# The Financial Consequences of Being Denied Benefit Access*

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**Abstract**

We examine the causal impact of being denied benefit access on the financial well-being of marginally-excluded households. Using experimental and quasi-random variation in the flexibility of mandatory intake interviews and a unique linkage between administrative Supplemental Nutrition Assistance Program (SNAP) and credit report data, we find that households screened out of SNAP due to administrative barriers suffer tangible downstream economic consequences. Specifically, we find that process-related denials increase debt and delinquencies, and decrease credit scores. These results highlight the economic importance of implementation design and its role in strengthening (or undermining) the financial protection provided by social insurance programs.

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# 1 Introduction

A substantial body of evidence documents the financial precarity of low-income households in the United States (Morduch and Schneider, 2017, Brown and Braga, 2019, Board of Governors of the Federal Reserve System, 2024, Kramer-Mills et al., 2024, Cox et al., 2024). 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 among excluded households.

Our paper addresses this open question by estimating the effects of procedural barriers to SNAP participation on the financial well-being of marginally-denied households. To do so, we link individual-level credit reports 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 County and recertification rates in San Francisco County (Giannella et al., 2024, Homonoff and Somerville, 2021).[1] 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,

[1]Prior work finds that missed interviews account for up to half of all SNAP denials (Kim et al., 2025), highlighting the importance of studying this particular barrier.

allows us to estimate the effect of the interventions on various measures of financial health, such as credit card debt and delinquencies, 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 granted greater autonomy over the scheduling of their interview via randomly-assigned access to an interview call center 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, resulting in marginally significant increases in credit scores by the end of the study period. We find similar effects of recertification interview flexibility in San Francisco: participants assigned to earlier recertification interviews, which allow more time to reschedule missed interviews before the deadline, experience significant decreases in credit card delinquencies and increases in credit scores.

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 marginally-approved SNAP recipients experience reductions in credit card balances of \$146, \$1,129, and \$1,982 in each of the first three years post-application. These declines represent a 44 percent decrease relative to the control mean by the end of the study period and are similar in magnitude to the annual maximum SNAP benefit for a single-person household. The fraction of households with a delinquent account decreases by 6 to 11 percentage points while severe delinquencies decrease by up to 15 percentage points. This represents a decline of up to one quarter for overall delinquencies and three quarters for severe delinquencies, suggesting meaningful improvements in payment behavior over time. In San Francisco, marginally re-enrolled SNAP recipients experience a steady decrease in the likelihood of having any delinquency with a decrease of 12 percentage points (a 57 percent decrease) after three years. Additionally, earlier interviews all but

eliminate the prevalence of severe delinquencies for these marginal enrollees. We observe decreases in credit card balances of up to $1,000, but these changes in debt are not statistically significant. In both counties, we observe marginally significant improvements in credit scores of around 20 points.

Our findings provide new causal evidence on the effects of SNAP on financial health. A large literature documents effects of SNAP on food security (Shapiro, 2005, Gregory and Smith, 2019, Hastings et al., 2021), long-term health and economic self-sufficiency (Almond et al., 2011, Hoynes et al., 2016, Bailey et al., 2024), and the 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, which may suffer from small sample sizes and potential mismeasurement (Meyer et al., 2022), 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.[3]

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

[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] 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 does not directly observe SNAP participation.

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 measuring changes in take-up among different socioeconomic groups, 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 among excluded households. 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 leads to declines in financial health.

# 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 (Le Menestrel and Duncan, 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 identifies 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, all applicants are assigned a scheduled interview appointment, usually conducted via phone. 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 an invitation 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 marginally-approved cases receiving an average monthly benefit of $375. Although some applicants who were initially denied due to interview inflexibility 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 impact 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. 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, however, for the one-quarter of households that remained off SNAP for a full year, these losses reached over $2,000.

# 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]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.

## 4.1 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 item, such as account type (e.g., credit card), status, balance, and payment history. The sample includes 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.[6] We report effects on any delinquency, defined as late payments over 30 days past due, as well as severe delinquencies (over 90 days past due). When an account is severely delinquent, it can trigger bankruptcy proceedings. We calculate bankruptcy filings in a given quarter to capture more extreme measures of financial distress. 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.

## 4.2 Administrative SNAP Data

Our first SNAP dataset comes from the Los Angeles flexible interview experiment analyzed 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-

[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 because they are more common in our population are plausibly more responsive to unexpected changes in household resources.

used online application portal at the time. For each household, we observe experimental status, 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, and 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, we observe demographic characteristics provided in the application.

We link this data to the credit report data following the process described in Online Appendix B. This appendix provides details on match rates and case characteristics by match status. We find that the vast majority of cases have at least one household member matched in the credit report data (93 percent in Los Angeles and 89 percent in San Francisco).

Tables A.1 and A.2 explore potential bias induced by our high, yet imperfect match rate, in each of the two counties. A non-match may be due to incorrect or missing social security numbers or other identifying information used in our matching process or, alternatively, due to the absence of a credit history. As a result, a less than perfect match rate presents two potential concerns. First, 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.

Panel A 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. Panel B 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 important 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.

Finally, we restrict our sample to the roughly 85% of 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 sample consists of 52,015 applications (61,967 individuals) in Los Angeles and 22,956 recertifications (28,639 individuals) in San Francisco.

## 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.

In Los Angeles, sample members hold an average of nearly $5,000 in credit card debt. Forty-one percent have at least one delinquent credit card account, 19 percent have at least one severely delinquent account, and hold an average of 1.4 delinquent accounts. In San Francisco, indicators of financial health are somewhat stronger: average credit card balances are just under $2,000, 21 percent of sample members have at least one delinquent credit card account, 9 percent have at least one severely delinquent account, and the average number of delinquent accounts is 0.5. These more favorable baseline measures may in part reflect

timing differences, as the Los Angeles study period overlapped with the first year of the Covid-19 pandemic. 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.

Appendix Tables A.3 and A.4 present demographic characteristics of the two samples and reassess balance checks conducted in Giannella et al. (2024) and Homonoff and Somerville (2021) for our matched sample. As in Table 1, these summary statistics underscore the economic vulnerability of our study population. Applicants in Los Angeles report average household income in the past 30 days of $779, with 53 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 differences in baseline characteristics across experimental groups in both sites; these differences, while statistically significant, are economically small and are unlikely to substantially affect our estimates, especially after controlling for the baseline characteristics themselves.

# 5 Empirical Strategy

We use the following econometric models 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, $\text{Treatment}_i$ 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.3 or A.4, along with application/recertification quarter fixed effects, office fixed effects (Los Angeles), and assignment group strata (San Francisco). $\beta_k$, 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 (see Figure 1). 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, $\text{SNAP}_i$ 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).

Our main analyses make one additional sample restriction. Since the intervention we study does not change eligibility criteria, only the administration of eligibility determination processes, we focus on a high-impact sample of likely-eligible households.[7]

The coefficients $\eta_{1l}$ 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 or recertification application approved 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. However, if participating in SNAP leads to participation in other safety net programs (Cook and East, 2023, Schmidt et al., 2024, Cholli and Wu, 2025), our estimates will incorporate those spillovers as well.

At the same time, it is worth noting that marginally-denied cases may successfully reapply for the program. As a result, our IV estimates will be an underestimate of the effect of overall SNAP participation on financial health if the first stage effect on longer-term participation is smaller than effect on approval and recertification rates, as is the case in our study.

# 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

[7]We define likely-eligible as having household income below 130% of the federal poverty line at the time of application or recertification, respectively. This sample restriction results in a decrease of 18% of our Los Angeles sample and 16% of our San Francisco sample.

to three years after random assignment for our primary outcomes: credit card balances, delinquencies, credit score, and bankruptcy filings.[8]

Starting with balances, 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, credit card balances are approximately $130 lower in the treatment group. These results do not appear to be driven by changes in access to credit through a change in the number of credit card accounts (see Figure A.1), but rather by holding less debt in existing accounts.

Turning to delinquencies (Panels B through D), we find that treatment group members are significantly less likely to have a delinquent credit card account. By the end of the follow-up period, treatment members are 0.76 percentage points less likely to have a delinquency and 0.99 percentage points less likely to have a severe delinquency, i.e., over 90 days past due. We also observe decreases in the number of delinquent credit card accounts in the first year after the intervention.

Panel E 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 effects on debt and delinquencies and becoming statistically significant by the end of the study period. We do not find significant effects on new bankruptcy filings, our measure of more extreme financial distress (Panel F).

Figure 1, Panel 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 sample. As in the

[8]Table A.5, Panel A provides complementary regression estimates for all financial outcomes at the annual level.

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

Table A.6 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 $146, $1,129, and $1,982 in the three years post-randomization, respectively. This represents a 43 percent decrease relative to the pre-randomization control mean of $4,554 by the end of the study period. For context, the decreases in debt are roughly equivalent to the annual SNAP benefit for a single-person household at the time of the study.

Column 2 shows that marginally-approved cases experience significant decreases in the likelihood of having a delinquent account of 6.2, 9.1, and 11.3 percentage points in years 1, 2, and 3, respectively, relative to a baseline delinquency rate of 41 percent. The effects are somewhat larger when considering severe delinquencies: marginally-approved cases are 14.7 percentage points less likely to have a credit card account over 90 days past due by year 3, a 76 percent decrease relative to baseline. These changes in financial health also present as sizable increases in credit score of 18 points by the end of the study period. The average baseline credit score in our sample is 632, 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.[9]

## 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, Panel B, presents first stage effects of receiving an earlier recertification interview on SNAP participation by week for the year

[9]Appendix Table A.6 repeats these analyses for the full sample, i.e., includes the 18% of cases with wage income above 130% FPL and finds very similar results.

post-randomization using our matched sample of likely-eligible households. 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 3 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, credit scores, and bankruptcy filings. Here too, we find that increased flexibility in the interview process leads to significant improvements in financial well-being. As in Los Angeles, the largest treatment effects emerge toward the end of the three-year study period.

Table A.6, Panel B presents our treatment-on-the-treated estimates. We find that participants who successfully recertify for the program due to earlier interview dates are significantly less likely to hold a credit card delinquency with effects growing over the study period. By the third year, the likelihood of having any delinquent credit card accounts decreases by 11.6 percentage points (57% decrease off the control mean) and a 12.0 percentage point decrease in severe delinquencies, all but eliminating the prevalence of this negative financial outcome. We observe decreases in credit card balances of up to $1,000 (50% off the control mean), but these effects are not statistically significant. Finally, these marginally enrolled participants experience significant increases in their credit scores ranging from 17 to 22 points, though the effects are only significant in certain years. Again, we find no effect of flexible interviews on bankruptcy filings.

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

# 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 effects of two interventions—flexible enrollment interviews in Los Angeles County and earlier re-certification 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. While some advocate for the use of ordeals as a means to improve benefit targeting, our results suggest that the financial consequences for excluded households, which have received limited attention in the literature, are not trivial.

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 if only temporarily——leads to measurable credit deterioration.

Our study contributes to a growing literature on administrative burdens by demonstrating 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 to $70 in annual savings for borrowers, suggesting that the increases in credit scores we observe among marginal SNAP enrollees translate to savings of over $100 per year. Taken together, our results underscore how implementation design can amplify, or undermine, the financial protection provided by social insurance programs.

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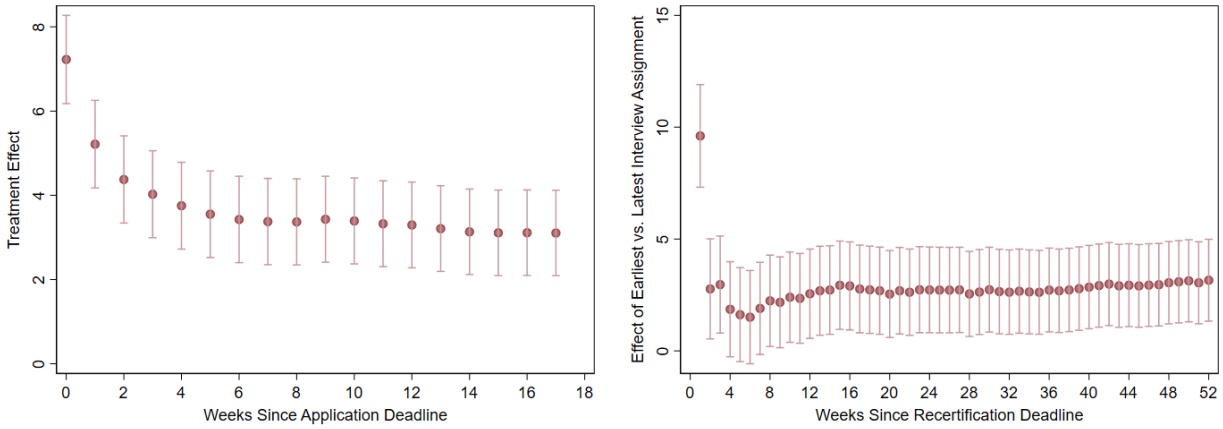
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Figure 1: First Stage: Effects of Flexible Interviews on SNAP Participation

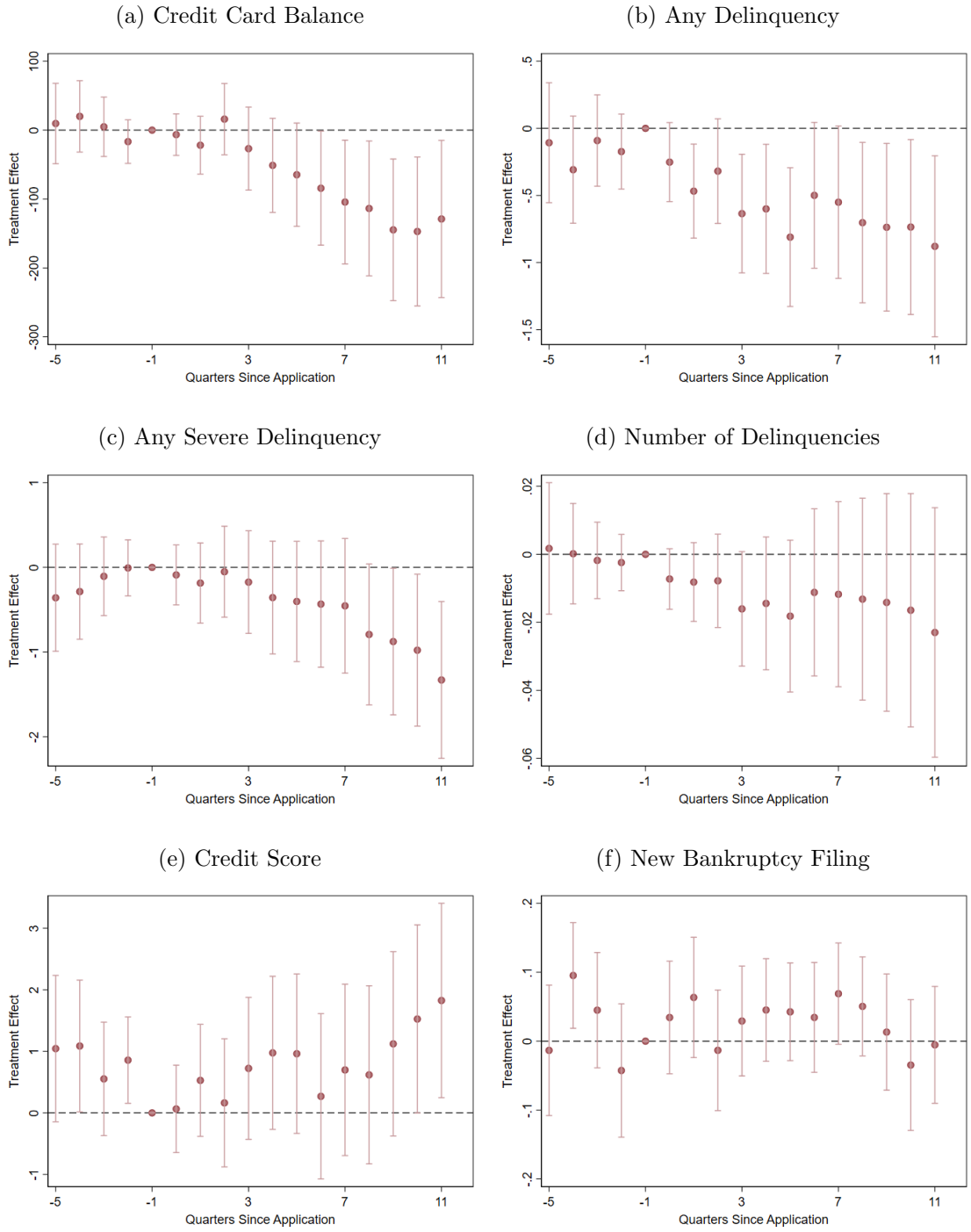
(a) Los Angeles

(b) San Francisco




Notes: This figure presents the effects of the flexible interview interventions on SNAP participation. Panel (a) presents the estimated effects of receiving 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.3 and A.4 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.

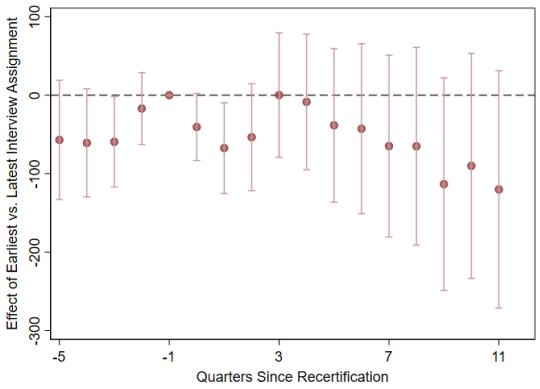
Figure 2: Effect of Interview Flexibility on Financial Well-Being by Quarter, Los Angeles



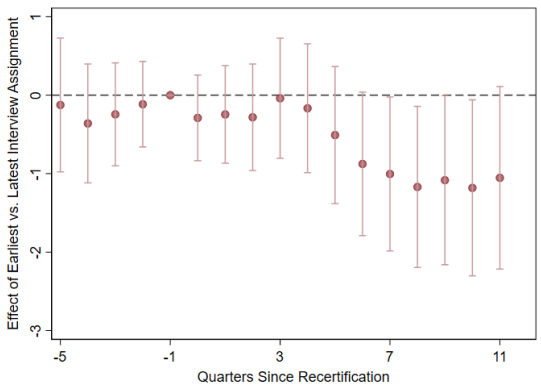

Notes: This figure presents event-study estimates of assignment to treatment communications regarding flexible interview availability in Los Angeles in Los Angeles on various financial outcomes by quarter relative to the individual's SNAP recertification deadline. Treatment effects correspond to the event study coefficients ($\hat{\beta}_k$) estimated using Equation (1). Controls include baseline demographics listed in Appendix Table A.3 as well as week, day-of-week, and SNAP office fixed effects. Outcomes in each panel are: (a) credit card balances (in dollars), (b) indicator of any credit card account 30 or more days past due, (c) indicator of any credit card account 90 or more days past due, (d) number of delinquent credit card accounts, (e) credit score, and (f) indicator of new bankruptcy filing. Coefficient estimates for indicator variables are reported in percentage points (0–100). Bars denote the 95 percent confidence interval with standard errors clustered at the recertification case level.

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

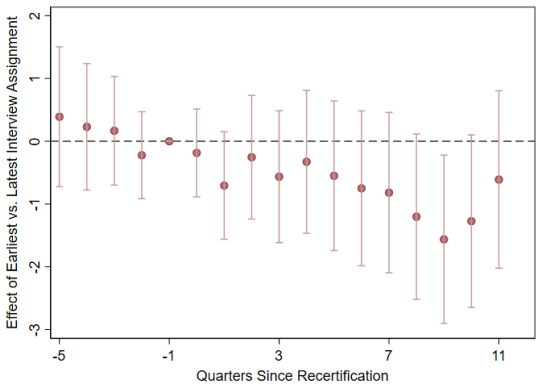
(a) Credit Card Balance



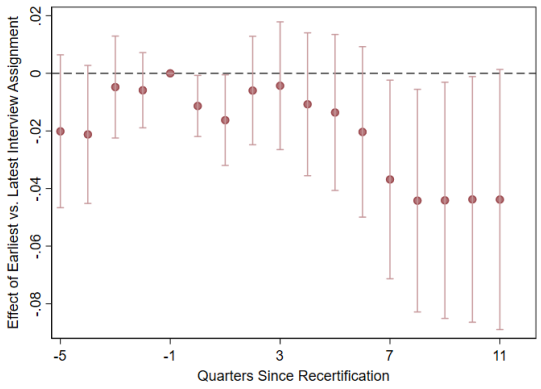

(b) Any Delinquency



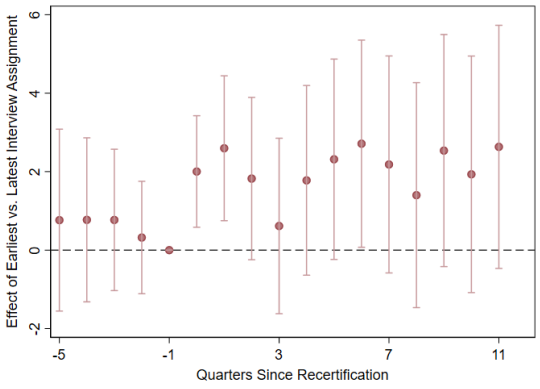

(c) Any Severe Delinquency



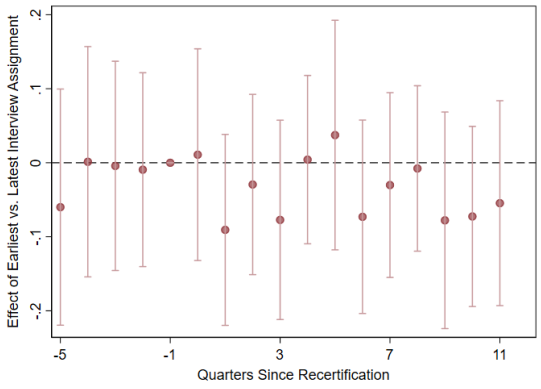

(d) Number of Delinquencies




(e) Credit Score




(f) New Bankruptcy Filing




Notes: This figure presents event-study estimates of being assigned to the earliest versus latest scheduled recertification interview in San Francisco 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.4 as well as interview scheduling strata fixed effects. Outcomes in each panel are: (a) credit card balances (in dollars), (b) indicator of any credit card account 30 or more days past due, (c) indicator of any credit card account 90 or more days past due, (d) number of delinquent credit card accounts, (e) credit score, and (f) indicator of new bankruptcy filing. Coefficient estimates for indicator variables are reported in percentage points (0–100). 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 ($) | 4,554 | 4,576 | 4,488 | 0.31 | 1,955 | 1,966 | 1,943 | 0.70 |
| Any Delinquency (30+ days) | 41.32 | 41.42 | 41.01 | 0.44 | 20.24 | 19.98 | 20.50 | 0.29 |
| Any Severe Delinquency (90+ days) | 19.35 | 19.42 | 19.13 | 0.48 | 8.88 | 8.89 | 8.88 | 0.98 |
| Number of Delinquent Accounts | 1.34 | 1.35 | 1.32 | 0.29 | 0.46 | 0.46 | 0.45 | 0.85 |
| Credit Score | 631.75 | 632.06 | 630.81 | 0.26 | 627.19 | 626.46 | 627.92 | 0.32 |
| New Bankruptcy Filing | 0.06 | 0.05 | 0.08 | 0.37 | 0.08 | 0.11 | 0.05 | 0.07 |
| Number of Observations | 50,526 | 37,973 | 12,553 | | 30,931 | 15,583 | 15,348 | |

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. Outcomes include: credit card balance, indicator of any credit card delinquency (30+ days past due), indicator of any severe credit card delinquency (90+ days past due), number of delinquent credit card accounts, credit score, and whether the individual had a new bankruptcy filing.

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

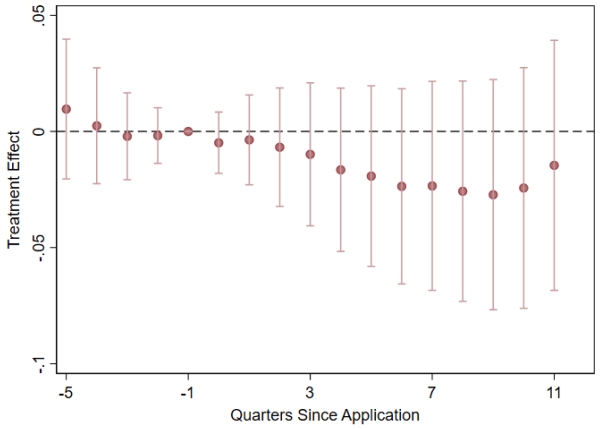
| | (1) Credit Card Balance | (2) Any Delinquency | (3) Any Severe Delinquency | (4) Number of Delinquencies | (5) Credit Score | (6) New Bankruptcy Filing |
|---|---|---|---|---|---|---|
| Panel A: Los Angeles | | | | | | |
| Year 1 | -146.15 | -6.21** | -1.85 | -0.15+ | 5.32 | 0.42 |
| | (302.37) | (2.43) | (3.24) | (0.09) | (5.99) | (0.50) |
| Year 2 | -1129.31** | -9.12** | -6.10 | -0.21 | 10.44 | 0.71 |
| | (568.17) | (3.77) | (5.11) | (0.17) | (8.90) | (0.49) |
| Year 3 | -1981.58** | -11.32** | -14.74** | -0.25 | 18.31+ | 0.09 |
| | (786.56) | (4.66) | (6.37) | (0.24) | (10.54) | (0.50) |
| N | 201,892 | 201,892 | 201,892 | 201,892 | 181,000 | 201,892 |
| Mean, Dep. Var. | 4554.28 | 41.32 | 19.35 | 1.34 | 631.75 | 0.06 |
| Panel B: San Francisco | | | | | | |
| Year 1 | -418.15 | -2.22 | -4.43 | -0.10 | 17.52** | -0.48 |
| | (290.57) | (2.91) | (4.13) | (0.08) | (8.64) | (0.59) |
| Year 2 | -400.81 | -6.62 | -6.33 | -0.21 | 22.37+ | -0.16 |
| | (504.59) | (4.44) | (5.99) | (0.15) | (12.59) | (0.59) |
| Year 3 | -1006.82 | -11.63** | -12.04+ | -0.46** | 21.16 | -0.55 |
| | (717.91) | (5.67) | (6.95) | (0.22) | (14.69) | (0.59) |
| N | 123,724 | 123,724 | 123,724 | 123,724 | 106,076 | 123,724 |
| Mean, Dep. Var. | 1954.89 | 20.24 | 8.88 | 0.46 | 627.19 | 0.08 |

Notes: 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 are provided for the quarter prior to randomization. Outcomes include: credit card balance, indicator of any credit card delinquency (30+ days past due), indicator of any severe credit card delinquency (90+ days past due), number of delinquent credit card accounts, credit score, and whether the individual had a new bankruptcy filing. Coefficient estimates for indicator variables are reported in percentage points (0–100). Outcomes are measured at the individual level. Standard errors in parentheses are clustered at the application/recertification level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

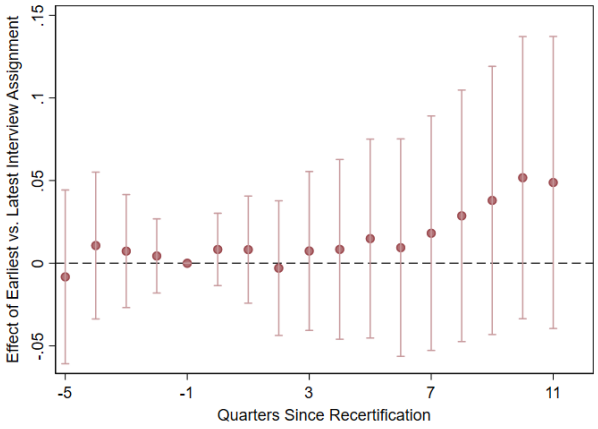
# Appendix A: Tables and Figures

Appendix Figure A.1: Effect of Interview Flexibility on Number of Open Accounts

(a) Los Angeles




(b) San Francisco




Notes: This figure presents event-study estimates of the effects of receiving communications regarding the flexible interview in Los Angeles (Panel A) and San Francisco (Panel B) on the number of open credit card accounts 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). Bars denote the 95 percent confidence interval with standard errors clustered at the recertification level.

Appendix Table A.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 A.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.

Appendix Table A.3: Baseline Characteristics, Los Angeles

| | Full Sample | Treatment | Control | p-value |
|---|---|---|---|---|
| Household size | 1.7 | 1.7 | 1.7 | 0.00 |
| Female submitter (%) | 58.6 | 58.7 | 58.4 | 0.67 |
| Submitter age | 38.3 | 38.3 | 38.1 | 0.24 |
| English speaking (%) | 86.9 | 86.9 | 87.2 | 0.43 |
| Elderly or disabled (%) | 16.5 | 16.6 | 16.1 | 0.23 |
| Receives SSI (%) | 10.0 | 10.1 | 9.6 | 0.13 |
| Stable housing (%) | 59.3 | 59.5 | 59.0 | 0.40 |
| Rent or mortgage ($) | 764.3 | 757.3 | 785.2 | 0.40 |
| Any income in past 30 days (%) | 35.6 | 35.6 | 35.5 | 0.84 |
| Income past 30 days ($) | 381.5 | 382.8 | 377.5 | 0.47 |
| Any money on hand (%) | 54.7 | 54.6 | 55.1 | 0.39 |
| Money on hand ($) | 1024.3 | 1016.7 | 1047.3 | 0.76 |
| Has non-job income (%) | 15.9 | 15.9 | 15.9 | 0.97 |
| Expedited(%) | 57.1 | 56.9 | 57.8 | 0.10 |
| Estimated eligible (%) | 92.9 | 92.9 | 93.0 | 0.83 |
| Number of cases | 42,799 | 32,106 | 10,693 | |
| Number of individuals | 50,526 | 37,973 | 12,553 | |

Notes: This table shows summary statistics of baseline characteristics for the Los Angeles sample. Data is based on self-reported measures included in the initial application. 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.4: Baseline Characteristics, San Francisco

| | Full Sample | Early Interview | Late Interview | p-value |
|---|---|---|---|---|
| Household size | 1.5 | 1.5 | 1.5 | 0.80 |
| Female submitter (%) | 46.3 | 45.9 | 46.8 | 0.17 |
| Submitter age | 43.3 | 43.2 | 43.5 | 0.12 |
| English speaking (%) | 74.8 | 74.6 | 75.0 | 0.57 |
| Any children (%) | 26.9 | 27.0 | 26.8 | 0.77 |
| US citizen (%) | 83.8 | 83.3 | 84.3 | 0.06 |
| Non-white (%) | 77.0 | 77.1 | 77.0 | 0.86 |
| Years since first on SNAP | 4.6 | 4.5 | 4.6 | 0.74 |
| Monthly benefits ($) | 228.9 | 229.4 | 228.3 | 0.58 |
| Max SNAP benefits (%) | 63.3 | 63.3 | 63.4 | 0.91 |
| Quarterly wage income ($) | 940.2 | 925.0 | 956.1 | 0.28 |
| No wage income (%) | 71.3 | 71.6 | 71.1 | 0.49 |
| Wage income below 200% FPL (%) | 99.1 | 99.1 | 99.1 | 0.76 |
| Number of cases | 19,588 | 10,013 | 9,575 | |
| Number of individuals | 24,223 | 12,387 | 11,836 | |

Notes: This table shows summary statistics of baseline characteristics for the San Francisco sample. Data is based on self-reported demographic measures included in the application, administrative SNAP participation data, and earnings information based on Unemployment Insurance wage data from the California Employment Development Department. 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.

Appendix Table A.5: Effect of Interview Flexibility on Financial Well-Being by Year (Reduced Form)

| | (1) Credit Card Balance | (2) Any Delinquency | (3) Any Severe Delinquency | (4) Number of Delinquencies | (5) Credit Score | (6) New Bankruptcy Filing |
|---|---|---|---|---|---|---|
| Panel A: Los Angeles | | | | | | |
| Year 1 | -9.85 | -0.42*** | -0.12 | -0.01+ | 0.37 | 0.03 |
| | (20.37) | (0.16) | (0.22) | (0.01) | (0.41) | (0.03) |
| Year 2 | -76.13** | -0.61** | -0.41 | -0.01 | 0.73 | 0.05 |
| | (37.79) | (0.25) | (0.34) | (0.01) | (0.61) | (0.03) |
| Year 3 | -133.59** | -0.76** | -0.99** | -0.02 | 1.27+ | 0.01 |
| | (51.90) | (0.31) | (0.42) | (0.02) | (0.72) | (0.03) |
| N | 201,892 | 201,892 | 201,892 | 201,892 | 181,000 | 201,892 |
| Mean, Dep. Var. | 4554.28 | 41.32 | 19.35 | 1.34 | 631.75 | 0.06 |
| Panel B: San Francisco | | | | | | |
| Year 1 | -40.34 | -0.21 | -0.43 | -0.01 | 1.76** | -0.05 |
| | (27.62) | (0.28) | (0.39) | (0.01) | (0.83) | (0.06) |
| Year 2 | -38.67 | -0.64 | -0.61 | -0.02 | 2.25+ | -0.02 |
| | (48.43) | (0.42) | (0.57) | (0.01) | (1.22) | (0.06) |
| Year 3 | -97.13 | -1.12** | -1.16+ | -0.04** | 2.13 | -0.05 |
| | (68.11) | (0.53) | (0.65) | (0.02) | (1.44) | (0.06) |
| N | 123,724 | 123,724 | 123,724 | 123,724 | 106,076 | 123,724 |
| Mean, Dep. Var. | 1954.89 | 20.24 | 8.88 | 0.46 | 627.19 | 0.08 |

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 (30+ days past due), indicator of any severe credit card delinquency (90+ days past due), number of delinquent credit card accounts, credit score, and whether the individual had a new bankruptcy filing. Coefficient estimates for indicator variables are reported in percentage points (0–100). 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: Effect of Marginally Enrolling in SNAP on Financial Well-Being

| | (1) Credit Card Balance | (2) Any Delinquency | (3) Any Severe Delinquency | (4) Number of Delinquencies | (5) Credit Score | (6) New Bankruptcy Filing |
|---|---|---|---|---|---|---|
| Panel A: Los Angeles | | | | | | |
| Year 1 | -235.84 | -5.09** | -1.38 | -0.16+ | 2.35 | -0.05 |
| | (317.93) | (2.46) | (3.30) | (0.09) | (6.12) | (0.57) |
| Year 2 | -1393.52** | -7.58** | -3.24 | -0.16 | 6.42 | 0.34 |
| | (599.58) | (3.80) | (5.17) | (0.18) | (9.11) | (0.55) |
| Year 3 | -2435.76*** | -10.10** | -13.00** | -0.18 | 17.25 | -0.07 |
| | (835.24) | (4.73) | (6.46) | (0.25) | (10.85) | (0.56) |
| N | 246,156 | 246,156 | 246,156 | 246,156 | 223,124 | 246,156 |
| Mean, Dep. Var. | 4777.79 | 41.37 | 19.21 | 1.37 | 634.19 | 0.09 |
| Mean, Compliers | 4972.56 | 45.90 | 18.97 | 1.38 | 639.97 | 0.24 |
| Panel B: San Francisco | | | | | | |
| Year 1 | -500.99+ | -2.31 | -5.00 | -0.12 | 14.56+ | -0.36 |
| | (262.94) | (2.76) | (3.76) | (0.08) | (7.79) | (0.52) |
| Year 2 | -309.29 | -4.66 | -3.90 | -0.21 | 19.02+ | -0.20 |
| | (459.30) | (4.16) | (5.49) | (0.14) | (11.47) | (0.49) |
| Year 3 | -986.96 | -7.75 | -10.19 | -0.41** | 16.39 | -0.48 |
| | (651.73) | (5.20) | (6.36) | (0.21) | (13.23) | (0.49) |
| N | 148,084 | 148,084 | 148,084 | 148,084 | 127,888 | 148,084 |
| Mean, Dep. Var. | 1962.95 | 20.81 | 9.22 | 0.47 | 625.91 | 0.07 |
| Mean, Compliers | 2417.62 | 22.64 | 7.10 | 0.52 | 646.40 | 0.44 |

Notes: 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 are provided for the quarter prior to randomization. Outcomes include: credit card balance, indicator of any credit card delinquency (30+ days past due), indicator of any severe credit card delinquency (90+ days past due), number of delinquent credit card accounts, credit score, and whether the individual had a new bankruptcy filing. Coefficient estimates for indicator variables are reported in percentage points (0–100). 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 B: Data Linkage

To link the two administrative SNAP datasets to the UC-CCP at the individual level, we received encrypted personally identifying information (PII) that used a common algorithm across data sets. Data partners 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. Our final samples consist of 52,015 Los Angeles cases (61,967 individuals) and 22,956 San Francisco cases (28,639 individuals).