us to estimate the effect of the interventions on various measures of financial health, such as credit card debt and delinquencies, accounts in collections, bankruptcy filings, and credit scores.

For both interventions, we find that reducing administrative burdens leads to significant improvements in the financial well-being of individuals seeking to gain or maintain access to SNAP. In Los Angeles, applicants randomly assigned to receive communications regarding a newly-established, client-initiated interview process are less likely to have a delinquent account and have lower credit card debt. These effects emerge toward the end of the first year post-application and steadily increase over the three-year follow-up period. We find similar effects of recertification interview flexibility in San Francisco: participants assigned to later recertification interviews, which leave less time to reschedule missed interviews or complete other components of the recertification process before the deadline, experience increases in credit card balances and number of delinquent accounts immediately following the recertification quarter. These effects attenuate, then reemerge at the end of the study period, consistent with recertification failure causing short-term benefit losses for some (benefit "churn") and long-term losses for others. In both Los Angeles and San Francisco, we find marginally significant increases in credit scores following the observed decreases in debt and delinquencies. We find no effects on bankruptcies in either county.

To understand the magnitude of the effects of being denied program access, we provide treatment-on-the-treated estimates that scale our reduced form estimates by the first stage effect of the two interventions on rates of SNAP approval and recertification, respectively. In Los Angeles, we find that the marginal SNAP approval results in reductions in credit card balances of $226, $1,374, and $2,386 in each of the first three years post-application and reduces the likelihood of having a delinquent account by 5.0, 7.5, and 10.1 percentage points. These represent declines of up to 50 percent for balances and 24 percent for delinquencies relative to the control mean, suggesting meaningful debt relief over time. In San Francisco, we find that the marginal disenrolled SNAP recipient experiences an increase in their credit card balance of $500 (a 26 percent increase relative to the control mean) and a decrease in their credit score of 15 points in the year following recertification failure.[1] By the third year,

[1]To put these numbers into context, Homonoff and Somerville (2021) find that the marginal disenrolled case loses an average of $550 in benefits in the year following recertification—an amount just slightly larger

the effects on credit card balance and credit score are somewhat larger, though no longer statistically significant, but the number of delinquent credit card accounts increases by a significant 0.41 (an 87 percent increase).

To explore how these effects vary across the income distribution, we conduct heterogeneity analyses by baseline earnings. In both counties, reductions in credit card balances are concentrated among households with pre-period earnings. In contrast, improvements in delinquency rates and credit scores tend to be more pronounced among households reporting no earnings at baseline, although this pattern does not hold uniformly across all outcomes and years. These findings suggest that SNAP benefits help participants avoid different types of financial consequences depending on their income level, mitigating debt burdens for some households and preventing repayment failures and credit deterioration for others.

Our findings provide new causal evidence on the effects of SNAP on financial health. A large literature documents effects of SNAP on nutrition and food security (Shapiro, 2005, Hastings et al., 2021), long-term benefits of childhood SNAP exposure on health and economic self-sufficiency (Almond et al., 2011, Hoynes et al., 2016, Bailey et al., 2024), and employment incentives (or disincentives) of the program's design (Hoynes and Schanzenbach, 2012, Harris, 2021, Han, 2022, Gray et al., 2023, Cook and East, 2023, 2024). Most related to our focus, several studies examine the effect of SNAP on financial well-being (Edin et al., 2013, Shaefer and Gutierrez, 2013, Han, 2016, Dodini et al., 2024).[2] We complement this last strand of research—which largely relies on qualitative interviews or survey data[3] coupled with descriptive or quasi-experimental methods—by using experimental variation in SNAP participation combined with, to our knowledge, the first linkage of credit bureau data with administrative SNAP microdata.

than our estimated effects on credit card balances.

[2]A related literature studies the effect of recent healthcare expansions on medical debt as well as non-medical financial hardship (Gross and Notowidigdo, 2011, Finkelstein et al., 2012, Mazumder and Miller, 2016, Hu et al., 2018, Brevoort et al., 2020).

[3]These data sources suffer from small sample sizes and potential mismeasurement (Meyer et al., 2022). One notable exception, Dodini et al. (2024), uses individual-level credit report data to estimate the effects of county-level SNAP policies on financial health, though, is limited in its ability to identify SNAP participants.

Notably, these prior studies focus on the effects of expanding or contracting program eligibility. In contrast, our study examines the downstream effects of increases in SNAP participation stemming from changes in administrative burdens (Currie, 2006, Herd and Moynihan, 2019) without altering rules regarding program eligibility. Prior literature consistently documents the outsized effects of minor administrative changes in eligibility determination processes on program take-up (Finkelstein and Notowidigdo, 2019, Wu and Meyer, 2023, Cook and East, 2023, Unrath, 2024, Kim et al., 2025); however, there remains less consensus regarding whether these barriers to access improve or worsen targeting of the program (Nichols and Zeckhauser, 1982, Goldin and Reck, 2018, Deshpande and Li, 2019). This empirical literature typically assesses the targeting properties of a particular burden by comparing changes in take-up across households with observable characteristics associated with higher or lower levels of need, yet recent work suggests that commonly-used characteristics (e.g., current income) may be an imperfect proxy for lifetime consumption, a more plausible measure of need (Rafkin et al., 2023).

Our paper asks a separate but related question by directly estimating the financial consequences of the burden. If ordeals effectively screen out households that derive less value from participation, as suggested in Nichols and Zeckhauser (1982), removing minor costs to participation should result in comparably minor changes to the economic well-being of the marginal SNAP recipient. However, we find just the opposite: administrative burdens associated with the SNAP application and recertification process lead to meaningful declines in the financial health of marginally denied applicants. Our findings complement research demonstrating that informational barriers to applying for health or social insurance worsen the health of would-be applicants (Aizer, 2007, Goldin et al., 2021, Saunders et al., 2025) and are consistent with concurrent work by Bhardwaj and Tabak (2025) who find that automation and outsourcing of welfare enrollment processes resulted in worsened financial outcomes.

2 Institutional Background

The Supplemental Nutrition Assistance Program (SNAP) is the largest food assistance program in the country and a central component of the U.S. social safety net (NAS, 2019) providing $100 billion in benefits to approximately 42 million individuals nationwide in FY2024. SNAP provides households with monthly food benefits averaging $332 per household, a substantial share of household income for many recipients (U.S. Department of Agriculture, 2023). In spite of the significant financial benefits of the program, take-up of SNAP is far from universal: in FY2022, California enrolled just 81 percent of eligible individuals, among the lowest in the country, up from only 67 percent two years prior (Cunnyngham, 2025).

Prior research has identified administrative burdens as contributing factors to lower take-up rates (Herd et al., 2023). For example, enrollment in SNAP requires completion of a multi-step eligibility determination process. Applicants must submit supporting documentation such as verification of income, employment, expenses, and immigration and student status and complete a federally-mandated eligibility interview. These interviews are used to verify documentation and resolve inconsistencies, but they also constitute a strict procedural requirement: applications are denied if the interview is not completed on time, regardless of whether the household successfully submitted their application materials and appears eligible for the program.

Once enrolled, households must periodically verify continued eligibility through a process known as recertification. This process closely mirrors the initial application process: recipients must complete a recertification form, submit income verification documents, and complete a caseworker interview at least once per year to maintain program access. As a result, recertification creates a series of additional, nearly identical administrative hurdles each year for applicants who successfully completed the initial enrollment process and gained access to SNAP.

3 Interview Flexibility Experiments

Prior research identifies the caseworker interview as a key barrier to program access. In many counties, missed interviews are the primary reason for SNAP denials, often accounting for more than half of all denials (Giannella et al., 2024, Kim et al., 2025). As a result, SNAP administrators interested in improving take-up have designed interventions to alleviate barriers associated with the interview process. We describe two such interventions in the state of California below.

3.1 Los Angeles County Experiment

In the standard enrollment process, applicants are assigned a scheduled phone interview appointment. If the applicant cannot be reached, the interview is marked incomplete, and the individual must contact the county to reschedule within 30 days of the application or the application is summarily denied.

In 2020, the Los Angeles Department of Public Social Services (DPSS) implemented a large-scale experiment to evaluate the effect of increasing flexibility in the SNAP enrollment interview process. All applicants were assigned a pre-scheduled interview date and received electronic communications regarding the interview along with a mailed appointment letter. However, for applications assigned to the treatment group, the standard electronic reminders were modified to include information inviting them to complete their interview by calling a dedicated caseworker hotline at their convenience. If a treatment group member failed to call the hotline, the county attempted to reach them through the standard channels. The intervention did not alter eligibility rules or documentation requirements—only ease of completing the required interview.

Giannella et al. (2024) finds that access to flexible interviews significantly increased SNAP take-up. Just over half of treatment members called the flexible interview hotline and SNAP approval rates rose by 6.2 percentage points with an average monthly benefit of $375. Al-

though some control group applicants eventually entered the program, SNAP participation remained 2.2 percentage points higher in the treatment group five months after the intervention.

3.2 San Francisco County Experiment

In addition to overcoming barriers associated with initial enrollment, SNAP recipients must document continued eligibility through an annual recertification process. As part of this process, recipients must complete a recertification interview scheduled by the county by the end of the calendar month in which their certification period ends. In San Francisco County, these interview dates are randomly assigned across cases and staggered throughout the month to smooth caseworker workloads.[4] Because all interviews must be completed and paperwork submitted by the end of the month, scheduling of the initial interview creates meaningful differences in the amount of time households have to complete these steps, such as rescheduling missed interviews or submitting additional eligibility documentation.

This administrative feature has a large effect on program continuity. Homonoff and Somerville (2021) analyze the universe of SNAP recertification cases in San Francisco to estimate the effect of interview timing on recertification success. Cases assigned to interviews at the end of the month were 10 percentage points less likely to recertify than those assigned to interviews at the beginning of the month. These effects extended beyond recertification: while some cases who failed recertification due to later interview assignment reapplied successfully and experienced only temporary losses in benefits, others exited the program for over a year. The financial consequences were substantial: among cases that failed recertification due to late interviews, the average benefit loss over the subsequent year was $558, but for the one-quarter of these households that remained off SNAP for a full year, it was nearly $2,000.

[4]Specifically, for a given recertification month, cases are grouped by interview language and appointment type (phone versus in-person) then randomly assigned an interview date between the first and 28th of the calendar month. Controlling for month-by-language fixed effects provides as-good-as-random assignment.

4 Data

In this paper, we link administrative SNAP data associated with these two flexible interview interventions to a newly-available 100 percent sample of credit reports in the state of California. To our knowledge, this is the first study linking individual-level administrative SNAP and credit report data.

4.1 Administrative SNAP Data

Our first SNAP dataset comes from the analysis of the Los Angeles experiment in Giannella et al. (2024). The data include the universe of SNAP applications in Los Angeles County between October 2020 and May 2021 that were submitted via GetCalFresh, a widely used online application portal at the time, with individual SNAP offices entering the sample in a staggered roll-out. For each household, we observe experimental status (75 percent receive information regarding flexible interview availability), application status (approved, denied, or pending), and any reapplications for five months. The dataset also includes demographic information from the initial application, such as household size, applicant age and gender, and recent income.

Our second administrative SNAP dataset contains information on the universe of recertifications scheduled in San Francisco between 2014 and 2016, as analyzed in Homonoff and Somerville (2021). For each case, we observe the recertification month, assigned interview date, interview completion status, and final recertification outcome. The data also include the interview language and whether it was conducted over the phone—characteristics that, together with the recertification month, constitute the strata within which interview dates are randomly assigned. The data include subsequent reapplications, allowing us to track whether denied cases reapply or remain disenrolled. Finally, the data include demographic characteristics that are provided in the initial SNAP application and periodically updated.

For each of the two data sets, we make one modification to facilitate linkage to the credit

report data. Namely, the original data sets in Giannella et al. (2024) and Homonoff and Somerville (2021) are at the case (i.e., household) level, while credit outcomes are at the individual level. We find a match rate close to 90 percent (93 percent in Los Angeles and 89 percent in San Francisco) with no evidence of differential match rates by experimental status. Our final data set consist of 52,015 applications (61,967 individuals) in Los Angeles and 22,956 recertifications (28,639 individuals) in San Francisco with a four-year balanced credit panel corresponding to roughly 80 percent of the original intervention samples. We describe this data linkage process and characteristics of our matched sample in detail in Online Appendix B.

4.2 Consumer Credit Data

Our financial outcomes data comes from the University of California Consumer Credit Panel (UC-CCP), a longitudinal dataset of credit bureau records for the universe of individuals with a credit file in California. The UC-CCP includes quarterly credit reports from 2004 to 2024 provided by one of the three nationwide consumer reporting agencies. This data includes credit scores, public records such as bankruptcy filings, and tradeline-level information about each loan or collections item, such as account type (e.g., credit card), status, balance, and payment history. The UC-CCP provides this information for individuals who ever lived in California, even if they do not reside in California in the given quarter.[5]

Our main measures of financial well-being focus on credit card balances and delinquencies (defined as late payments over 30 days past due).[6] When an account is severely delinquent, it can be transferred to collections or trigger bankruptcy proceedings. We therefore calculate two additional outcomes—new collection items and bankruptcy filings in a given quarter—to capture more extreme measures of financial distress. These measures have the added benefit

[5]Specifically, the UC-CCP includes historical information dating back to 2004 for individuals with credit histories that ever lived in California between 2004 and 2019. For individuals who first appear in California in 2020 or later, the UC-CCP follows their credit histories from that point forward.

[6]While our data include other types of tradelines, such as auto loans, mortgages, home equity lines, and student loans, we focus on credit card accounts, first, because they are more common in our population and, second, because they are likely to be more responsive to unexpected changes in household resources.

of including unpaid obligations outside the formal financial sector, such as medical and utility bills, as well. These items are often purchased by collections agencies and can provide insights into the financial health of unbanked individuals. Finally, the consumer credit score serves as a measure of overall financial health, as it incorporates several individual financial outcomes across multiple accounts into one measure. Separately, the score itself has important welfare implications, as it may directly inform the decisions of employers, landlords, and lenders.

4.3 Summary Statistics

We present baseline measures of financial well-being for our two experimental samples in Table 1 for the full sample and by experimental status—treatment versus control in Los Angeles, and early versus late interview assignment in San Francisco.[7]

Table 1 highlights the economic vulnerability of both study populations. In Los Angeles, sample members hold an average of nearly $5,000 in credit card debt and 41 percent have at least one delinquent credit card account with an average of 1.4 delinquent accounts. In San Francisco, average credit card balances are lower, at just under $2,000, possibly due to our Los Angeles study period overlapping with the first year of the Covid-19 pandemic, with 21 percent holding at least one delinquent credit card account and an average of 0.5 delinquent accounts. At the same time, average credit scores are somewhat lower in San Francisco—626 versus 635 in Los Angeles—though scores in both samples fall within the subprime range. Baseline financial characteristics are similar across experimental groups in both counties, supporting the validity of the research design.

Comparison of the demographic characteristics of the two samples highlights modest differences across the two counties. Applicants in Los Angeles are, on average, younger (38 versus 43 years old), more likely to be female (61 versus 48 percent), and more likely to speak English (87 versus 75 percent) than current participants facing recertification in San

[7]Additionally, Appendix Tables A.1 and A.2 reassess balance checks conducted in Giannella et al. (2024) and Homonoff and Somerville (2021) for our sample of cases that have at least one member with a match to the credit report data.

Francisco. Income levels are also somewhat higher in Los Angeles: applicants report average household income in the past 30 days of $779, with 47 percent reporting no income at all. In contrast, San Francisco recertification cases report average quarterly income of $1,759 (equivalent to $586 per month), with 62 percent reporting no income in the quarter prior to recertification.

We find a few small, but statistically significant differences in baseline characteristics across experimental groups in both sites. For example, in Los Angeles, treatment and control households differ modestly in household size (1.8 versus 1.7) and SSI receipt (8.6 versus 9.2 percent). In San Francisco, we observe differences in the applicant's age (42.7 in late interview group versus 42.3 in early) and the share of U.S. citizens (84.6 versus 83.6 percent). While these differences suggest that conditioning on appearing in the credit report data may have introduced some bias, these differences are economically small and are unlikely to substantially affect our estimates, especially after controlling for the baseline characteristics themselves.

5 Empirical Strategy

We use two complementary research designs to estimate the causal effect of SNAP access on financial health. First, we conduct reduced-form event studies to estimate the effect of the flexible interview interventions using the following specification:

$$Y_{it} = \alpha + \sum_{k=-5}^{11} \mathbb{1}[q_{it} = k]\big(\beta_k \cdot \text{Treatment}_i + \gamma_k\big) + \delta \cdot \text{Treatment}_i + \theta X_i + \varepsilon_{it} \qquad (1)$$

where Y_{it} denotes the financial outcome for individual i in calendar quarter t, and $\mathbb{1}[q_{it} = k]$ is an indicator for event time k, defined relative to the quarter of SNAP application or recertification (event time 0), depending on the intervention. We omit $k = -1$ as the reference period. In Los Angeles, Treatment_i is an indicator for assignment to the treatment communications regarding the flexible interview process. In San Francisco, the treatment

variable is the number of days between the assigned interview date and the recertification deadline. To estimate the effect of moving from the latest to the earliest possible interview date, we multiply the coefficient—which captures the impact of an additional day—by the full range of possible interview dates (28 days). The vector X_i includes the demographic characteristics summarized in Table A.1 or A.2, along with application/recertification quarter fixed effects, office fixed effects (Los Angeles), and assignment group strata (San Francisco). β_k, our coefficients of interest, estimate the reduced-form effects of the flexible interview interventions on financial outcomes over a three-year period following randomization as well as five pre-period quarters. Standard errors are clustered at the case level, the unit of randomization.

Second, we provide treatment-on-the-treated estimates that scale our reduced form estimates by the first stage effects of the interventions on SNAP participation. Specifically, we estimate an instrumental variables (IV) regression in which we instrument SNAP approval/[recertification] success with random assignment to client-initiated interview access/[earlier interview dates] using the following joint specification:

$$SNAP_i = \delta_0 + \sum_{l=1}^{3} \mathbb{1}[\text{Year}_t = l](\delta_{1l} \cdot \text{Treatment}_i + \delta_2) + \delta_3 \text{Treatment}_i + \delta_4 X_i + \varepsilon_{it} \quad (2)$$

$$Y_{it} = \eta_0 + \sum_{l=1}^{3} \mathbb{1}[\text{Year}_t = l](\eta_{1l} \cdot \text{SNAP}_i + \eta_2) + \eta_3 \text{SNAP}_i + \eta_4 X_i + \varepsilon_{it} \quad (3)$$

where Y_{it} is individual i's financial outcome in post-randomization event-year t averaged across quarters, SNAP_i is an indicator for whether individual i's initial application was approved (Los Angeles) or whether they successfully recertified (San Francisco), and $\mathbb{1}[\text{Year}_t = l]$ is an indicator for event-year l. We omit the quarter prior to treatment as the reference period. The control vector X_i includes the same set of covariates as in Equation (1).

The coefficients η_{1l} identify the financial consequences of gaining or maintaining access

to SNAP among applicants and current participants who would otherwise have been denied program benefits were it not for the intervention. Specifically, they capture the effect of having an initial application approved or a recertification application denied for these marginal cases. If the interventions affect financial health through channels other than the likelihood of approval, our estimates will be biased due to an exclusion restriction violation. However, the interventions we study alter only the scheduling of interviews, holding all other eligibility criteria constant. Given their narrow scope, we believe it is unlikely that they had an effect on financial health other than through SNAP participation.

At the same time, it is worth noting that marginally denied cases may, and do, successfully reapply for the program. As a result, our IV estimates are likely an underestimate of the effect of overall SNAP participation on financial health.[8]

6 Results

6.1 Los Angeles County

Figure 2 presents reduced form estimates of the effect of access to flexible interviews in Los Angeles. The figure plots quarterly event-time treatment effects from one year before to three years after random assignment for our primary outcomes: credit card balances, the presence and number of delinquent credit card accounts, and credit score.

Panel A shows a gradual but persistent decline in credit card debt among individuals who received treatment communications detailing the availability of flexible interviews, relative to those in the control group who received standard communications about the scheduled interview process. While we find no evidence of differential trends prior to the start of the intervention, we observe relative decreases in debt balances for the treatment group starting around the third quarter post-randomization. By the end of the three-year follow-up period,

[8]The length of our SNAP panel data limits our ability to estimate cumulative SNAP exposure over our study period.

credit card balances are approximately $150 lower in the treatment group.

Turning to delinquencies (Panels B and C), we find that treatment group members are significantly less likely to have a delinquent credit card account. As with credit card balances, these treatment effects grow over time: by the end of the follow-up period, treatment members are 0.75 percentage points less likely to have a delinquent account than control members. We also find significant decreases in the number of delinquent credit card accounts. This effect emerges and is marginally significant in the first full post-randomization quarter, however, the estimates are not significant in any other quarter.

Panel D presents the effect of the intervention on credit scores. We find that assignment to the treatment group results in modest increases in credit score that grow over time, following the trends in debt and delinquencies and becoming statistically significant by the end of the study period. We do not find significant effects of new accounts in collection or bankruptcy filings, our measures of more extreme financial distress (Panels E and F).[9]

These results demonstrate that removing barriers to SNAP access generated significant improvements in creditworthiness and overall financial well-being. It is worth noting that "treatment" in our context refers to receiving information about flexible interviews, not take-up, and that only half of treatment group members actually called the flexible interview hotline. As a result, the intent-to-treat estimates we provide may underestimate the true effect of interview flexibility. By this same logic, if we believe that the intervention affects financial health only through its impact on SNAP participation, we can calculate treatment-on-the-treated estimates by scaling the reduced form estimates by the effect on participation.

Figure 1(a) presents the effect of the flexible interview intervention on SNAP participation by week for the five months post-randomization—the "first stage". These analyses mirror those in Giannella et al. (2024), but for our matched CCP sample. As in the original study,

[9]Table A.3, Panel A provides complementary regression estimates for all financial outcomes at the annual level. As a robustness check, Table A.4, Panel A repeats these analyses excluding the 10 percent of cases determined to be ineligible based on the self-reported income and demographic information provided in their SNAP application. We find that this sample restriction generally leads to larger treatment effects measured with greater precision, consistent with the inclusion of ineligible households, which should not be affected by interview flexibility, biasing our estimates toward zero.

we find that treatment group assignment increased approval rates by just over six percentage points. While the treatment effect decreases over time, due to the control group successfully reapplying for the program after being denied, we still observe a three percentage point increase in SNAP participation at the end of the follow-up period.

Table 2 reports treatment-on-the-treated estimates of the effect of SNAP on financial well-being. Panel A shows that SNAP access leads to substantial reductions in credit card debt and delinquencies. The marginal SNAP approval results in a reduction in credit card balances of $226, $1,374, and $2,386 in the three years post-randomization, respectively. This represents a 50 percent decrease relative to the pre-randomization control mean of $4,778 by the end of the study period. Column 2 shows that marginally approved cases experience significant decreases in the likelihood of having a delinquent account of 5.0, 7.5, and 10.1 percentage points in years 1, 2, and 3, respectively, relative to a baseline delinquency rate of 41 percent. These changes in financial health also present as sizable increases in credit score of 17 points by the end of the study period. The average baseline credit score in our sample is 634, suggesting that the increases we observe, while somewhat modest and only statistically significant in the final quarter, have the potential to move recipients out of the subprime range.

6.2 San Francisco County

We next examine the effects of maintaining SNAP access at the time of recertification among current SNAP recipients in San Francisco. Figure 1(b) presents first stage effects of receiving an earlier recertification interview on SNAP participation by week for the year post-randomization using our matched sample. As in Homonoff and Somerville (2021), we find that assignment to the earliest (versus latest) recertification interview leads to a 10 percentage point increase in the recertification rate and a 2 percentage point increase in long-term SNAP participation. These results suggest that recertification flexibility in the form of earlier interviews leads to large decreases in short-term benefit loss (i.e., program churn) and

smaller, yet still significant, decreases in long-term program exit.

Figure 3 replicates the reduced-form event study analyses in Figure 2 for the San Francisco experiment to estimate the effects of recertification interview flexibility on our main financial outcomes: credit card balances, delinquencies, and credit scores. Here too, we find that flexible interviews lead to significant improvements in financial well-being. As in Los Angeles, the largest treatment effects emerge toward the end of three year study period. One exception is that we also observe immediate, significant improvements in credit card debt, delinquencies, and credit score among participants with earlier interview dates in the quarter following recertification that attenuate shortly after, consistent with the intervention's large effect on SNAP churn. These findings suggest that even short-term loss in benefits can lead to measurable financial distress. Again, we find no effect of flexible interviews on collections or bankruptcy filings.

Table 2, Panel B presents our treatment-on-the-treated estimates. We find that participants who are disenrolled due to later interview dates increase their credit card debt by a marginally significant $500 in the first year post-recertification, compared to a baseline mean of $1,963, and just under $1,000 by the end of the study period, though the effect is no longer significant. These effects are smaller than those observed in Los Angeles in absolute terms, but similar when calculated as a percent of the baseline mean. Marginal disenrollments lead to imprecisely-estimated reductions in the likelihood of having any delinquent account of 2.3, 4.7, and 7.7 percentage points by year, respectively, but a precisely-estimated 0.41 decrease in the number of delinquent accounts (87 percent relative to the baseline mean). Finally, these disenrolled participants experience decreases in their credit scores ranging from 15 to 19 points, though the decreases are only marginally significant.[10]

Taken together, the findings from our two experimental settings provide complementary

[10]Table A.4, Panel B restricts the sample to the 95 percent of cases that appear eligible, here defined as having wage income below 200 percent of the Federal Poverty Level, the gross income limit for SNAP eligibility in California, in the quarter prior to recertification. Excluding these likely-ineligible cases from our sample increases the size and precision of our estimates; for example, effects on credit card balances and delinquency rates become marginally significant by the end of the study period.

evidence that administrative burdens associated with the interview requirement have significant downstream effects on financial health both at initial application and recertification. Importantly, the patterns of these effects suggest that both temporary and longer-term loss of benefits can lead to tangible measures of financial distress.

6.3 Heterogeneity by Income

We next explore heterogeneity in our estimates by baseline income. Specifically, we compare effects for households with and without income just prior to randomization—defined by reporting zero income in the past 30 days on the SNAP application (Los Angeles) or having no UI wage earnings in the quarter prior to recertification (San Francisco).[11] Table A.5 presents the reduced form estimates separately for individuals with and without household income in Los Angeles (Panel A) and San Francisco (Panel B), respectively.

The results suggest that reducing barriers to SNAP access may serve different financial functions across the income distribution. In both counties, treatment effects on credit card balances are concentrated among households with baseline income. There is some evidence that households without income experience greater improvements in delinquency rates and credit scores, though this pattern is not consistent across all outcomes and years.

Differences in the effect of the intervention on financial outcomes across income groups could, alternatively, reflect differences in the effect of the intervention on SNAP take-up for households with and without income. However, we find no evidence of differential treatment effects on SNAP participation between the income groups in either county (Table A.6). This allows us to conclude that the heterogeneity in financial outcomes we observe arises from variation in the impact of SNAP access, rather than differential program take-up. Separately, Table A.6 allows us to assess the targeting properties of inflexible interviews, following the methods common in the targeting literature. The (lack of) heterogeneity in the first stage suggests that removing this barrier to participation did not worsen targeting.

[11] Forty-seven percent of individuals reported no income in Los Angeles and 62 percent in San Francisco.

7 Conclusion

This paper provides new causal evidence that removing administrative barriers to SNAP access reduces financial distress among low-income individuals. We construct a novel linkage between SNAP administrative data and individual credit bureau records to study the the effects of two interventions—flexible enrollment interviews in Los Angeles County and earlier recertification appointments in San Francisco County—on financial well-being. We find that these administrative changes lead to meaningful improvements in financial health over time, including reductions in credit card debt and delinquency, and increases in credit scores.

Importantly, our results suggest that even short-term benefit losses lead to financial distress. Related research finds that small expenses such as traffic fines or car repairs lead to increased collections activity, lower credit scores, and even reduced employment (Mello, 2024, Barr et al., 2025). These studies mirror our setting, where the financial shock of losing SNAP benefits——even when modest in dollar terms——leads to measurable credit deterioration.

Our study contributes to a growing literature on how administrative burdens shape access to safety net programs and, in turn, affect economic outcomes. By linking administrative SNAP records to individual credit data, we show that procedural features of benefit delivery can generate downstream consequences for household financial stability—extending beyond food security and material hardship to affect debt repayment and creditworthiness. These indirect financial effects substantially increase the estimated value of SNAP. For example, Brevoort et al. (2020) estimates that a 10-point increase in credit score yields $45–$70 in annual savings for borrowers, suggesting that the increases in credit scores we observe among the marginal SNAP enrollees translate to savings of over $100 per year. Taken together, our results highlight the economic importance of implementation design and its role in amplifying (or undermining) the financial protection provided by social programs.

References

Aizer, A. (2007). Public health insurance, program take-up, and child health. *The Review of Economics and Statistics 89*(3), 400–415.

Almond, D., H. W. Hoynes, and D. W. Schanzenbach (2011). Inside the war on poverty: The impact of food stamps on birth outcomes. *The review of economics and statistics 93*(2), 387–403.

Bailey, M. J., H. W. Hoynes, M. Rossin-Slater, and R. Walker (2024). Is the social safety net a long-term investment? large-scale evidence from the food stamps program. *Review of Economic Studies 91*(3), 1291–1330.

Barr, A., J. Eggleston, S. Mello, and A. Smith (2025). The big economic consquences of small shocks: Evidence from car repairs. Technical report, Mimeo.

Bhardwaj, S. and D. Tabak (2025). Effect of redetermination of safety-net on financial distress and local crime: Evidence from indiana. Working paper.

Board of Governors of the Federal Reserve System (2024, May). Report on the economic well-being of u.s. households in 2023.

Brevoort, K., D. Grodzicki, and M. B. Hackmann (2020). The credit consequences of unpaid medical bills. *Journal of Public Economics 187*, 104203.

Brown, K. S. and B. Braga (2019, February). Financial distress among american families: Evidence from the well-being and basic needs survey.

Cook, J. B. and C. N. East (2023). The effect of means-tested transfers on work: evidence from quasi-randomly assigned snap caseworkers. Technical report, National Bureau of Economic Research.

Cook, J. B. and C. N. East (2024). Work requirements with no teeth still bite: Disenrollment and labor supply effects of snap general work requirements. Technical report, National Bureau of Economic Research.

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

Cunnyngham, K. E. (2025). Estimates of state supplemental nutrition assistance program participation rates in 2022. *Mathematical Policy Research*.

Currie, J. (2006). The take-up of social benefits. In A. Auerbach, D. Card, and J. Quigley (Eds.), *Poverty, The Distribution of Income, and Public Policy*, pp. 80–148. New York: Russell Sage.

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), 213–48.

Dobkin, C., A. Finkelstein, R. Kluender, and M. J. Notowidigdo (2018). The economic consequences of hospital admissions. *American Economic Review 108*(2), 308–352.

Dodini, S., J. Larrimore, and A. Tranfaglia (2024). Financial repercussions of SNAP work requirements. *Journal of Public Economics 229*, 105034.

Edin, K., M. Boyd, J. Mabli, J. Ohls, J. Worthington, S. Greene, N. Redel, and S. Sridharan (2013). Snap food security in-depth interview study. *Mathematica Policy Research*.

Finkelstein, A. and M. J. Notowidigdo (2019). Take-up and targeting: Experimental evidence from snap. *The Quarterly Journal of Economics 134*(3), 1505–1556.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and O. H. S. Group (2012). The oregon health insurance experiment: Evidence from the first year. *The Quarterly Journal of Economics 127*(3), 1057–1106.

Giannella, E., T. Homonoff, and J. Somerville (2024). Administrative burden and procedural denials: Experimental evidence from SNAP. *American Economic Journal: Economic Policy*. Forthcoming.

Goldin, J., I. Z. Lurie, and J. McCubbin (2021). Health insurance and mortality: Experimental evidence from taxpayer outreach. *The Quarterly Journal of Economics 136*(1), 1–49.

Goldin, J. and D. Reck (2018). Rationalizations and mistakes: optimal policy with normative ambiguity. In *AEA Papers and Proceedings*, Volume 108, pp. 98–102. American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203.

Gray, C., A. Leive, E. Prager, K. Pukelis, and M. Zaki (2023, February). Employed in a snap? the impact of work requirements on program participation and labor supply. *American Economic Journal: Economic Policy 15*(1), 306–41.

Gross, T. and M. Notowidigdo (2011). Health insurance and the consumer bankruptcy decision: Evidence from expansions of medicaid. *Journal of Public Economics 95*(7-8), 767–778.

Han, J. (2016). The impact of snap on material hardships: Evidence from broad-based categorical eligibility expansions. *Southern Economic Journal 83*(2), 464–486.

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

Harris, T. F. (2021). Do snap work requirements work? *Economic Inquiry 59*(1), 72–94.

Hastings, J., R. Kessler, and J. M. Shapiro (2021). The effect of snap on the composition of purchased foods: Evidence and implications. *American Economic Journal: Economic Policy 13*(3), 277–315.

Herd, P., H. Hoynes, J. Michener, and D. Moynihan (2023). Introduction: Administrative burden as a mechanism of inequality in policy implementation. *RSF: The Russell Sage Foundation Journal of the Social Sciences 9*(5), 1–23.

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

Homonoff, T. and J. Somerville (2021). Program recertification costs: Evidence from snap. *American Economic Journal: Economic Policy 13*(4), 271–98.

Hoynes, H., D. W. Schanzenbach, and D. Almond (2016). Long-run impacts of childhood access to the safety net. *American Economic Review 106*(4), 903–34.

Hoynes, H. W. and D. W. Schanzenbach (2012). Work incentives and the food stamp program. *Journal of Public Economics 96*(1-2), 151–162.

Hu, L., R. Kaestner, B. Mazumder, S. Miller, and A. Wong (2018). The effect of the affordable care act medicaid expansions on financial wellbeing. *Journal of Public Economics 163*, 99–112.

Kim, J. Y., P. Herd, S. Jilke, D. Moynihan, and K. Rodden (2025). Administrative checkpoints, burdens, and human-centered design: Increasing interview access to raise snap participation. *Journal of Policy Analysis and Management*.

Ko, W. and R. A. Moffitt (2024). Take-up of social benefits. *Handbook of Labor, Human Resources and Population Economics*, 1–42.

Kramer-Mills, C., A. Nair, B. Rodriguez, and J. Scally (2024, January). The state of low-income america: Credit access & housing.

Mazumder, B. and S. Miller (2016). The effects of the massachusetts health reform on household financial distress. *American Economic Journal: Economic Policy 8*(3), 284–313.

Mello, M. (2024). The effects of consumer bankruptcy protection. *Review of Economic Studies*. Advance online publication.

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

NAS (2019). *A roadmap to reducing child poverty*. National Academies of Sciences, National Academies Press.

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

Rafkin, C., A. Solomon, and E. Soltas (2023). Self-targeting in us transfer programs. *Available at SSRN 4495537*.

Saunders, T., P. Herd, S. Jilke, D. Moynihan, and E. Safran (2025). Burden reduction in a social safety net program reduces mortality. Working paper.

Shaefer, H. L. and I. A. Gutierrez (2013). The supplemental nutrition assistance program and material hardships among low-income households with children. *Social Service Review 87*(4), 753–779.

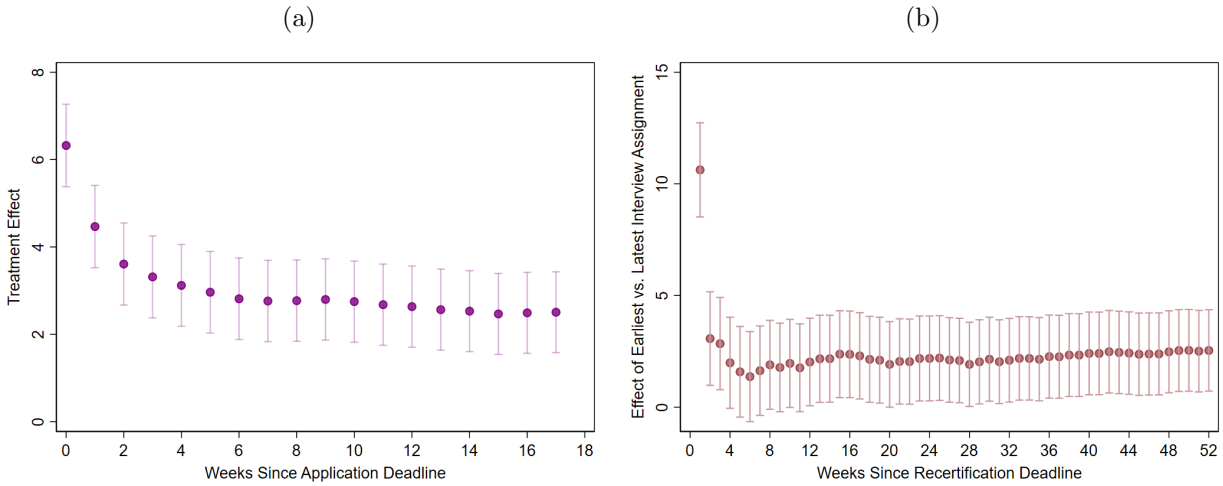
Shapiro, J. M. (2005). Is there a daily discount rate? evidence from the food stamp nutrition cycle. *Journal of Public Economics 89*(2), 303–325.

Unrath, M. (2024). Targeting, screening, and retention: Evidence from california's food stamps program. *Mimeo*.

U.S. Department of Agriculture (2023). Supplemental nutrition assistance program (snap): Fiscal year 2023 characteristics report. Accessed: May 15, 2015.

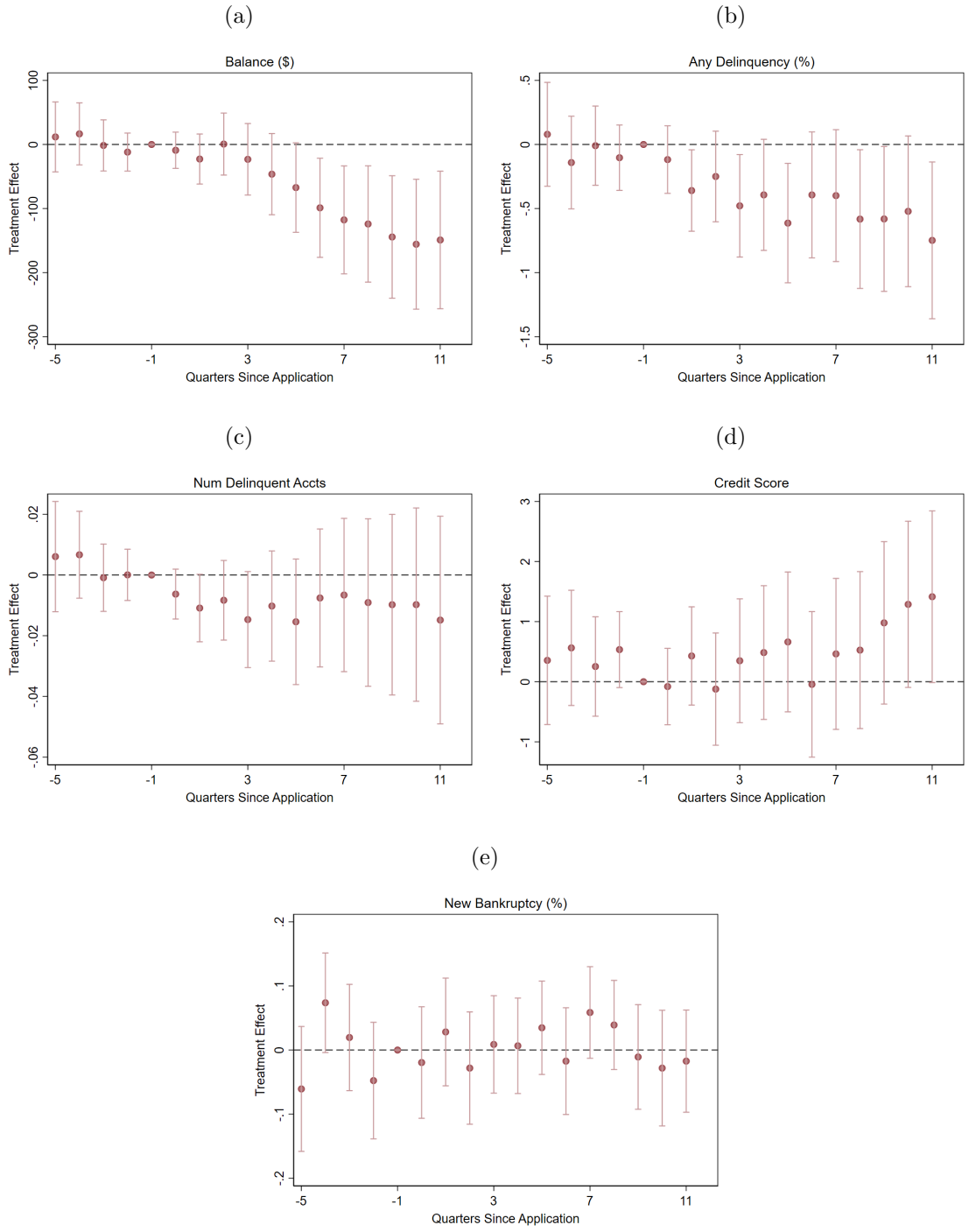
Wu, D. and B. D. Meyer (2023). Certification and recertification in welfare programs: What happens when automation goes wrong? Technical report, National Bureau of Economic Research.

Figure 1: First Stage: Effects of Flexible Interviews on SNAP Participation



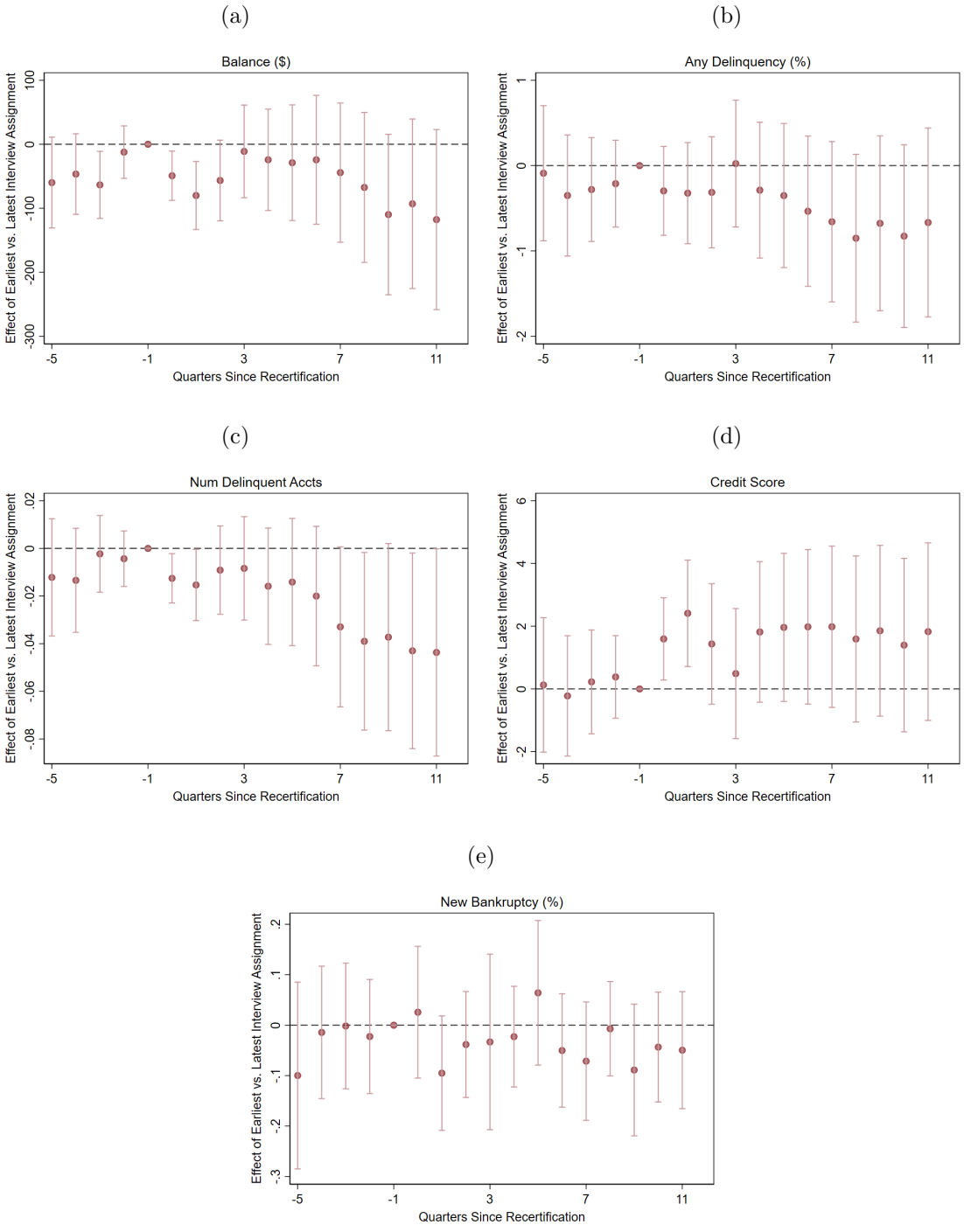
Notes: This figure presents the effects of the flexible interview interventions on SNAP participation. Panel (a) presents the estimated effects of receiving treatment communications regarding flexible interview availability in Los Angeles. Panel (b) presents the estimated effects of being assigned to the earliest versus latest scheduled recertification interview in San Francisco. In both panels, the outcome is an indicator for ever receiving SNAP by a given week since the application or recertification deadline, respectively. Regressions include controls for baseline characteristics listed in Appendix Tables A.1 and A.2 as well as week, day-of-week, and SNAP office fixed effects (Los Angeles) and interview scheduling strata (San Francisco). Coefficient estimates are reported in percentage points (0–100). Bars denote the 95 percent confidence interval with standard errors clustered at the application level.

Figure 2: Effect of Interview Flexibility on Financial Well-Being by Quarter, Los Angeles



Notes: This figure presents event-study estimates of the effects of receiving communications regarding the flexible interview in Los Angeles County on various financial outcomes by quarter relative to the individual's SNAP application date. Treatment effects correspond to the event study coefficients ($\hat{\beta}_k$) estimated using Equation (1). Controls include baseline demographics listed in Appendix Table A.1 as well as week, day-of-week, and SNAP office fixed effects. Outcomes in each panel are: (a) credit card balances, (b) indicator of any credit card delinquency, (c) number of delinquent credit card accounts, (d) credit score, and (e) indicator of new bankruptcy filing. Bars denote the 95 percent confidence interval with standard errors clustered at the application level.

Figure 3: Effect of Interview Flexibility on Financial Well-Being by Quarter, San Francisco



Notes: This figure presents event-study estimates of being assigned to the earliest versus latest scheduled recertification interview in San Francisco County on various financial outcomes by quarter relative to the individual's SNAP recertification deadline. Treatment effects correspond to the event study coefficients ($\hat{\beta}_k$) estimated using Equation (1). Controls include baseline demographics listed in Appendix Table A.2 as well as interview scheduling strata fixed effects. Outcomes in each panel are: (a) credit card balances, (b) indicator of any credit card delinquency, (c) number of delinquent credit card accounts, (d) credit score, and (e) indicator of new bankruptcy filing. Bars denote the 95 percent confidence interval with standard errors clustered at the recertification case level.

Table 1: Baseline Credit Report Outcomes

	Los Angeles				San Francisco			
	Full Sample	Treatment	Control	p-value	Full Sample	Early Interview	Late Interview	p-value
Credit Card Balance ($)	4,778	4,784	4,761	0.78	1,963	1,972	1,954	0.73
Any Delinquent Account (%)	41.38	41.45	41.17	0.55	20.81	20.57	21.05	0.30
Num Delinquent Accounts	1.37	1.38	1.36	0.59	0.47	0.47	0.47	0.94
Credit Score	634.19	634.33	633.76	0.57	625.91	625.34	626.47	0.39
New Bankruptcy Filing (%)	0.09	0.08	0.09	0.83	0.07	0.09	0.04	0.11
Number of Observations	61,967	46,508	15,459		37,021	18,528	18,493	

Notes: This table presents baseline financial outcomes in the quarter prior to randomization for our study samples, overall and by experimental group (treatment versus control in Los Angeles and early versus late interviews in San Francisco). P-values are associated with with a test for equality of means across experimental groups. Financial outcomes include: total credit card balance, indicator of any credit card delinquency, number of delinquent credit card accounts, credit score, whether the person files for bankruptcy, and indicator of any new collections.

Table 2: Effect of Marginally Enrolling in SNAP on Financial Well-Being

	Balance	Any Delinquency	Num Del. Account	New Bankruptcy	Credit Score
Panel A: Los Angeles					
Year 1	-225.96	-5.02**	-0.17+	-0.04	2.35
	(317.08)	(2.45)	(0.09)	(0.57)	(6.12)
Year 2	-1374.26**	-7.49**	-0.17	0.34	6.42
	(597.73)	(3.79)	(0.17)	(0.55)	(9.11)
Year 3	-2385.97***	-10.13**	-0.18	-0.07	17.25
	(831.13)	(4.72)	(0.25)	(0.56)	(10.85)
N	246,132	246,132	246,132	246,132	223,124
Dep. Var. Mean	4778.07	41.38	1.37	0.09	634.19
Panel B: San Francisco					
Year 1	-500.99+	-2.31	-0.12	-0.36	14.56+
	(262.94)	(2.76)	(0.08)	(0.52)	(7.79)
Year 2	-309.29	-4.66	-0.21	-0.20	19.02+
	(459.30)	(4.16)	(0.14)	(0.49)	(11.47)
Year 3	-986.66	-7.68	-0.41**	-0.48	16.39
	(651.72)	(5.20)	(0.21)	(0.49)	(13.23)
N	148,084	148,084	148,084	148,084	127,888
Dep. Var. Mean	1962.95	20.81	0.47	0.07	625.91

This table presents estimates of the effects of being marginally enrolled in SNAP due to interview flexibility on financial well-being. These treatment-on-the-treated estimates correspond to the coefficients $\hat{\eta}_1$ derived from a two-stage least-squares specification in equations (2) and (3). Panel A instruments application approval with assignment to treatment communications regarding flexible interview availability in Los Angeles. Panel B instruments recertification success with scheduled interview timing. Each row of estimates corresponds to effects on average quarterly financial outcomes in the first, second, or third year after the recertification month. Dependent variable means provided for the quarter prior to randomization. Outcomes include: credit card balance, indicator of any credit card delinquency, number of delinquent credit card accounts, whether the person files for a new bankruptcy, and credit score. Outcomes are measured at the individual level. Standard errors in parentheses are clustered at the application/recertification level. $^{+}$ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A: Tables and Figures

Appendix Table A.1: Baseline Characteristics, Los Angeles

	Full Sample	Treatment	Control	p-value
Household size	1.8	1.8	1.7	0.00
Female submitter (%)	60.5	60.5	60.4	0.71
Submitter age	38.0	38.0	37.9	0.38
English speaking (%)	86.8	86.7	87.1	0.28
Elderly or disabled (%)	16.1	16.2	15.9	0.30
Receives SSI (%)	9.0	9.2	8.6	0.04
Stable housing (%)	63.3	63.4	63.0	0.39
Rent or mortgage ($)	837	833	848	0.58
Any income in past 30 days (%)	47.0	47.0	47.1	0.86
Income past 30 days ($)	779	778	781	0.82
Any money on hand (%)	56.8	56.7	57.2	0.28
Money on hand ($)	1,102	1,103	1,102	1.00
Has non-job income (%)	18.0	18.0	17.9	0.76
Expedited (%)	49.6	49.5	50.1	0.22
Estimated eligible (%)	89.5	89.5	89.5	0.85
Number of cases	52,015	38,988	13,027	
Number of individuals	61,967	46,508	15,459	

Notes: This table shows summary statistics of baseline characteristics for the Los Angeles sample. Mean values are reported for the full sample (column 1), the treatment group (column 2), and the control group (column 3). Column 4 contains the p-value associated with a test for equality of means from columns 2 and 3. Application characteristics include: household size, sex and age of the application submitter, whether the case language is English, whether the household contains an elderly or disabled member, whether the household receives SSI, an indicator for having stable housing, estimate of monthly rent, income in the past 30 days (indicator and dollar amount), any money on hand (indicator and dollar amount), an indicator for non-job income, and estimates of whether the household qualifies for expedited benefits or appears eligible for SNAP based on the households' self-reported information in their applications.

Appendix Table A.2: Baseline Characteristics, San Francisco

	Full Sample	Early Interview	Late Interview	p-value
Household size	1.6	1.6	1.6	0.96
Female submitter (%)	48.0	47.7	48.2	0.45
Submitter age	42.5	42.3	42.7	0.05
English speaking (%)	75.4	75.0	75.7	0.18
US citizen (%)	84.1	83.6	84.6	0.04
Non-white (%)	78.0	78.1	77.9	0.76
Any children (%)	28.8	29.2	28.3	0.15
Years since first on SNAP	4.5	4.5	4.5	0.72
Monthly benefits ($)	229	230	228	0.41
Max SNAP benefits (%)	61.7	61.7	61.7	0.93
Quarterly wage income ($)	1,759	1,759	1,759	1.00
No wage income (%)	61.7	61.8	61.5	0.58
Wage income below 200% FPL (%)	94.7	94.6	94.9	0.29
Number of cases	22,956	11,708	11,248	
Number of individuals	28,639	14,575	14,064	

Notes: This table shows summary statistics of baseline characteristics for the San Francisco sample. Mean values are reported for the full sample (column 1), cases assigned recertification interview dates in the first half of the month, i.e. before the 14th (column 2), and cases assigned interview dates in the second half of the month (column 3). Column 4 contains the p-value associated with a test for equality of means from columns 2 and 3. Recertification case characteristics include: household size, the sex, age, citizenship, and race of the household head, whether the case language is English, presence of children, years since first SNAP application, prior year monthly SNAP benefit, and an indicator for receiving the maximum SNAP benefit as well as quarterly wage earnings, an indicator for receiving any wages, and an indicator for receiving wages below the gross income limit (200 percent of the Federal Poverty Level) in the quarter prior to recertification based on Unemployment Insurance wage data from the Employment Development Department.

Appendix Table A.3: Effect of Interview Flexibility on Financial Well-Being by Year

	Balance	Any Delinquency	Num Del. Account	New Bankruptcy	Credit Score
Panel A: Los Angeles					
Year 1	-13.57	-0.30**	-0.01^{+}	-0.00	0.14
	(19.00)	(0.14)	(0.01)	(0.03)	(0.37)
Year 2	-82.53**	-0.45**	-0.01	0.02	0.39
	(35.23)	(0.22)	(0.01)	(0.03)	(0.55)
Year 3	-143.30***	-0.61**	-0.01	-0.00	1.05
	(48.50)	(0.28)	(0.02)	(0.03)	(0.65)
N	246,132	246,132	246,132	246,132	223,124
Dep. Var. Mean	4778.07	41.38	1.37	0.09	634.19
Panel B: San Francisco					
Year 1	-49.26^{+}	-0.23	-0.01	-0.04	1.48^{+}
	(25.26)	(0.27)	(0.01)	(0.05)	(0.77)
Year 2	-30.41	-0.46	-0.02	-0.02	1.94^{+}
	(45.08)	(0.41)	(0.01)	(0.05)	(1.14)
Year 3	-97.02	-0.76	-0.04**	-0.05	1.67
	(63.22)	(0.51)	(0.02)	(0.05)	(1.32)
N	148,084	148,084	148,084	148,084	127,888
Dep. Var. Mean	1962.95	20.81	0.47	0.07	625.91

Notes: This table presents reduced form estimates of the flexible interview interventions on financial well-being by year. Panel A presents the estimated effects of receiving treatment communications regarding flexible interview availability in Los Angeles. Panel B presents the estimated effects of being assigned to the earliest versus latest scheduled recertification interview in San Francisco. All specifications include a set of yearly event time indicators, their interactions with the treatment variable, the treatment variable itself, and the control variables in Equation (1). Dependent variable means provided for the quarter prior to randomization. Outcomes include: credit card balance, indicator of any credit card delinquency, number of delinquent credit card accounts, whether the person files for a new bankruptcy, and credit score. Outcomes are measured at the individual level. Standard errors in parentheses are clustered at the application/recertification level. $^{+}$ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.4: Effect of Interview Flexibility on Financial Well-Being by Year Among Likely-Eligible Households

	Balance	Any Delinquency	Num Del. Account	New Bankruptcy	Credit Score
Panel A: Los Angeles					
Year 1	-20.15	-0.34**	-0.01**	0.02	0.43
	(19.79)	(0.16)	(0.01)	(0.04)	(0.40)
Year 2	-84.91**	-0.53**	-0.02	0.03	0.78
	(36.40)	(0.24)	(0.01)	(0.04)	(0.59)
Year 3	-142.61***	-0.72**	-0.02	0.01	1.59**
	(49.89)	(0.30)	(0.02)	(0.04)	(0.69)
N	220,528	220,528	220,528	220,528	199,068
Dep. Var. Mean	4608.91	41.20	1.35	0.08	632.79
Panel B: San Francisco					
Year 1	-46.80+	-0.28	-0.01	-0.05	1.63**
	(25.78)	(0.27)	(0.01)	(0.05)	(0.79)
Year 2	-30.62	-0.62	-0.02	-0.02	1.94+
	(46.04)	(0.41)	(0.01)	(0.05)	(1.16)
Year 3	-107.88+	-0.96+	-0.04**	-0.04	1.74
	(64.61)	(0.52)	(0.02)	(0.05)	(1.36)
N	140,096	140,096	140,096	140,096	120,720
Dep. Var. Mean	1957.40	20.54	0.46	0.07	626.93

Notes: This table presents reduced form estimates of the flexible interview interventions on financial well-being by year among likely-eligible households. The table repeats the analyses presented in Appendix Table A.3 excluding the 10 percent of cases in Los Angeles determined to be ineligible based on the self-reported information provided in their application and the 5 percent of cases in San Francisco with quarterly wage earnings above the SNAP eligibility threshold in the quarter prior to recertification. Panel A presents the estimated effects of receiving treatment communications regarding flexible interview availability in Los Angeles. Panel B presents the estimated effects of being assigned to the earliest versus latest scheduled recertification interview in San Francisco. All specifications include a set of yearly event time indicators, their interactions with the treatment variable, the treatment variable itself, and the control variables in Equation (1). Dependent variable means provided for the quarter prior to randomization. Outcomes include: credit card balance, indicator of any credit card delinquency, number of delinquent credit card accounts, whether the person files for a new bankruptcy, and credit score. Outcomes are measured at the individual level. Standard errors in parentheses are clustered at the application/recertification level. $^{+}$ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.5: Effect of Interview Flexibility on Financial Well-Being by Income

	No Income				Any Income			
	Balance	Any Delinquency	Num Del. Account	Credit Score	Balance	Any Delinquency	Num Del. Account	Credit Score
Panel A: Los Angeles								
Year 1	6.54	-0.37^{+}	-0.00	0.39	-34.04	-0.23	-0.02**	-0.09
	(26.53)	(0.20)	(0.01)	(0.54)	(27.24)	(0.21)	(0.01)	(0.51)
Year 2	-66.85	-0.58^{+}	-0.01	1.14	-98.39^{+}	-0.32	-0.01	-0.32
	(48.89)	(0.32)	(0.01)	(0.80)	(50.79)	(0.32)	(0.02)	(0.77)
Year 3	-39.07	-0.69^{+}	-0.02	1.10	-248.51***	-0.53	-0.00	1.00
	(66.07)	(0.39)	(0.02)	(0.94)	(71.06)	(0.39)	(0.02)	(0.91)
N	124,056	124,056	124,056	108,744	122,076	122,076	122,076	114,380
Dep. Var. Mean	4452.34	42.91	1.37	623.52	5104.90	39.85	1.38	644.65
Panel B: San Francisco								
Year 1	-12.91	-0.05	-0.01	2.30**	-97.85**	-0.47	-0.01	0.47
	(33.84)	(0.34)	(0.01)	(1.03)	(37.98)	(0.44)	(0.01)	(1.16)
Year 2	-46.79	-0.20	-0.02	1.82	-8.18	-0.79	-0.02	2.08
	(58.29)	(0.50)	(0.02)	(1.50)	(71.03)	(0.68)	(0.02)	(1.74)
Year 3	-106.18	-0.77	-0.06**	2.40	-83.83	-0.73	-0.02	0.76
	(81.24)	(0.63)	(0.03)	(1.76)	(99.43)	(0.83)	(0.03)	(2.00)
N	85,020	85,020	85,020	70,944	63,064	63,064	63,064	56,944
Dep. Var. Mean	1985.10	20.80	0.47	617.95	1933.10	20.83	0.47	636.44

Notes: This table presents annual reduced form estimates of the flexible interview interventions on financial well-being by baseline income. The table repeats the analyses presented in Appendix Table A.3 for cases with no household income (columns 1 through 4) versus with some household income (columns 5 through 8). In Los Angeles, baseline income is measured as reporting no income in the past 30 days on the SNAP application while in San Francisco, baseline income is measured using unemployment insurance wage data for the quarter prior to randomization. Panel A presents the estimated effects of receiving treatment communications regarding flexible interview availability in Los Angeles. Panel B presents the estimated effects of being assigned to the earliest versus latest scheduled recertification interview in San Francisco. All specifications include a set of yearly event time indicators, their interactions with the treatment variable, the treatment variable itself, and the control variables in Equation (1). Dependent variable means provided for the quarter prior to randomization. Outcomes include: credit card balance, indicator of any credit card delinquency, number of delinquent credit card accounts, and credit score. Outcomes are measured at the individual level. Standard errors in parentheses are clustered at the application/recertification level. $^{+}$ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.6: First Stage: Effects of Flexible Interviews on SNAP Participation by Baseline Income

	Los Angeles		San Francisco	
	Approved	SNAP 150 Days	Recertified	SNAP at 12 months
Treatment	6.86***	2.68***	9.41***	2.58*
	(0.67)	(0.64)	(1.53)	(1.35)
Any Income	-6.77***	-7.16***	-8.56***	-7.82***
	(1.01)	(1.01)	(1.39)	(1.32)
Treatment × Any Income	-1.15	-0.38	0.72	-0.52
	(0.96)	(0.95)	(2.38)	(2.24)
Dep. Var. Mean	53.83	62.89	43.57	72.81
Total Cases	51,608	51,608	22,956	22,956

Notes: This table presents the effects of the flexible interview interventions on SNAP participation by baseline income. Treatment refers to receipt of treatment communications regarding flexible interview availability in Los Angeles (Panel A) and assignment to the earliest versus latest scheduled recertification interview in San Francisco (Panel B). "Any Income" is an indicator for reporting some income in the the past 30 days on the SNAP application in Los Angeles and having at least some unemployment insurance wage data for the quarter prior to random assignment in San Francisco. Outcomes include indicators for: application approval (column 1), ever enrolling in SNAP within five months of application submission (column 2), recertification success (column 3), and ever participating in SNAP within one year of recertification deadline (column 4). Coefficient estimates are reported in percentage points (0–100). Standard errors clustered at the application level.

Appendix B: Data Linkage

To link the two administrative SNAP datasets to the UC-CCP at the individual level, each of the three data partners encrypted personally identifying information (PII) with a common algorithm. They each appended an obfuscating string of characters known as a "salt" to the original PII before encryption; this practice ensures that researchers cannot reverse engineer the PII as the salt is not revealed to them. The PII include Social Security Numbers (SSNs), first and last names, and date of birth.

We matched individuals by ranking potential matches of PII. For the San Francisco sample, individuals needed to share at least the same SSN with those in the UC-CCP to be considered a match, and multiple matches were resolved by assigning higher scores to those sharing more PII in common. For the Los Angeles sample, we additionally accepted perfect matches on all non-SSN variables in lieu of a SSN match due to greater missingness of the SSN in the dataset, but assigned higher scores to matches on SSN. In Los Angeles, 59,966 of the 64,711 SNAP cases (~93%) had at least one individual matched to the UC-CCP data, for a total of 74,733 matched individuals. In San Francisco, 26,954 of the 30,241 cases (~89%) had at least one individual matched, for a total of 39,016 matched individuals.

We retain matched individuals with a balanced panel of credit outcomes from five quarters before to eleven quarters after randomization, i.e., the quarter of application in Los Angeles or the recertification deadline in San Francisco. Our final analytical samples consist of 52,015 LA cases (61,967 individuals) and 22,956 SF cases (28,639 individuals).

It is worth noting that a non-match may be due to bad PII or the absence of a credit history. As a result, a less than perfect match rate presents two potential concerns. First, and most importantly, if the match rate differs by experimental status, for example, if SNAP access has a causal effect on appearing in the credit data, this could introduce bias in our estimates. Separately, even if the match rate does not differ by treatment group, our findings, while unbiased, are nonetheless conducted on a select sample of households.

Tables B.2 and B.1 explore potential bias induced by our high, yet imperfect match rate,

in each of the two counties. The first panel demonstrates that the match rate is consistent across experimental groups in both Los Angeles and San Francisco, suggesting that the matching process does not introduce bias into our estimates. The second panel compares the baseline characteristics of households in the original interventions who did or did not have a match in the credit data. These analyses show that the nine percent of cases that do not match are substantially more vulnerable than the cases that match across a variety of economic characteristics. To the extent that the interventions have larger effects on SNAP participation for these higher-need households, as suggested in Giannella et al. (2024), or if the downstream effects of SNAP participation for these households are larger, their exclusion from our analyses will underestimate the effect financial distress for the full population of SNAP participants. Separately, these differences are interesting in and of themselves as they may shed light on the characteristics of households excluded from the credit bureau data among a population of low-income households.

Appendix Table B.1: Match Rates and Case Characteristics by Match Status, Los Angeles

Panel A: Match Rates

	Full Sample	Treatment	Control	p-value
Matched UC-CCP (%)	92.7	92.7	92.6	0.92
Matched, Balanced Panel (%)	80.7	80.7	80.8	0.26
Number of cases	64,711	48,524	16,187	
Number of individuals	112,315	84,630	27,685	

Panel B: Case Characteristics by Match Status

	Full Sample	Matched	Unmatched	p-value
Household size	1.7	1.7	1.4	0.00
Female submitter (%)	59.6	59.9	56.0	0.00
Submitter age	37.4	37.2	40.2	0.00
English speaking (%)	85.4	86.8	68.2	0.00
Elderly or disabled (%)	16.9	15.9	29.9	0.00
Receives SSI (%)	10.1	9.3	21.6	0.00
Stable housing (%)	59.6	60.6	47.6	0.00
Rent or mortgage ($)	768.3	789.9	487.5	0.00
Any income in past 30 days (%)	44.7	45.7	32.4	0.00
Income past 30 days ($)	710.1	737.6	352.8	0.00
Any money on hand (%)	55.4	55.9	49.2	0.00
Money on hand ($)	999.3	1,034.9	535.6	0.00
Has non-job income (%)	16.7	17.0	12.5	0.00
Expedited (%)	52.6	51.4	68.3	0.00
Estimated eligible (%)	90.5	90.1	95.3	0.00
Number of cases	64,711	59,966	4,745	

Notes: Panel A presents match rates between individuals associated with SNAP applications in the Los Angeles flexible interview intervention and the UC-CCP credit data, overall and by treatment status. The first row presents the fraction of applications with any household member appearing in the UC-CCP data; the second row presents the fraction of applications with any household member appearing in the UC-CCP data for five quarters before and eleven quarters after random assignment. Panel B presents baseline characteristics for the full intervention sample and for applications that did or did not appear in the UC-CCP data. P-values are associated with a test for equality of means between columns 2 and 3.

Appendix Table B.2: Match Rates and Case Characteristics by Match Status, San Francisco

Panel A: Match Rates

	Full Sample	Early Interview	Late Interview	p-value
Matched UC-CCP (%)	89.1	89.1	89.2	0.57
Matched, Balanced Panel (%)	76.0	76.1	75.9	0.66
Number of cases	30,241	15,406	14,835	
Number of individuals	60,650	30,977	29,673	

Panel B: Case Characteristics by Match Status

	Full Sample	Matched	Unmatched	p-value
Household size	1.5	1.5	1.2	0.00
Female submitter (%)	46.3	46.8	42.4	0.00
Submitter age	42.4	42.2	44.1	0.00
English speaking (%)	71.2	73.7	51.1	0.00
Any children (%)	29.7	29.3	32.7	0.00
US citizen (%)	78.3	80.9	56.4	0.00
Non-white (%)	77.7	77.9	76.6	0.10
Years since first on SNAP	4.4	4.5	4.0	0.00
Monthly benefits ($)	225.0	227.1	207.6	0.00
Max SNAP benefits (%)	64.4	63.3	73.6	0.00
Quarterly wage income ($)	1,475.8	1,619.7	295.5	0.00
No wage income (%)	66.9	64.0	90.3	0.00
Wage income below 200% FPL (%)	95.5	95.1	99.1	0.00
Number of cases	30,241	26,954	3,287	

Notes: Panel A presents match rates between individuals associated with SNAP recertification cases in the San Francisco interview timing intervention and the UC-CCP credit data, overall and by treatment status. The first row presents the fraction of recertification cases with any household member appearing in the UC-CCP data; the second row presents the fraction of recertification cases with any household member appearing in the UC-CCP data for the five quarters before and eleven quarters after random assignment. Panel B presents baseline characteristics for the full recertification sample and for cases that did or did not appear in the UC-CCP data. P-values are associated with a test for equality of means between columns 2 and 3.