

The Financial Consequences of Being Denied Benefit Access^{*}

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July 24, 2026

Abstract

We estimate the causal impact of the Supplemental Nutrition Assistance Program (SNAP) on financial well-being by exploiting experimental variation from two interventions that reduced administrative barriers for individuals seeking to gain or maintain SNAP access. Linking administrative SNAP records to consumer credit reports, we estimate the effect of benefits among applicants approved because of increased flexibility in mandatory intake interviews. Reducing process-related denials improves financial outcomes for marginal beneficiaries, decreasing debt and delinquencies while increasing credit scores. These results demonstrate that implementation design not only shapes access to social insurance programs, but materially affects their capacity to reduce financial distress.

JEL Codes: I38, H53, D14

Keywords: SNAP, administrative burden, financial well-being

^{*}For helpful comments and suggestions, we thank Francesca Costa, Jason Cook, Julie Cullen, Samuel Dodini, Itzik Fadlon, Eric Giannella, Jacob Goldin, and Angela Wyse and seminar participants at Better Government Lab, the Federal Reserve Bank of Dallas, Notre Dame, and the University of California-San Diego. We thank Vince Dorie and Gwen Rino at Code for America, Peri Weisberg at San Francisco Human Service Agency, and Nikta Akhavan, Erika Brown, Brett Fischer, 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.

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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, 2025), 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 participation, 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 reducing procedural barriers to SNAP participation on the financial well-being of marginally approved households. To do so, we link individual-level credit reports to administrative SNAP data from two interventions aimed at minimizing administrative burdens associated with the application interview. Prior work finds that missed interviews account for up to half of all SNAP denials (Kim et al., 2025), highlighting the importance of this particular barrier. 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 (Giannella et al., 2024a) and recertification rates in San Francisco County (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, allows us to estimate the effect of the interventions

on various measures of financial health, such as credit card debt, delinquencies, and credit scores.

To estimate the causal effect of SNAP on financial health, we instrument SNAP approval rates with randomization status in each intervention—a treatment-on-the-treated analysis. As in Giannella et al. (2024a) and Homonoff and Somerville (2021), we find a large first stage effect of reductions in administrative burdens on SNAP participation. In Los Angeles, applicants granted greater autonomy over the scheduling of their interview via randomly assigned access to an interview call center are 7.1 percentage points (14 percent) more likely to be approved. In San Francisco, participants assigned to earlier recertification interviews, which allow more time to reschedule missed interviews before the deadline, experience increases in recertification rates of 11.4 percentage points (20 percent).

In both interventions, we find that these increases in SNAP access lead to significant improvements in financial well-being. The effects emerge in the first year post-application then steadily increase over the three-year follow-up period, patterns consistent with prior work studying the effect of income shocks, such as unemployment or unexpected expenses.¹ By the end of the study period, we find that individuals in Los Angeles who are marginally approved for SNAP experience a decrease in their credit card balance of just under \$2,000 (44 percent). The number of individuals with a delinquent account decreases by 11 percentage points while severe delinquencies decrease by up to 15 percentage points by the end of our study; this represents a decline of roughly 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 12 percentage point (a 57 percent decrease) decrease in the likelihood of having any delinquency and all but eliminate severe delinquencies by the end of the study period. We observe decreases in credit card balances of up to \$1,000, but these changes in debt are not statistically

¹This literature suggests that when households cannot access social insurance programs, they turn to consumer borrowing to cover their expenses (Sullivan, 2008) with effects emerging over time as households exhaust one source of income, such as cash-on-hand, and turn to another, such as borrowing or delays in debt payments (Mello, 2025).

significant. In both counties, we find marginally significant improvements in credit scores of roughly 20 points.

Separately, we find evidence that eliminating barriers to participation in SNAP may have large effects on take-up of other social insurance programs as well. We find that over one-third of individuals in San Francisco who were approved for SNAP due to the intervention were also marginally enrolled in at least one additional means-tested program. Specifically, each marginal SNAP recertification is associated with a 37 percentage point increase in the likelihood of participating in Medicaid a year after randomization and 7 percentage point increase in the likelihood of participating in the Temporary Assistance for Needy Families (TANF) program. These findings are consistent with recent research on multiple program participation (Schmidt et al., 2024, Cholli and Wu, 2025) and highlight the importance of accounting for spillovers across social insurance programs.

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 uses descriptive or quasi-experimental methods and relies on qualitative interviews or survey data, which may suffer from small sample sizes and potential mismeasurement (Meyer et al., 2022)—by using experimental variation in SNAP participation combined with administrative data. While a subset of this literature uses similar credit bureau data to study the effects of county-level SNAP policies on financial health (Dodini et al., 2024), we are, to our knowledge,

²A 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).

the first study to link of credit bureau data with administrative SNAP microdata.

Notably, these prior studies focus on the effects of expanding or contracting program eligibility, such as changes in income eligibility thresholds or the implementation of work requirements (Han, 2016, Dodini et al., 2024). 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 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: removing administrative burdens associated with the SNAP application and recertification processes leads to meaningful improvements in the financial health of marginally approved 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 (Cunyngham, 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 an application along with supporting documentation such as verification of income, employment, expenses, residency, and immigration and student status. In addition, applicants must 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 application denials (Giannella et al., 2024a, 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 involving the administration of SNAP eligibility interviews in the state of California below. These interventions were first analyzed in Giannella et al. (2024a) and Homonoff and Somerville (2021), respectively, with the goal of estimating their effect on SNAP take-up and targeting. We provide the core details of these interventions below.

3.1 Los Angeles County Experiment

In the standard enrollment process, all applicants are assigned an interview appointment when they submit their application. These appointments are typically conducted via phone in the applicant’s preferred language within ten days of receiving the application. These interviews are scheduled without prior consultation with applicants regarding their availability, potentially resulting in conflicts with work or family obligations. If the applicant cannot be reached, the interview is marked incomplete, and the individual must contact the county to reschedule and complete an interview within 30 days of the application submission or the application is summarily denied. Denied applicants may reapply without delay, but must resubmit all components of the application anew.

In 2020, the Los Angeles Department of Public Social Services (DPSS) and Code for

America, a nonprofit organization that administers one of the primary online application portals in the state of California (GetCalFresh.org), implemented a large-scale experiment to evaluate the effect of increasing flexibility in the SNAP enrollment interview process.³ The sample included all applicants in Los Angeles County applying for SNAP between October 2020 and May of 2021 via GetCalFresh, the most common mode of application in the county at the time of the study.

Upon application submission, all applicants were assigned a pre-scheduled interview appointment following standard processes which included receiving electronic communications regarding the interview along with a mailed appointment letter.⁴ However, for the three-quarters of applications assigned to the treatment group, the standard electronic communications were modified to include an option to complete an interview at their convenience by calling a dedicated caseworker hotline.⁵ Treatment group members who did not call the hotline were contacted by the county at the time of their scheduled appointment. The intervention did not alter eligibility rules or documentation requirements—only ease of completing the required interview.

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, these interviews take place as early as the first day of the calendar month in which the

³This intervention was registered in the American Economic Association Registry for randomized control trials under trial number AEARCTR-0009728 (Giannella et al., 2024b).

⁴Mailed appointment letters included the date and time of the scheduled interview along with information on application deadlines and a number where applicants could call to reschedule their interview. Standard electronic communications sent through GetCalFresh stated “Your county should call you within 10 days for your interview. The call may come from a blocked/unlisted phone number. Make sure to pick up all calls.”

⁵These treatment messages stated “You can complete your interview by calling [hotline number]. Hours: M-F 7:30am–7:30pm, Sat 8:00am–4:30pm. The sooner you complete your interview, the sooner you may get your benefits.”

case’s certification period ends, and as late as two days prior to the end of the month. As with initial application interviews, these recertification interview appointments are scheduled without consulting the participant regarding their availability. Cases may reschedule their interview prior to their scheduled appointment, but in practice rarely do. Recipients who fail to complete their interview by the end of the month are discontinued from the program; these cases may submit a new application in order to regain access, but are not retroactively eligible for benefits between the date of recertification denial and the approval of the new application.

In San Francisco County, recertification interview dates are randomly assigned across cases and staggered throughout the month. 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. Because all interviews must be completed and paperwork submitted by the end of the calendar month, the timing of the scheduled 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 scheduling process was designed in an effort to smooth caseworker workloads throughout the month but, in doing so, it created a natural experiment by which to evaluate the effect of these administrative features of the interview process on recertification success. Specifically, the scheduling process allows researchers to evaluate the effect of earlier versus later interview assignments on continued program access since the process creates as-good-as-random assignment to the interview appointment date after controlling for month-by-language fixed effects.

4 Conceptual Framework

In this paper, we use these two interventions to study the downstream effects on credit outcomes. Our conceptual framework follows prior work studying the effects of income loss, such as unemployment (Sullivan, 2008) or unexpected expenses (Dobkin et al., 2018, Mello, 2025), on financial health. These studies suggest that many households are not fully insured against such shocks: for example, Sullivan (2008) finds that the median working-age employee has only enough liquid assets to cover three weeks of lost earnings, so must turn to other means of consumption smoothing, such as social insurance, when faced with a loss in income.

However, when households cannot access these safety net programs, they may turn instead to consumer borrowing. Hu et al. (2018) hypothesize that Medicaid access, which significantly reduces the cost of medical expenses, will reduce delinquencies and financial distress, though note that the effects on consumer borrowing are ambiguous: unsecured debt may decrease due to lower need for consumption smoothing, but may increase if individuals increase their debt-financed consumption due to a decreased need for precautionary saving.

Mello (2025) finds evidence of a hierarchy of sources of income for covering unexpected expenses. When faced with a negative income shock, households first draw down on available cash-on-hand, then turn to credit card borrowing, finally turning to delays in debt payments (i.e., incurring delinquencies). These patterns suggest that effects on credit balances and delinquencies emerge slowly over time after households spend down other sources of income. This paper also highlights the potential for negative credit outcomes to snowball over time as defaults and lower credit scores can result in higher interest rates. Consistent with this model, he finds that after incurring a traffic fine, credit scores steadily decrease with effects persisting for at least three years.

Taken together, these studies suggest that access to safety net programs may prevent the need for households to rely on consumer borrowing or delay debt payments to finance their expenses during times of economic need. Conversely, barriers to accessing social insurance

programs may lead to escalating financial distress, potentially playing out over long time horizons.

5 Data

To explore the effect of SNAP access on financial well-being, we link administrative SNAP data associated with the Los Angeles and San Francisco flexible interview interventions to a newly available data set containing the universe 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.

5.1 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 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.⁶

Our main measures of financial well-being focus on credit card balances and delinquencies. While our data include other types of tradelines, such as auto loans, mortgages, home equity lines, and student loans, our main outcomes focus on credit card accounts because they are more common in our population and are plausibly more responsive to unexpected changes in household resources. 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). Finally,

⁶Specifically, 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.

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.

5.2 Administrative SNAP Data

Our first SNAP dataset comes from the Los Angeles flexible interview experiment analyzed in Giannella et al. (2024a). The data include the universe of SNAP applications in Los Angeles County between October 2020 and May 2021 that were submitted via GetCalFresh. 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 in San Francisco between November 2014 and November 2016, as analyzed in Homonoff and Somerville (2021).⁷ For each case, we observe the recertification month, assigned interview date, and recertification outcome. The data also include the interview language and whether the interview was conducted over the phone—characteristics that, together with the recertification month, constitute the strata within which interview dates are randomly assigned—as well as demographic characteristics provided in the application. The data include subsequent reapplications, allowing us to track whether denied cases reapply or remain disenrolled. We also observe administrative quarterly wage data from the Employment Development Department (EDD) as well as TANF and Medicaid applications and approvals for one year post-randomization and in the quarter prior to randomization.

Our main analyses make an additional sample restriction. Since the intervention we study does not change eligibility criteria, only the administration of eligibility determination

⁷This sample excludes cases participating in TANF at application as the interview scheduling process differs somewhat for these cases, though a small fraction (2 percent) participate in TANF as of recertification.

processes, we focus on a high-impact sample of likely eligible households. We define a household to be likely eligible if it has household income below 130% of the federal poverty line at the time of application or recertification, respectively.⁸

5.3 Linking SNAP and Credit Report Data

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.⁹ 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.

Tables A.1 and A.2 report the match rates in each of the two counties. We find that the vast majority of households in both interventions were matched in the credit report data. In Los Angeles, 92% of the 54,148 SNAP cases had at least one individual matched to the UC-CCP data. In San Francisco, 88% of the 26,482 cases had at least one individual matched. These match rates provide some of the first evidence demonstrating the representation of SNAP households in credit bureau data.

These high match rates are encouraging for our study design; however, any less-than-perfect match rate allows for the potential for bias in our experimental estimates. A non-match may be due to incorrect or missing social security numbers or other identifying in-

⁸This sample restriction results in a decrease of 18% of our Los Angeles sample and 16% of our San Francisco sample.

⁹Data 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.

formation 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 of Tables A.1 and A.2 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 cases that do not match are substantially more vulnerable than the cases that match across a variety of economic characteristics. For example, unmatched households are less more likely to have any income and are more likely to be a non-English speaking household. 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 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 cases 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 42,796 cases (50,522 individuals) in Los Angeles and 19,594 cases (24,223 individuals) in San Francisco.

5.4 Summary Statistics

Table 1 presents baseline measures of financial well-being for our two interventions 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 roughly \$4,500 in credit card debt in the quarter prior to randomization. Forty-one percent have at least one delinquent credit card account, with an average of 1.3 delinquent accounts, and 19 percent have at least one severely delinquent account. In San Francisco, indicators of financial health are somewhat stronger: average credit card balances are just under \$2,000, 20 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 differences in timing of the interventions, as the Los Angeles study period overlapped with the first year of the Covid-19 pandemic. At the same time, average credit scores are modestly lower in San Francisco—627 versus 632 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 Figures A.1 and A.2 present the trends in these same outcomes for the five quarters prior to the intervention and three years post-intervention. In both counties, we see a sizable increase in balances, delinquencies, and credit scores over the study period. However, only in Los Angeles do we observe a trend break at randomization, i.e., at the initial SNAP application quarter. Specifically, prior to applying for SNAP, trends in credit card balances and severe delinquencies were somewhat flat, then increased sharply in the quarters following application. Similarly, credit scores were increasing prior to randomization, then plateaued shortly after the quarter of SNAP application. These findings are consistent with a model in which individuals apply for benefits at a time of financial distress, similar to prior work demonstrating evidence of negative earnings shocks leading individuals to apply for assistance (Ganong and Liebman, 2018, East and Simon, 2024). In contrast, in San Francisco, we see no trend break at randomization, consistent with the fact that recertification timing (which usually occurs annually following an initial application) is unlikely to be correlated with

trends in the household’s economic well-being.

Appendix Tables A.3 and A.4 present demographic characteristics of the two samples and reassess balance checks conducted in Giannella et al. (2024a) 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 \$382, with 64 percent reporting no income at all. San Francisco recertification cases report average quarterly income of \$940 (equivalent to \$313 per month), with 71 percent reporting no income in the quarter prior to recertification. In terms of balance, we find one significant difference in baseline characteristics across experimental groups in each site; these differences, while statistically significant, are economically small and are unlikely to substantially affect our estimates, especially after controlling for the baseline characteristics themselves.¹⁰

6 Empirical Strategy

We use the following econometric model to estimate the causal effect of SNAP access on financial health. Our main analyses estimate an instrumental variables (IV) regression in which we instrument SNAP approval with random assignment to flexible interview access or earlier assigned interview dates, depending on the intervention. These analyses provide treatment-on-the treated estimates measuring the effect of being marginally approved for SNAP due to interview flexibility on various credit report outcomes using the following joint specification:

$$SNAP_i = \beta_0 + \sum_{k=-5}^{11} \mathbb{1}[q_{it} = k] (\beta_{1k} \cdot \text{Treatment}_i + \beta_{2k}) + \beta_3 \cdot \text{Treatment}_i + \beta_4 X_i + \varepsilon_{it} \quad (1)$$

¹⁰Specifically, we find that, in Los Angeles, household size is modestly larger among treatment group members (1.75 versus 1.70) and that, in San Francisco, cases with early interviews are less likely to be US citizens than cases with later interviews (83.3 versus 84.3 percent).

$$Y_{it} = \alpha_0 + \sum_{k=-5}^{11} \mathbb{1}[q_{it} = k](\alpha_{1k} \cdot \text{SNAP}_i + \alpha_{2k}) + \alpha_3 \cdot \text{SNAP}_i + \alpha_4 X_i + \varepsilon_{it} \quad (2)$$

Y_{it} denotes individual i 's credit report outcome in calendar quarter t , our outcome of interest. SNAP_i is an indicator for whether individual i 's initial application was approved in Los Angeles and whether their recertification application was approved in San Francisco. 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. The vector X_i includes the baseline demographic characteristics summarized in Table A.3 or A.4, along with application quarter fixed effects, office fixed effects (Los Angeles), and assignment group strata (San Francisco). $\mathbb{1}[q_{it} = k]$ is an indicator for event time k , defined relative to the quarter of randomization (event time 0). We omit the quarter prior to randomization as the reference period. We also conduct analyses at the yearly level in which Y_{it} is individual i 's financial outcome in post-randomization event-year y averaged across quarters.

The coefficients α_{1k} 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. In section 8 we explore conditions under which our estimates can more broadly be interpreted as the effect of SNAP on financial health.

7 Results

7.1 Effect of Flexible Interview Interventions on SNAP Approval

Table 2 presents estimates of the effect of the interventions on SNAP participation in the several months following randomization—the “first stage.” The analyses mirror those provided in Giannella et al. (2024a) and Homonoff and Somerville (2021), but for our matched

sample of likely eligible households.¹¹

Panel A presents the effect of the Los Angeles flexible interview intervention on whether the initial application was approved by the 30-day deadline (Column 1) and on SNAP participation as of the end of the follow-up period, i.e., five months post-randomization or 120 days after the application deadline (Column 2). We find that treatment group assignment increased approval rates by 7.1 percentage points. This treatment effect decreases over time, due to differential reapplication rates among treatment and control group members after a denial. However, we still observe a 3.1 percentage point increase in SNAP participation at the end of the follow-up period.

Panel B estimates the effect of scheduled interview timing on recertification success and longer-term SNAP participation in San Francisco. Specifically, the regression estimates the effect of a one-day change in the scheduling of a case’s recertification interview using a linear regression model, then multiplies the coefficient by 28 days to measure the effect of moving from the latest to earliest assigned interview date in the month. We find that cases assigned to the earliest interview appointments are 11.4 percentage points more likely to successfully recertify for SNAP. Here too, we find that control group members successfully reapply for the program over the following year at higher rates than the control group, reducing the treatment effect over time. However, we still observe significant effects of earlier interview dates a year after recertification: cases assigned to the earliest interviews are 2.2 percentage points more likely to participate in SNAP one year post-recertification than cases assigned to the latest interview date.

Figure 1 provides a complementary analysis which estimates the effect of each intervention on SNAP participation by week. For both interventions, we find that the treatment effects decrease substantially in the month following the application deadline, then remain constant through the end of the follow-up period. Taken together, these results suggest that administrative flexibility in the form of client-initiated interviews in Los Angeles and earlier

¹¹Since approvals are decided at the application level (initial or recertification) rather than at the individual level, these analyses are conducted at the application level with standard errors clustered accordingly.

interviews in San Francisco leads to large increases in short-term benefit gain and smaller, yet still significant, increases in longer-term program participation.

Columns 4 and 5 of Table 2 estimate the cumulative effect of being marginally approved for SNAP on benefit receipt in the year post-randomization.¹² Specifically, these analyses use the same model as in Section 6 in which we instrument for SNAP approval with random assignment status to estimate the effect of marginal approvals on months of benefit receipt and benefit dollars received. In Los Angeles, we find that cases that are marginally approved for SNAP due to the intervention receive an additional 6.7 months of benefits in the post-randomization year for an average of \$2,754 benefit dollars. These estimates are smaller in San Francisco: cases that marginally recertified due to earlier scheduled interviews receive an additional 2.2 months of benefits for an average of \$521 in the year following randomization. These differences are due to differences in the rates at which control group members successfully reapply as well as differences in generosity of SNAP benefits across the two study periods.¹³

7.2 Complier Characteristics

Tables A.5 and A.6 explore the characteristics of the compliers across the two interventions. The tables provide estimates of the baseline characteristics listed in Tables A.3 and A.4, respectively, for individuals who were approved for SNAP due to the treatment (compliers) in Column 1 and individuals in the control group who were not approved for SNAP (potential compliers) in Column 2. A few differences across the two groups stand out: compliers in San Francisco are twice as likely to have children and significantly less likely to speak English or

¹²Recall that while we observe SNAP participation data for the full year in San Francisco, we only observe participation data in Los Angeles for five months of follow-up. However, Figure 1 shows that the treatment effects are largely unchanged after the first month following the application deadline. Therefore, for this analysis, we assume that SNAP participation remains constant from the end of our follow-up period through the end of the year.

¹³For example, the maximum benefits for a family of four at the end of the Los Angeles intervention was \$835, while in San Francisco it was \$649. Additionally, all households in Los Angeles received the statutory maximum benefit during our study period due to temporary benefit expansions provided through the Coronavirus Aid, Relief, and Economic Security (CARES) Act.

be a US citizen, while compliers in Los Angeles are somewhat more likely to be elderly or disabled. In terms of need, compliers in both counties are less likely to have any income at the time of application, but are also less likely to qualify for expedited (emergency) benefits or for the statutory maximum benefit amount.

This comparison suggests mixed evidence regarding relative need of the compliers versus potential compliers. Nevertheless, Column 1 suggests a very high level of need among this group of applicants who would have been denied were it not for the interventions we study. The vast majority of these marginally approved cases have no earnings or income at the time of application and close to half are eligible for expedited benefits or the statutory maximum benefit amount, i.e., households determined to have the highest need.¹⁴

7.3 Effect of SNAP Approval on Financial Health

7.3.1 Los Angeles

Figure 2 presents our treatment-on-the-treated estimates of the effect of SNAP approval on financial well-being 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, delinquencies, and credit score.

Starting with credit card balances, Panel A shows a gradual but persistent decline in credit card debt among individuals who were marginally approved for SNAP. While we find no evidence of differential trends prior to the start of the intervention, we observe relative decreases in debt balances for marginally approved individuals starting around the third quarter post-randomization. These effects grow steadily over our follow-up period, suggesting that applicants whose SNAP application was not approved may be accumulating debt over time. 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.3), but rather by

¹⁴For example, expedited benefit status is awarded to individuals with less than \$150 in gross monthly income and less than \$100 in liquid resources.

holding less debt in existing accounts.

Turning to delinquencies (Panels B through D), we find that individuals who were approved for SNAP due to changes in the interview process are significantly less likely to have a delinquent credit card account. The effects on having any delinquency emerge shortly after randomization, then grow throughout the follow-up period, similar to the effects on credit card balances. The effects on severe delinquencies (defined as 90 or more days past due) emerge more slowly with treatment effects becoming significant only in the third year of follow-up.

Panel E presents the effect of the intervention on credit scores. We find that a marginal SNAP approval 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.

Table 3, Panel A reports these same estimates at the annual level, averaging outcomes across quarter. We find that the marginal SNAP approval results in a reduction in credit card balances of \$143, \$1,131, and \$1,945 in the three years post-randomization, respectively (Column 1). 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 decrease in debt in year three is roughly 70% of the annual benefit dollar gains for marginally approved cases estimated in Table 2.

Column 2 shows that marginally approved cases experience significant decreases in the likelihood of having a delinquent account of 6.2, 9.0, and 11.4 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 14.6 percentage points less likely to have a credit card account over 90 days past due by the end of the study period, a 75 percent decrease relative to baseline. These changes in financial health also present as a sizable increase in credit score, 19 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 quarters of the study period, have the potential to move recipients out of the subprime range.

7.3.2 San Francisco

Figure 3 presents the event study analyses for the San Francisco experiment exploring the effect of maintaining SNAP access at the time of recertification due to receiving earlier interview appointments. Here too, we find that increased flexibility in the interview process leads to significant improvements in financial well-being with effects growing over the three-year follow-up period.

Table 3, Panel B presents our treatment-on-the-treated estimates at the annual level. We find that SNAP participants who successfully recertify have lower credit card balances, consistent with our findings on the effects of interview flexibility in the initial application in Los Angeles. These effects are smaller in magnitude (just over \$1,000 in year 3) than in Los Angeles and not statistically significant, however, the baseline means in San Francisco are also much lower than in Los Angeles. As a result, when considering the magnitude of these effects as a percent of the pre-period means, we find very similar effects across the two interventions: cases that are marginally approved for SNAP saw a decrease in the debt balances of roughly one half at the end of the follow-up period, albeit imprecisely measured in San Francisco.

Columns 2 through 4 estimate the effect of recertification approval on the prevalence and number of delinquent credit card accounts. 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. By the third year, the likelihood of having any delinquent credit card accounts decreases by 11.5 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 marker of financial distress. We also observe a significant decrease in the number of delinquent accounts.

Finally, Column 5 shows that marginally approved participants experience significant increases in their credit scores ranging from 17 to 22 points, though the effects are only significant in certain years.

7.3.3 Comparison of Interventions

The two interventions we study have many similarities—both decrease administrative burdens associated with the SNAP application process by increasing the flexibility of the mandatory interview—but also several key differences.

First, the way in which interview flexibility is operationalized differs across the two interventions. In Los Angeles, treated applicants receive access to a caseworker hotline, allowing them to complete an unscheduled interview at a time of their choosing; in San Francisco, treated participants are assigned interview appointments further in advance of the recertification deadline. While similar in spirit, the specific administrative barriers are somewhat different and, therefore, the two interventions may result in different treatment effects or complier populations.

Second, the two interventions occur at different stages of the eligibility determination process. The intervention in Los Angeles occurs during the initial application process, i.e., the study population is not currently participating in SNAP, while the San Francisco intervention takes place during recertification, targeting a population that has received SNAP for at least one year, possibly much longer. Prior research studying the effect of caseworker automation, which led to significant decreases in approval rates at both initial application and recertification, finds differences in the targeting properties of this administrative change: less needy individuals are screened out at the initial application stage, while needier individuals are screened out at recertification (Wu and Meyer, 2023). This suggests that there may be differences in downstream effects of SNAP access for those marginally enrolled across the two stages. While we study a different administrative change with potentially different targeting properties, we find no evidence that the effect of being marginally approved differs

across the two process stages.

Third, the economic context in which each of these studies takes place differs substantially. The San Francisco experiment takes place from 2014 to 2016 during a period of declining unemployment rates and economic recovery, while the Los Angeles experiment takes place roughly a year after the onset of the Covid-19 pandemic. These differences can be seen clearly in the baseline levels of financial health: balances and delinquencies are twice as high among the Los Angeles sample versus the San Francisco sample.

Despite these differences, we find very similar effects similar across the two interventions, especially in the longer run. Both interventions reduce outstanding balances by approximately 50 percent, lower delinquency rates by roughly one-third, and nearly eliminate severe delinquencies. Indeed, the substantial contextual differences between the interventions make the similarity of the results especially noteworthy. The consistency of the estimates suggests that the observed effects are neither specific to the Covid-19 period nor confined to participation changes arising at a particular stage of the eligibility determination process.

Taken together, the findings from our two experimental settings provide complementary evidence that administrative burdens associated with the interview requirement have significant negative downstream effects on financial well-being both at initial application and recertification.

7.3.4 Robustness Checks

We include several additional analyses for robustness and completeness. First, Appendix Table A.7 provides complementary reduced form analyses, mirroring the outcomes in Table 3, that estimate the direct effect of receiving communications regarding availability of flexible interviews and receiving earlier scheduled interviews.

Second, while we observe balances and delinquencies for all person-quarters in our data, a small fraction of person-quarters are missing a credit score. This is due to the fact that

scores are only generated when the credit bureau has a sufficiently “scoreable” file.¹⁵ As a result, the samples used in our our main results table are not consistent across all outcomes.¹⁶ Appendix Table A.8 repeats our main analyses limiting the sample to individuals that have a credit score in all sample quarters. We find similar, if not larger, effects for this always-scored sample.

Third, we explore two additional loan types beyond credit cards: unsecured loans and auto loans. Appendix Table A.9 considers unsecured loans which include unsecured personal and installment loans, but excludes credit cards, student loans, and secured forms of borrowing such as auto loans and mortgages.¹⁷ The amount of non-credit card unsecured borrowing is much lower than the credit card balances we observe (roughly 5 to 10%), so many of our estimates focusing on other unsecured debt categories are imprecisely estimated. However, we observe suggestive evidence that the patterns we observe for credit card accounts are similar for other types of unsecured loans as well. When combining all unsecured loans including credit cards, we find similar, yet somewhat larger effects on loan balances in both counties than for credit card debt alone with the year-three effects in San Francisco becoming significant. The effect on 30-day delinquencies increases in San Francisco, yet decreases in Los Angeles and the effects on severe delinquencies remain similar in both counties.

Appendix Table A.10 considers effects on having an outstanding auto loan balance. Column 1 finds that the marginal SNAP approval increases the likelihood that individuals holding an auto loan at baseline fully pay off their loan by the end of the follow-up period, though these estimates are somewhat imprecisely measured. We then consider the effect of SNAP access on the likelihood of obtaining a new auto loan among individuals without an existing loan at baseline, a potential proxy for car ownership, an alternative measure of

¹⁵A credit file is considered unscorable if it contains insufficient payment history or if the record has become stale due to lack of recent payment information (Brevoort et al., 2016). In our data, we find evidence of both possibilities as some individuals become scorable over time while others transition from scored to unscorable.

¹⁶Specifically, Column 5 of Table 3 excludes the 9 and 13 percent of the sample that is missing a credit score in any quarter of our study period in the two counties, respectively.

¹⁷We exclude student debt, following Dodini et al. (2024), as these loans are for a specific expense that may be less likely to be affected by SNAP access.

financial health.¹⁸ Column 2 shows mixed, somewhat noisy evidence on the effect of SNAP on this measure of economic self-sufficiency.

8 Interpreting Results

Our main regression models instrument for the SNAP application approval rate (initial or recertification) with random assignment status, meaning that our estimates provide the effect of being marginally approved for SNAP on financial well-being. However, if the interventions we study affect financial health through channels other than the likelihood of approval, our estimates will be biased due to an exclusion restriction violation. Our interventions alter only the scheduling of interviews, holding all other eligibility criteria constant; however, we cannot rule out the possibility that completing the caseworker interview itself has an independent effect on the credit outcomes we study. For example, discussing one’s finances may have a direct effect on credit outcomes among those who are interviewed, but not approved.¹⁹ However, given their narrow scope, we believe it is unlikely that the interventions had an effect on the credit outcomes we study other than through SNAP participation.

At the same time, there are several reasons why the coefficients we present may differ from the direct effect of SNAP participation on financial health. First, marginally denied cases may subsequently successfully reapply for the program (see Table 2). 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.

Second, it is possible that households may change other behaviors in response to being approved (or not) for SNAP. For example, an applicant may enter or exit the labor force in response to enrolling in SNAP. Similarly, participating in SNAP may lead to participation

¹⁸While we do not observe car ownership, roughly two-thirds of individuals who purchase a vehicle take out a loan to finance that purchase (Board of Governors of the Federal Reserve System, 2016).

¹⁹Note that caseworkers collect information on a household’s income and expenses, but not debts or delinquencies, i.e., not the measures of financial well-being that we analyze. Nevertheless, these conversations may cause financial health to be top of mind more generally.

in other safety net programs (Cook and East, 2023, Schmidt et al., 2024, Cholli and Wu, 2025). While neither of these scenarios constitutes an exclusion restriction violation—the behavioral change still operates in response to being approved for SNAP—understanding these potential responses is important for interpretation of the coefficients as our estimates will incorporate the effects of these responses as well.

Table 4 explores the effect of being marginally approved for SNAP on labor supply and participation in other safety net programs in San Francisco using administrative quarterly wage data and Medicaid and TANF application data.²⁰ Column 1 finds mostly positive, but imprecisely estimated effects of SNAP approval on having any wage earnings in the year following the intervention. Column 2 estimates the effect on quarterly earnings and finds similarly noisy estimates with the sign of the coefficient fluctuating by quarter.

In contrast, Columns 3 and 4 show significant positive effects of SNAP approvals on participation in other means-tested programs. Individuals who recertify for SNAP due to earlier interviews are 37 percentage points more likely to participate in Medicaid a year after randomization. In other words, among marginal SNAP approvals, roughly one-third marginally enroll in Medicaid as well. These effects are smaller, but still statistically significant for TANF, consistent with lower income eligibility thresholds for TANF. Individuals marginally approved for SNAP are 7 percentage points more likely to be enrolled in TANF one year post-randomization.

These findings have two important implications. First, they show that changes in the administration of one safety net program may have meaningful spillovers on take-up of other programs as well. Second, they suggest that the estimates we present should be interpreted as the effect of participation in the broader social safety net on financial health.

²⁰These analyses are restricted to our San Francisco sample as we do not have data on labor force or safety net participation for our Los Angeles sample other than at the time of application.

9 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 and earlier recertification interview appointments in San Francisco—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.

We find effects that are qualitatively consistent with prior research on the effects of expanding or contracting SNAP eligibility (Han, 2016, Dodini et al., 2024); however, that need not have been the case. For example, households that exit SNAP due to an inability to satisfy work requirements may experience very different downstream consequences from those who exit SNAP due to administrative hassles. To this point, Gray et al. (2023) find that SNAP work requirements have a disproportionately larger impact on SNAP exits among the most vulnerable households, including homeless participants and households with no income. This suggests that the households exiting SNAP in response to changes in work requirements, such as those studied in Dodini et al. (2024), may be precisely the households most likely to experience financial distress in response to a loss in benefits.

In contrast, our study contributes to a growing literature on administrative burdens by demonstrating that procedural features of benefit delivery can also generate downstream consequences for household financial stability. 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 strengthen, or undermine, the financial protection provided by social insurance programs.

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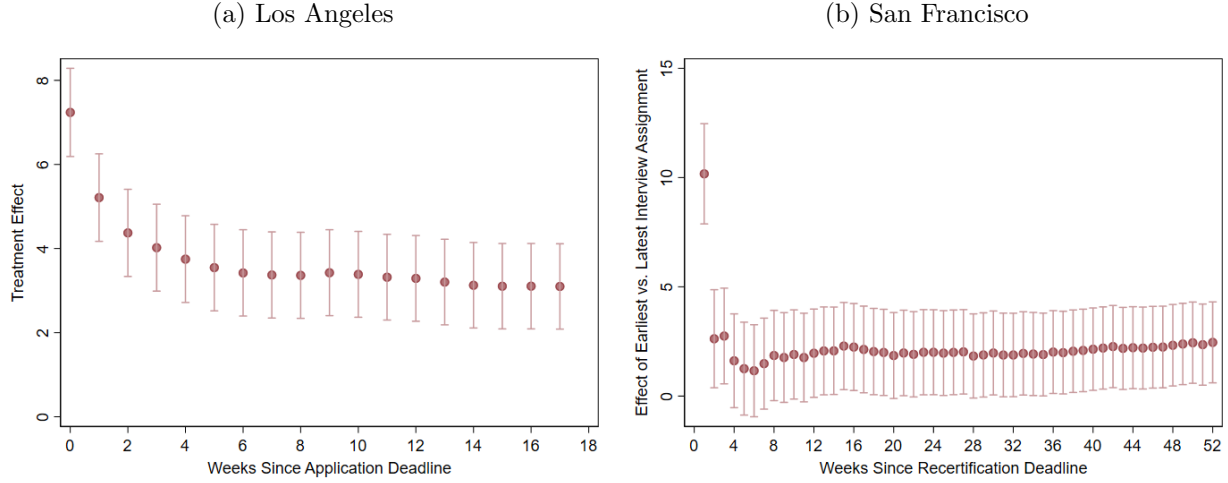
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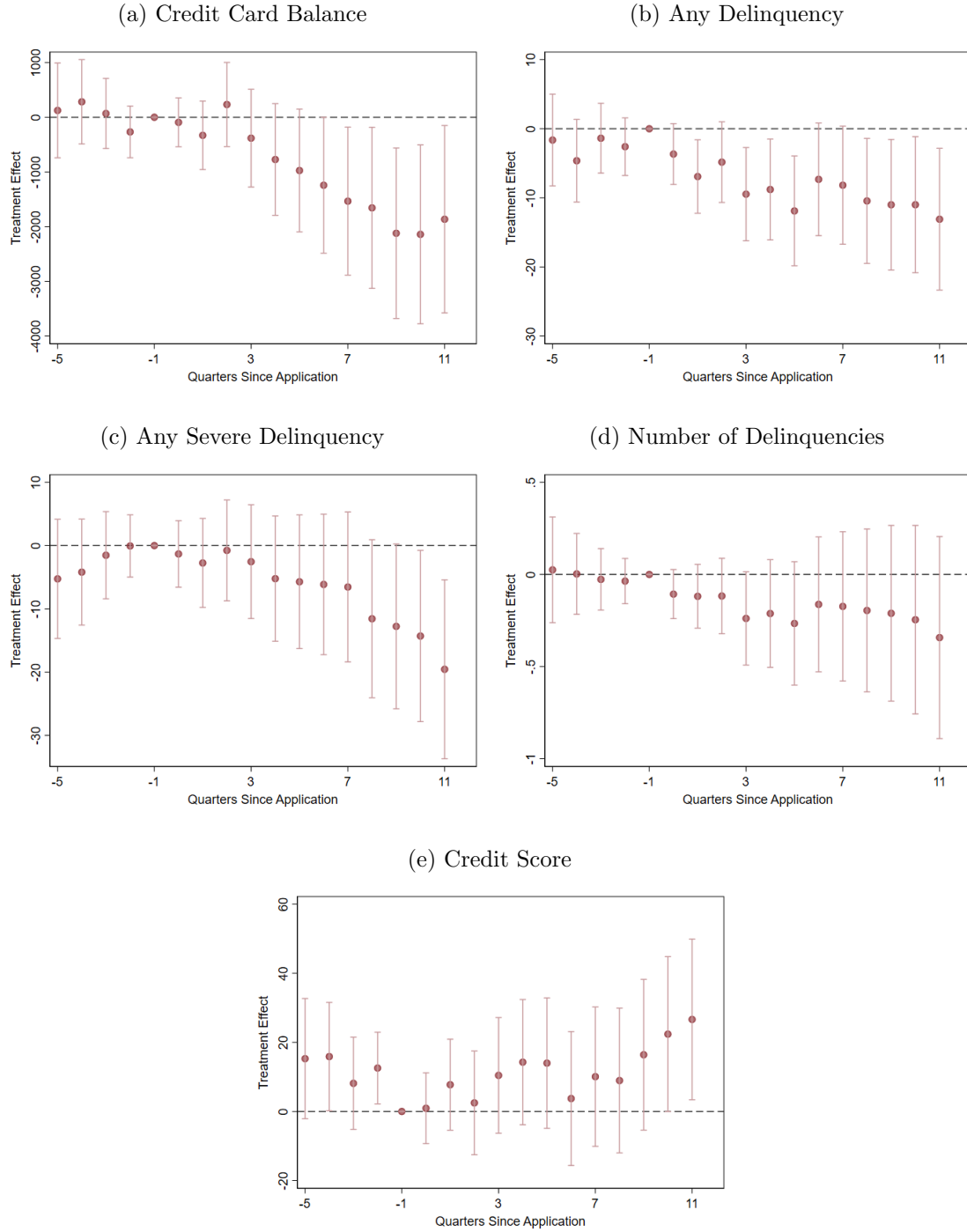
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Figure 1: First Stage: Effects of Flexible Interviews on SNAP Participation



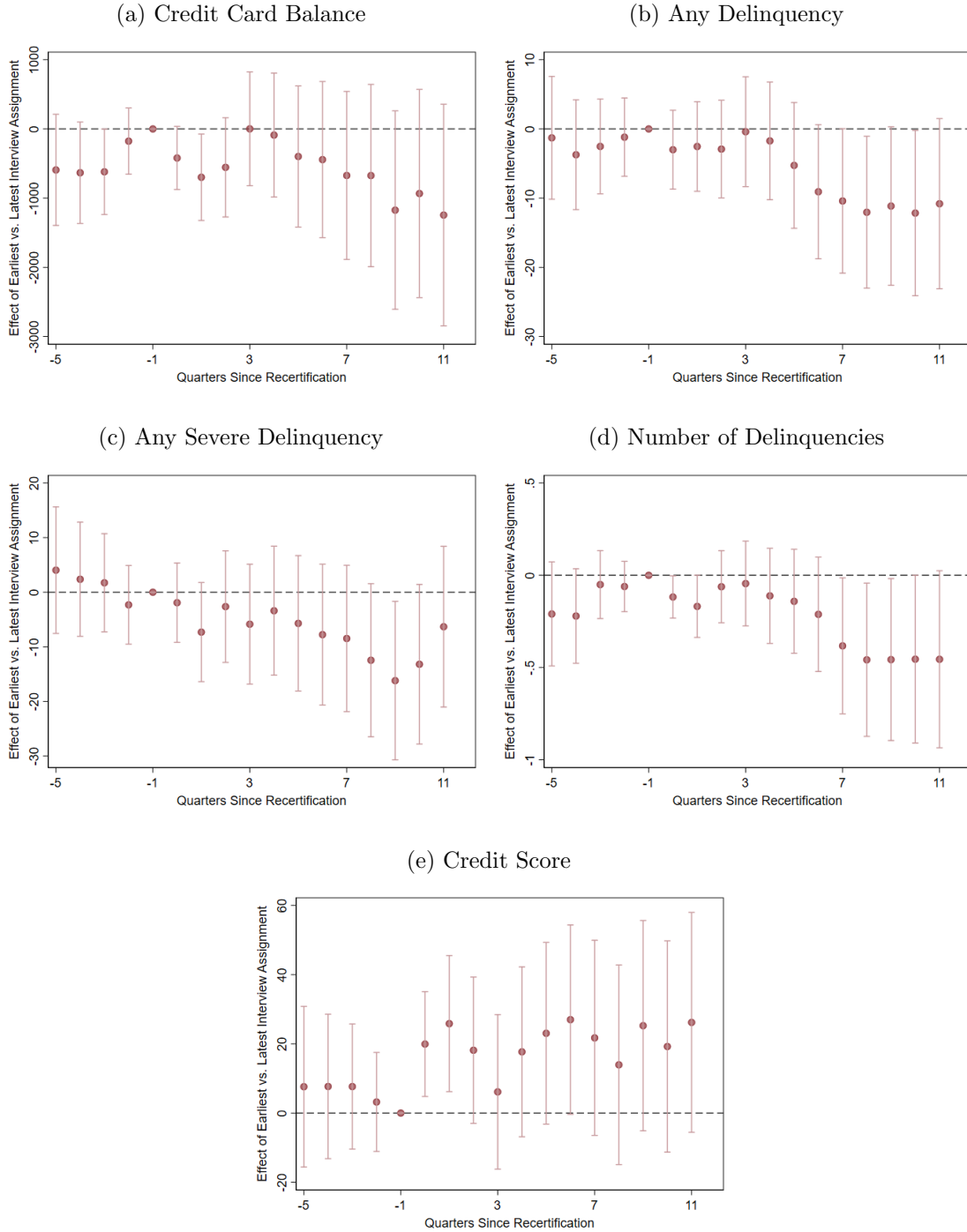
Notes: This figure presents the effects of the flexible interview interventions on SNAP participation. Panel (a) presents the estimated effects of receiving communications regarding flexible interview availability in Los Angeles. Panel (b) presents the estimated effects of being assigned to the earliest 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 SNAP Approval on Financial Well-Being by Quarter, Los Angeles



Notes: This figure presents treatment-on-the-treated event-study estimates of the effect of marginal SNAP approval on financial outcomes by quarter relative to the individual's SNAP application. SNAP approval and its interactions with event time are instrumented using assignment to treatment communications regarding flexible interview availability. 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 delinquency (30+ days past due), (c) indicator of any severe credit card delinquency (90+ days past due), (d) number of delinquent credit card accounts, and (e) credit score. 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 application level.

Figure 3: Effect of SNAP Approval on Financial Well-Being by Quarter, San Francisco



Notes: This figure presents treatment-on-the-treated event-study estimates of the effect of marginal SNAP recertification on financial outcomes by quarter relative to the individual's SNAP recertification deadline. Recertification and its interactions with event time are instrumented using the randomly assigned recertification interview date. 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 delinquency (30+ days past due), (c) indicator of any severe credit card delinquency (90+ days past due), (d) number of delinquent credit card accounts, and (e) credit score. 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: Summary Statistics: 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 (\$)	4,555	4,576	4,489	0.31	1,955	1,966	1,943	0.70
Any Delinquency (30+ days)	41.32	41.42	41.02	0.45	20.24	19.98	20.50	0.29
Any Severe Delinquency (90+ days)	19.35	19.42	19.13	0.49	8.88	8.89	8.88	0.98
No. Delinquent Accounts	1.34	1.35	1.32	0.29	0.46	0.46	0.45	0.85
Credit Score	631.75	632.06	630.80	0.26	627.19	626.46	627.92	0.32
Missing Credit Score	3.73	3.78	3.59	0.31	3.98	3.85	4.12	0.25
Number of Observations	50,522	37,972	12,550		30,931	15,583	15,348	

Notes: This table reports baseline credit report outcomes measured in the quarter prior to randomization, overall and by experimental group (treatment versus control in Los Angeles and early versus late interviews in San Francisco). 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 an indicator for a missing credit score. p-values are from tests of equality of means across experimental groups within each county.

Table 2: First Stage: Effects of Flexible Interviews on SNAP Participation

	(1) Approval Rate	(2) On SNAP by Day 120	(3) On SNAP at 12 Months	(4) Enrollment Months IV	(5) Annual Benefits IV
Panel A: Los Angeles					
Treatment Group	7.08*** (0.57)	3.07*** (0.57)			
Approved by day 30				6.65*** (0.57)	2753.95*** (391.61)
Number of Observations	50,513	50,513		50,513	50,513
Control Mean	50.65	63.20		7.18	2761.69
F-Stat	151.92	28.67			
Panel B: San Francisco					
Interview Day	11.37*** (1.27)	2.00** (1.02)	2.21** (0.96)		
Recert Success				2.19** (0.91)	520.97** (258.15)
Number of Observations	30,924	30,924	30,924	30,924	30,924
Mean, Dep. Var.	55.56	79.09	82.73	9.61	2477.90

Notes: This table presents first-stage estimates of the effects of the interview flexibility interventions on SNAP participation and benefits. 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. Outcomes in Columns 1 through 3 are indicators for initial/recertification application approval, for ever receiving SNAP within 120 days of the application deadline, and for ever receiving SNAP in the post-application year (San Francisco only). Columns 4 and 5 instrument SNAP approval rates with experimental status to estimate the effect of being marginally approved for SNAP on the number of months receiving SNAP and the benefit dollars received in the post-randomization year, respectively. Coefficient estimates for indicator variables are reported in percentage points (0–100). Outcomes are measured at the case level. Standard errors in parentheses are clustered at the application level. $^+p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Effect of Being Marginally Approved for SNAP on Financial Well-Being

	(1) Credit Card Balance	(2) Any Delinquency	(3) Any Severe Delinquency	(4) Number of Delinquencies	(5) Credit Score
Panel A: Los Angeles					
Year 1	-143.06 (302.13)	-6.22** (2.43)	-1.84 (3.24)	-0.15 ⁺ (0.09)	5.40 (6.00)
Year 2	-1130.59** (567.87)	-9.04** (3.77)	-5.91 (5.11)	-0.20 (0.17)	10.52 (8.93)
Year 3	-1945.35** (784.10)	-11.38** (4.67)	-14.55** (6.37)	-0.25 (0.24)	18.60 ⁺ (10.58)
N	202,052	202,052	202,052	202,052	181,156
Mean, Dep. Var.	4554.75	41.32	19.35	1.34	635.95
Panel B: San Francisco					
Year 1	-418.15 (290.57)	-2.22 (2.91)	-4.43 (4.13)	-0.10 (0.08)	17.52** (8.64)
Year 2	-400.81 (504.59)	-6.62 (4.44)	-6.33 (5.99)	-0.21 (0.15)	22.37 ⁺ (12.59)
Year 3	-1006.45 (717.90)	-11.54** (5.66)	-12.04 ⁺ (6.95)	-0.46** (0.22)	21.16 (14.69)
N	123,724	123,724	123,724	123,724	106,076
Mean, Dep. Var.	1954.85	20.24	8.88	0.46	635.88

Notes: This table presents estimates of the effects of being marginally approved for 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 (1) and (2). 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 post-randomization. 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, and credit score. 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. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

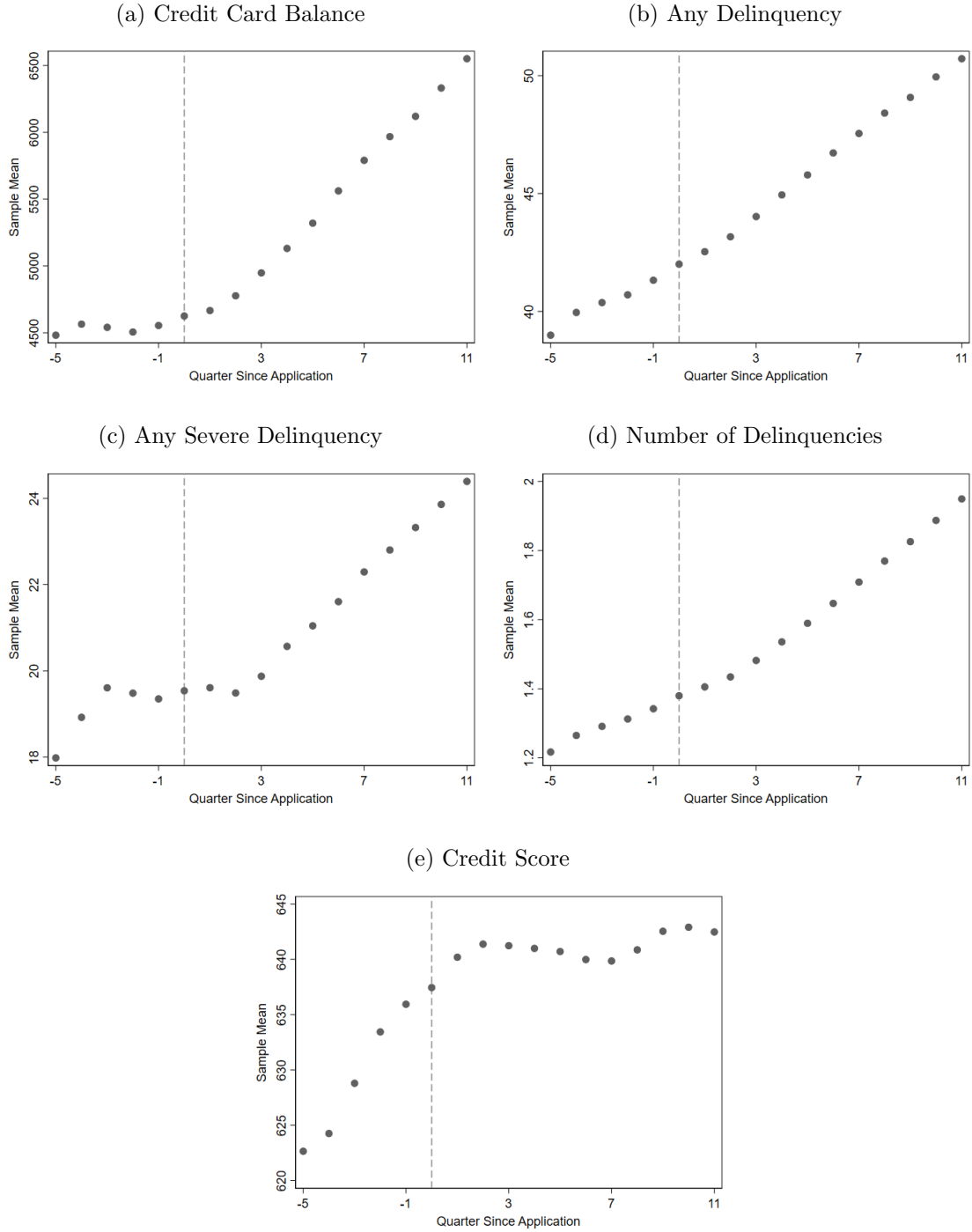
Table 4: Effect of SNAP Approval on Employment and Program Participation, San Francisco

	(1) Any Earnings	(2) Quarterly Earnings	(3) On Medicaid	(4) On TANF
Quarter 1	-1.90 (8.61)	-704.82 (491.33)	10.21 (7.55)	3.52 (2.31)
Quarter 2	3.14 (9.92)	-575.74 (696.64)	24.02** (9.54)	6.58** (3.03)
Quarter 3	5.59 (10.74)	598.35 (791.97)	36.91*** (11.26)	7.39** (3.42)
Quarter 4	5.29 (11.08)	-232.58 (869.93)	37.31*** (11.81)	7.13+ (3.74)
N	154,655	154,655	154,655	154,655
Mean, Dep. Var.	32.98	1028.53	48.49	1.67

Notes: This table presents estimates of the effects of being marginally approved for SNAP due to interview flexibility on employment and participation in other social insurance programs. These treatment-on-the-treated estimates correspond to the coefficients $\hat{\eta}_1$ derived from a two-stage least-squares specification in equations (1) and (2). Recertification success is instrumented for with scheduled interview timing. Each row of estimates corresponds to effects in the first, second, third, or fourth quarter after the recertification month. Dependent variable means are provided for the quarter prior to randomization. Outcomes include: any earnings, earnings, Medi-Cal (Medicaid) participation in the 3rd month of the quarter, and CalWorks (TANF) participation in the third month of the quarter. Coefficient estimates for indicator variables are reported in percentage points (0–100). Outcomes are measured at the recertification case level. Standard errors in parentheses are clustered at the recertification level. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

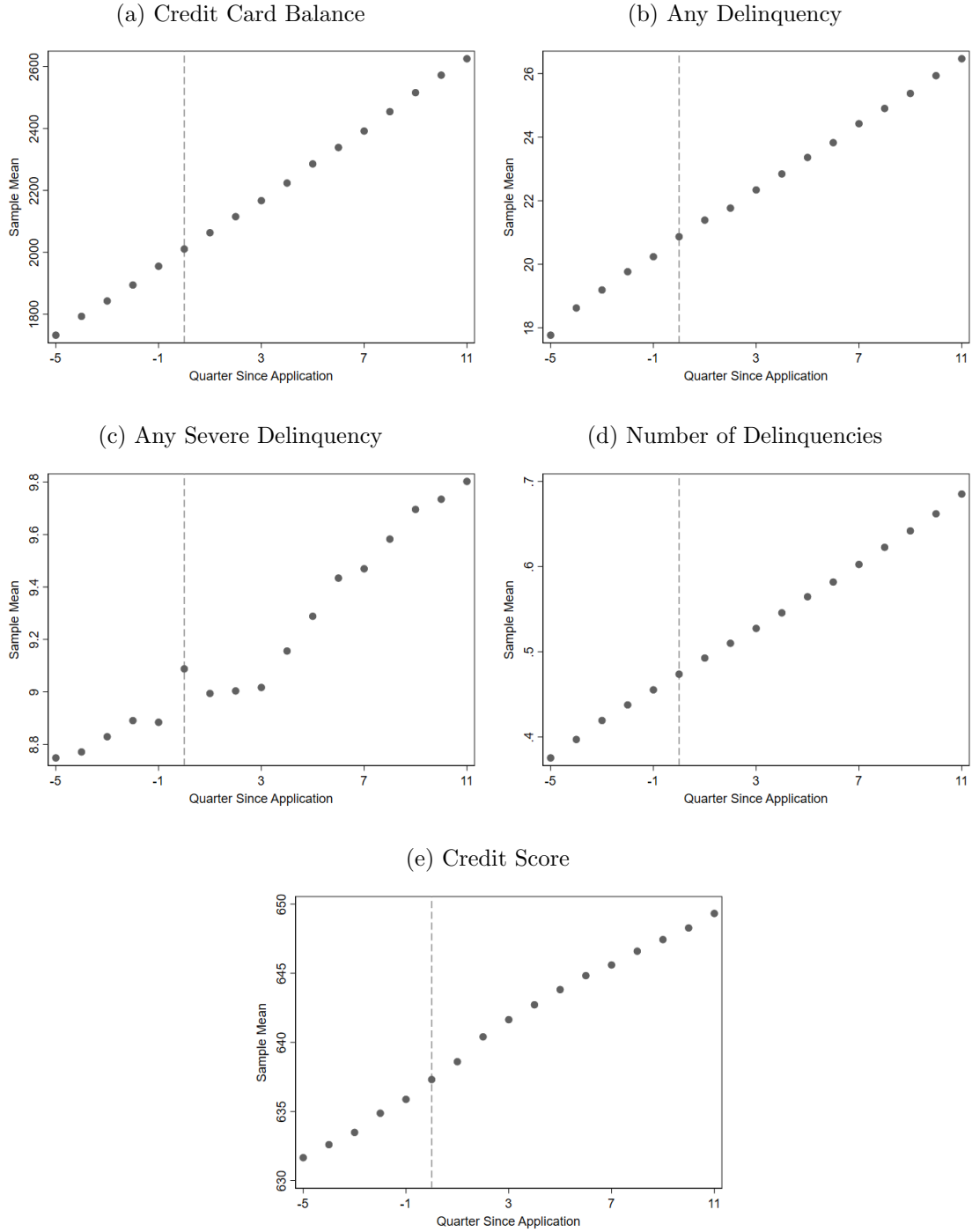
Appendix A: Tables and Figures

Figure A.1: Financial Outcomes by Event-Time, Los Angeles



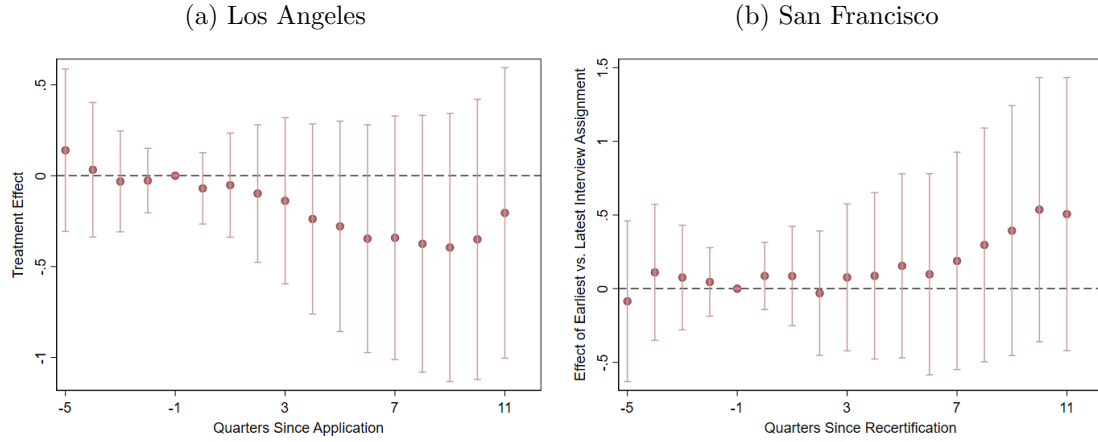
Notes: This figure plots the mean value of each financial outcome by quarter since SNAP application. Outcomes in each panel are: (a) credit card balances (in dollars), (b) indicator of any credit card delinquency (30+ days past due), (c) indicator of any severe credit card delinquency (90+ days past due), (d) number of delinquent credit card accounts, and (e) credit score. Indicator outcomes are expressed in percentage points (0–100). The vertical dashed line denotes the quarter of SNAP application.

Figure A.2: Financial Outcomes by Event-Time, San Francisco



Notes: This figure plots the mean value of each financial outcome by quarter since the SNAP recertification deadline. Outcomes in each panel are: (a) credit card balances (in dollars), (b) indicator of any credit card delinquency (30+ days past due), (c) indicator of any severe credit card delinquency (90+ days past due), (d) number of delinquent credit card accounts, and (e) credit score. Indicator outcomes are expressed in percentage points (0–100). The vertical dashed line denotes the quarter of SNAP recertification.

Figure A.3: Effect of SNAP Approval on Number of Open Accounts



Notes: This figure presents treatment-on-the-treated event-study estimates of the effect of marginal SNAP approval on the number of open credit card accounts by quarter relative to the individual's SNAP application (Los Angeles) or recertification deadline (San Francisco). SNAP approval and its interactions with event time are instrumented using assignment to the treatment communications regarding flexible interview availability (Los Angeles) or scheduled interview timing (San Francisco). Bars denote the 95 percent confidence interval with standard errors clustered at the application/recertification case level.

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 (%)	91.8	91.8	91.7	0.73
Matched, Balanced Panel (%)	79.0	79.0	79.1	0.96
Number of cases	54,148	40,624	13,524	
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 (%)	57.9	58.1	56.0	0.01
Submitter age	37.7	37.5	40.4	0.00
English speaking (%)	85.8	87.2	69.7	0.00
Elderly or disabled (%)	17.5	16.3	30.3	0.00
Receives SSI (%)	11.2	10.2	22.3	0.00
Stable housing (%)	55.8	56.6	46.9	0.00
Rent or mortgage (\$)	698.0	716.2	495.2	0.00
Any income in past 30 days (%)	34.0	34.7	26.1	0.00
Income past 30 days (\$)	352.5	363.3	231.8	0.00
Any money on hand (%)	53.1	53.7	46.4	0.00
Money on hand (\$)	913.0	956.7	426.8	0.00
Has non-job income (%)	14.8	15.1	11.8	0.00
Expedited(%)	59.7	58.7	70.2	0.00
Number of cases	54,148	49,686	4,462	

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 matched and unmatched cases.

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 (%)	87.8	87.9	87.7	0.55
Matched, Balanced Panel (%)	74.0	74.1	73.9	0.75
Number of cases	26,482	12,900	13,582	
Panel B: Case Characteristics by Match Status				
	Full Sample	Matched	Unmatched	p-value
Household size	1.5	1.5	1.3	0.00
Female submitter (%)	44.7	45.2	41.6	0.00
Submitter age	43.4	43.3	44.3	0.00
English speaking (%)	70.4	73.1	51.0	0.00
Any children (%)	28.3	27.6	33.3	0.00
US citizen (%)	77.7	80.7	55.9	0.00
Non-white (%)	77.3	77.3	76.8	0.49
Years since first on SNAP	4.8	4.8	4.3	0.00
Monthly benefits (\$)	223.2	225.2	208.7	0.00
Max SNAP benefits (%)	65.1	63.9	73.1	0.00
Quarterly wage income (\$)	789.4	872.8	188.7	0.00
No wage income (%)	75.7	73.4	92.5	0.00
Number of cases	26,482	23,253	3,229	

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 matched and unmatched cases.

Table A.3: Baseline Characteristics, Los Angeles

	Full Sample	Treatment	Control	p-value
Household size	1.74	1.75	1.70	0.00
Female submitter (%)	58.62	58.68	58.44	0.66
Submitter age	38.27	38.32	38.12	0.24
English speaking (%)	86.95	86.87	87.17	0.43
Elderly or disabled (%)	16.47	16.60	16.11	0.24
Receives SSI (%)	10.00	10.12	9.62	0.13
Stable housing (%)	59.35	59.46	59.01	0.41
Rent or mortgage (\$)	764.30	757.28	785.38	0.40
Any income in past 30 days (%)	35.59	35.62	35.52	0.85
Income past 30 days (\$)	381.50	382.80	377.61	0.48
Any money on hand (%)	54.72	54.60	55.07	0.39
Money on hand (\$)	1024.28	1016.67	1047.13	0.76
Has non-job income (%)	15.90	15.91	15.89	0.97
Expedited(%)	57.12	56.89	57.81	0.10
Number of cases	42,796	32,105	10,691	
Number of individuals	50,522	37,972	12,550	

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.

Table A.4: Baseline Characteristics, San Francisco

	Full Sample	Early Interview	Late Interview	p-value
Household size	1.54	1.54	1.54	0.80
Female submitter (%)	46.33	45.85	46.83	0.17
Submitter age	43.31	43.16	43.46	0.12
English speaking (%)	74.78	74.60	74.96	0.57
Any children (%)	26.86	26.95	26.77	0.77
US citizen (%)	83.78	83.29	84.28	0.06
Non-white (%)	77.03	77.08	76.97	0.86
Years since first on SNAP	4.55	4.54	4.56	0.74
Monthly benefits (\$)	228.88	229.40	228.34	0.58
Max SNAP benefits (%)	63.32	63.29	63.36	0.91
Quarterly wage income (\$)	940.21	925.01	956.10	0.28
No wage income (%)	71.35	71.57	71.12	0.49
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, and an indicator for receiving any wages in the quarter prior to recertification.

Table A.5: Complier Analysis, Los Angeles

	(1) Compliers	(2) Potential Compliers
Household Size	2.05 (0.11)	2.10 [0.605]
Applicant Age (in years)	40.43 (1.33)	37.02 [0.011]
Applicant Female (%)	56.03 (4.46)	59.38 [0.001]
Elderly or Disabled (%)	19.04 (3.40)	15.39 [0.000]
English (%)	85.22 (3.07)	85.13 [0.908]
Spanish (%)	9.31 (2.63)	10.90 [0.015]
Any Income Past 30 Days (%)	39.60 (4.17)	43.20 [0.001]
Income in Past 30 Days (\$)	425.59 (60.44)	522.95 [0.112]
Any Money on Hand (%)	59.79 (4.43)	57.70 [0.047]
Money on Hand (\$)	1279.95 (998.10)	1196.55 [0.934]
Has Non-Job Income (%)	20.93 (3.41)	15.90 [0.000]
SSI (%)	11.88 (2.73)	8.15 [0.000]
Stable Housing (%)	67.29 (4.41)	62.79 [0.000]
Rent or Mortgage (\$)	1249.46 (186.38)	990.91 [0.188]
Expedited (%)	48.62 (4.44)	52.68 [0.000]
Sample Share	6.73 (0.52)	12.26

Notes: This table presents summary statistics of baseline application characteristics for compliers (Column 1) and potential compliers (Column 2). Potential compliers are applicants in the control group whose initial SNAP application was not approved. Complier means are estimated using the approach of Marbach and Hangartner (2020). Bootstrapped standard errors are reported in parentheses, and p-values from tests of equality between complier and potential complier means are reported in brackets. The final row reports the estimated share of the sample represented by each group.

Table A.6: Complier Analysis, San Francisco

	(1) Compliers	(2) Potential Compliers
Household Size	2.08 (0.18)	1.79 [0.162]
Applicant Age (in years)	43.74 (1.83)	41.75 [0.321]
Applicant Female (%)	48.27 (7.38)	52.00 [0.327]
Any Kids (%)	53.50 (7.30)	26.50 [0.000]
English(%)	65.45 (6.87)	81.00 [0.000]
Citizen(%)	76.28 (6.07)	86.50 [0.001]
Non-white (%)	73.98 (6.97)	78.45 [0.200]
Earnings Prior Quarter (\$)	1094.00 (319.28)	1123.27 [0.934]
No Earnings Prior Quarter (%)	78.42 (7.37)	67.00 [0.000]
Eligible Prior Quarter (\$)	98.66 (1.28)	99.00 [0.695]
SNAP Amount (%)	280.31 (21.79)	249.64 [0.200]
Maximum SNAP (%)	42.07 (7.71)	63.00 [0.000]
Years since initial (%)	4.12 (0.60)	5.71 [0.015]
Sample Share	4.00 (0.57)	0.65

Notes: This table presents summary statistics of baseline recertification characteristics for compliers (Column 1) and potential compliers (Column 2). Cases assigned interview dates in the first half of the recertification month comprise the treatment group, while cases assigned interview dates in the second half of the month comprise the control group. Potential compliers are cases in the control group that did not successfully recertify for SNAP. Complier means are calculated using the approach of Marbach and Hangartner (2020). Bootstrapped standard errors are reported in parentheses, and p-values from tests of equality between complier and potential complier means are reported in brackets. Recertification characteristics include household size, applicant age and sex, presence of children, interview language, citizenship, race, prior-quarter earnings, prior-quarter SNAP eligibility, SNAP benefit amount, maximum benefit status, and years since initial SNAP enrollment.

Table A.7: Reduced Form: Effect of Interview Flexibility on Financial Well-Being

	(1) Credit Card Balance	(2) Any Delinquency	(3) Any Severe Delinquency	(4) Number of Delinquencies	(5) Credit Score
Panel A: Los Angeles					
Year 1	-9.64 (20.34)	-0.42*** (0.16)	-0.12 (0.22)	-0.01 ⁺ (0.01)	0.37 (0.41)
Year 2	-76.16** (37.75)	-0.61** (0.25)	-0.40 (0.34)	-0.01 (0.01)	0.73 (0.61)
Year 3	-131.05** (51.76)	-0.77** (0.31)	-0.98** (0.42)	-0.02 (0.02)	1.29 ⁺ (0.72)
N	202,052	202,052	202,052	202,052	181,156
Mean, Dep. Var.	4554.60	41.32	19.35	1.34	631.75
Panel B: San Francisco					
Year 1	-40.34 (27.64)	-0.21 (0.28)	-0.43 (0.40)	-0.01 (0.01)	1.76** (0.83)
Year 2	-38.67 (48.47)	-0.64 (0.42)	-0.61 (0.57)	-0.02 (0.01)	2.25 ⁺ (1.22)
Year 3	-97.09 (68.16)	-1.11** (0.53)	-1.16 ⁺ (0.65)	-0.04** (0.02)	2.13 (1.44)
N	123,724	123,724	123,724	123,724	106,076
Mean, Dep. Var.	1954.85	20.24	8.88	0.46	627.19

Notes: 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. Each row of estimates corresponds to effects on average quarterly financial outcomes in the first, second, or third year after the application or 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, and credit score. 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. ⁺ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Effect of Being Marginally Approved for SNAP on Financial Well-Being, Always-scored Sample

	(1) Credit Card Balance	(2) Any Delinquency	(3) Any Severe Delinquency	(4) Number of Delinquencies	(5) Credit Score
Panel A: Los Angeles					
Year 1	2.04 (317.75)	-5.86** (2.45)	-1.60 (3.34)	-0.13 (0.09)	5.40 (6.00)
Year 2	-941.19 (593.74)	-9.64** (3.80)	-7.29 (5.27)	-0.19 (0.18)	10.52 (8.93)
Year 3	-1964.34** (827.81)	-13.54*** (4.74)	-16.86** (6.61)	-0.27 (0.26)	18.60 ⁺ (10.58)
N	181,156	181,156	181,156	181,156	181,156
Mean, Dep. Var.	4906.77	42.98	20.70	1.43	635.95
Panel B: San Francisco					
Year 1	-488.16 (323.63)	-3.44 (3.19)	-4.06 (4.51)	-0.16 ⁺ (0.09)	17.52** (8.64)
Year 2	-531.91 (561.51)	-8.97 ⁺ (4.85)	-7.36 (6.56)	-0.32** (0.16)	22.37 ⁺ (12.59)
Year 3	-1248.52 (798.77)	-12.73** (6.12)	-13.06 ⁺ (7.59)	-0.54** (0.25)	21.16 (14.69)
N	106,076	106,076	106,076	106,076	106,076
Mean, Dep. Var.	2214.13	22.15	10.01	0.51	635.88

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 $\hat{\gamma}_1$ derived from a two-stage least-squares specification in equations (1) and (2). 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 application or recertification month. Dependent variable means are provided for the quarter prior to randomization. The estimation sample is restricted to individuals with non-missing credit scores in all quarters of the study period. 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, and credit score. 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. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Additional Outcomes: Unsecured Loan Outcomes

	Unsecured Loans Only			Unsecured Loans + Credit Cards		
	(1) Balance	(2) Any Delinquency	(3) Any Severe Delinquency	(4) Balance	(5) Any Delinquency	(6) Any Severe Delinquency
Panel A: Los Angeles						
Year 1	-189.64 (116.40)	0.81 (1.29)	0.75 (1.52)	-332.70 (325.67)	-3.39 (2.43)	-1.48 (3.33)
Year 2	-402.94 ⁺ (235.24)	1.18 (2.25)	0.64 (2.44)	-1533.52** (632.95)	-4.42 (3.74)	-5.42 (5.23)
Year 3	-324.90 (298.76)	-0.37 (3.05)	-0.99 (3.19)	-2270.25*** (872.01)	-6.79 (4.61)	-14.94** (6.56)
N	202,052	202,052	202,052	202,052	202,052	202,052
Mean, Dep. Var.	528.29	7.65	3.43	5083.04	43.11	21.00
Panel B: San Francisco						
Year 1	-41.97 (63.37)	-0.69 (0.79)	1.38 (1.18)	-460.12 (300.12)	-2.63 (2.94)	-4.05 (4.21)
Year 2	-169.84 (124.63)	-2.95** (1.46)	0.17 (1.68)	-570.66 (536.43)	-7.82 ⁺ (4.53)	-6.75 (6.11)
Year 3	-299.59 ⁺ (171.22)	-3.30 ⁺ (2.00)	0.90 (2.01)	-1306.04 ⁺ (775.24)	-12.32** (5.75)	-11.17 (7.08)
N	123,724	123,724	123,724	123,724	123,724	123,724
Mean, Dep. Var.	89.28	1.66	0.79	2044.13	20.95	9.46

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 the two-stage least-squares specification in equations (1) and (2). 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 post-randomization. Dependent variable means are provided for the quarter prior to randomization. Outcomes include balances, any delinquency, any severe delinquency, and the number of delinquent accounts, measured separately for unsecured loans excluding credit cards (Columns 1 to 3) and for unsecured loans including credit cards (Columns 4 to 6). 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 or recertification level. ⁺ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Additional Outcomes: Any Auto Loan

	(1)	(2)
Panel A: Los Angeles		
Year 1	0.21 (3.47)	1.77 (5.57)
Year 2	-3.93 (5.57)	-4.64 (8.73)
Year 3	-12.41 ⁺ (6.75)	-20.28 ⁺ (10.59)
N	126,196	75,856
Mean, Dep. Var.	0.00	100.00
Panel B: San Francisco		
Year 1	0.97 (3.61)	-2.20 (14.53)
Year 2	-2.08 (5.95)	-8.57 (24.10)
Year 3	5.28 (7.20)	-21.17 (29.31)
N	107,464	16,260
Mean, Dep. Var.	0.00	100.00
Sample Restriction	No auto loan at baseline	Has auto loan at baseline

Notes: This table presents estimates of the effects of being marginally approved for SNAP due to interview flexibility on having an auto loan. These treatment-on-the-treated estimates correspond to the coefficients $\hat{\eta}_1$ derived from the two-stage least-squares specification in equations (1) and (2). 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 the probability of having an auto loan in the first, second, or third year post-randomization. Dependent variable means are provided for the quarter prior to randomization. Columns (1) and (2) restrict the sample based on auto loan status in the quarter prior to randomization ($q = -1$). Coefficient estimates are reported in percentage points (0–100). Outcomes are measured at the individual level. Standard errors in parentheses are clustered at the application or recertification level. ⁺ $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$.