Unemployment Insurance as a Financial Stabilizer: Evidence from Large Benefit Expansions *

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Abstract

To what extent does unemployment insurance (UI) attenuate aggregate financial responses to unemployment shocks? We answer this question using administrative credit bureau records and the unprecedented changes in unemployment and UI generosity during the Covid-19 pandemic. We first find that aggregate sensitivity to the unemployment rate decreased by 50% for auto loans and 66% for credit cards between January 2017 and March 2021. To isolate the effect of UI from other contemporaneous policies shifting unemployment shock responsiveness, we employ a staggered event study design around state-level withdrawals from federal UI programs in late 2021. We find that almost all of the pandemic sensitivity drop is attributable to UI expansions. Our two designs are qualitatively robust to placebo tests on plausibly unaffected credit types, potential demand-side responses for increased credit, and alternate estimation specifications. In a back-of-the-envelope calculation, we calculate that UI expansions prevented about 59% of total potential delinquency-months. Taken together, these results imply that federal UI expansions have had a substantially stabilizing effect during the Covid-19 pandemic. Our findings thus provide powerful empirical support for a largely theoretical body of research on the role of UI as an automatic stabilizer of aggregate economic conditions.

JEL Classification: E2, J6, G5

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1 Introduction

Job loss induces substantial financial stress: Households experiencing temporary unemployment spells are more likely to default on their loans (Braxton et al., 2020; Hurd and Rohwedder, 2010) or to file for bankruptcy (Keys, 2018). Liquidity—as opposed to wealth—seems to be a crucial determinant of consumption smoothing behavior, with liquidity-constrained households appearing much more sensitive to adverse shocks (Gerardi et al., 2017; Ganong and Noel, 2020; Ganong et al., 2020a). An important policy question is the extent to which targeted liquidity provision from unemployment insurance (UI) benefits insulate households from these adverse financial effects of job loss. Empirical evidence in this area has primarily focused on the micro-level impacts of the UI system. Using survey data, Hsu et al. (2018) leverage heterogeneity in UI generosity across states and over time to show that workers' job loss translates into less financial distress during more generous benefit regimes.

At the macroeconomic level, Kekre (2021) shows that UI can stabilize aggregate economic conditions if unemployed households have higher marginal propensities to consume (MPCs) than employed households or if UI alleviates precautionary savings motives. In a similar vein, McKay and Reis (2021) show that unemployment insurance can insulate households from uninsurable income shocks and unemployment, potentially making counter-cyclical increases in UI generosity optimal. Finally, Landais et al. (2018) show that counter-cyclical UI benefit expansions can be welfare enhancing in matching models by increasing labor market tightness in slumps. However, *empirical* evidence assessing the magnitudes of these potential mechanisms is relatively limited. Hsu et al. (2018) use their micro results to argue that UI expansions during the Great Recession prevented 1.3 million foreclosures, but this result relies on a partial equilibrium analysis extrapolation that simply multiplies the micro elasticities with the benefit extensions during the Great Recession. The best empirical evidence of the role of UI in smoothing aggregate economic conditions comes from Di Maggio and Kermani (2016). Leveraging heterogeneity in local benefit generosity and estimating the effects of Bartik shocks on local economies, Di Maggio and Kermani (2016) find that more generous UI regimes attenuate the effect of adverse shocks on employment and earnings growth. The key mechanism in their analysis is the financial accelerator channel: Delinquencies rise by much less in more generous UI regimes, preventing banks from tightening lending standards in economic downturns.

We shed new light on this role of UI as a stabilizer of aggregate financial conditions, leveraging the enormous increases in unemployment rates during the Covid-19 pandemic. As part of its policy response, the United States engaged in an unprecedented expansion of the unemployment insurance system: Maximum benefit durations were increased from lows of 12 weeks to highs of up to 76 weeks and supplemental payments from \$300 to \$600 increased replacement rates to close to or substantially over 100% (Ganong et al., 2020b). In addition, eligibility requirements were loosened so that virtually all unemployed workers were eligible for UI, even independent contractors or those with inadequate pre-displacement earnings. Importantly, many of these changes mimic previous policy recommendations intended to make the unemployment insurance system a better tool for macroeconomic stabilization (Chodorow-Reich and Coglianese, 2019), making this period an ideal setting to empirically investigate the macroeconomic stabilization potential of the unemployment system.

To illustrate the Covid policy response's unparalleled magnitude, in Figure 1 we map unemployment and delinquency rates at the county level between 2019 and 2021. This exercise reveals a stark geographic pattern. Looking first at Panels (a) and (c), we see stark increases in county-level unemployment rates between 2019 and 2020. However, Panels (b) and (d) reveal that county-level delinquencies actually *decreased* over the same time period, reflecting a decoupling of unemployment shocks and delinquencies. This phenomenon stands in stark contrast to the Great Recession, in which both measures simultaneously spiked. Turning now to Panels (e) and (f), we see unemployment rates falling back down between 2020 and 2021 accompanied by a small continued decline in delinquencies. To what extent does this new disconnect between local unemployment and delinquency rates throughout the pandemic reflect the efficacy of different Covid policy choices in local financial stabilization?

In this paper, we show that an important driver of this decoupling between delinquency and unemployment is attributable to Covid-era expansions in the UI system rather than other contemporaneous policy responses. We leverage a nationally representative sample of administrative credit records from Experian, aggregated to the county-month level. As a benchmark, we estimate the delinquency-unemployment sensitivity over time, using state-by-month fixed effects to absorb contemporaneous policy changes (which were generally set or carried out at the state level). We show that prior to the pandemic, local financial conditions were highly sensitive to local economic conditions with county-level unemployment rates being highly predictive of county-level delinquency rates. We then show that the sensitivity of local financial conditions to local unemployment rates collapsed during the time expanded UI was in effect. The sensitivity of auto loan delinquencies with respect to the local unemployment rate fell by 50%. For consumer credit cards, this decline in the delinquency-unemployment rate sensitivity was 66%.

In order to isolate the effect of UI expansion from other Covid-era policy responses that would have simultaneously changed unemployment sensitivity, we employ a staggered event study design around the withdrawal from the Federal UI program in late 2021. We argue that withdrawal was plausibly driven by political and ideological concerns about the generosity of the UI system rather than a response to state-level economic conditions. The delinquency-unemployment rate sensitivity increases substantially after withdrawal—by about the same magnitude as prior estimates of sensitivity drops—suggesting that the Covid-era drop in the delinquency-unemployment rate sensitivity was due to expanded UI benefits. These findings are qualitatively robust to alternate specification choices, and we also present placebo treatments that show that we do not see increases in delinquencies on loan types that are likely to be unaffected by UI policies. Using a back-of-the-envelope calculation that keeps the delinquency-unemployment rate sensitivity fixed at pre-UI expansion levels, we estimate that UI expansions prevented about 59% of counterfactual delinquency-months. This aggregate financial stabilization effect was in addition to other widely acknowledged benefits of UI benefit provision, such as sustaining aggregate household consumption.

This paper contributes to a growing literature on the effects of pandemic UI policies. Ganong et al. (2022) use bank account data to show that expanded UI played a significant role in explaining aggregate consumption dynamics but had very limited labor market effects. Replacement rates were so high that households receiving UI built up substantial savings buffers despite one-month MPCs out of UI ranging from 0.26 to 0.43. Similarly, Coombs et al. (2022) show that workers affected by the abrupt withdrawal from federal UI had relatively small job finding responses while the MPC out of the benefit cut was 0.52. Similar to our results on the effects of aggregate financial conditions, these high MPCs combined with the sheer magnitude of the UI policy response suggest substantial aggregate effects of the Covid-era UI benefit expansions. Unlike these more micro-level papers, our paper explicitly focuses on the aggregate effects of UI expansions. Rather than estimating whether any one household is more insulated from adverse shocks under a more generous UI regime, our paper therefore answers the question whether *macroeconomic* conditions can be stabilized with UI as a policy instrument.[1]

We also add to a nascent *empirical* macro literature on the benefits of UI provision. A large empirical micro tradition has attempted to separately estimate both the consumption-smoothing benefits (Gruber, 1997; Ganong and Noel, 2019) and the job search disincentive costs of UI (Katz and Meyer, 1990; Card et al., 2007) towards calibrating models of optimal benefit provision (Baily, 1978; Chetty, 2006, 2008; Schmieder and von Wachter, 2017). On the other hand, past empirical research in macroeconomics has mainly focused on estimating aggregate labor market disincentive effects of the UI system (Hagedorn et al., 2013; Johnston and Mas, 2018; Chodorow-Reich et al., 2019; Boone et al., 2021). Our paper fills this research gap by providing direct evidence that counter-cyclical increases in UI generosity can have substantial benefits in improving aggregate *financial* conditions. Our paper adds to a recent set of papers (Ganong et al. 2022, Di Maggio and Kermani 2016) providing empirical support for the theoretical work on the financial macro stabilization effects of UI.

Section 2 details the numerous federal stabilization policies enacted during the pandemic, with a focus on the UI system. Section 3 describes our credit bureau microdata and aggregation procedure in detail. Turning to empirics, Section 4 explains our first estimation strategy and estimates aggregate financial sensitivity to unemployment shocks over time. Section 5 then explains how we use the 2021 staggered federal UI withdrawal to calibrate UI-specific effects in attenuating financial sensitivity during the pandemic. Section 6 uses these estimates to provide an aggregate estimate for delinquencies prevented by pandemic UI policies. Finally, Section 7 concludes.

[1]An important caveat to our results is that we cannot separately identify whether our results are due to liquidity provision to the unemployed (potentially stimulating aggregate demand through high MPCs among the unemployed) versus reductions in pre-cautionary savings motives or spillovers to employed households. We offer suggestive county-level evidence in favor of the former view by estimating heterogeneous treatment effects of UI benefit withdrawal on counties with low and high unemployment rates (see Section 5 for more details).

2 The Pandemic Policy Environment

2.1 Unemployment Insurance Policies

In this paper, we analyze the aggregate financial effects of introduction and withdrawal from pandemic unemployment programs enacted during the Covid-19 pandemic. These programs, which were first created as part of the 2020 CARES Act, had three major components. The Pandemic Emergency Unemployment Compensation (PEUC) first extended the maximum duration of unemployment benefits by 13 weeks. This largely mirrored prior ad-hoc federal benefit extensions during the Great Recession (see Figure 3, which plots the evolution of federal UI benefit duration extensions over time). Together with existing state-level policies—both existing statutory durations and automated cyclical UI extension triggers—total eligible benefit durations totalled up to a maximum of 99 weeks. The other two components of pandemic unemployment policy were novel and reflected a broad desire to provide rapid liquidity to affected workers. The Federal Pandemic Unemployment Compensation (FPUC) program introduced a $600 supplement to existing weekly benefit amounts, which increased replacement rates above 100% for low to medium wage workers.[2] The Pandemic Unemployment Assistance (PUA) program additionally extended benefits to otherwise ineligible workers, such as those who had otherwise exhausted benefit eligibility, independent contractors, or those with an insufficient working history.[3]

While PEUC and PUA were authorized through December 31st, the CARES Act originally set FPUC supplements to expire on July 26th. The program was not reauthorized despite congressional efforts[4] and was partially replaced by the Lost Wages Assistance Program (LWA), which instead provided a temporary six-week $300 UI benefit supplement until September 6th.[5] On December 27th, all three CARES programs—FPUC, PUA, and PEUC—were extended until March 13th as part of the new Continued Assistance Act.[6] PEUC and PUA duration extensions were renewed for a further 11 weeks, with FPUC reauthorized for a smaller $300 supplement. These policies were extended for a final time on March 11th as part of the American Rescue Plan, which reauthorized i) the $300 FPUC supplement as well as ii) new 29 week benefit duration extensions for PUA and PEUC claimants. As part of the bill, each pandemic unemployment insurance pro-

[2] Ganong et al. (2020b) show that statutory replacement rates exceeded 145%. In sum, this component of the program paid out over $263 billion in benefits, totaling 7% of total personal income over this period.

[3] More information on unemployment agencies' implementations of these policies can be found [here]. The California UI system also provides an excellent and accessible breakdown of the various UI programs and their resulting changes to pandemic benefits [here].

[4] The 2020 HEROES Act, which would have extended benefit supplements, passed the House but was not taken up in the Senate [link].

[5] The program was made possible by presidential order, as LWA program funding came from redirected FEMA disaster relief funds originally earmarked in the 2020 CARES Act [link]. Using Chase bank account data, Ganong et al. (2022) find that receipt of LWA supplements was inconsistent and depended highly on state agencies; while most benefits were paid in September, some Wisconsin and New Jersey recipients received benefits well into October. Given the haphazard nature of LWA payments, we are unable to cleanly assign receipt for different counties over time and do not include the program variation in our analysis.

[6] The CAA also authorized Mixed Earner Unemployment Compensation (MEUC), which provided $100 supplements for self-employed workers receiving benefits.

gram was designed to expire September 4, 2021.

Following a weak jobs report in May 2021, however, some state governors expressed concern that UI benefit availability had suppressed workers' job search and was impeding economic recovery. As we highlight in Section 5 when discussing our empirical strategy, this belief was arguably driven by ideological, rather than financial, concerns.[7] 26 states consequently terminated access to FPUC benefits ahead of the scheduled September expiration (22 states in June, three in July, and one in August), generating relatively sharp state-level variation in both access and generosity of UI benefits. Figure 2, which plots UI continued claims throughout 2021, highlights the stark nature of benefit expiry: almost 4.5 percent of the labor force (nearly 5 million people) lost access to UI in September, with another 1.5 percent of the labor force losing UI access during the early phase-out from June through August.[8] Other authors have leveraged this variation across states as a shock to UI benefit access, finding relatively small increases in job-finding rates but large MPCs out of UI benefits for benefit losers (Coombs et al., 2022).

In sum, these pandemic programs made the UI program substantially more generous even compared to past recessions. Workers eligible under regular claims had a potential benefit duration up to 99 weeks in some states, equalling benefit durations at the height of the Great Recession. Moreover, FPUC supplements greatly increased benefit levels; replacement rates for some workers almost tripled compared to normal program levels. The introduction of PUA also dramatically expanded access to benefits for otherwise ineligible workers. Figure 4, which plots the insured (IUR, red line) and regular unemployment rate (UR, in blue) over time from January 2000 to December 2021, depicts an immense increase in aggregate insured for even regular workers. Indeed, the IUR-UR ratio nearly doubled in March 2020 compared to the Great Recession, from about 50% to almost 100%. Taken together with special programs like PUA[9] (green line), the IUR-UR ratio was around 150% until federal program expiry in late 2021.[10]

Many of these Covid-era changes to the UI system correspond to existing policy proposals improving the macroeconomic stabilization component of UI. Writing before the pandemic, Chodorow-Reich and Coglianese (2019) point out that UI had historically played a minor role in macroeconomic stabilization. Duration expansions are usually implemented with lags and only affect a small subset of workers (since relatively few workers become long-term unemployed). Baseline take-up rates of UI are also quite low at 30-50% (Blank and Card, 1991), implying limited scope for UI to stabilize aggregate economic conditions. In order to make UI into a macro stabilization tool, Chodorow-Reich and Coglianese (2019) made five recommendations: (i) increased eligibility

[7]As an illustrative example, South Carolina governor Henry McMaster claimed in early May that "[the] labor shortage is being created in large part by the supplemental unemployment payments that the federal government provides claimants on top of their state unemployment benefits . . . [it] has turned into a dangerous federal entitlement, incentivizing and paying workers to stay at home rather than encouraging them to return to the workplace" [link].

[8]In addition, workers in early phase-out states who continued to receive UI benefits through the regular UI program lost access to the $300 supplement.

[9]The regular IUR, taken from the BLS, does not include federal programs like PUA. We construct the insured unemployment rate including pandemic programs by 1) computing the ratio of all-programs and continuing claims weeks (which include both regular claims and special federal programs), 2) multiplying by the regular IUR.

[10]Note that the ratio can surpass 100% since the two statistics' underlying populations do not exactly line up.

and take-up of regular UI, (ii) full federal financing of the expanded benefit (EB) program, (iii) removing look-back provisions for EB, (iv) automatic extensions of benefit durations in times of very high unemployment, and (v) automatic increases in UI generosity during recessions. During Covid, UI eligibility and access increased massively through relaxations of earnings tests as well as the reduction of administrative hurdles. The EUC and PUA programs both extended benefit durations and provided supplemental payments that significantly increased UI replacement rates. In effect, we can think of Covid-era UI as a temporary implementation of (i), (iv), and (v). Therefore, these UI expansions provide an excellent framework to test whether these changes actually help in stabilizing macroeconomic conditions.

2.2 Other Pandemic Policies

It is important to note that the Covid legislative response included many non-UI policies that may have affected credit outcomes. For example, the CARES Act also instituted a mortgage forbearance program that allowed borrowers with federally backed mortgages to defer payments for up to 18 months.[11] Since forbearance immediately affects payment status on mortgage loans by deferring payments, we assume all changes in mortgage delinquencies are driven by either forbearance or the general Covid policy response outside of expanded UI. Another policy immediately affecting financial conditions is the ongoing moratorium on federal student loan payments. Payments were paused effective March 20th and the Office of Federal Student Aid also stopped collections on defaulted loans and set the interest rate on Department of Education-backed loans to 0%. Given that the vast majority of student loans are federal loans, this policy meant that most student loans were reported to creditors as "current" starting on March 20, 2020.

Beyond credit market policies, the Covid policy response included many actions aimed at providing liquidity and insulating households from the economic fallout of the pandemic. The federal government provided three rounds of Economic Impact Payments ("stimulus checks") ranging from $500 to $1,400 per household member. The American Rescue Plan provided an expanded and fully refundable Child Tax Credit of $3,600 per child under the age of six (and $3,000 per child between the ages of six and seventeen). The CARES Act also instituted the Paycheck Protection Program, a policy designed to keep existing labor market matches intact by providing forgivable loans to employers provided they were mostly used to make payroll payments. For a more detailed description and analysis of many of these policies, see Chetty et al. (2020). Our empirical setup is designed to control for many of these federal policy changes, as well as other policies at the state level.[12]

[11] Initially, the program allowed for 180 days of forbearance with a borrower-side option to extend forbearance for another 180 days. Borrowers with mortgages backed by Freddie Mac or Fannie Mae could extend forbearance for up to 180 more days, provided their account when into forbearance before February 28, 2021. Households with mortgages backed by the Department for Housing and Urban Development, the Department of Veterans Affairs, and the Rural Housing Service could request an additional 180 days of forbearance provided they first entered forbearance before June 30, 2020.

[12] Several states enacted their own policies to expand or mimic federal reforms: for example, California provided two rounds of stimulus checks for state residents.

Our general approach for isolating the effect of changes to the unemployment insurance system on local financial conditions is as follows. First, we restrict primary attention to credit cards and auto loans, which were both unaffected by explicit policy responses. Second, our baseline analysis focuses on county within state-by-month dynamics. As such, all common variation in delinquencies driven by the general policy response will be absorbed by the state-by-month fixed effect. Third, we focus on the effects of the local unemployment rate on local financial conditions. Many of the other policy responses were not directly targeted at the unemployed (and some of them were explicitly attempting to keep labor market matches intact): To the extent that we see declines in the sensitivity of local financial conditions to local unemployment, it is very likely that this "dampening" is driven by increases in the generosity of the UI system. We return to these points while discussing our design in Section 4.[13]

3 Data

Our analysis principally makes use of aggregated credit bureau microdata matched to county-level Local Area Unemployment Statistics (LAUS) data from the Bureau of Labor Statistics.

3.1 Credit Data

Our credit bureau microdata comes from the University of California's Consumer Credit Panel (UC-CCP), which covers a nationally representative random 2% sample of households (together with associated borrowers and household members) with their associated credit histories for each quarter from 2004 to 2021. The data, which originates from Experian and is made possible through a data use agreement with the California Policy Lab, contains detailed information about credit holders over time: person-level variables include geographic identifiers, demographic information, credit scores, bankruptcies, and new inquiries for credit. A novel aspect of our data relative to other credit bureau data sets is that we additionally also see raw tradeline-level information about each loan, such as monthly payment history, credit limits and balances, loan type (e.g., credit card vs auto loan), delinquency status, and deferments.[14]

Our principal goal is to construct detailed measures of aggregate financial distress over the Covid-19 pandemic. We therefore begin by extracting person-level records between the first quarter of 2017 and the first quarter of 2021. By leveraging the loan-level payment history information, we then reconstruct a monthly panel of loan-level delinquencies, linked to each consumer and their county of residence over time.[15] We aggregate these person-level records to the county-month

[13] An important caveat is that Covid-era policies like mortgage forbearance or the student loan moratorium let households allocate a larger share of their budget to the repayment of credit cards or auto loans.

[14] By comparison, other credit panels (such as the New York Fed's Consumer Credit Panel) are often "rolled-up" to the person-level and may not include associated borrowers or household members. Further background information on this data and comparisons to other credit panels can be found on the UC-CCP's website here.

[15] In particular, we utilize the fact that for each loan Experian also reports the last 64 months of payment history. We extract and reshape these payment histories to form a monthly dataset. In Figure A1, we benchmark our constructed panel against public CFPB mortgage delinquency data, finding that a very similar percentage of mortgages are delin-

level, separated by loan type (e.g., credit cards, auto loans, mortgages), to form our main analysis data set. To better understand potential aggregate demand-side credit responses to unemployment shocks, we similarly construct and merge on county-month counts of new loans, new loan balances, and new loan inquiries. Table 1 describes our final aggregate credit data set, which covers a balanced panel of 3,107 counties, 5.7 million unique consumers, and over 30 million unique loans between January 2017 and March 2022.

3.2 Employment Data

We obtain county-level monthly employment and unemployment rates from the Local Area Unemployment Statistics (LAUS), as published by the Bureau of Labor Statistics.[16] County-level LAUS data are not seasonally adjusted and are available for virtually all counties and county-equivalents in the US. Our main measure for local economic conditions is the the county-level unemployment rate. One potential issue with using not seasonally adjusted data is that the unemployment rate is highly cyclical (see again Figure 4). If households are forward-looking with respect to *seasonal* unemployment, then we would expect the insurance value of UI to be larger with respect to cyclical than to seasonal fluctuations. However, since realizations of seasonal income risk (like realizations of cyclical and idiosyncratic income risk) have a substantial random component, it seems reasonable to think that unemployment insurance insures against both business cycle and seasonal fluctuations. That being said, our results are robust to seasonally adjusting both the unemployment and the delinquency rates as most of the seasonal variation of unemployment will get absorbed by a state-by-month fixed effect in our baseline specification.

Recent work by Boone et al. (2021) has argued that LAUS data may not be the best measure when estimating the aggregate labor market effects of UI policies, as LAUS relies on state-level information to impute county-level unemployment rates. We are interested in the interaction between local economic conditions and local financial distress, however, rather than conditions themselves. In addition, our main specification will include a state-by-month fixed effect that should purge local unemployment rates of the common state component of unemployment for all counties in a given state. A related but separate concern in using LAUS data is that small county populations may generate large sampling error in the unemployment rate or delinquency rates. To mitigate this concern, 1) we drop small counties with credit data on fewer than 50 people and 2) weight by population size in all specifications.[17] Our results are robust to increasing the county size restriction.

quent over time in both data sets. Indeed, the principal differences for 90+ day delinquencies stems from the fact that the CFPB's publicly available data is rounded to the nearest tenth.

[16]The LAUS data can be downloaded here. Our data was downloaded as of May 3, 2021.

[17]Imposing our size restriction drops around 10% of all counties as we can see in A2. Given that our credit data is a 2% representative sample, this implies that we drop counties with credit-scored populations smaller than 2500 people on average.

4 How Elastic are County Delinquencies to Local Unemployment?

We start by qualitatively examining how county-level delinquencies respond to increases in local unemployment rates. The intuition for our analysis here is straightforward: since UI benefits provide needed liquidity to otherwise constrained households (Ganong and Noel, 2019), benefit expansions should attenuate aggregate delinquency responses to unemployment (Di Maggio and Kermani, 2016). Descriptively, we would therefore expect a reduced effect of unemployment rate increases on local delinquency rates after Covid UI policies are enacted. To begin, Figure 5 plots a binned scatter plot of overall county delinquencies against local unemployment rates separately both during Covid (March 2020 to August 2021) and pre-Covid (January 2017 to February 2020). We find the stabilization prediction bears out in the data: we see a much larger pre-pandemic (in red) slope compared to during the pandemic (in blue).

To better understand these patterns, we extend this setup to a regression framework with explicit dynamics and disaggregation by credit type (e.g., credit cards or auto loans).[18] First, we define the county-level delinquency rate for credit type k and county c in state $s(c)$ and month t as $y_{s(c),k,t}$. We regress $y_{s(c),k,t}$ on a state-by-month fixed effect and the local unemployment rate interacted by time dummies in the following estimating question:

$$\text{Delinquency Rate}_{s(c),k,t} = \delta_{s(c),t} + \sum_{\tau}^{T} \beta_t \, UR_{c,t} \cdot \mathbb{1}\{t = \tau\} + \varepsilon_{c,t} \tag{1}$$

Our main objects of interest, $\{\beta_t\}_{\tau}^{T}$, summarize the impact of a 1 percentage point increase in county-level unemployment on the county's aggregate delinquency rate for each month from January 2017 to March 2022. To reduce expositional clutter in what follows, we refer more concisely to these treatment effects β_t as the *delinquency-unemployment sensitivity* in each period. Our choice of estimating equation is motivated by three considerations. First, UI expansion was not the only policy response during this time period: other policies, both directly within the credit market (mortgage forbearance, the student loan payment moratorium) and in providing stimulus (e.g., economic impact payments and the expanded Child Tax Credit) could have also affected this sensitivity over time. Since these reforms were largely invariant to the local unemployment rate, direct effects should be captured in the time component of the fixed effect $\delta_{s(c),t}$. Many other policies that may be affected by unemployment shocks, such as Extended Benefit triggers, are set at the state level and so separating out the state-time component using our fixed effect is also important for dealing with these contemporaneous confounders. Third, since the regular unemployment insurance system is a state-run program, treatment variation is at the state level.

Turning now to results, Figure 6 plots the coefficients $\{\beta_t\}_{\tau}^{T}$ separately for credit cards, auto loans, mortgages, and student loans for each month between January 2017 and March 2022. We

[18]Disaggregation by credit type is particularly important during the Covid pandemic period. As discussed in Section 2, policies such as the student loan payment moratorium or mortgage forbearance affected discrete credit groups, so estimating any-delinquency outcomes masks substantial heterogeneity across types.

prefer to report disaggegated estimates this way to ensure comparability across our sample time period, since mortgage and student loan payment obligations changed during the pandemic. In all plots, the shaded area shows periods when pandemic UI policies including UI supplements were in effect: darker grey implies full pandemic UI, while light grey starting in June 2021 denotes the beginning of UI phase-outs.[19]

We focus on auto loans and credit cards, which were not subject to any Covid policies or credit reporting changes. A first striking feature of these graphs is their cyclicality: the delinquency-unemployment sensitivity tends to rise in the fall months and fall each spring during the pre-pandemic period. After the start of the pandemic, this pattern changes: credit cards, for example, exhibit a mostly flat estimated sensitivity while pandemic UI is in effect. A second notable result is the substantial drop in estimated sensitivity during the pandemic period. Looking first at credit cards throughout the pandemic period in Panel (b), a 1 percentage point increase in the county unemployment rate is associated with roughly a 0.075 percentage point increase in county-level delinquencies (about 0.05 in the first shaded UI period, 0.1 in the second period). This average represents a 66% drop in the average sensitivity compared to the pre-pandemic period value of 0.225, indicating that Covid policies were associated with to substantial reductions in county-level credit card delinquency risk. Panel (a) highlights a similarly large effect for auto loans, at least early in the pandemic: between March and August of 2020, the average delinquency-unemployment sensitivity was about 50% lower than its pre-pandemic average (0.4 to 0.2).

A third takeaway from these plots is that these drops are largely coincident with the shaded areas when UI policies are in effect, and estimates appear to increase in reaction to policy withdrawals. For credit cards, for example, this picture is especially stark: the only increases in the delinquency-unemployment sensitivity are during the unshaded non-pandemic UI periods. We interpret this timing as suggestive potential evidence that our results flow through a UI-liquidity channel, as most other policies were unaffected by contemporaneous UI expirations. This pattern is intuitively quite plausible given the notably more generous pandemic UI policy environment. Recall that UI benefit replacement rates often exceeded 100% (Ganong et al., 2020b), so unemployed workers were receiving more income than before during employment. Looking at this in bank account microdata, Ganong et al. (2022) find that both income and aggregate checking account balances for the unemployed were about 20% and 50% higher respectively than *employed* workers (matched on pre-displacement characteristics).[20] Given this context, and assuming roughly similar debt spend-down out of UI and earned income, the additional benefits appear

[19]While major Covid economic stabilization policies began at the end of March 2020 with the CARES Act, March 2020 can be regarded as potentially treated due to lenders preemptively waiving delinquency reporting in expectation of federal legislation. In contrast to later federal legislation only covering student loans and mortgages, many prominent *credit card* issuers (including Goldman Sachs, US Bank, Truist, and Discover) also announced temporary forgiveness programs for March 2020. This MarketWatch article provides an illustrative sample of popular news coverage on pre-emptive supply-side credit policies at the time [link]. Anecdotally, lenders ceased idiosyncratic delinquency waivers after the introduction of the CARES Act.

[20]Using a constructed series of redistributed national accounts data, Blanchet et al. (2022) additionally find that UI distributions constituted about a third of monthly income for bottom 50% households (see Figure 8, in particular, of their paper).

a strong candidate explanation for these sensitivity drops. We return to this point in Section 5, where we utilize the staggered expiration of UI benefits to directly estimate the proportion of the delinquency-unemployment sensitivity drop that is attributable to UI.

One concern is that we may be measuring reductions in *reported* financial distress instead of actual financial distress: creditors may have simply not reported delinquencies during the pandemic. As a data validation check, panels (c) and (d) of Figure 6 reestimates Eq. 1 for student loans and mortgages, where we know delinquencies were not reported. Looking first at student loans in Panel (c), while we see similar (though more muted) pre-pandemic cyclicality to credit cards and auto loans, we see consistently near zero sensitivity during the pandemic. We interpret this as a useful check on our data: due to the student loan payment moratorium, we should indeed see no reported delinquencies. Panel (d), covering mortgages, also provides a similar validation check as a federal mortgage forbearance policies were in effect between March 2020 and August 2021. In this case, however, our estimates are relatively small rather than zero. This finding reflects two factors. First, not all mortgages were necessarily subject to forbearance policies; the CARES Act policy only applied to federally-backed mortgages, such as those through Fannie Mae, Freddie Mac, Veterans Affairs, or the Federal Housing Administration. While some private mortgage servicers may have followed the federal policy, in the data we see some servicers reporting delinquencies during the pandemic. As of 2018, federally-backed mortgages reflected about 70% of all mortgages (Housing Finance Policy Center, 2020); we therefore interpret this percentage as a lower bound on the number of mortgages potentially affected by forbearance. Secondly, forbearance policies were enacted upon request rather than automatically through servicers: while we do not observe forbearance enactment for individual mortgages, incomplete take-up of this option may further explain nonzero estimated sensitivity. Even despite these two factors, however, we see a large and relatively consistent drop in the mortgage delinquency-unemployment sensitivity during the pandemic.[21]

Next, we probe our estimates for robustness. For our results so far, we follow the typical definition and define loans as delinquent if they are over 30 days past due. A reasonable question for the financial stabilization interpretation is whether our results largely reflect continued nonpayment on older loans or new nonpayments. To examine this point, we re-estimate Equation 1 by instead using the shorter term 30-89 day delinquency rate to better capture short-run nonpayments. The results are qualitatively very similar; auto loan sensitivity seems largely driven by short-term delinquencies, while credit cards are more evenly split between short and longer-term delinquencies (our estimated levels and drop are about half of the previous all-delinquency estimate). One other concern with our outcome variable construction is that our results could be mechanically driven by demand-side responses for additional credit during the pandemic: if consumers take out additional loans, then the aggregate delinquency rate (delinquencies as a fraction of all loans) would mechanically decrease. Figure 8 thus re-estimates Equation 1 but replaces the delinquency rate

[21]Our estimates for mortgages increase substantially towards the end of our sample period, possibly reflecting the fact that the mortgage forbearance program ended in August 2021.

with new loans per capita, disaggregating into auto loans and credit cards. We find little evidence of compensating loan count increases that would drive our results: while some point estimates for credit cards are statistically significant, they are largely precisely estimated near zero. Indeed, the largest estimates for credit cards imply a 0.005 increase in loans per capita for each percentage point increase in the unemployment rate.

5 Event-Study Evidence from Staggered the Pandemic UI Phase-Out

To what extent do these reductions in the delinquency-unemployment sensitivity reflect UI versus other contemporaneous Covid policy changes? In this section, we disentangle these effects by exploiting the aforementioned staggered loss of benefits for UI claimants across states between July and September 2021.[22] These withdrawals happened relatively quickly: looking across the 22 states that withdrew from federal UI programs in June, public announcements typically gave a month or less of forewarning for the policy change. These withdrawals were unlikely to have been driven by local government budgetary conditions: the federal program would have expired in September regardless, and all spending on UI benefits was covered by federal funds.

A common public interpretation was that the withdrawals were motivated by political considerations rather than labor market conditions, consistent with other research that highlights the role of political polarization as impetus for recent state-level policy changes (DellaVigna and Kim, 2022). Indeed, an illustrative public announcement from Gov. Brad Little of Idaho signalled broader ideological opposition to continued UI benefits, saying in mid-May that his *"decision* [*was*] *based on a fundamental conservative principle – we do not want people on unemployment"* [link]. Reflecting this consideration, 21 of the 22 early withdrawal states were led by Republican governors; the sole Democratic governor, John Bel Edwards of Louisiana, led a largely Republican-leaning state (58.5%-39.9% Republican-Democrat vote shares during the 2020 presidential election).

We exploit the sharp timing of these changes in an event study framework to examine how UI withdrawal affected the delinquency-unemployment sensitivity. The key variation is across different states' month of exit from federal UI policies: given that these withdrawals were politically motivated, we see these events as plausibly uncorrelated with local credit market conditions. Following our previous specification, we estimate a dynamic event study variation of Equation 1 that also includes state-by-month fixed effects:

$$\text{Delinquency Rate}_{c,t,m} = \delta_{s(c),t} + \sum_{\tau}^{T} \beta_t \, UR_{c,t} \cdot D_{s(c),\tau} + \varepsilon_{c,t} \tag{2}$$

where now $D_{s(c),\tau}$ is an indicator that equals one if county c in state s withdrew from federal pandemic UI programs in month τ. In all regressions, we use a balanced sample of counties and plot

[22] Coombs et al. (2022) use the same variation in related work to examine employment and earnings responses in payroll-linked banking data, finding relatively small increases in job-finding rates and aggregate earnings increases of $900 million for benefit-losing workers in early withdrawal states. These workers also lost access to about $7.6 billion total in UI transfers, however, constituting a substantial aggregate net loss in income for affected households.

estimates for 6 months before and 5 months after the policy change to allow for visual inspection of pre-trends. As before, we again disaggregate delinquency rates by loan type to ensure comparability over time and to the previous set of results.

We present our estimates for auto loans and credit cards in Figure 9. As before, we begin by discussing results for the first two categories. We see little evidence of pre-trends for auto loans or credit cards: point estimates before state-level UI withdrawals are near zero and statistically insignificant. Moreover, both credit types show a sharp effect of withdrawal on the delinquency-unemployment sensitivity: after about 4 months, the estimated sensitivity increases by about 0.2 percentage points (or 68%) for auto loans and 0.13 percentage points (144%) for credit cards. These treatment effects are qualitatively quite large, constituting 68% and 144% increases respectively compared to the month before withdrawal.

We now compare these treatment effects to the total sensitivity drops in Panels (a) and (b) of Figure 6. There, the sensitivity change after the introduction of Covid policies is about -0.15 for credit cards and -0.2 for auto loans. If we assume UI withdrawal had similar or symmetric effects on local financial stabilization to pandemic UI introduction, our phase-out estimates imply that the UI channel represents the vast majority of the total stabilization arising from Covid pandemic policies: almost all of the auto loans sensitivity drop, and 86% of the credit card sensitivity drop. Given the substantial amount of relief policies passed during the pandemic, both directly within the credit market (mortgage forbearance, the student loan payment moratorium) and in providing stimulus (e.g., economic impact payments and the expanded Child Tax Credit), we interpret this as strong evidence for substantial aggregate financial stabilization provided by the unemployment insurance system.

We conclude this section by considering three potential extensions and robustness checks for our estimates. In Figure 10, we re-estimate regressions for auto loans and credit cards using a 30-89 day delinquency measure to assess the extent to which our estimates may reflect newer or older nonpayments. Our results largely mirror the previous discussion of Figure 7: while the short-term auto loans estimates are about 2/3 of the total sensitivity increase (about 0.14 of the previous 0.2 increase after 4 months), about half of our credit card estimate appears to be driven by shorter-term delinquencies. We also again test whether our estimated sensitivity changes could be driven by demand-side changes in the number of loans taken out by consumers. Figure 11 estimates the effect of the phase-outs on the per-capita number of loans in each county. As before, our estimates are economically and generally statistically insignificant: the largest estimates, for auto loans after 4 months, imply a 0.002 change in per capita loans after a 1 percentage point change in the unemployment rate.

6 How Did Pandemic UI Affect Aggregate Delinquencies?

We conclude our analysis by providing a back-of-the-envelope calculation for the amount of aggregate delinquencies prevented by federal UI policies during the Covid pandemic. Our framework is motivated by our previous intuition for macro effects of UI: since benefit expansions provide increased liquidity to harder-hit counties, they effectively attenuate aggregate delinquency responses to unemployment shocks. To construct a macro counterfactual, we should thus reset the aggregate delinquency-unemployment sensitivity to empirical pre-pandemic levels, and calculate the difference between observed and otherwise predicted delinquencies over time. We illustrate these ideas, first in a simplified way and using Figure 12 as a visual aid. Panel (a) starts with a stylized reproduction of Figure 5, the empirical delinquency-unemployment relationship before and during the pandemic. As represented in panels (b) and (c), under a simplified attenuation framework UI policies can only impact delinquencies through a change in the curves' slope. Differences in intercepts thus reflect other existing Covid policies, such as stimulus checks or CTC expansion. Panel (d) illustrates our proposed calculation for aggregate delinquency effects: after removing intercept differences, the distance between the pre-Covid and during-Covid curves represent the prevented delinquencies at each value of the unemployment rate. We can thus sum across unemployment rates to yield the total number of delinquencies prevented.

We extend these base ideas to a fully dynamic framework, just as before in Section 4. One complication is that delinquencies are not an absorbing outcome, so a delinquency prevented in a given month does not imply that the delinquency cannot occur later on. We thus compute delinquency-months as our preferred measure of prevented financial distress. Our implementation proceeds in several steps. First, we re-estimate an augmented form of Equation 1:

$$\text{Delinquency Rate}_{s(c),k,t} = \delta_{s(c),t} + \alpha \mathbb{1}(t \in [\underline{\tau}, \bar{\tau}]) + \beta_t \, UR_{c,t} + \tilde{\beta} \mathbb{1}(t \in [\underline{\tau}, \bar{\tau}]) \, UR_{c,t} + \varepsilon_{c,t} \tag{3}$$

where $[\underline{\tau}, \bar{\tau}]$ is a shorthand for the Covid UI period, between March 2020 and August 2021. The first new coefficient in our estimation, α, provides for a level shift in delinquencies after the introduction of pandemic UI. The second term, $\tilde{\beta}$, separately estimates a direct shift in the delinquency-unemployment sensitivity in the same period. In essence, we will "turn off" these pandemic policy effects to construct our counterfactual delinquency series. Note that this way of constructing counterfactuals is quite conservative: We assume that increased UI generosity does not have any effect after August 2021, an assumption which undercounts prevented delinquencies if expanded UI benefits allowed households to build up precautionary savings. We use these estimates to construct two new monthly series for our counterfactual calculations, as seen in Figure 13. We begin with Panel (a), which proceeds for auto loans. The blue line plots fitted values from Equation 3, representing the estimated evolution of the delinquency rate. As a reassuring check on our estimation, this series roughly matches the dynamics of actual observed delinquencies over time (grey line). The red line, however, instead plots fitted values where the α and $\tilde{\beta}$ effects are removed from the blue line between March 2020 and August 2021. This second series thus represents a designed counterfactual where we have removed the effect of federal Covid policies. We can then calculate the number of monthly prevented delinquency-months as the difference between our estimated counterfactual (red) and estimated status quo (blue) series for each month, multiplied by the number of loans for that credit type in our data. To arrive at a total sum for delinquency-months prevented, we simply sum this measure over the Covid UI period, between March 2020 and August 2021.

This back-of-the-envelope calculation delivers stark results. For credit cards, we estimate that UI prevented about 59.3% of all potential delinquency-months in this time frame; for credit cards, we estimate a slightly larger net effect of about 59.6% of potential delinquency-months. While these effects are quite large, this came at a price: total federal pandemic UI program spending was about $674 billion[23], implying a cost of about $8,864 per delinquency-month prevented across the two credit types. Note that this estimate computes the direct cost; our results cannot identify the effects on other types of credit, overall credit smoothing, or aggregate spending-side responses that would all mitigate the final cost figure.

As a last step, we briefly review the robustness of our results to estimation design. One potential consideration is that comparisons across counties within a state-month are problematic due to county-level heterogeneity in responsiveness over time, and so within-county variation is better suited to our design. To address this, we re-estimate our results by replacing our state-month fixed effect with separate county and month fixed effects and reproduce our previous results in Appendix A as Figures A3-A8. Our estimates are qualitatively quite similar: we again find reduced seasonality and a large drop in the delinquency-unemployment sensitivity during the pandemic, though the drops here are larger in percentage terms (Figure A3). We also find a clear effect of the phase-out on the delinquency-unemployment sensitivity (Figure A6), though now somewhat smaller than our previous state-month fixed effect estimates. Altogether, these differences lead to a qualitatively similar conclusion that UI policies instead explain about 60% of the total delinquency-unemployment sensitivity drop during the pandemic (Figure A8). Though we prefer our prior estimates as better absorbing confounding state-level policies, we view this replication as broadly similar and reassuring evidence that our estimates are indeed quite robust.

7 Conclusion

In this paper, we use administrative credit bureau data to investigate the local financial effects of UI benefit expansions during the Covid-19 pandemic. At the micro level, if UI provides targeted liquidity to financially constrained households, then expansions should attenuate delinquency responses to unemployment. At the macro level, expanded UI represents large injections of liquidity into areas hit with adverse economic shocks and can be thought of as rapid counter-cyclical fiscal policy at the local level, targeted towards populations with potentially high marginal propensities to consume. Therefore, any micro stabilization might actually *understate* the effect of UI on aggregate economic conditions. We overcome this problem by directly estimating whether increasing the generosity of the UI system insulates aggregate financial conditions from economic shocks. We have three main findings. First, we estimate 50-66% reductions in the county-level delinquency-unemployment sensitivity after the introduction of Covid policies, driven both by changes in new

[23]Taken from Department of Labor official calculations of federal pandemic UI spending, available here.

delinquencies and continued nonpayment on existing delinquencies. Furthermore, this finding is qualitatively robust to placebo tests on unaffected credit types and demand-side responses.

At the same time, our first design cannot disentangle the effects of UI policies from other contemporaneous policies that would have also mitigated unemployment shocks. We thus next leverage the late 2021 staggered phase-out of federal UI to isolate the UI-specific component of the pandemic sensitivity drop. We estimate large sensitivity increases after UI withdrawal using a dynamic event study design, finding a 68-144% increase in sensitivity after 4 months (compared to the month before withdrawal). We find no evidence of pre-trends, supporting a casual interpretation of our results. As before, we again find that this result is robust to placebo tests and demand-side changes. Assuming that changes in the delinquency-unemployment sensitivity are symmetric with respect to UI expansions, our estimates imply that over 86% of our prior estimated Covid-era sensitivity drop is attributable to UI policies.

How should we think about these results in terms of delinquencies rather than sensitivities? In a last step, we assess the aggregate implications of our results and calculate the total delinquency-months prevented by UI policies. Using a simple framework to construct counterfactual delinquencies over the pandemic, we estimate that UI expansions prevented about 59% of potential delinquency-months for a cost of $8,864 per month. While this suggests that preventing any one delinquency month was very costly, these financial stabilization effects are in addition to the effects on micro-level household welfare and the aggregate *spending* effects. Ganong et al. (2022) estimate that the $600 and $300 dollar supplements boosted aggregate spending by 2.9% and 1.3%, respectively. Our estimates show that beyond the immediate effect of UI on aggregate demand, Covid-era expansions of UI also substantially stabilized aggregate financial conditions.

The Covid-19 pandemic saw unprecedented and prolonged increases in unemployment. Our results imply that UI policies were enormously successful in attenuating corresponding delinquency increases at the aggregate level. Ganong et al. (2022) show that the adverse labor market effects of UI expansion were small while the aggregate spending effects were large, a result mostly driven by the fact that substantial fraction of UI recipients seem to be high-MPC *types* rather than households with temporarily high MPCs because of liquidity constraints. This is consistent with our result that financial conditions became more sensitive to unemployment rates as soon as the UI expansions expired. Ganong et al. (2022) argue that their results suggest that front-loading of expanded benefits might be optimal policy in terms of trading off stimulating demand and increasing disincentives to work. Our results can be read as cautionary evidence that such front-loading may come at the cost of under-stabilizing financial conditions compared to smoother payout paths of UI supplements, presumably at levels that do not lead to median replacement rates substantially above 100%. An analysis of how to optimally trade off these two effects is a promising avenue for future research.

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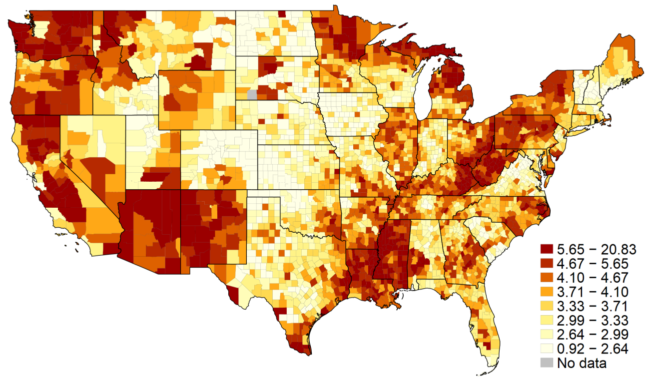
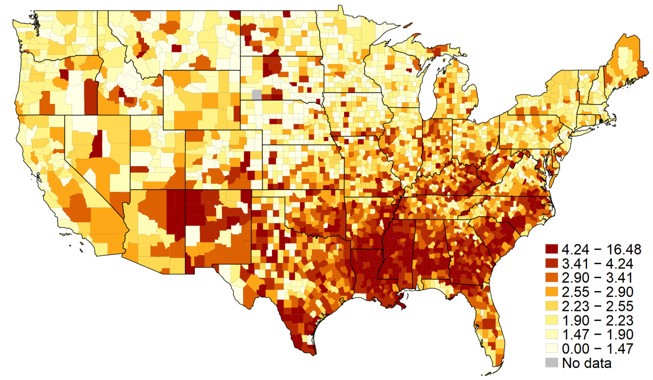
8 Tables and Figures

Figure 1: The Geography of Delinquency, Before and During the Pandemic

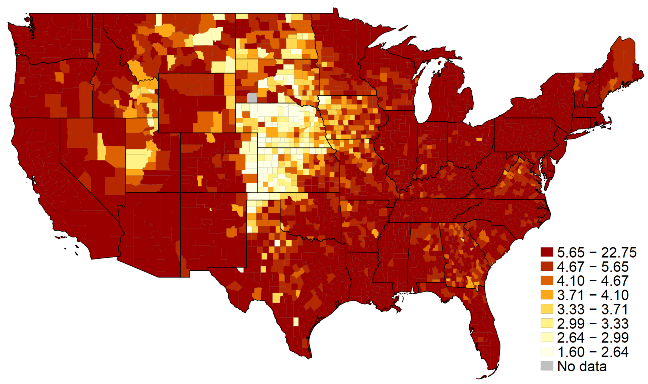
(**a**) Unemployment Rates, 2019



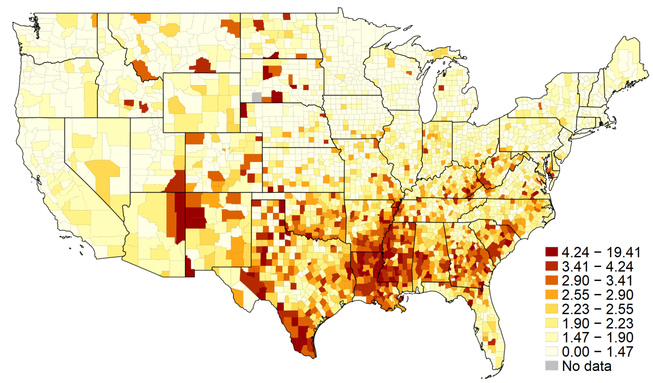
(**b**) All Delinquencies, 2019



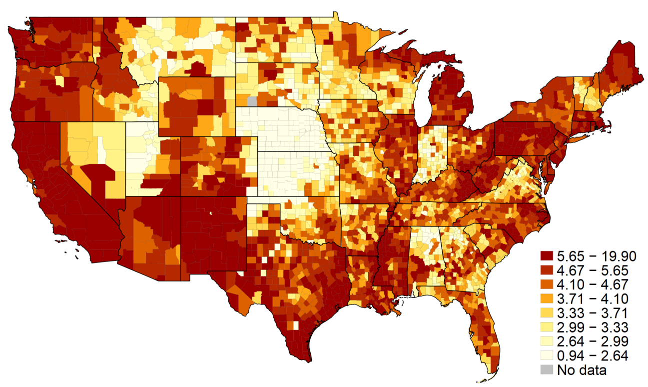
(**c**) Unemployment Rates, 2020



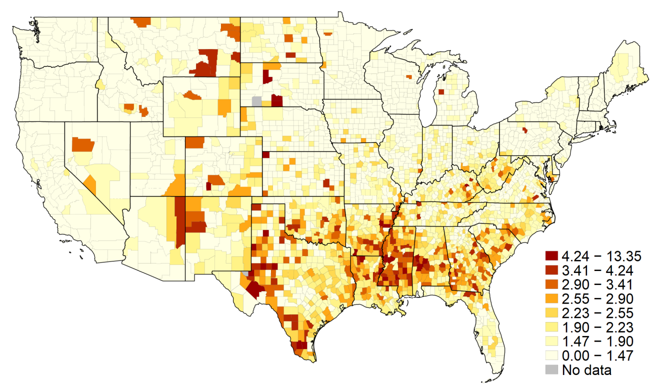
(**d**) All Delinquencies, 2020



(**e**) Unemployment Rates, 2021

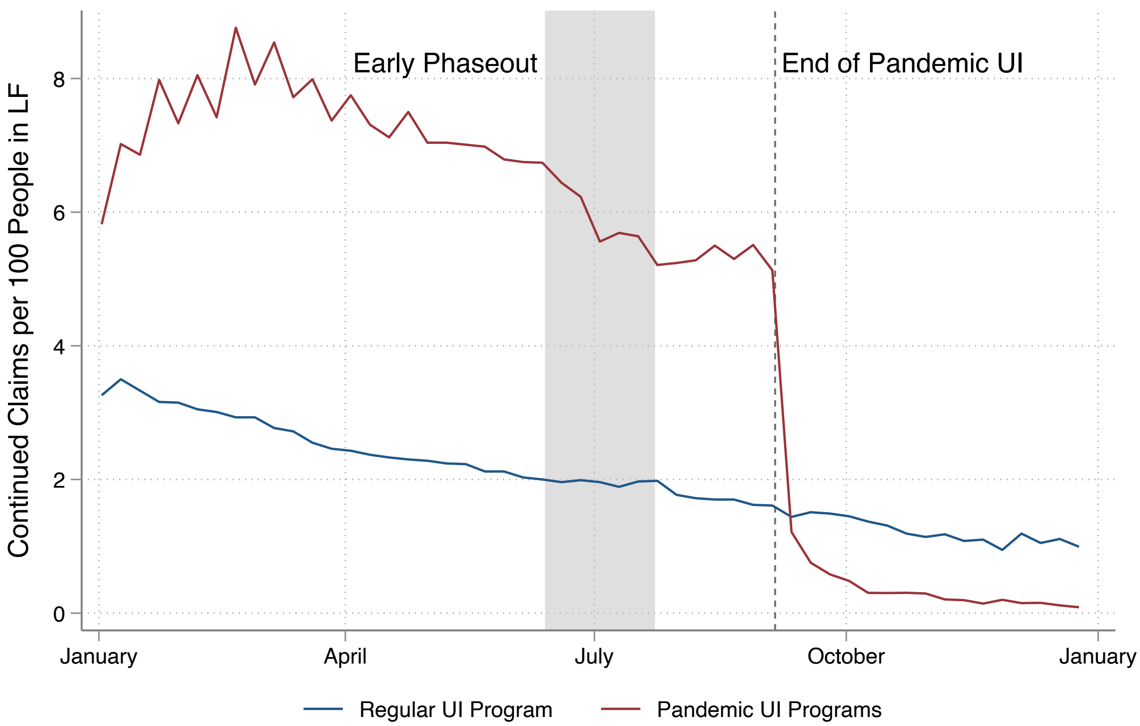


(**f**) All Delinquencies, 2021



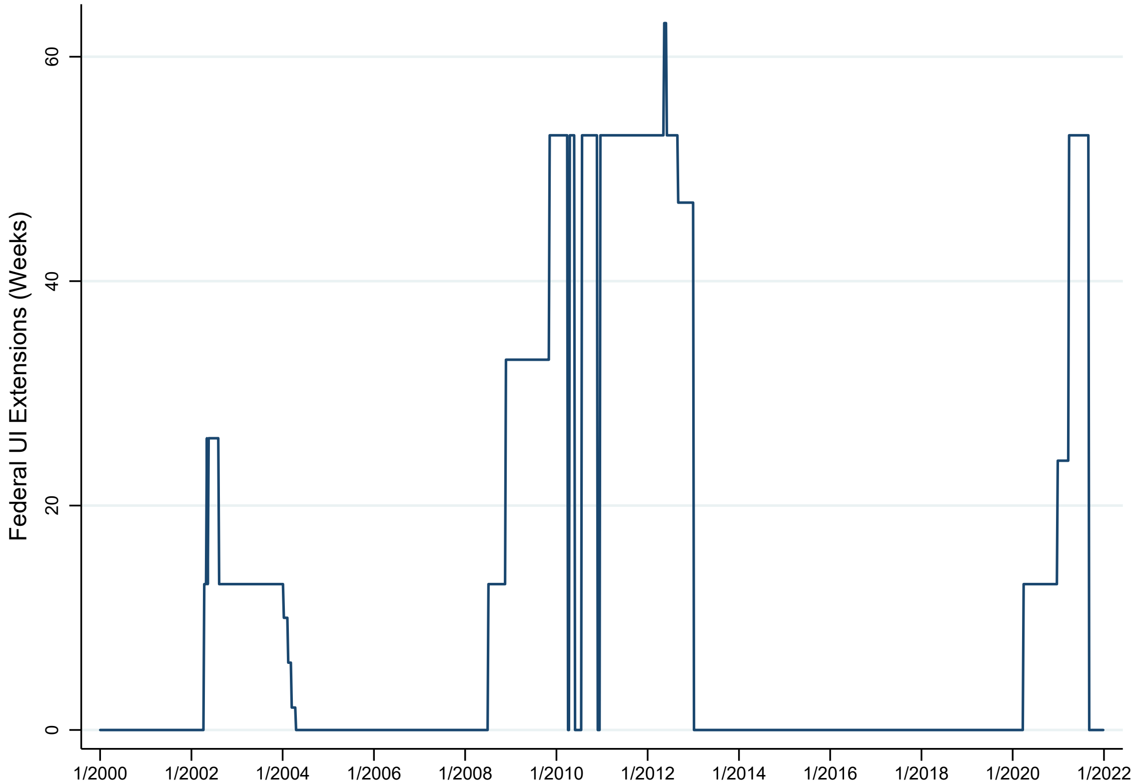
Notes: This figure graphs mean county-level delinquency and unemployment rates between 2019 and 2021. Shading for each measure represents 8 equally-spaced bins for 2019 values. Delinquency rates are constructed using our county-month aggregation of credit bureau microdata; more details on data construction can be found in Section 3.

Figure 2: The 2021 UI Phase-Out



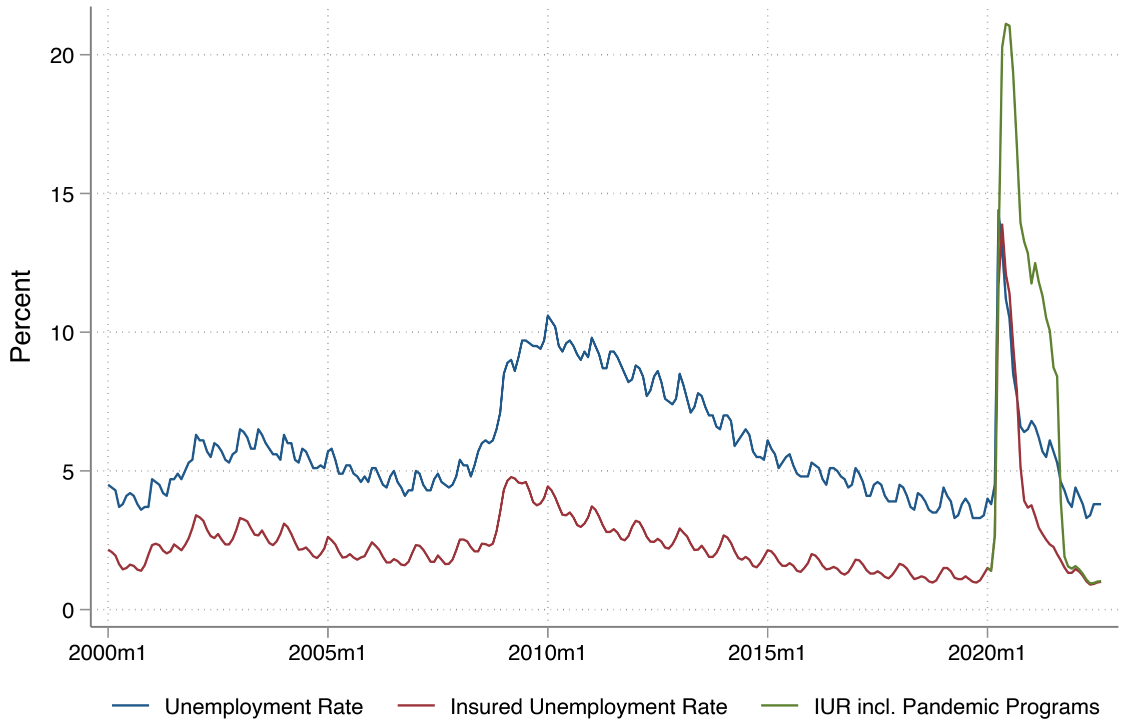
Notes: This figure plots continuing UI claims over time, for each week between 1/1/2021 and 1/1/2022, to highlight the stark drop in UI claimants after state-level withdrawals from federal pandemic UI programs. See Sections 2 and 5 for more details on the underlying policy variation. Our calculations are based on the Department of Labor's ETA 539 Weekly Claims data.

Figure 3: Federal UI Duration Extensions, 2000-2021



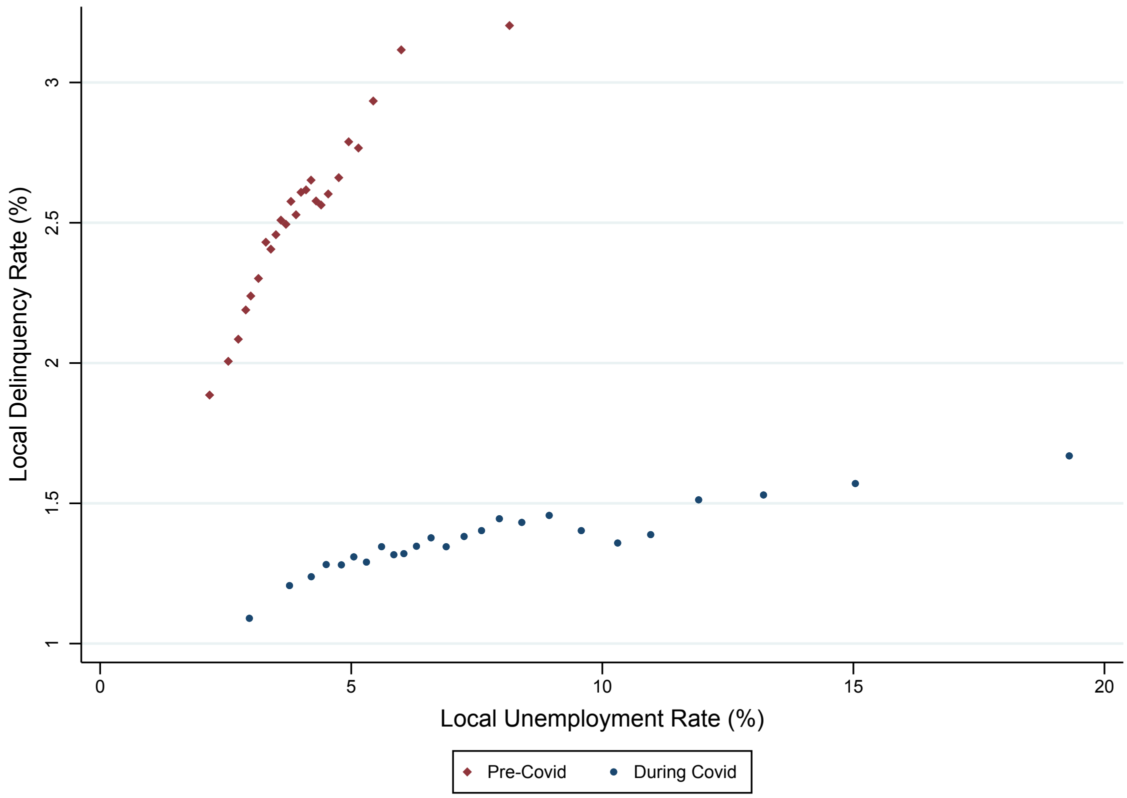
Notes: This figure plots the number of maximum total federal UI weeks available to new initial claimants for each week between January 2020 and December 2021. Importantly, this figure plots only federal weeks available to claimants: UI recipients could also access upto 48 total additional weeks from state-specific UI programs, depending on whether UI trigger policies were in effect.

Figure 4: Insured and Total Unemployment Rates, 2000-2021



Notes: This figure plots the unemployment rate (UR), insured unemployment rate (IUR), and our constructed insured unemployment rate including pandemic programs. Our constructed series adjusts for the large expansions of UI eligibility during the pandemic through the federal PUA program. The first two series are from the Bureau of Labor Statistics. We construct the all-programs insured unemployment rate here by 1) computing the ratio of all-programs (regular UI, PEUC, PUA) and continuing claims weeks (which include both regular claims and special federal programs), 2) multiplying by the regular IUR.

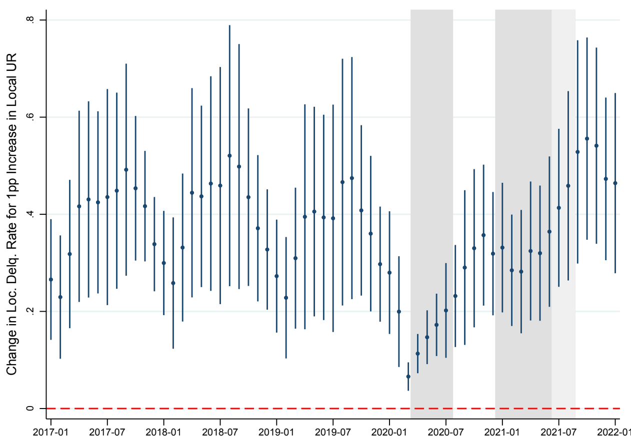
Figure 5: Delinquencies vs Unemployment, Before and During Covid



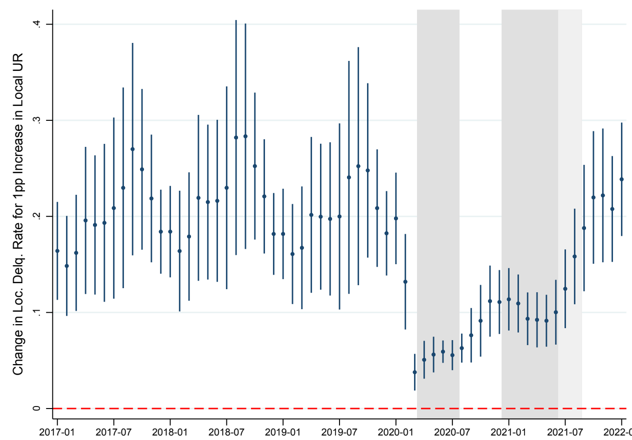
Notes: This figure displays a large attenuation in local responsiveness to unemployment rate shocks following the introduction of Covid policies. We perform a binned scatterplot of county-level any-loan delinquency rates against county-level unemployment rates, separately using county-months from January 2018 to February 2020 (red) and again using March 2020 to August 2021 (blue). Delinquency rates are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure 6: Delinquency-Unemployment Sensitivity Over Time

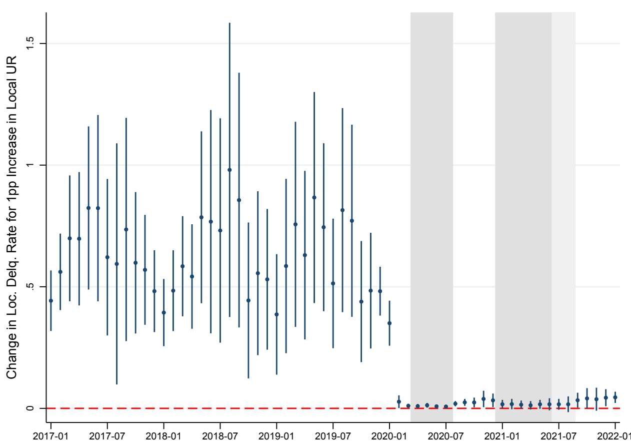
(**a**) Auto Loans



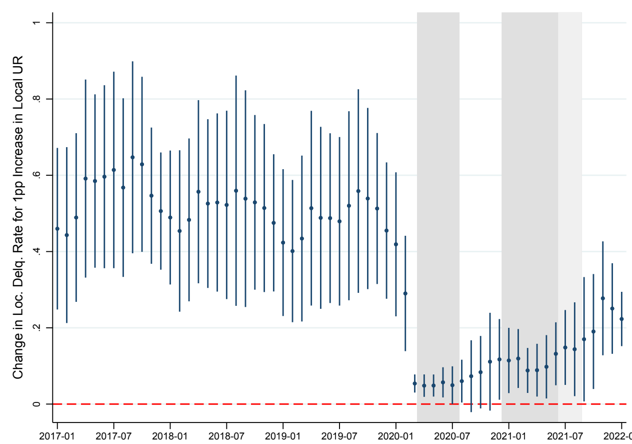
(**b**) Credit Cards



(**c**) Student Loans



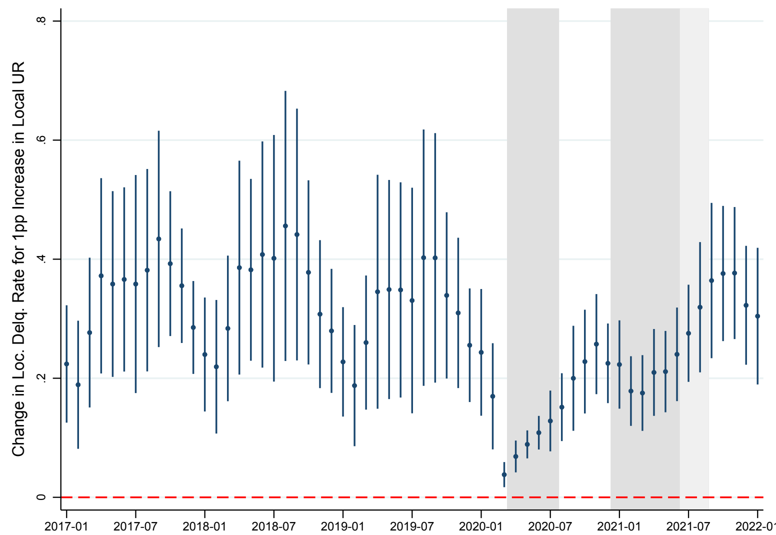
(**d**) Mortgages



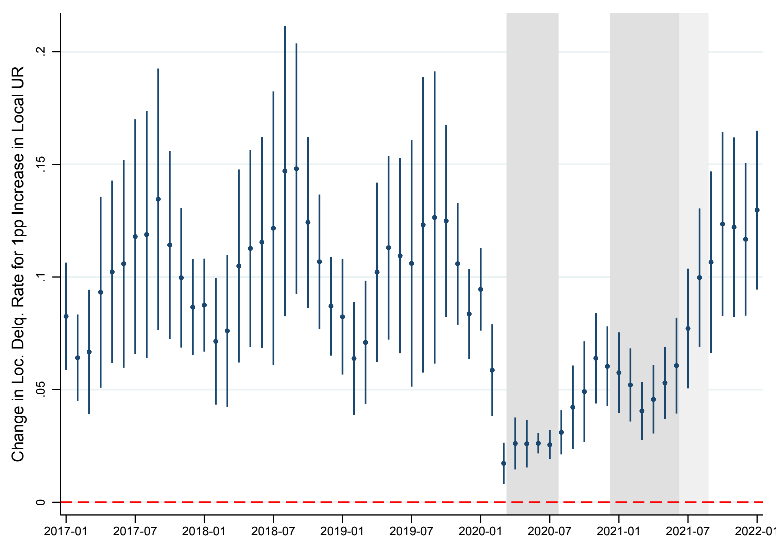
Notes: This figure shows the evolution of the estimated delinquency-unemployment sensitivity for each month between January 2017 and March 2022, separately for different credit types. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_\tau^T$ from our estimation of Equation 1. Delinquency rates are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure 7: Short-Term Delinquency-Unemployment Sensitivity Over Time

(**a**) Auto Loans, 30-89 Day Delinquencies



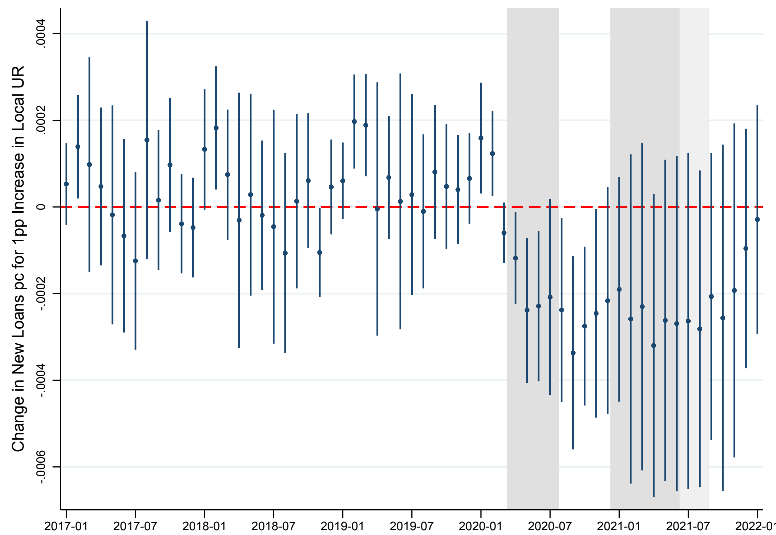
(**b**) Credit Cards, 30-89 Day Delinquencies



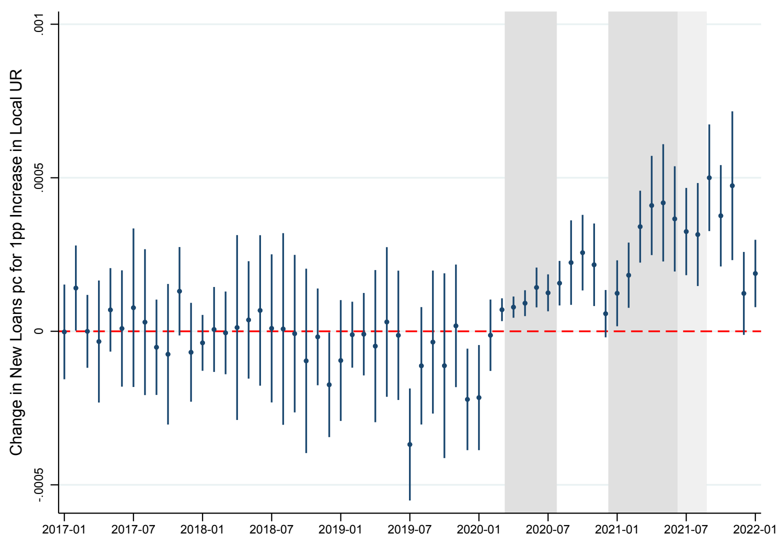
Notes: This figure shows the evolution of the estimated delinquency-unemployment sensitivity for each month between January 2017 and March 2022, separately for different credit types. In comparison to the previous figure, here we use the short-term 30-89 day delinquency rate as the dependent variable. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_\tau^T$ from our estimation of Equation 1. Delinquency rates are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure 8: New Loan Responses to Local Unemployment Shocks

(**a**) New Auto Loans



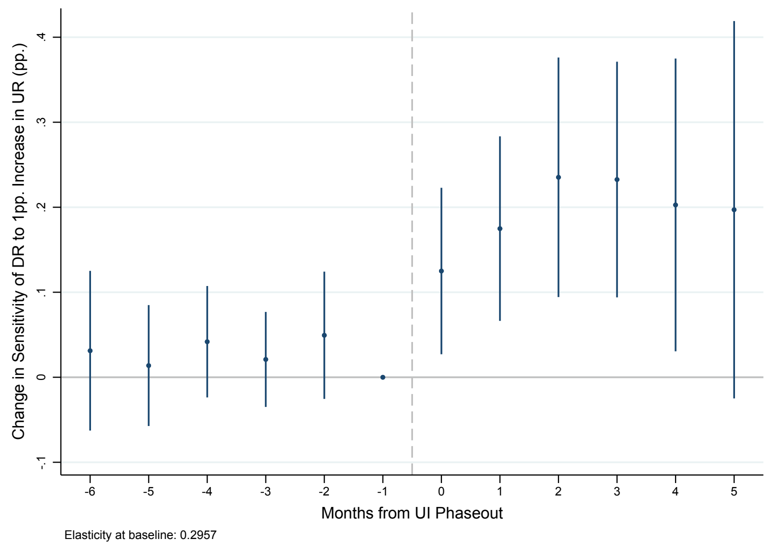
(**b**) New Credit Cards



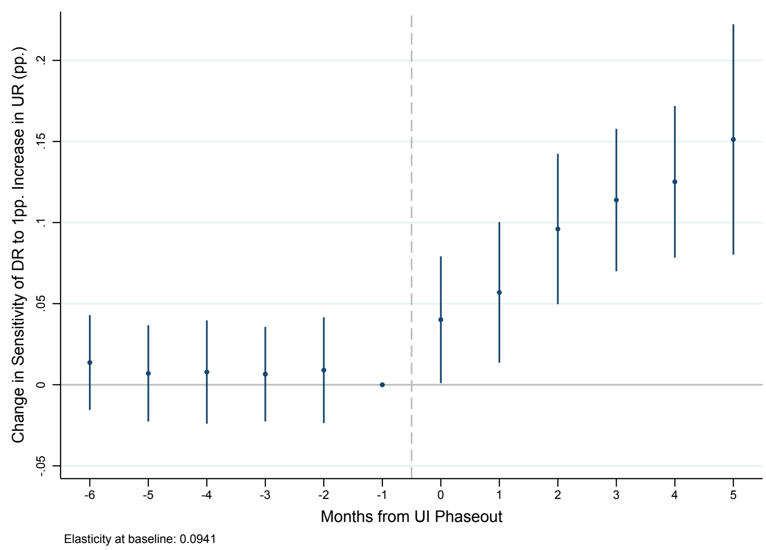
Notes: This figure assesses potential demand-side responses to local unemployment shocks, separately for each month between January 2017 and March 2022. In comparison to the previous figure, here we use the change in the number of per-capita loans (disaggregating into auto loans and credit cards) as the dependent variable. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_\tau^T$ from our estimation of Equation 1. New loans for each credit type are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure 9: Effects of UI Phase-Out on Delinquency-Unemployment Sensitivity

(**a**) Auto Loans



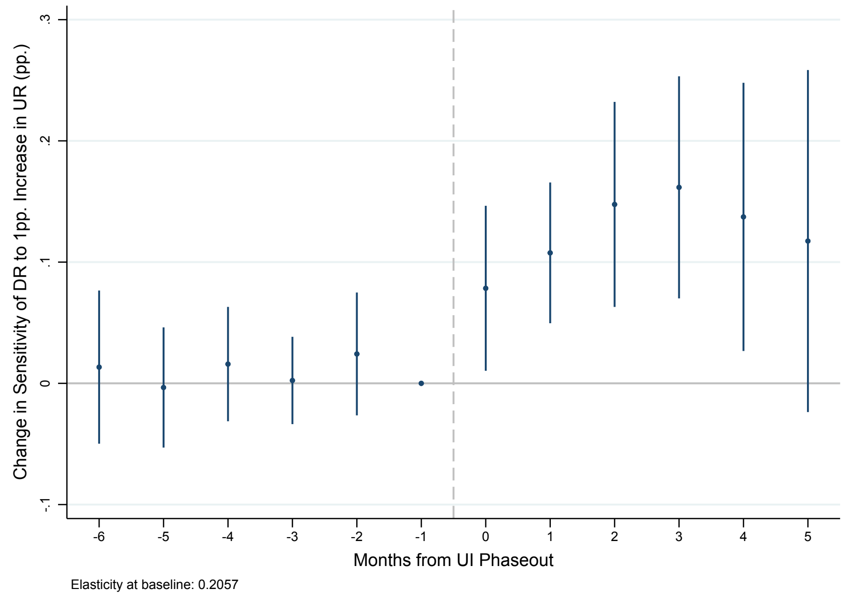
(**b**) Credit Cards



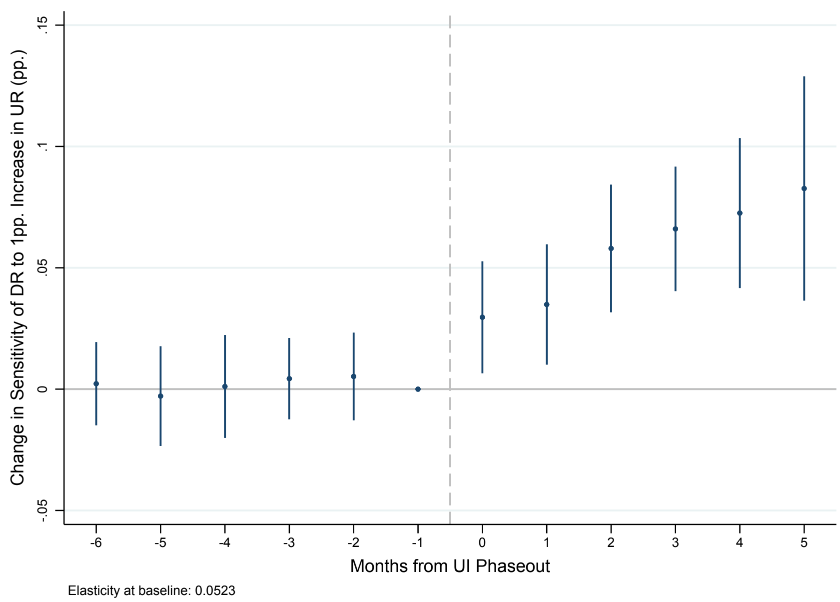
Notes: This figure assesses the impacts of federal UI withdrawals using a staggered event study design, leveraging the fact that different states withdrew at different times. In comparison to the previous monthly sensitivity graphs, here we estimate the effect of withdrawal on the delinquency-unemployment sensitivity (normalized to 0 in the period before withdrawal). More details on the estimation procedure and interpretation can be found in Section 5.

Figure 10: Effects of UI Phase-Out on Short-Term Delinquency-Unemployment Sensitivity

(**a**) Auto Loans, 30-89 Day Delinquencies



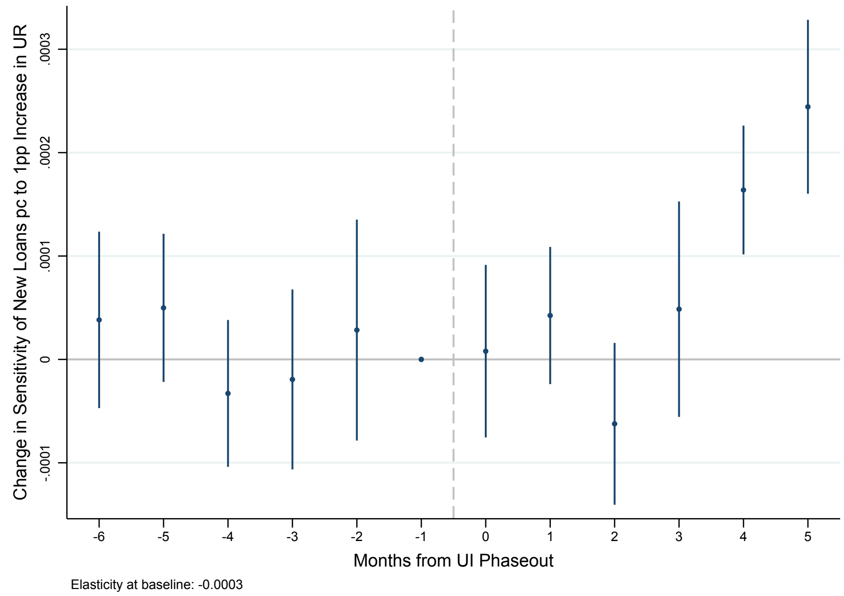
(**b**) Credit Cards, 30-89 Day Delinquencies



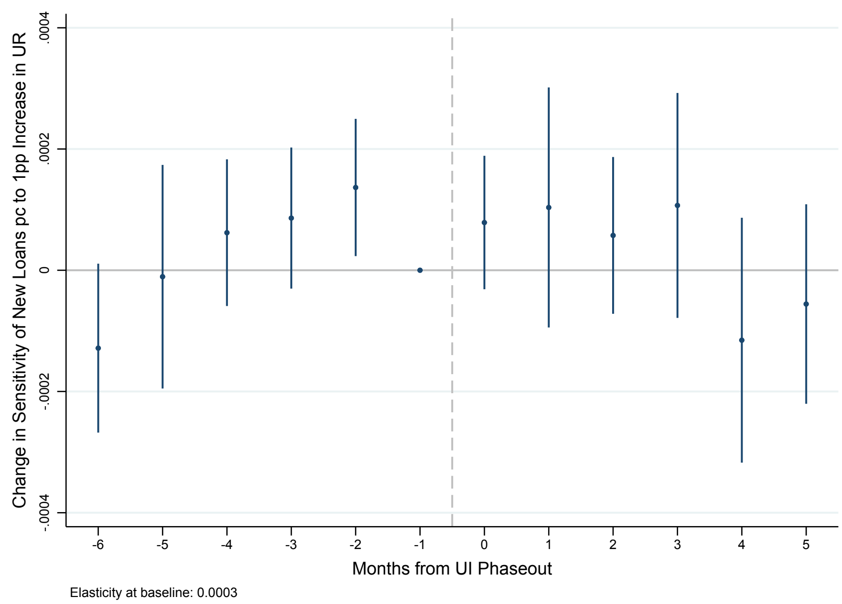
Notes: This figure assesses the impacts of federal UI withdrawals using a staggered event study design. In comparison to the previous monthly sensitivity graphs, here we estimate the effect of withdrawal on short-term delinquency-unemployment sensitivity (normalized to 0 in the period before withdrawal). More details on the estimation procedure and interpretation can be found in Section 5.

Figure 11: UI Phase-Out: New Loan Responses

(**a**) Auto Loans

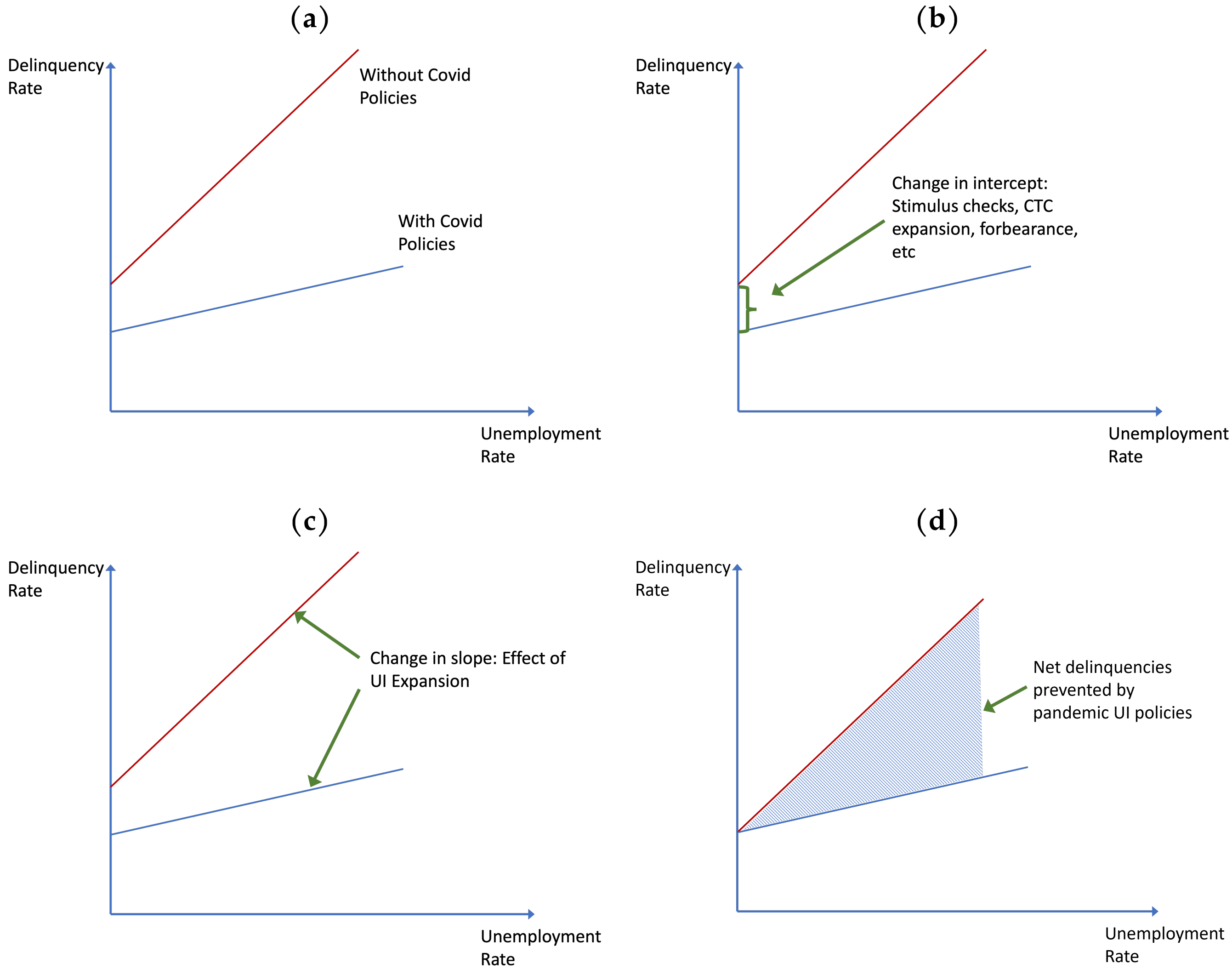


(**b**) Credit Cards



Notes: This figure assesses potential demand-side responses to state-level UI withdrawals. The outcome variable is the change in per capita new loans per percentage point change in the unemployment rate, relative to the period before withdrawal (-1, normalized to 0). More details on the estimation procedure and interpretation can be found in Section 5.

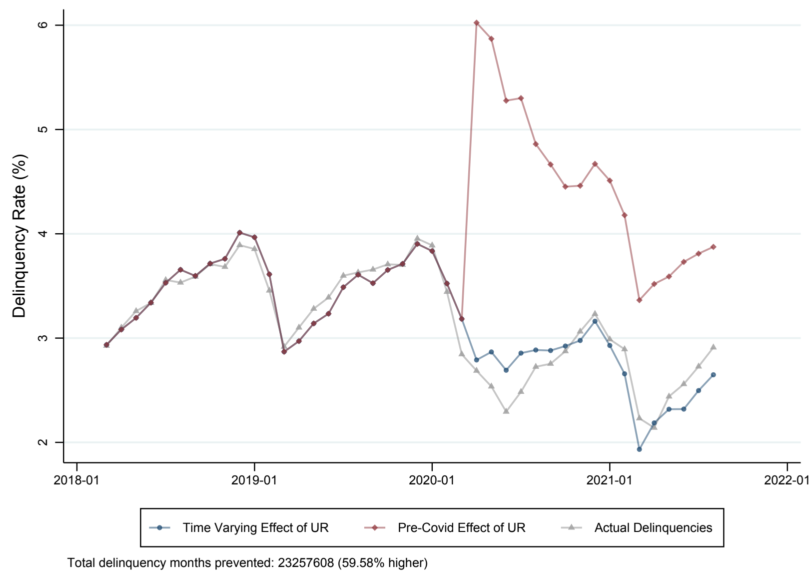
Figure 12: Construction of Counterfactual



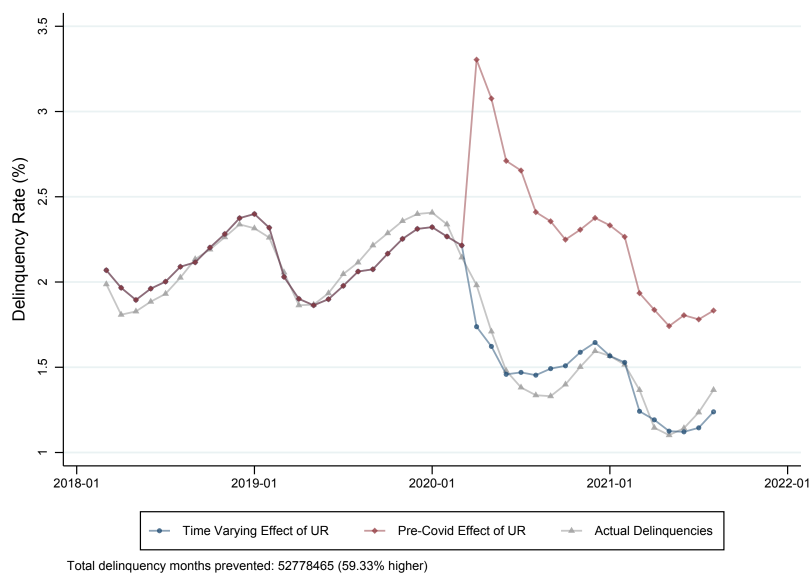
Notes: This figure presents a simplified visual aid to guide intuition for our counterfactual estimation procedure. Panel (a) starts with a reproduction of Figure 5, the empirical delinquency-unemployment relationship before and during the pandemic. As represented in panels (b) and (c), under a simplified attenuation framework UI policies can only impact delinquencies through a change in the curves' slope. Differences in intercepts thus reflect other existing Covid policies, such as stimulus checks or CTC expansion. Panel (d) illustrates our proposed calculation for aggregate delinquency effects: after removing intercept differences, the distance between the pre-Covid and during-Covid curves represent the prevented delinquencies at each value of the unemployment rate. We can thus sum across unemployment rates to yield the total number of delinquencies prevented. An expanded discussion of this figure and corresponding results can be found in Section 6.

Figure 13: Counterfactual Estimates, State-Month FE

(**a**) Auto Loans



(**b**) Credit Cards



Notes: This figure presents empirical, predicted, and counterfactual delinquency time series. The blue line plots fitted values from Equation 3 as an estimated evolution of the delinquency rate. As a reassuring check, this series roughly matches the dynamics of actual observed delinquencies over time (grey line). The red line, however, instead plots fitted values where the level and shift effects are removed from the blue line between March 2020 and August 2021. This second series thus represents a designed counterfactual where we have removed the effect of federal Covid policies. We can then calculate the number of monthly prevented delinquency-months as the difference between our estimated counterfactual (red) and estimated status quo (blue) series for each month, multiplied by the number of loans for that credit type in our data. To arrive at a total sum for delinquency-months prevented, we simply sum this measure over the Covid UI period, between March 2020 and August 2021, presented below each figure.

Table 1: Distribution of Key County-Level Variables

Variable	**Mean**	**p5**	**p10**	**p25**	**p50**	**p75**	**p90**	**p95**	**p99**
Labor Force									
2019	52,181	1,463	2,393	5,016	11,824	32,051	106,554	229,631	699,903
Pct Change, 2019-2020	.15	-3.2	-2.2	-.97	.12	1.2	2.5	3.3	6.7
Number of People									
2019	1331	37	63	140	329	873	2737	5761	16270
Pct Change, 2019-2020	-.9	-6.4	-4.9	-2.7	-.92	.7	2.7	4.6	9.2
Unemployment Rate									
2019	4.7	2.4	2.7	3.3	4.3	5.6	7.3	8.4	11
Change, 2019-2020	-.48	-1.2	-.98	-.72	-.45	-.22	-.025	.12	.49
DQ Share: Any Loan									
All Term: 2019	2.3	.73	.97	1.4	2.1	2.9	3.9	4.7	6.7
All Term: Change, 2019-2020	.075	-1.3	-.83	-.28	.082	.45	.98	1.4	2.5
Short Term: 2019	1.2	.4	.53	.77	1.1	1.5	2	2.3	3.2
Short Term: Change, 2019-2020	.075	-1.3	-.83	-.28	.082	.45	.98	1.4	2.5
DQ Share: Auto Loan									
All Term: 2019	3.4	.21	.94	1.9	2.9	4.4	6.2	7.8	12
All Term: Change, 2019-2020	-.069	-2.6	-1.6	-.64	-.019	.56	1.4	2.3	4.5
Short Term: 2019	2.7	0	.7	1.5	2.4	3.6	5.1	6.3	9.8
Short Term: Change, 2019-2020	-.069	-2.6	-1.6	-.64	-.019	.56	1.4	2.3	4.5
DQ Share: CC									
All Term: 2019	2	.44	.78	1.3	1.8	2.5	3.3	4	5.8
All Term: Change, 2019-2020	.15	-1.4	-.88	-.27	.13	.56	1.2	1.7	3.6
Short Term: 2019	1.1	.25	.44	.71	.99	1.3	1.7	2.1	3.1
Short Term: Change, 2019-2020	.15	-1.4	-.88	-.27	.13	.56	1.2	1.7	3.6
Number of Counties: 3,107									

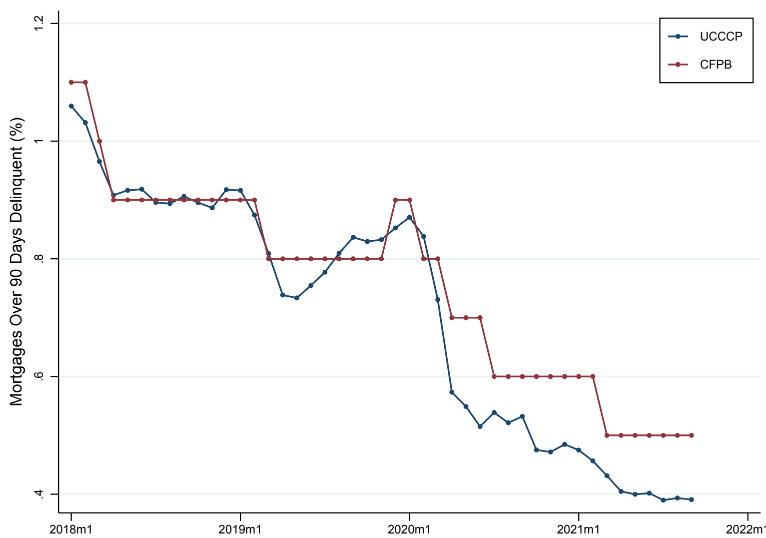
Notes: This table displays summary statistics for the balanced panel of counties in our analysis sample. The labor force size and unemployment rate are taken from the LAUS; person counts and delinquency shares are taken from our county-month aggregation of credit bureau microdata. See Section 3 for more information on the underlying data construction.

A Supplementary Figures and Tables

Figure A1: Comparison to CFPB Mortgage Delinquency Data

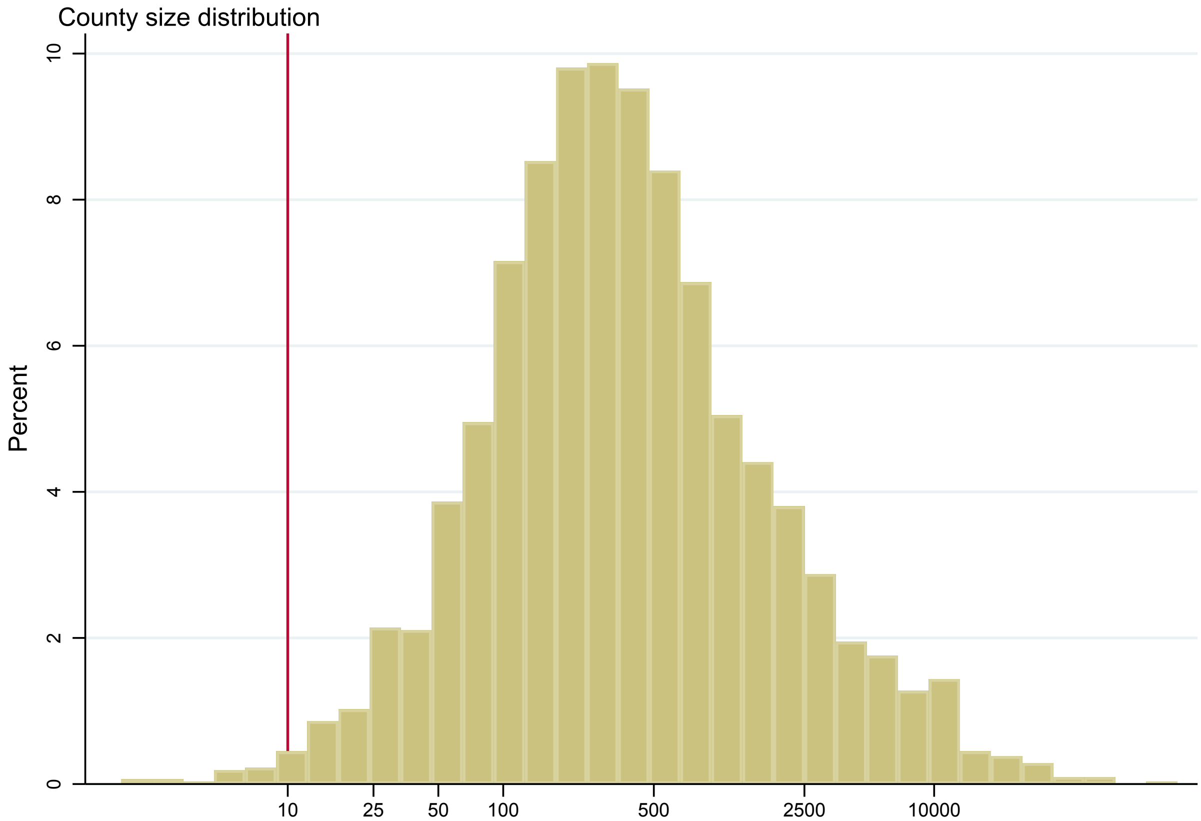
(**a**) Under 90 Days Delinquent

(**b**) Over 90 Days Delinquent



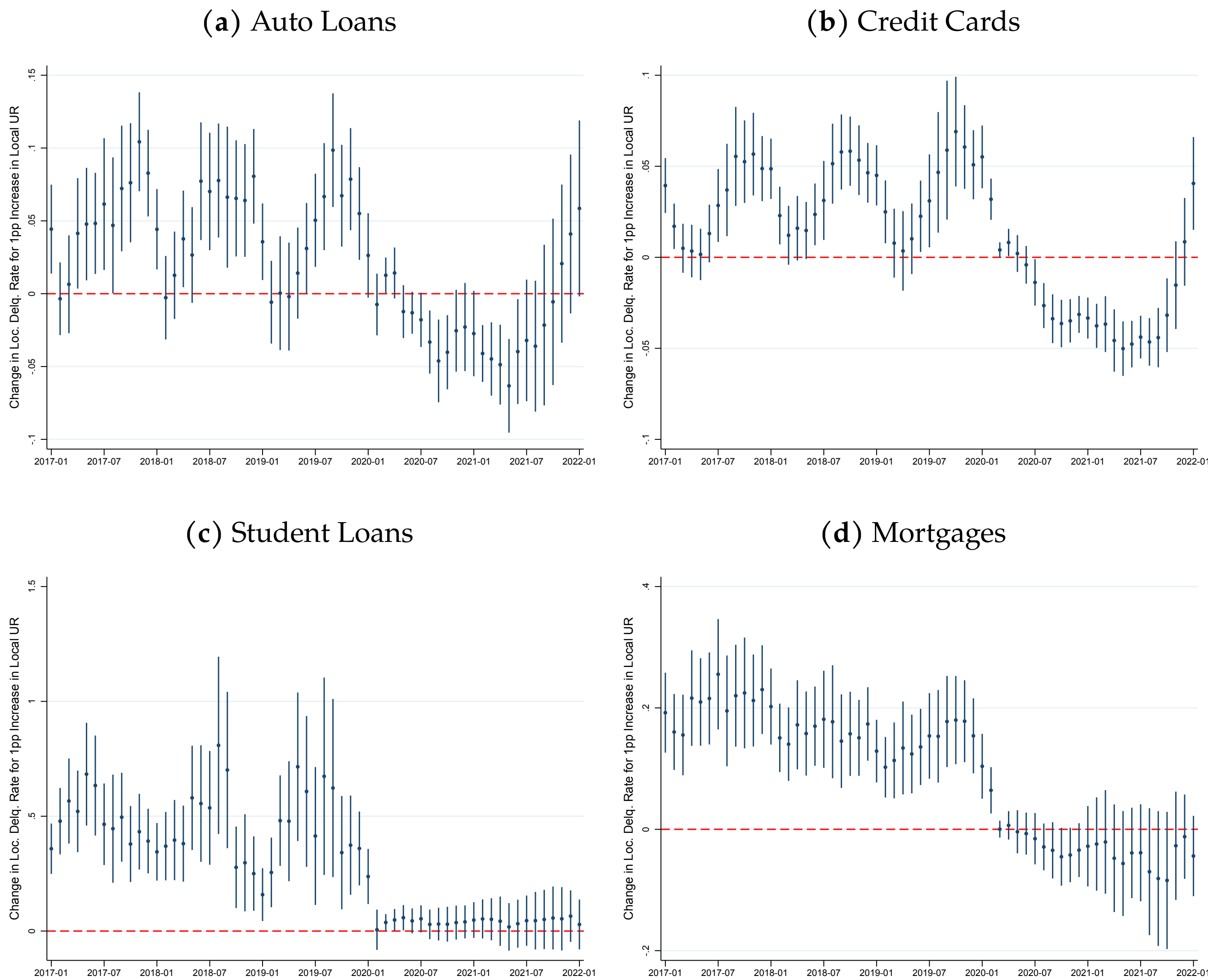
Notes: Our main analysis sample is a monthly reconstruction—using retrospective monthly loan payment status identifiers—of quarterly credit bureau archives. To validate our constructed data, we compare average mortgage delinquency rates in our microdata to public aggregates from the Consumer Financial Protection Bureau (other credit types are not available from the CFPB for a similar analysis). Small differences in Panel (b) are partially attributable to the CFPB's rounding of delinquency rates.

Figure A2: Distribution of Observed County Sizes



Notes: This figure shows the distribution of average county sizes (between 2017 and 2021) in our aggregated county-month analysis sample, before imposing a county size restriction. The horizontal axis is displayed in log scale (with corresponding level tick values). For our main analysis sample, we drop counties at the far left tail with less than 50 observed people on average between 2017 and 2021 (red line).

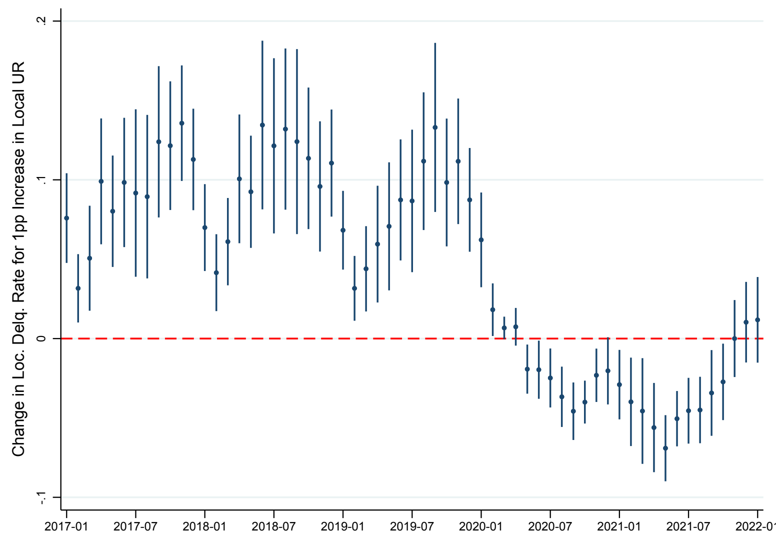
Figure A3: Delinquency-Unemployment Sensitivity Over Time



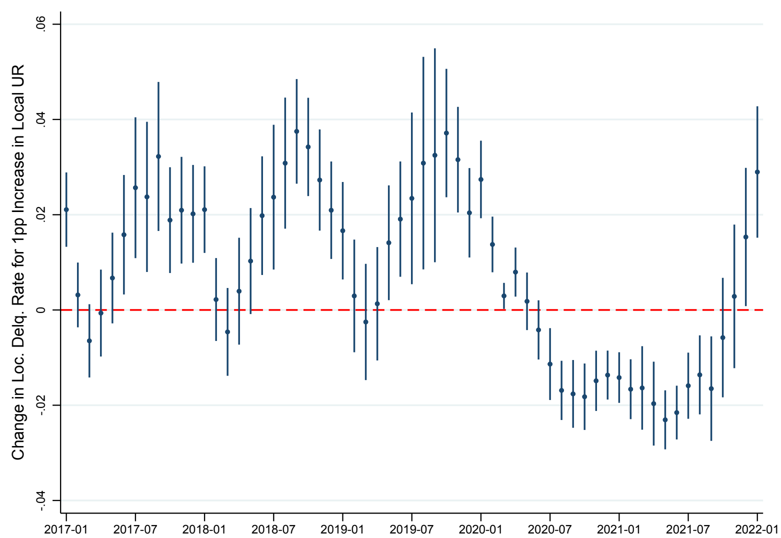
Notes: This figure shows the evolution of the estimated delinquency-unemployment sensitivity for each month between January 2017 and March 2022, separately for different credit types. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_\tau^T$ from a version of Equation 1 that replaces the state-month fixed effect with separate county and month fixed effects. Delinquency rates are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure A4: Short-Term Delinquency-Unemployment Sensitivity Over Time

(**a**) Auto Loans, 30-89 Day Delinquencies



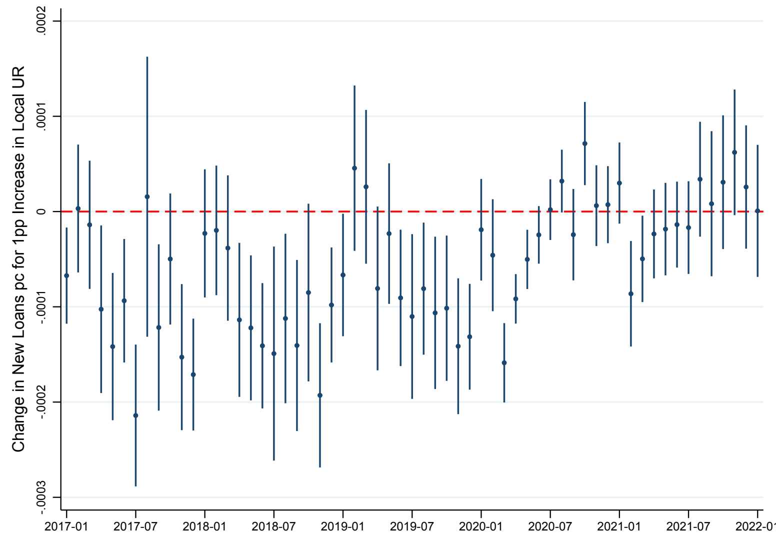
(**b**) Credit Cards, 30-89 Day Delinquencies



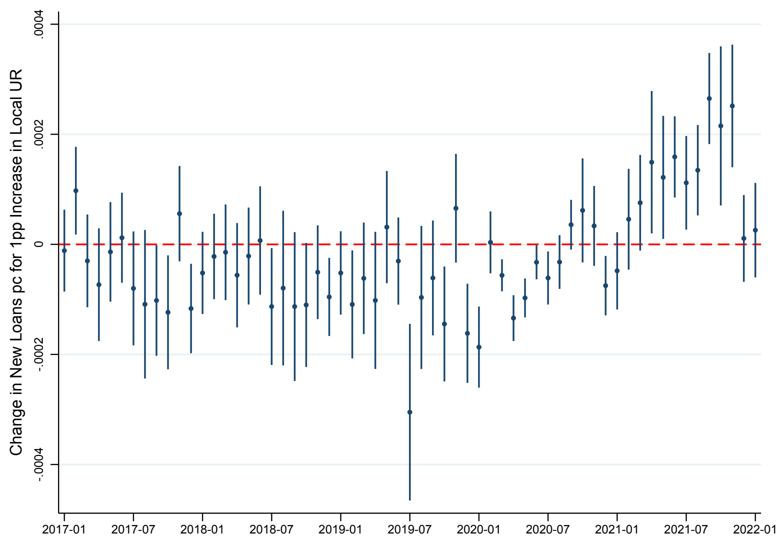
Notes: This figure shows the evolution of the estimated delinquency-unemployment sensitivity for each month between January 2017 and March 2022, separately for different credit types, now using the short-term 30-89 day delinquency rate as the dependent variable. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_{\tau}^{T}$ from a version of Equation 1 that replaces the state-month fixed effect with separate county and month fixed effects. Delinquency rates are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure A5: New Loans Responses to Local Unemployment Shocks

(**a**) New Auto Loans



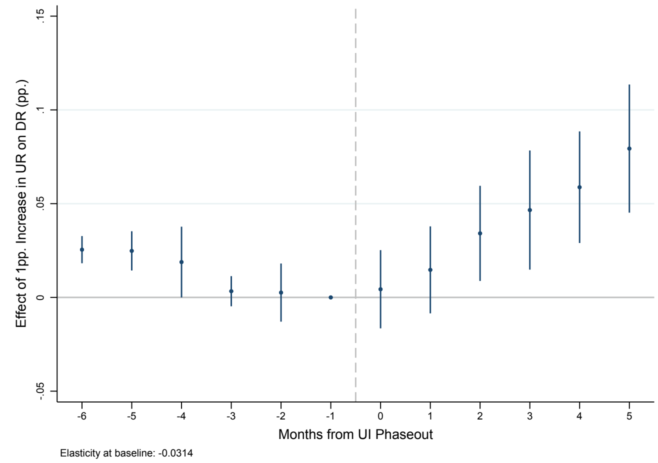
(**b**) New Credit Cards



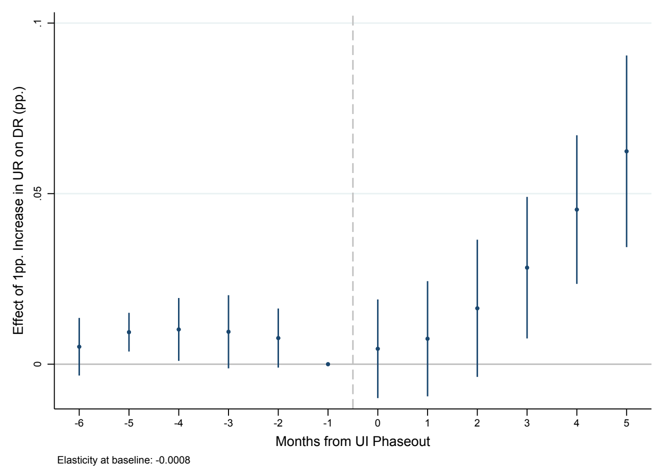
Notes: This figure assesses potential demand-side responses to local unemployment shocks, separately for each month between January 2017 and March 2022. In comparison to the previous figure, here we use the change in the number of per-capita loans (disaggregating into auto loans and credit cards) as the dependent variable. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_{\tau}^{T}$ from a version of Equation 1 that replaces the state-month fixed effect with separate county and month fixed effects. New loans for each credit type are constructed using our county-month aggregation of credit bureau micro-data, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure A6: Effects of UI Phase-Out on Delinquency-Unemployment Sensitivity

(**a**) Auto Loans



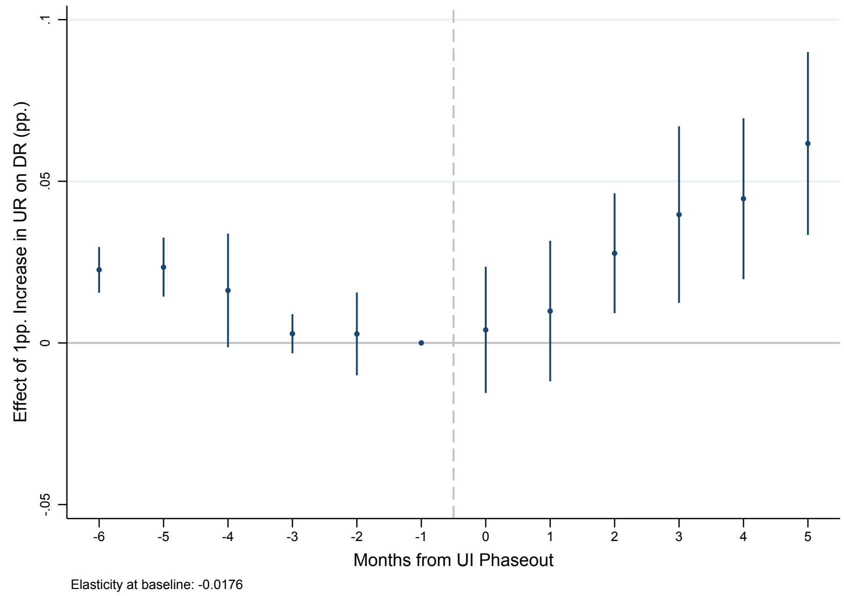
(**b**) Credit Cards



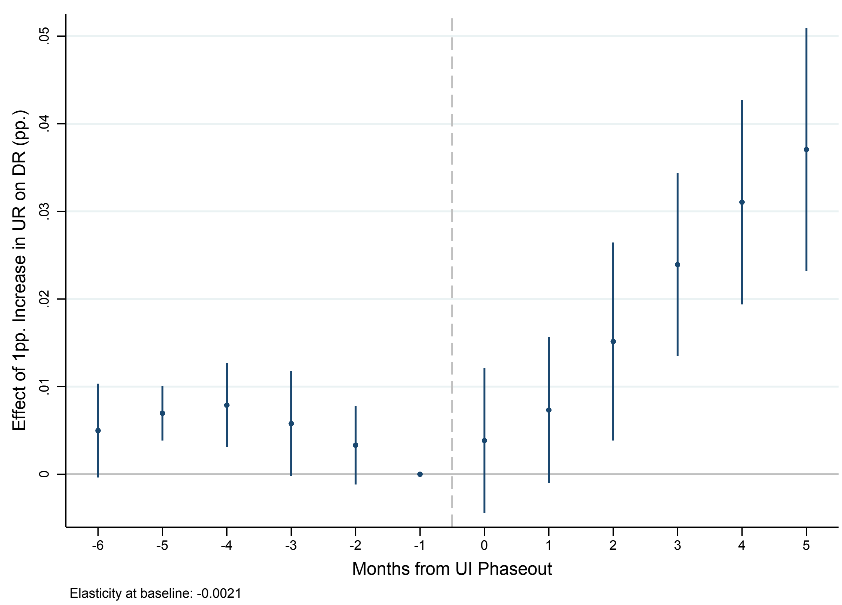
Notes: This figure assesses potential demand-side responses to local unemployment shocks, separately for each month between January 2017 and March 2022. In comparison to the previous figure, here we use the change in the number of per-capita loans (disaggregating into auto loans and credit cards) as the dependent variable. Each panel is a separate regression, plotting coefficients $\{\beta_t\}_\tau^T$ from estimating a version of Equation 1 that replaces the state-month fixed effect with separate county and month fixed effects. New loans for each credit type are constructed using our county-month aggregation of credit bureau microdata, and county unemployment rates are taken from the LAUS. More details on data and interpretation can be found in Sections 3 and 4 respectively.

Figure A7: Effects of UI Phase-Out on Short-Term Delinquency-Unemployment Sensitivity

(**a**) Auto Loans, 30-89 Day Delinquencies



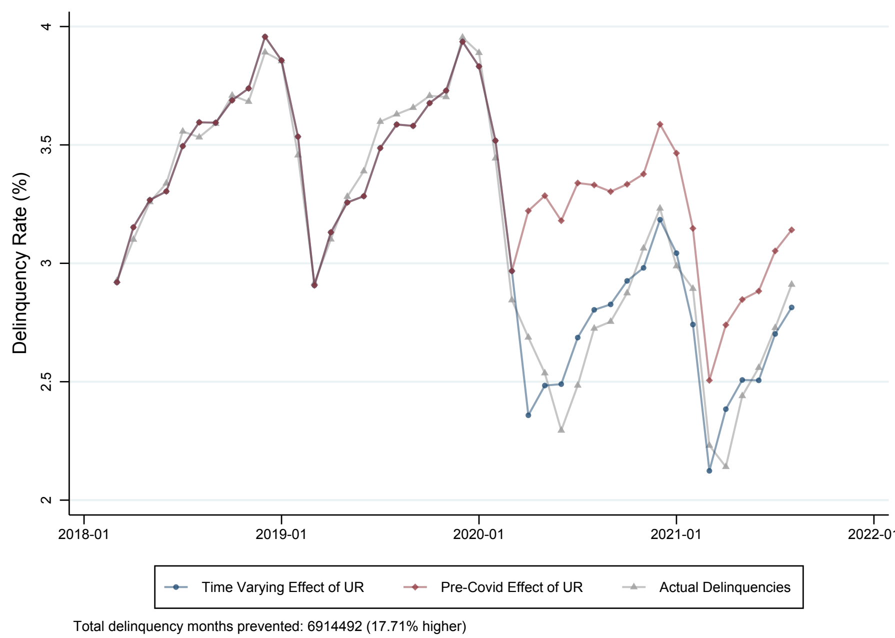
(**b**) Credit Cards, 30-89 Day Delinquencies



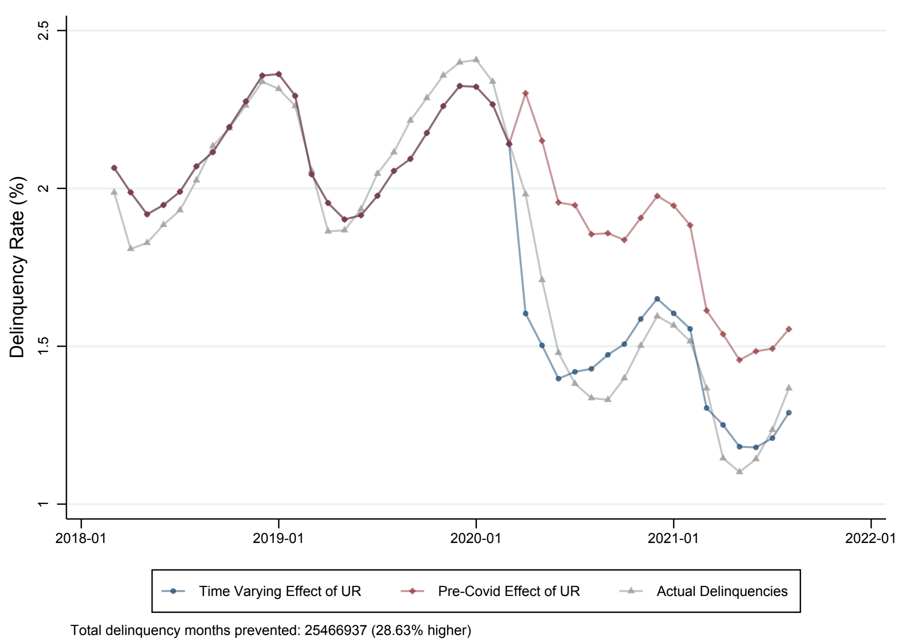
Notes: This figure assesses the impacts of federal UI withdrawals using a staggered event study design. In comparison to the previous monthly sensitivity graphs, here we estimate the effect of withdrawal on the short-term delinquency-unemployment sensitivity (normalized to 0 in the period before withdrawal). More details on the estimation procedure and interpretation can be found in Section 5.

Figure A8: Counterfactual Estimates

(**a**) Auto Loans



(**b**) Credit Cards



Notes: This figure presents empirical, predicted, and counterfactual delinquency rate time series during the pandemic, reproducing Figure 13 by replacing the state-month fixed effect with separate county and month fixed effects.