The Effects of Federal Funds Rate Changes on Household Debt

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Abstract

We study how monetary policy affects household debt in areas that are more and less exposed to changes in the Federal Funds Rate. Comparing more- and less-exposed counties, we find that monetary tightening increases the growth of adverse household debt outcomes, including delinquencies, foreclosures, and bankruptcies, and reduces credit score growth and borrowing for homes and cars in more-exposed counties relative to less-exposed counties. These declines in auto and mortgage borrowing are even larger in more-exposed counties that are low-income or have higher shares of subprime borrowers or Black residents. Furthermore, we find that the effects of monetary policy are asymmetric; monetary contraction has larger effects than monetary expansion. We find that the effects of monetary policy are largely mediated by borrowing cost changes in the short term and by changes in employment in the medium to long term following a rate change. Moreover, we find that the transmission of monetary policy is considerably more effective in areas and time periods that have higher shares of adjustable-rate mortgages.

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Key words: monetary policy, local projections, household debt, adjustable-rate mortgage, fixed-rate mortgage

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1 Introduction

Understanding how monetary policy shocks transmit to household debt is central to evaluating the broader effects of monetary policy. Many households in the bottom and middle of the U.S. income distribution carry substantial debt (Mian et al., 2020) and rely heavily on debt financing to purchase assets such as homes and cars. Monetary policy affects household debt through several channels. Changes in the Federal Funds Rate (FFR) affect the cost of debt by influencing the interest rates households face when originating and servicing debt. Monetary policy also affects household financial health through its impacts on the labor market, as earnings are the primary means through which many households service their debt and remain solvent. In addition, monetary policy affects house prices and hence housing wealth, which can influence households' ability to originate new debt and repay existing debt. Importantly, these effects may vary with households' exposure to monetary policy changes.

In this paper, we estimate the effects of monetary policy shocks on household debt in areas that are more exposed to such shocks relative to areas that are less exposed. We measure exposure using the ratio of household debt to income (DTI). Areas with higher debt relative to income are more leveraged and potentially more sensitive to changes in interest rates because a larger volume of debt must be financed and serviced relative to household income. Comparing areas with different levels of leverage therefore allows us to examine whether the transmission of monetary policy to household debt varies systematically with households' debt exposure.

Estimating the effects of monetary policy shocks is challenging because monetary policy responds to economic conditions. A naïve regression of household outcomes on monetary policy may therefore conflate the effects of monetary policy with those of the economic conditions to which policy is responding. We use several complementary empirical approaches to address this identification challenge. First, we exploit panel data on county-level outcomes and include time fixed effects, which absorb national economic conditions and other aggregate factors that vary over time. This choice substantially limits concerns about reverse causality from local household debt outcomes to national monetary policy. Second, we focus on differential responses across areas with higher

and lower leverage, allowing us to absorb common shocks and other factors that affect high- and low-leverage areas similarly. Third, we control for a long series of lags of monetary policy to account for economic conditions preceding a policy change as well as the prior path of monetary policy. Finally, we provide alternative estimates using monetary policy shocks identified from high-frequency changes in interest rate futures and asset prices around Federal Open Market Committee (FOMC) announcements. These high-frequency measures isolate unexpected changes in monetary policy around a narrow announcement window and provide an alternative approach to identifying plausibly exogenous monetary policy shocks.

We find that, following a monetary tightening, borrowing for home and car purchases grows more slowly in more leveraged areas than in less leveraged areas. We also find that credit card balances, mortgage payments, and mortgage denial rates grow faster in more leveraged areas, while mortgage refinancing falls more. Finally, more leveraged areas see a relative decline in financial health, specifically regarding the growth rate of credit scores, and relative rises in the growth rates of various adverse measures of financial health, such as delinquencies on auto loans, mortgages, and credit card loans, foreclosures, and bankruptcies.

To better understand the mechanisms behind these effects, we examine whether contractions and expansions have symmetric effects, how borrowing costs and employment shape the transmission of monetary policy, and whether transmission depends on the composition of household debt. First, we ask whether the (generally adverse) differential effects of monetary contraction on household debt, spending, and employment are symmetric in magnitude to the (generally beneficial) differential effects of monetary expansions. To do so, we flexibly allow positive and negative monetary shocks to have different effects on our outcomes. We find that contractionary shocks have larger effects in absolute magnitude than do expansionary shocks of the same size. Second, we examine the roles of borrowing costs, employment, and housing prices in transmitting monetary policy to household financial health. In the short run, changes in borrowing costs account for most of the response to monetary policy. Over time, the employment channel becomes increasingly important, and after roughly a year to a year and a half, most of the effects are mediated through changes in employment. By contrast, changes in housing prices appear to play little role. Third, we investigate whether the

stronger impacts in the high-leverage areas are driven by the predominance of a specific kind of debt or whether the effects are symmetric across various kinds of debt—in other words, whether the transmission of monetary policy is stronger for certain types of household debt. We find that transmission is markedly stronger in areas and periods with greater exposure to adjustable-rate mortgage (ARM) debt.

Finally, we investigate heterogeneity in these effects by county demographics, income, and underlying financial health. Some of these effects are more pronounced in areas that have a higher share of Black residents and those that have a higher share of subprime borrowers. In these areas, mortgage payments and the share of denied applications grow faster with leverage. Growth of borrowing for auto and home purchases falls even more rapidly with leverage in higher-share Black counties, while growth of delinquencies and other adverse measures of financial health does not. In contrast, these effects are muted in low-income counties, likely because of their lower connection to formal financial borrowing mechanisms.

Our paper relates to two strands of literature. The first studies household borrowing-cost shocks and monetary-policy transmission through mortgage markets. Di Maggio et al. (2017) exploit adjustable-rate mortgage resets as interest rates fell and find that lower mortgage payments increase car purchases and voluntary deleveraging; Beraja et al. (2019) study quantitative easing (QE), rather than conventional monetary policy, and find that low-equity regions refinance and increase spending less when interest rates fall; and Bosshardt et al. (2024) study the 2022–23 episode of monetary tightening and find that it raised borrower debt-to-income ratios and reduced mortgage credit supply. These papers establish mortgage payments, refinancing opportunities, and underwriting constraints as important channels through which interest rates affect households in the mortgage market. Their analyses, however, largely focus on a particular segment of the household credit market—mortgages—or on downstream consumption responses, rather than tracing monetary policy through the household debt market as a whole.

A second strand studies heterogeneity in monetary-policy transmission across households and local economies. Holm et al. (2021) use Norwegian administrative data to estimate how household responses to monetary policy vary with liquid-asset holdings, finding that direct interest-rate expo-

sure drives short-run consumption responses while income effects become more important over time; Andersen et al. (2023) show that the effects of monetary policy on income, wealth, and consumption vary sharply with household leverage; and Coibion et al. (2017) and Amberg et al. (2022) document broader distributional effects of monetary shocks, with Coibion et al. finding that contractions increase inequality in income, labor earnings, expenditures, and consumption. Closest in spirit to our empirical design, Bergman et al. (2022) interact monetary shocks with local labor-market tightness to compare employment responses in more- versus less-exposed areas. Whereas we focus on household leverage, they study heterogeneity by labor-market tightness and find larger employment gains from monetary expansions for workers with weaker labor-force attachment when labor markets are tighter. Their paper demonstrates the value of cross-sectional heterogeneity in the labor market for understanding monetary transmission in that market. Our use of local leverage (debt-to-income ratio) in the household debt market is similarly intended both to sharpen identification and to ask a substantive incidence question: how do areas that are ex ante more exposed to interest-rate changes in the household debt market respond relative to less-exposed areas?

Our paper advances this literature by answering three important questions about the transmission of conventional monetary policy through household debt markets. First, we ask how conventional monetary policy—both contractions and expansions—affects household borrowing and financial health across more- and less-leveraged areas, using a comprehensive set of household-debt outcomes. Using cross-sectional differences in local leverage, we trace monetary policy transmission through mortgage and auto borrowing, credit-card balances, debt payments, delinquencies, credit scores, foreclosures, bankruptcies, refinancing, and mortgage denials. These outcomes provide a unified picture of both the margins through which household debt adjusts and the incidence of those adjustments across more- and less-exposed areas. Second, we study the mechanisms underlying these responses. We show that transmission changes over time: the cost-of-borrowing channel is most important in the short run, while the employment channel becomes more important at medium and longer horizons. The housing market, by contrast, is not as important as these other channels in mediating the effects. Third, we show that mortgage contract structure amplifies transmission. Effects are larger in places and periods with greater adjustable-rate-mortgage exposure,

connecting interest-rate pass-through to broader changes in household borrowing and financial distress. Together, these results advance our understanding of monetary transmission by showing which households and local economies are most affected, how the effects persist and evolve over time, and the underlying mechanisms that drive these patterns.

2 Data

For our analysis, we rely on detailed household debt data from the Equifax/New York Fed Consumer Credit Panel (see Lee and van der Klaauw, 2010). This dataset contains a 5% sample of the universe of U.S. borrowers with credit activity, including data on mortgages, auto loans, credit card loans, student loans, delinquencies, foreclosures, bankruptcies, and a measure of their creditworthiness known as the Equifax Riskscore, which is typically used as a proxy for their credit score and has a similar scale. In our analysis, we aggregate these variables to the county level.

We use mortgage servicing data from Black Knight McDash. These data cover 60 to 80 percent of the mortgage market and are constructed using data from mortgage servicers, which are financial institutions that collect payments from borrowers. Using loan-level McDash, we distinguish mortgage refinancing from purchase mortgages and also observe scheduled payments. McDash also helps us separately identify the type of mortgage: fixed-rate mortgage versus adjustable-rate mortgage. This differentiation is important because it helps us explore whether our results are driven by areas with higher adjustable-rate mortgage shares or areas with higher fixed-rate mortgage shares. Additionally, McDash reports home value at mortgage origination for each property. We combine home values at origination with ZIP-code-level home price indices from CoreLogic to estimate each property's value from 2005 to 2023 (Fuster et al., 2018).

Household leverage and the composition of mortgage debt vary substantially across the country. Figure A.1 Panel A shows the distribution of county-level DTI across the country. DTI levels are highest in the East, the Southwest, and the Pacific. Figure A.1 Panel B shows that higher-DTI areas are more likely to be areas with higher home equity values. Panel C of Figure A.1 shows the distribution of ARM DTI and FRM DTI across the country. We find that there is wide spatial

variation in each of these measures, even though they are similarly distributed. This pattern is consistent with the fact that higher-income and higher-house-price areas have both higher ARM debt and higher FRM debt. Looking at the spatial distribution of ARM mortgage share and FRM mortgage share, we immediately see a difference between them (Figure A.1 Panel D). ARM mortgage shares are highest in the Northeast, Florida, the Southwest, and the Pacific, whereas FRM shares are relatively higher in the rest of the South and West. In general, though, Figure A.2 shows that ARM DTI balance varies positively with FRM DTI balance: areas that have higher ARM DTI balance also tend to have higher FRM balance, as the areas with higher balances are typically areas with higher housing prices and housing wealth. Figure A.3 shows that higher-DTI places tend to have higher ARM shares.

We use confidential Home Mortgage Disclosure Act (HMDA) data to analyze whether there were differences in mortgage applications and denials by race and ethnicity. HMDA data cover 90 percent of mortgage applications. We aggregate the loan-level McDash data and the application-level HMDA data to the county-quarter level, which is the unit of all our analyses.

To understand whether and to what extent the effects on household debt are driven by changes in households' employment outcomes, we obtain data on total employment by county and time from the Quarterly Workforce Indicators (QWI), which covers 96% of wage and salary civilian jobs.

To investigate heterogeneity across different types of counties, we consider several different demographic categorizations. We refer to counties that fall in the top quartile of the weighted distribution of the share of Black residents as higher-share Black counties. These counties include the Black-majority counties in the U.S. Southeast as well as some metropolitan counties with a high share of Black residents. We define counties that fall in the top quartile of the weighted distribution of the share of subprime (credit score ≤ 620) borrowers as higher-share subprime counties. Finally, we define counties that fall in the bottom quartile of the population-weighted distribution of county median income as low-income counties. The date ranges of our data sources are reported in Table C.1. Table C.2 provides summary statistics for our explanatory variables (FFR, DTI, and other related variables), while Table C.3 provides summary statistics for our main outcome variables.

3 Empirical Strategy

To identify the effects of monetary policy shocks on household debt in more-exposed areas relative to less-exposed areas, we build on Bergman et al. (2022, hereafter BMW) and estimate a local projections model on county-by-quarter panel data, interacting our measure of the monetary policy stance with county-level exposure measures. This approach differs from much of the literature on estimating the effects of monetary policy on inequality (Coibion et al., 2017; Bartscher et al., 2022; Andersen et al., 2023; Holm et al., 2021) because it focuses on the heterogeneity of the effect of monetary policy across locations rather than on its main effect on the economy, and because it leverages both cross-sectional and time-series variation for identification. Our method differs from BMW in that we account for monetary policy lags in our model. Since there is strong serial correlation in monetary policy across time, controlling for lags enables us to capture the effects of current monetary policy innovations while controlling for the past path of monetary policy (Coibion et al., 2017). This choice also allows us to better align the persistence of our outcomes and our regressors. To that end, we control for lags of the interaction of monetary policy and county-level exposure.

We aim to recover how the effect of monetary policy varies across more and less financially exposed areas, which requires less stringent identification assumptions than the existing literature, which typically focuses on time series variation (Coibion et al., 2017). In a time series approach, the policy stance can be correlated with other time series macro outcomes and might confound impacts; however, in our case, our estimates will be affected only if the other macro-outcomes that are correlated with monetary policy also affect outcomes of interest differently in places with different exposure.

Let c index counties and t index time (at quarterly frequency), and let FFR_t be the average Federal Funds Rate prevailing in quarter t. Let $DTI_{c,t}$ be the debt-to-income ratio associated with county c at time t that is indicative of the exposure of county c to the monetary policy shock at time t. Additionally, let $G_{c,t} = (y_{c,t+4} - y_{c,t}) / y_{c,t}$ be the year-on-year growth rate of an outcome of interest y in county c at period t. Then, to estimate the impulse response of outcome y to a

monetary shock at time $t-1$ at horizon $t+h$, we run the local projection regression (Jordà, 2005):

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} \times DTI_{c,t-j} + \sum_{j=1}^{J} \gamma_j^{(h)} DTI_{c,t-j} + \varepsilon_{c,t+h} \tag{1}$$

For our baseline estimates, $J = 8$ and $H = 16$, meaning that we control for eight quarters (two years) of lags and look up to four years ahead. Standard errors are clustered at the county level. The county fixed effects $\alpha_c^{(h)}$ control for fixed county differences in growth rates of outcomes that are unrelated to FFR changes or to current dynamics of the exposure variable, while the time fixed effects $\lambda_t^{(h)}$ control for national trends that are common across counties. The sequence of coefficients $\left\{\beta_1^{(h)}\right\}_{h=0}^{H}$ traces out the impulse response of $y_{c,t}$ to a shock to FFR_{t-1}, as a function of $DTI_{c,t-1}$. Note that each coefficient $\beta_1^{(h)}$ is conditional on the preceding sequence of $J-1$ lags of monetary policy realizations and reflects only the component of FFR_{t-1} that could not have been predicted by them, so our specification allows for past monetary policy to shape what is considered a monetary policy innovation in the current period. Allowing for lags thus helps both alleviate omitted variable bias and isolate the stationary part of the identifying variation. If $\beta_1^{(h)}$ is equal to zero, that means that at horizon h, a shock to FFR_{t-1} affects the growth rate of $y_{c,t}$ the same way for counties regardless of the value of $DTI_{c,t}$. If $\beta_1^{(h)}$ is greater than zero, then at horizon h a shock to FFR_{t-1} increases the growth rate of $y_{c,t}$ for counties with higher $DTI_{c,t}$ by more than it does for counties with lower $DTI_{c,t}$, and vice versa for $\beta_1^{(h)} < 0$.

One concern is that FFR_{t-1} may be endogenous. If equation (1) were a pure time series regression of an outcome on FFR, this concern would be much more critical because it is well known that the Federal Funds Rate is correlated with other business cycle measures. However, as it is a panel regression, these correlations are captured by the quarter fixed effects. Moreover, as our outcomes are measured at the level of the county c and as FFR_t is determined at the national level, it is very unlikely that reverse causation from any county-level outcome is affecting the national monetary policy stance. It is also not an issue that $\left\{\beta_1^{(h)}\right\}_{h=0}^{H}$ may be reflecting the effects of changes to the exposure measure $DTI_{c,t}$ as we control for $DTI_{c,t}$ directly. Finally, the fact that FFR_{t-1} enters in an interaction with $DTI_{c,t}$ implies that any potential confounder

needs to be a nationwide macroeconomic variable that affects different counties differently based on that county's value of $DTI_{c,t}$. To address this remaining concern, we present additional estimates in which FFR is replaced by the running sum of high-frequency monetary policy surprise shocks (Gertler and Karadi, 2015; Jarociński and Karadi, 2020; Nakamura and Steinsson, 2018; Miranda-Agrippino and Ricco, 2021). We primarily rely on the shocks in Nakamura and Steinsson (2018) for this alternative measure. These surprise shocks measure the change in Fed Funds Rate futures or other asset prices in a very short time window around the monetary policy announcement after an FOMC meeting, plausibly capturing any information revealed by the announcement (any monetary policy announcement perfectly anticipated by the markets should not move them). We will see that using these alternative monetary policy surprise series in place of the FFR does not qualitatively change our estimates.[1]

We also investigate whether monetary contraction has asymmetric effects relative to monetary expansion on our outcomes of interest. To do so, we define z_t as the indicator that the Nakamura–Steinsson monetary policy shock is positive at time t. We then estimate the specification

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} DTI_{c,t-j} + \sum_{j=1}^{J} \tilde{\beta}_j^{(h)} z_{t-j} FFR_{t-j} DTI_{c,t-j} + \sum_{j=1}^{J} \gamma_j^{(h)} DTI_{c,t-j} + \sum_{j=1}^{J} \tilde{\gamma}_j^{(h)} DTI_{c,t-j} z_{t-j} + \varepsilon_{c,t+h} \tag{2}$$

The coefficients $\left\{\beta_1^{(h)} + \tilde{\beta}_1^{(h)}\right\}_{h=0}^{H}$ trace out the impulse response function after a positive shock, while the coefficients $\left\{\beta_1^{(h)}\right\}_{h=0}^{H}$ trace out the impulse response function after a negative shock. Testing the coefficients $\tilde{\beta}_1^{(h)}$ allows us to see whether the effects are statistically significantly asymmetric

[1] An alternative to using the high-frequency monetary policy surprise shocks would have been to use the shocks of Romer and Romer (2000), which are based on taking residuals from the regression of FFR changes on Green Book forecasts and other information known to the central bank at the time of the FOMC meeting. However, such shocks cannot be constructed for years after 2007, when the FFR hit the zero lower bound. Moreover, such shocks have been criticized for failing to capture all the predictable variation in FFR; for example, Cochrane and Piazzesi (2002) show that the substantial interest rate decrease during the September 2001 meeting is classified as a monetary shock by the Romer and Romer methodology yet was very closely predicted by markets as a reasonable response to the 9/11 attacks.

for any time horizon h.

We also attempt to decompose the overall effect of FFR shocks on outcomes into direct and indirect effects. We do this by (a) following Holm et al. (2021) and controlling for leads of employment growth between period t and period $t+h$ and (b) replacing the leads of employment growth with standard Bartik labor-demand shocks, which interact county employment sectoral shares with national employment sectoral growth rates (Notowidigdo, 2020), in order to avoid the potential endogeneity of employment growth leads in this regression. The specification for (b) is below, while (a) uses employment growth in place of the Bartik labor-demand shocks. The coefficients $\left\{\beta_1^{(h)}\right\}_{h=0}^{H}$ in equation (3) trace out the direct effect of FFR shocks, netting out the indirect labor market effects.[2]

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} \times DTI_{c,t-j} + \sum_{j=1}^{J} \gamma_j^{(h)} DTI_{c,t-j} + \sum_{j=1}^{h} \delta_j^{(h)} B_{c,t+j} + \varepsilon_{c,t+h} \quad (3)$$

$$B_{c,t} = \sum_{k=1}^{K} \varphi_{i,k,t-\tau} \left(\frac{v_{-i,k,t} - v_{-i,k,t-\tau}}{v_{-i,k,t-\tau}}\right) \quad (4)$$

where $\varphi_{i,k,t}$ is the employment share of industry k in county i and $v_{i,k,t}$ is the national employment share of industry k excluding county i.

Finally, we investigate whether our baseline results are driven by places with high ratios of FRM debt to income or places with high ratios of ARM debt to income. To do so, we include interactions of the FFR with both FRM DTI and ARM DTI in the regression, as well as the main effects of the two types of DTI. We denote $DTI_{c,t}^{F}$ as the FRM DTI and $DTI_{c,t}^{A}$ as the ARM DTI for county c at time t in the following specification:

[2] We use 3-year rolling Bartik shocks, setting $\tau = 12$ quarters.

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} \times DTI_{c,t-j}^{A} + \sum_{j=1}^{J} \tilde{\beta}_j^{(h)} FFR_{t-j} \times DTI_{c,t-j}^{F} + \sum_{j=1}^{J} \gamma_j^{(h)} DTI_{c,t-j}^{A} + \sum_{j=1}^{J} \tilde{\gamma}_j^{(h)} DTI_{c,t-j}^{F} + \varepsilon_{c,t+h} \tag{5}$$

where $\left\{\beta_1^{(h)}\right\}_{h=0}^{H}$ and $\left\{\tilde{\beta}_1^{(h)}\right\}_{h=0}^{H}$ trace out the impulse response functions for ARM DTI and FRM DTI, respectively.

4 Results

4.1 Baseline Results for Credit Outcomes

Figure 1 presents estimates of $\beta_1^{(h)}$, the interaction coefficient on FFR and the county's debt-to-income ratio at time t on the growth rate in the outcome at time $t+h$. Panel A shows the impulse responses for the share of people in a county with an auto loan, the share with a mortgage loan, and the overall credit card balance. We see that after an FFR increase, people in counties with higher DTI reduce borrowing growth for investment-type goods, such as cars and houses. In contrast, credit card balances grow faster in more leveraged counties, which is explained by faster growth in credit card borrowing to consume and, to a lesser extent, by growth in credit card delinquencies (last chart in Panel B). In particular, one year after a 100 bps FFR increase, the value of $\beta_1^{(h)}$ for credit card balance growth is around 2.5 and is statistically significantly different from zero, meaning that after a 100 bps increase in the FFR, counties with a one standard deviation higher DTI (about 40 percentage points) see credit card balances grow about 1 percentage point faster. This effect is somewhat under half of the average growth rate of credit card balances in the sample.

Next, Panel B considers how FFR increases differentially affect the propensity of borrowers to become delinquent. We see that for all three categories of auto, mortgage, and credit card debt, the share of borrowers who are delinquent grows faster in more leveraged counties for nearly four years

after the FFR increase. The magnitude of this differential is also sizable, suggesting that a one standard deviation increase in DTI raises the growth rate of the share of delinquent borrowers for auto loans by nearly a third of the average growth rate. For mortgage loans and credit card loans, these differentials are over three times the average rate and over half the average rate, respectively. FFR increases result in considerable additional financial stress in areas already experiencing high levels of borrowing relative to income. We note that even though the impulse response function eventually goes to zero after three to four years, the difference in shares delinquent, which is the cumulative sum of the growth rate differentials, reaches its maximum at that horizon.[3]

Panel C focuses on the growth rates of payments on auto, mortgage, and credit card debts. For each of the three categories of debt, payment growth increases with an FFR increase, with the impulse response slowly declining over time. Counties with a one standard deviation higher DTI experience increases of 1.5–2 percentage points in the growth rate of each of the three types of payments following a 100 bps FFR increase, which is about a tenth of the standard deviation of the growth rates for each of these measures. The positive impulse response is intuitive because the interest rates on the payments increase with FFR increases. It is notable that mortgage payment growth stops increasing and starts decreasing about six quarters after the shock. This could result from mortgage holders paying off their mortgages or becoming delinquent on them. The decline in the growth rate of mortgage balances around this time after the FFR shock shown in Panel A is consistent with mortgage repayment being a significant mechanism.

Finally, Panel D focuses on credit scores, new foreclosures, and new bankruptcies, which are summary measures of households' financial conditions. Both new foreclosures and new bankruptcies grow at a faster rate with a county's leverage, although these differential effects fade to zero after two to three years. Credit score growth also declines more in more leveraged counties, with the differential effect converging to zero after four years.

We further investigate mortgage-related outcomes in Figure 2. Using data from the confidential

[3]Appendix Figure A.6 presents impulse response functions for the outcomes in Panels A and B, in which we use the dollar shares of balances and delinquencies. The results are qualitatively similar, except for dollar auto balances, which consistently shrink following an FFR increase in more leveraged relative to less leveraged counties. This may be explained by consumers originating smaller auto loans when faced with higher interest rates and already high leverage.

Home Mortgage Disclosure Act (HMDA) database, we examine how the share of mortgage applications resulting in denials changes differentially by county DTI after an FFR increase. We see that the growth rate of the share of applications resulting in denials rises as a function of county DTI, with the effect size being large—a county with a one standard deviation higher DTI expects to see a 2% higher rise in the share of applications leading to denials, which is more than half the average growth in this variable. However, the growth rate starts falling with DTI a year after the FFR increase, and within the next three years the differential in the share of mortgage applications being denied as a function of DTI shrinks considerably.[4]

4.2 Results Using Monetary Surprise Measures

As we discussed in Section 3, there are many reasons to be concerned that the Federal Funds Rate is endogenous to broader economic conditions. Several features of our design already mitigate this concern: time fixed effects absorb aggregate economic conditions common across counties, county-level outcomes are unlikely to influence national monetary policy, and our estimates identify differential responses across counties with different levels of leverage rather than the aggregate effect of FFR changes. However, FFR changes may still be correlated with other macroeconomic factors—such as expectations—that differentially affect counties based on their debt-to-income ratios.

To address this concern, we replace FFR in equation (1) with the running sum of monetary policy surprises, obtained by looking at Fed Funds Rate futures and other asset price changes in a small (usually 30-minute) window around the time of a monetary policy announcement.[5] We consider the shocks calculated in Nakamura and Steinsson (2018, henceforth NS), which are based on the first principal component of Fed Funds Rate futures of various maturities. These shocks measure the unexpected information revealed through FOMC announcements about current and future monetary policy, and if asset markets are reasonably efficient, they should not be correlated with macroeconomic information known prior to the announcement.

Figure 3 replicates Figure 1 (our baseline results for credit outcomes) using the running sum

[4] Symmetric dynamics hold for the share of mortgage applications ending in originations.

[5] BMW also construct their monetary shock using a running sum of monetary policy surprises. This analysis can be interpreted as a "reduced form" of an instrumentation of FFR with the monetary policy surprise running sum.

of NS shocks in place of FFR in specification (1). We rescale the running sum of the NS shocks to match the standard deviation of the Federal Funds Rate for easier comparison across results. The trends of the impulse responses in Figure 3 closely track our baseline results in Figure 1. In particular, more leveraged counties have more negative growth of the shares of people with auto and mortgage loans as well as of credit scores, and substantially more positive growth of credit card balances, all types of delinquencies, new foreclosures, and bankruptcies. The magnitudes of the impulse responses at their peak are somewhat larger (e.g., 1–1.5 times) than the magnitudes of our baseline estimates. Figure 4 presents the corresponding mortgage-denial result using NS shocks. The pattern is again very similar, with somewhat larger magnitudes.[6][7]

4.3 Robustness Checks

In addition to using the monetary surprise measures, we perform further robustness checks in the Appendix. Figure A.8 replicates Figure 1 but uses only one FFR lag on the right-hand side of equation (1) rather than 8 lags (as done by BMW and Holm et al. (2021)). The results are similar to the baseline. Figure A.9 replicates Figure 1 while controlling for lags of the employment-to-population ratio (EPOP). Figure A.10 shows the summary household debt results when FFR is replaced by its first difference. Figure A.11 controls for a number of time-varying county-level covariates from the ACS, interpolated across quarters, such as industrial composition, average wages, unemployment rates, and demographic makeup. Figures 5 and A.12 present our main results for different sub-periods of our sample. It is clear that our results largely come from the 1999–2008 subperiod, which is the only subperiod with high interest rates for which we have follow-up data. Figures A.13A and A.13B document that our results are not driven by the Covid-19 pandemic.

[6] A concern may be that the starting value of this running sum is unknown, but this concern is effectively addressed by the inclusion of the main effects of $DTI_{c,t}$ and its lags as covariates in equation (1).

[7] We also investigate the robustness of our results to using shocks from Jarociński and Karadi (2020) in Appendix Figures A.7A and A.7B, with analogous results.

4.4 Are the Effects of FFR Changes Symmetric?

We next examine whether the effects of FFR shocks are symmetric across FFR increases and FFR decreases. Figure 6 replicates Figure 1, allowing the coefficients on the FFR interaction and its lags, as well as on the main effects of DTI and its lags, to be different depending on whether that quarter's change in the FFR was positive or negative. For each regression, both impulse response functions are plotted on the same axes. The FFR variable is defined in such a way that if the effects are symmetric, then the impulse response for FFR increases should be the same as for FFR decreases. We see that the coefficients on FFR when the change in FFR is positive are larger than the coefficients when the change in FFR is negative, suggesting that a monetary contraction has a stronger differential effect on household debt outcomes as a function of county leverage than the countervailing effect of a monetary expansion, consistent with the existing literature that finds stronger effects of contraction on output and prices (Matthes and Barnichon, 2015). Most of these differences are statistically significant, as can be seen in Appendix Figure A.14. The results of Figure 6 remain similar if we replace the FFR variable with monetary policy shocks from Nakamura and Steinsson (2018), providing further evidence that the effects of contractionary monetary policy are stronger than those of expansionary policy (Appendix Figure B.3A).

4.5 Direct and Indirect Effects of FFR Changes

We now examine what fraction of the baseline effects measured in Figure 1 comes from the direct effects of interest rate changes and what fraction is mediated by the effect of interest rates on local labor markets. First, we present evidence that more leveraged counties experience a greater decline in employment growth with interest rate increases than less leveraged counties do. Figure 7A presents estimates of the impulse response function for the growth of the employment rate. We see that employment growth differentially falls contemporaneously as the interest rate rises, and continues to decline differentially for four years after the interest rate increase. Counties with a one standard deviation higher DTI experience a 0.32 percentage point greater decrease in employment growth following a 100 bps FFR increase, equal to the mean increase in employment growth for a

county in a given year in our sample. Therefore, interest rate increases reduce employment growth, which may be a channel through which they affect financial health. Following Holm et al. (2021), we can estimate the direct effects of interest rate changes by running our baseline regression (1), additionally controlling for the leads of employment growth between horizon t and horizon $t + h$, and taking the resulting impulse response coefficients as the direct effects. The indirect effects are estimated as the difference between the baseline impulse response and the direct effect impulse response. A concern with this methodology is that the leads of employment growth may be endogenous. We overcome this concern by replacing the leads of employment growth with standard Bartik labor-demand shocks, specifically the weighted average of national-level employment growth across industries where the weights are county-level industry employment shares (Bartik, 1991). If the Bartik labor-demand shocks capture exogenous variation in local employment growth, controlling for them in the structural regression will allow us to obtain consistent estimates of the impulse response of household financial health to FFR shocks while holding the employment channel constant. Figure 7B presents a decomposition of our baseline results on the effects of FFR increases on household financial outcomes into direct and indirect effects, estimated using specification (3). We see that for the first year after a monetary shock, almost all of the baseline effect comes from the direct effect. Subsequently, direct effects diminish, while indirect effects coming through changes to local employment growth increase. By the third year after a monetary shock, nearly all of the baseline effect on financial outcomes is mediated by leads of local employment growth. Appendix Figure A.15 presents estimates of the indirect effects, or the component of the overall effect that is mediated by changes to employment growth. We see indirect effects following patterns similar to those of the direct effects, but shifted later in time, peaking at about six quarters after the FFR shock. For completeness, we also estimate results using employment growth leads rather than Bartik labor-demand shocks (Figure A.16). In Appendix Figure A.17, we present the indirect effects estimated using this procedure; that is, Appendix Figure A.17 is an alternative version of Appendix Figure A.15, where we use employment growth instead of Bartik labor-demand shocks. The results in Appendix Figure A.16 look very similar to Figure 7B, while the results in Appendix Figure A.17 look very similar to those in Appendix Figure A.15.

Furthermore, Appendix Figure B.4 presents the baseline results when shocks from Nakamura and Steinsson (2018) are used in place of the FFR. The qualitative and quantitative pattern of the direct and indirect effects is the same, with all effects being slightly larger in magnitude. Our finding that the direct effects of interest rate increases are prominent early on, but then slowly give way to indirect effects operating through the labor market, is consistent with similar findings by Holm et al. (2021).

4.6 Which Types of Debt Matter More for Monetary Policy Transmission?

Our main finding is that highly leveraged areas experience greater financial stress than less leveraged areas do following a contractionary monetary shock. However, do all kinds of leverage matter equally in the transmission of monetary policy, or are some types of debt more important for transmitting monetary policy than others? Redefining the DTI variable in our baseline equation (1) to capture different types of debt, we find no heterogeneity by credit card or auto debt-to-income ratios, but we do find that the effects differ between fixed-rate and adjustable-rate mortgage debt, leveraging data on the prevalence of each mortgage type from McDash.

In Figure 8, we investigate whether our baseline results are driven by places with high ratios of FRM debt to income or places with high ratios of ARM debt to income. To do this, we estimate equation (5), described in Section 3. Specifically, we include interactions of the FFR with both the FRM debt-to-income ratio and the ARM debt-to-income ratio (as well as the main effects of the two debt-to-income ratio measures) in the regression. We find that, holding FRM debt-to-income ratios fixed, counties with greater ARM debt-to-income ratios experience impulse responses to FFR shocks relative to counties with smaller ARM debt-to-income ratios in ways that are qualitatively similar to our baseline estimates. In contrast, holding ARM debt-to-income ratios fixed, counties with greater FRM debt-to-income ratios do not experience particularly strong or consistent impulse responses to FFR shocks relative to counties with smaller FRM debt-to-income ratios. These results, together with the fact that higher-DTI areas have higher ARM shares (Figure A.3), indicate that ARM debt is an important driver of responses to FFR shocks, further supporting the conclusion

that monetary transmission is larger in areas with higher ARM shares.

To further understand the role of ARM debt in monetary policy transmission, we separately estimate our specification for two subsamples: 1999–2008 and 2015–2023. The ARM share was considerably higher during the earlier period than during the later one, as seen in Figures A.4 and A.5. We exclude 2009–2015 because of two factors: (a) the interest rate was close to the zero lower bound and hence had very little variation, and (b) dropping this period gives us two periods that are distinctly different in terms of ARM shares. The impacts we find during the 1999–2008 period closely mirror the overall effects (Figure 5), while the effects are considerably muted or indistinguishable from zero for the 2015–2023 period (Appendix Figure A.12). This finding reinforces the mechanism described above: transmission of monetary policy is driven by the presence of variable-rate ARM debt. Areas and periods that are characterized by a larger presence of ARM debt are more affected by monetary policy.

A natural question that arises is why areas with a greater presence of ARM debt experience stronger pass-through of monetary policy while that is not the case in areas with a greater presence of credit card debt. First, while both are types of floating debt, households' credit card balances are considerably smaller than their mortgage balances, so a given rate change results in meaningfully larger interest charges on mortgage debt, leading to stronger transmission through ARM debt. Second, credit card interest rates are markedly higher than mortgage rates. Hence, a given change in interest rates is much more meaningful relative to mortgage rates than to credit card rates.

4.7 Heterogeneity

We now turn to examining heterogeneity in our results across counties with different demographics. We consider three types of heterogeneity: (a) by the fraction of county residents who are Black, (b) by county income, and (c) by credit score. Appendix Tables C.4 and C.5 provide summary statistics for counties falling in these three categories.

We first examine how the response to FFR shocks varies with leverage for counties with a higher share of Black residents (higher-share Black counties). We define these counties as the ones in the top quartile of the population-weighted county distribution of the share of residents who

are Black. Figure 9A presents the impulse responses for higher-share Black counties. We see from Panel A that among higher-share Black counties, those that are more leveraged experience an early, strong decline in the growth of the share of people with auto and mortgage debt, with the impulse response becoming statistically indistinguishable from zero after about six quarters. The initial impulse response for credit card balances in higher-share Black counties is also smaller, although it converges to the baseline impulse response within a few quarters. These results are consistent with interest rate increases acting as more of a barrier to the origination of new auto, mortgage, and credit card debt in higher-share Black counties than in all counties on average.

Panel B suggests that the impulse response for delinquency growth in higher-share Black counties is only slightly lower than the overall response. Hence, despite having a relatively larger reduction in the growth rate of borrowing activity, counties with a higher share of Black residents that are highly leveraged do not see a significant reduction in the growth rate of delinquencies when the FFR increases, consistent with them having a greater propensity to see lending become delinquent even at the same level of DTI. The responses of payments, credit scores, foreclosures, and bankruptcies to FFR changes as a function of county leverage (Panels C and D) are similar to the baseline for higher-share Black counties.

Turning next to income heterogeneity, we define low-income counties as counties whose median income is in the bottom quartile of the population-weighted county median income distribution. Figure 9B reproduces the baseline Figure 1 for low-income counties, including the baseline impulse responses for reference. We observe similar patterns to the heterogeneity for higher-share Black counties, such as low-income counties experiencing stronger declines in auto and mortgage balance growth with an FFR increase than typical counties do. We also see that the impulse responses of delinquencies for low-income counties are slightly lower than the baseline.

Lastly, we consider counties with a high share of subprime borrowers. We define these counties as the ones in the top quartile of the population-weighted county distribution of the fraction of borrowers that are subprime. Figure 9C presents the impulse responses for these counties together with the baseline impulse responses for reference. Similar to higher-share Black and low-income counties, counties with large fractions of subprime borrowers experience declines in balance growth

with an FFR increase, but growth in delinquencies remains similar to the baseline.[8]

5 Conclusion

In this paper, we have documented that the effects of monetary policy on household debt depend importantly on household leverage, measured by the debt-to-income ratio (DTI). Following monetary tightening, more leveraged areas experience greater increases in adverse debt outcomes, along with larger declines in spending and employment growth. We find that these effects are asymmetric, with monetary contractions having larger effects than expansions of similar size. We also find that the transmission mechanism changes over time. Borrowing costs play a larger role initially, while indirect effects through employment dominate in the longer run. Exploring mechanisms, we find that the transmission of monetary policy is much more effective in time periods and geographies characterized by higher ARM shares than in those with more fixed-rate mortgage debt or other kinds of floating debt. The intuition here is that ARM debt is considerably larger in size than other kinds of floating debt (such as credit card debt); hence, increases in interest charges are much more salient for ARM debt. On the other hand, fixed-rate mortgage debt is not affected by changes in interest rates unless the mortgage reaches the end of its term. Thus, the bite of monetary policy in an area and period critically depends on the composition of household debt in that area and period.

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[8] Appendix Figure A.18 shows heterogeneity in mortgage denials for low-income and higher-share subprime counties. Both groups experience greater growth in the share of mortgage applications denied as a function of leverage following an FFR increase. We also investigate heterogeneity in the impulse responses of employment in Appendix Figure A.19. The impulse responses for low-income and higher-share subprime counties are generally negative but occasionally statistically indistinguishable from zero.

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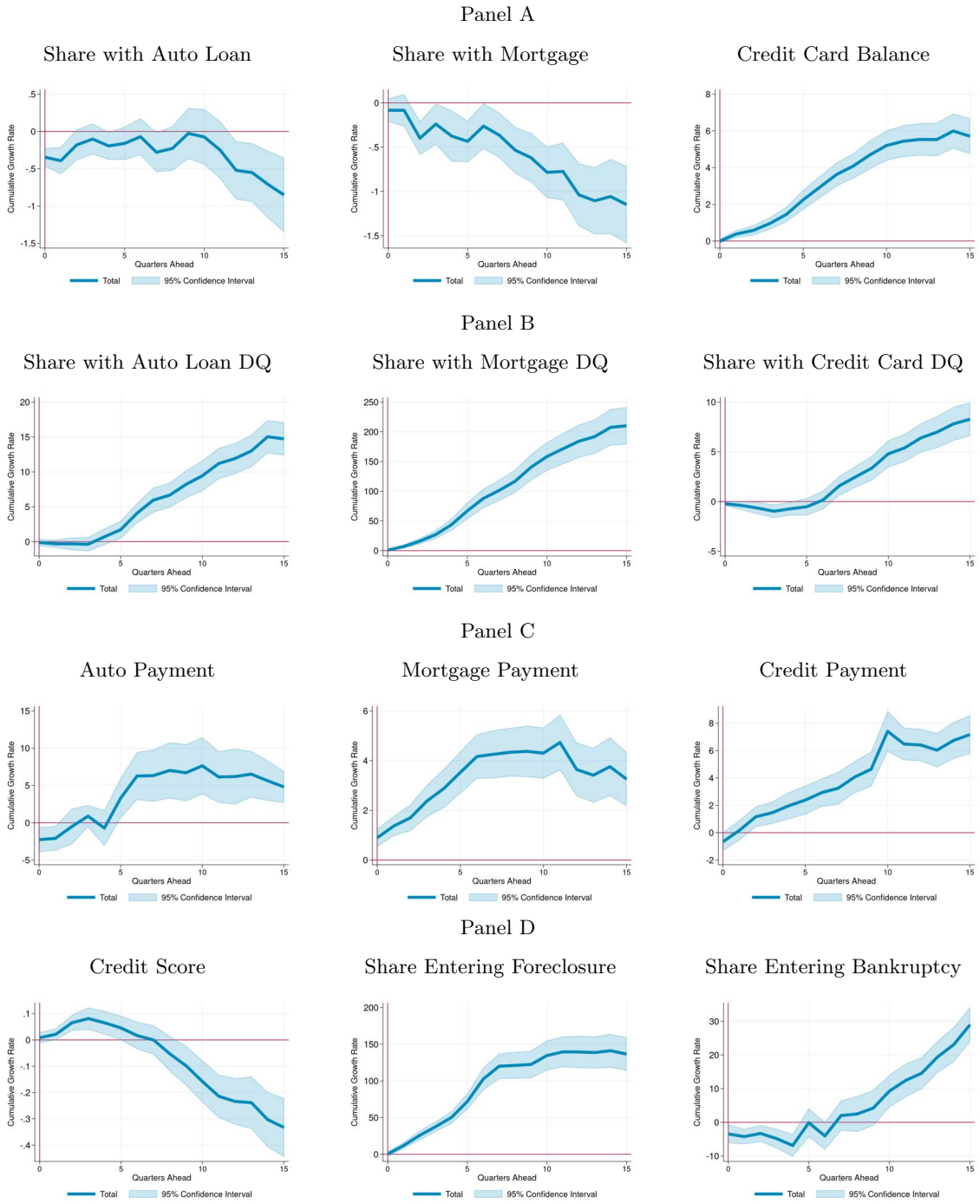
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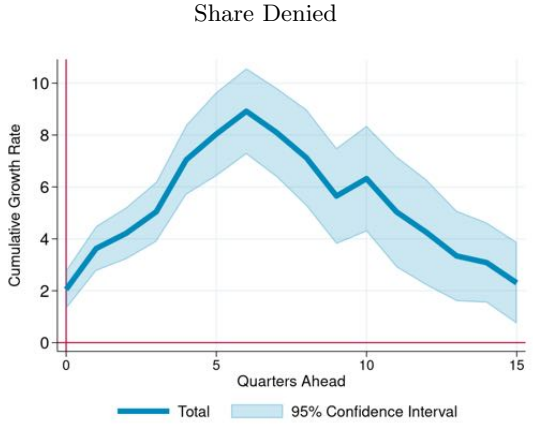
Figures

Figure 1: Household Debt Outcomes



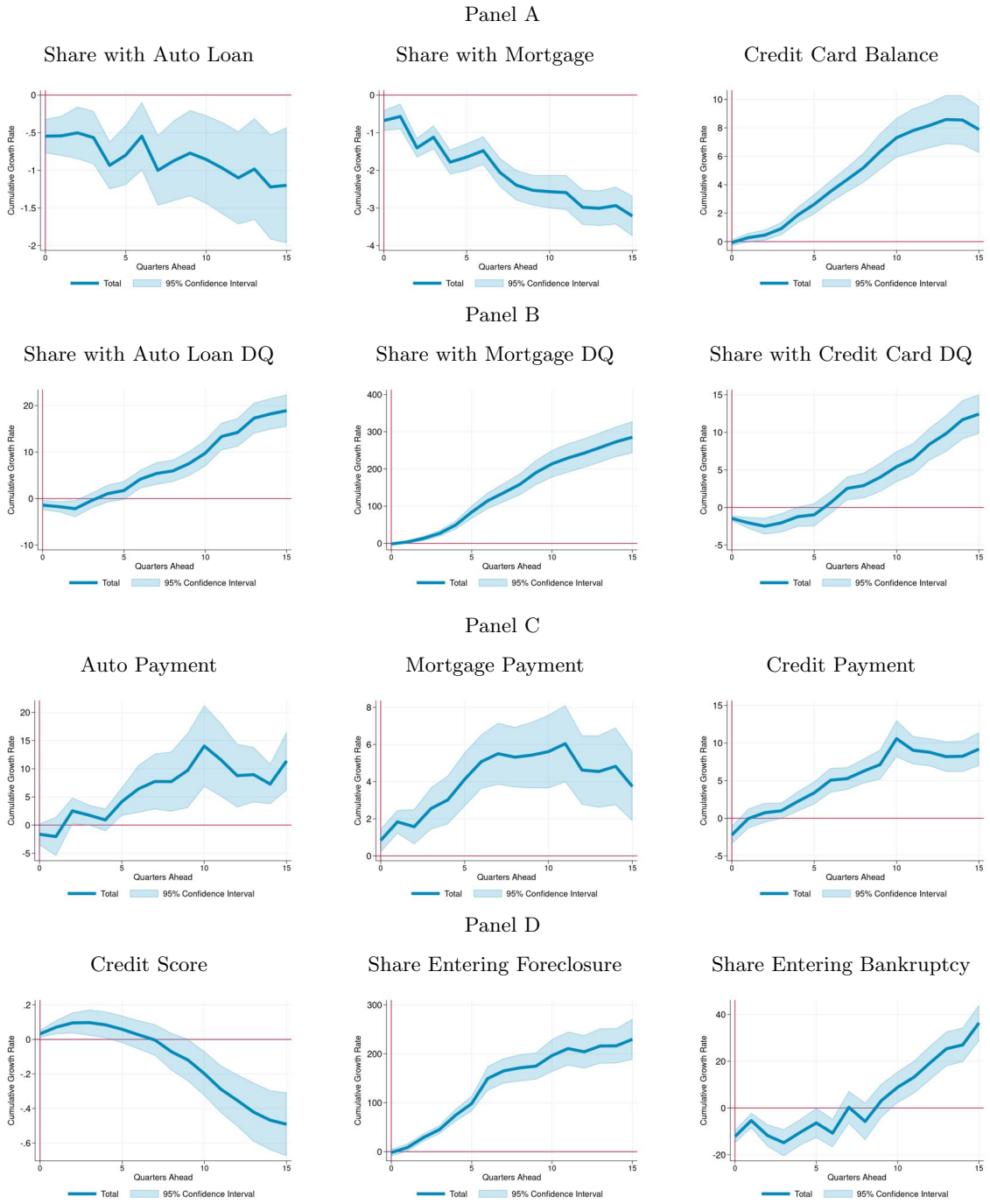
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 2: Mortgage Outcomes



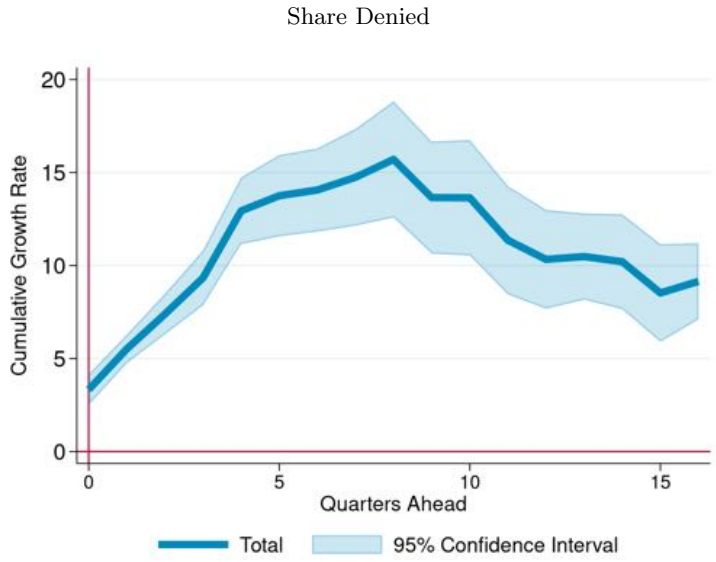
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1–2022Q4.

Figure 3: Household Debt Outcomes, Monetary Policy Shocks from Nakamura and Steinsson (2018)



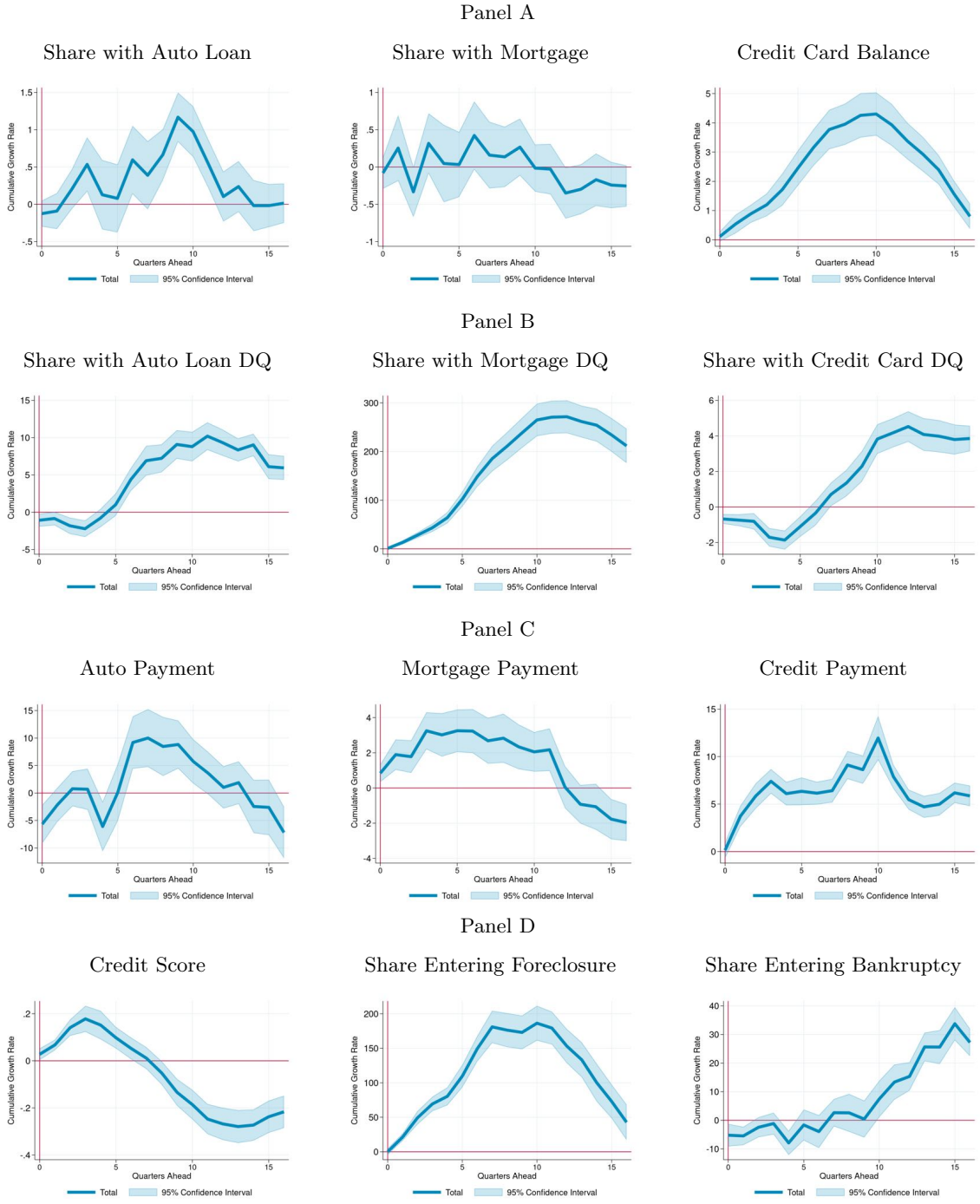
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2022Q3. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 4: Mortgage Outcomes, Using Monetary Policy Shocks from Nakamura and Steinsson (2018)



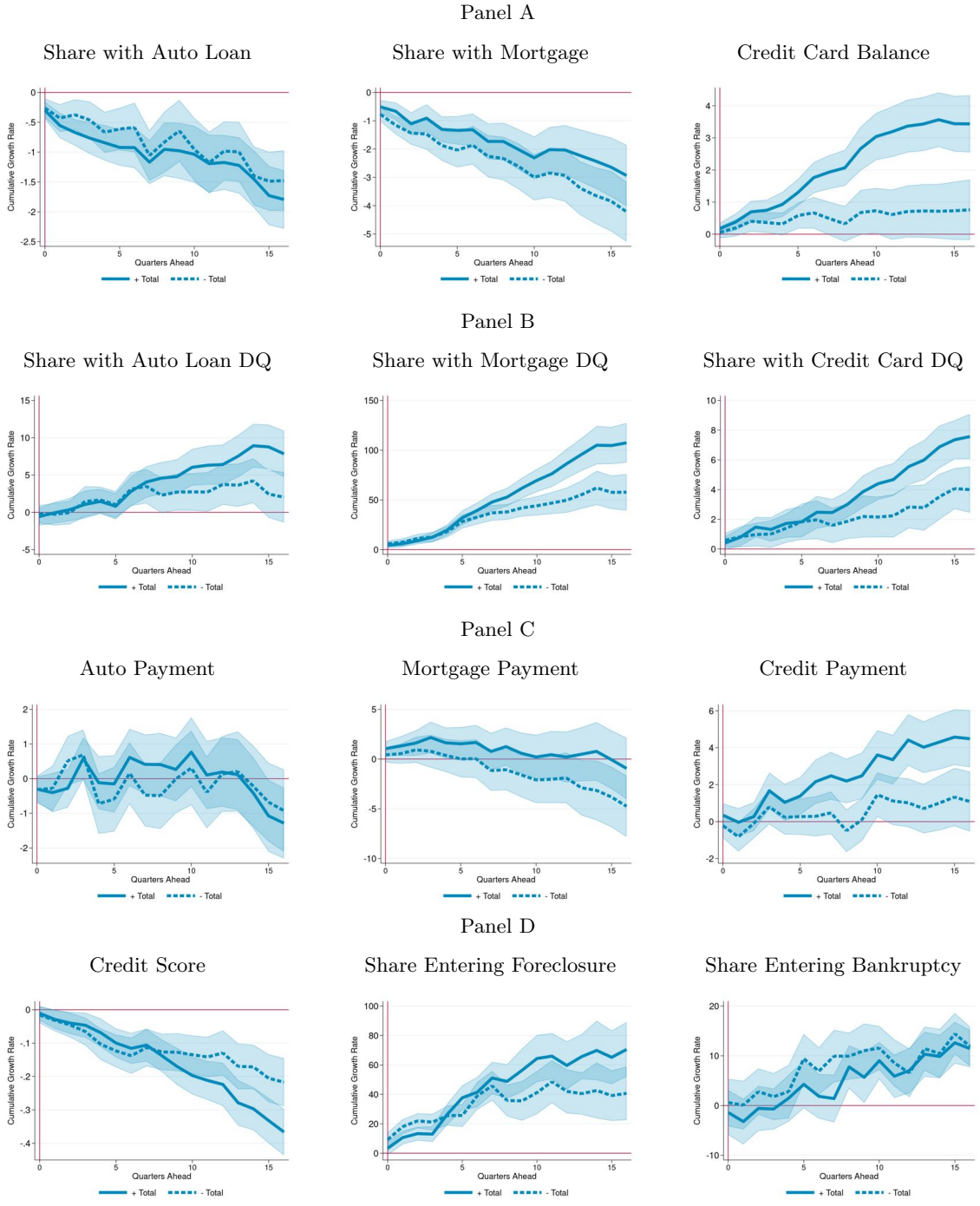
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1–2022Q4.

Figure 5: Household Debt Outcomes, 1999-2008



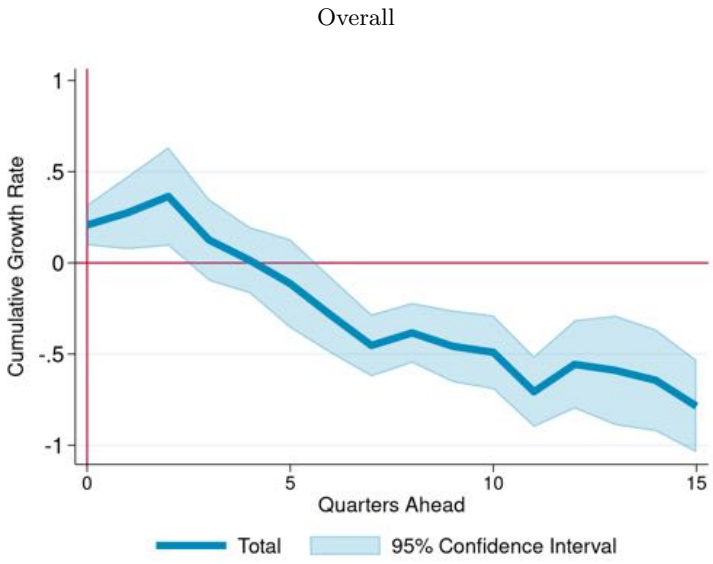
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2008q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 6: Household Debt Outcomes, Asymmetric Effects of Positive and Negative Shocks



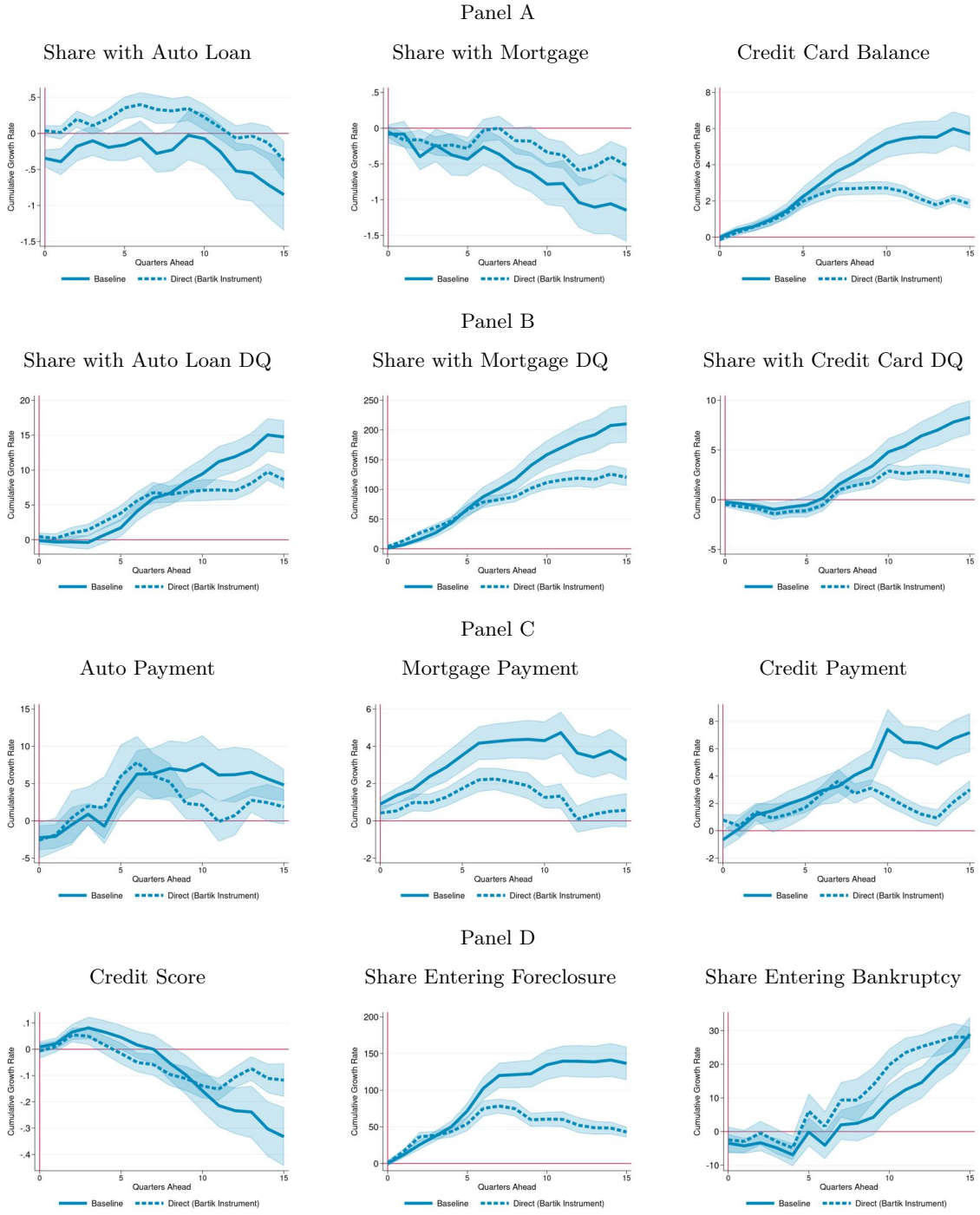
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates impulse response for positive FFR shocks and dashed line for negative FFR shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 7A: Total Employment, DTI as Exposure Measure



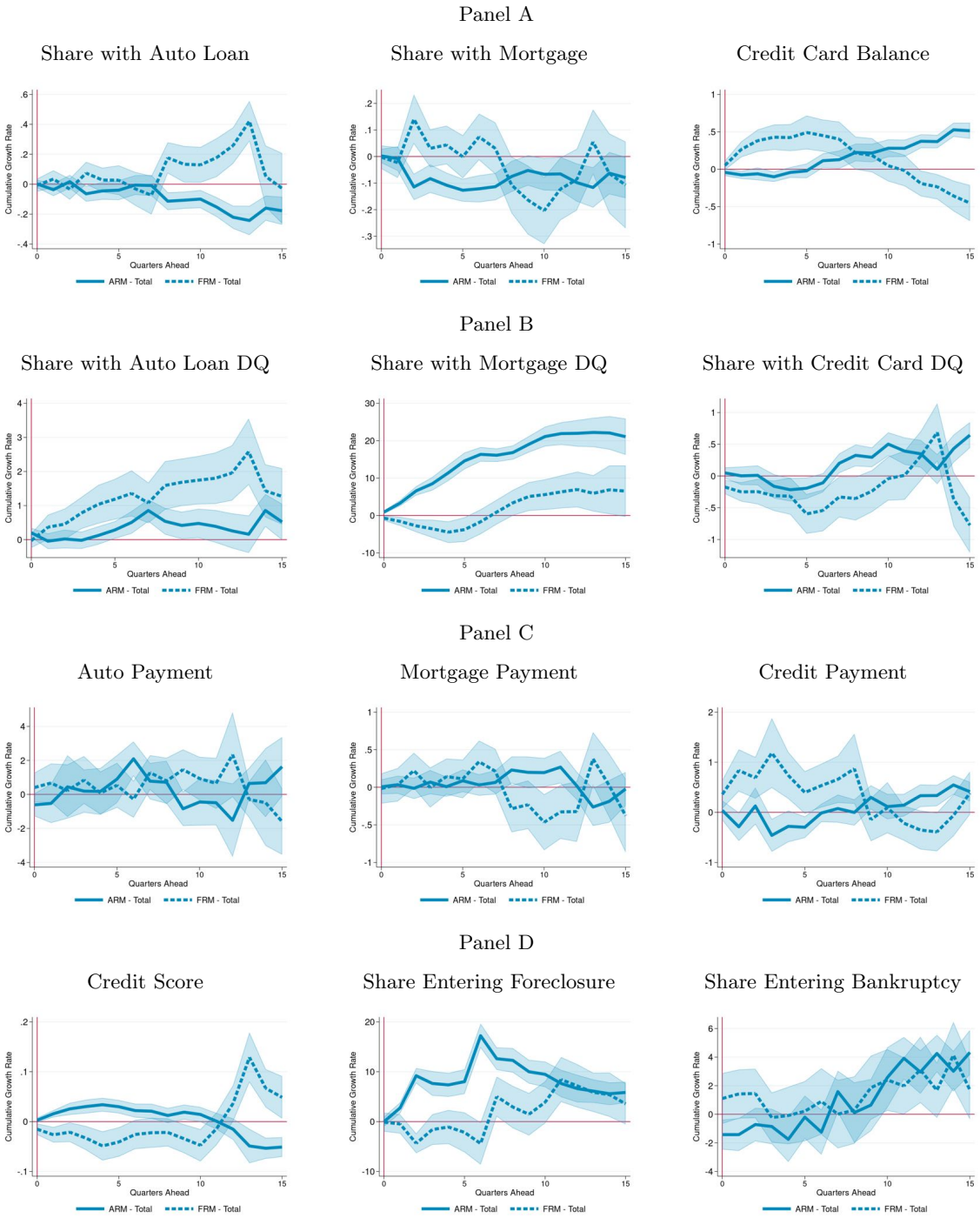
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the U.S. Census Bureau Quarterly Workforce Indicators.

Figure 7B: Household Debt Outcomes (Total vs. Direct Effects using Bartik labor-demand shocks)



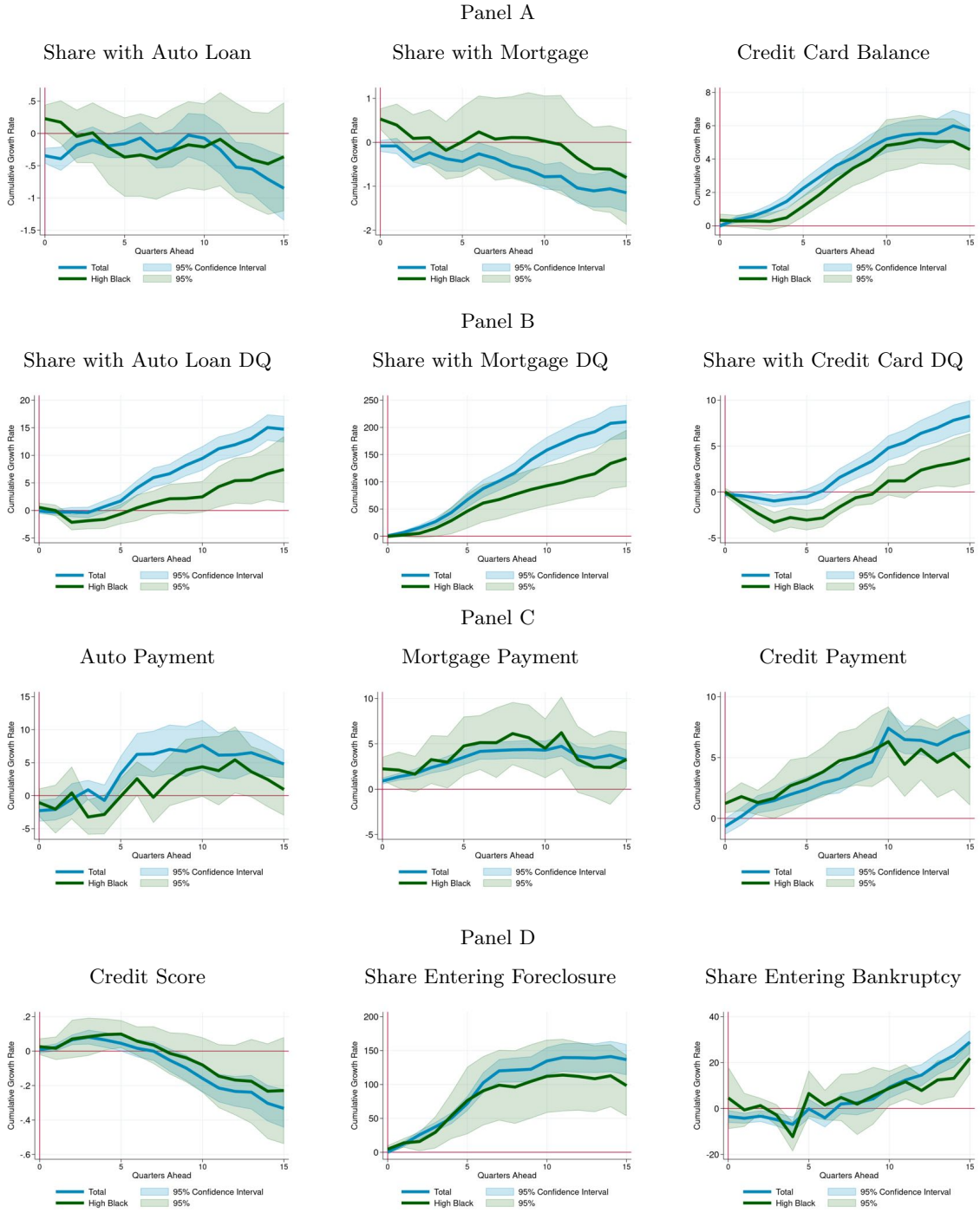
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (3), which uses Bartik labor-demand shocks to isolate the direct effect. The solid blue line indicates the total impulse response, while the dashed blue line indicates the impulse response when using the Bartik labor-demand shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure 8: Household Debt Outcomes, ARM vs. FRM Horse Race. **ARM and FRM DTI are standardized**



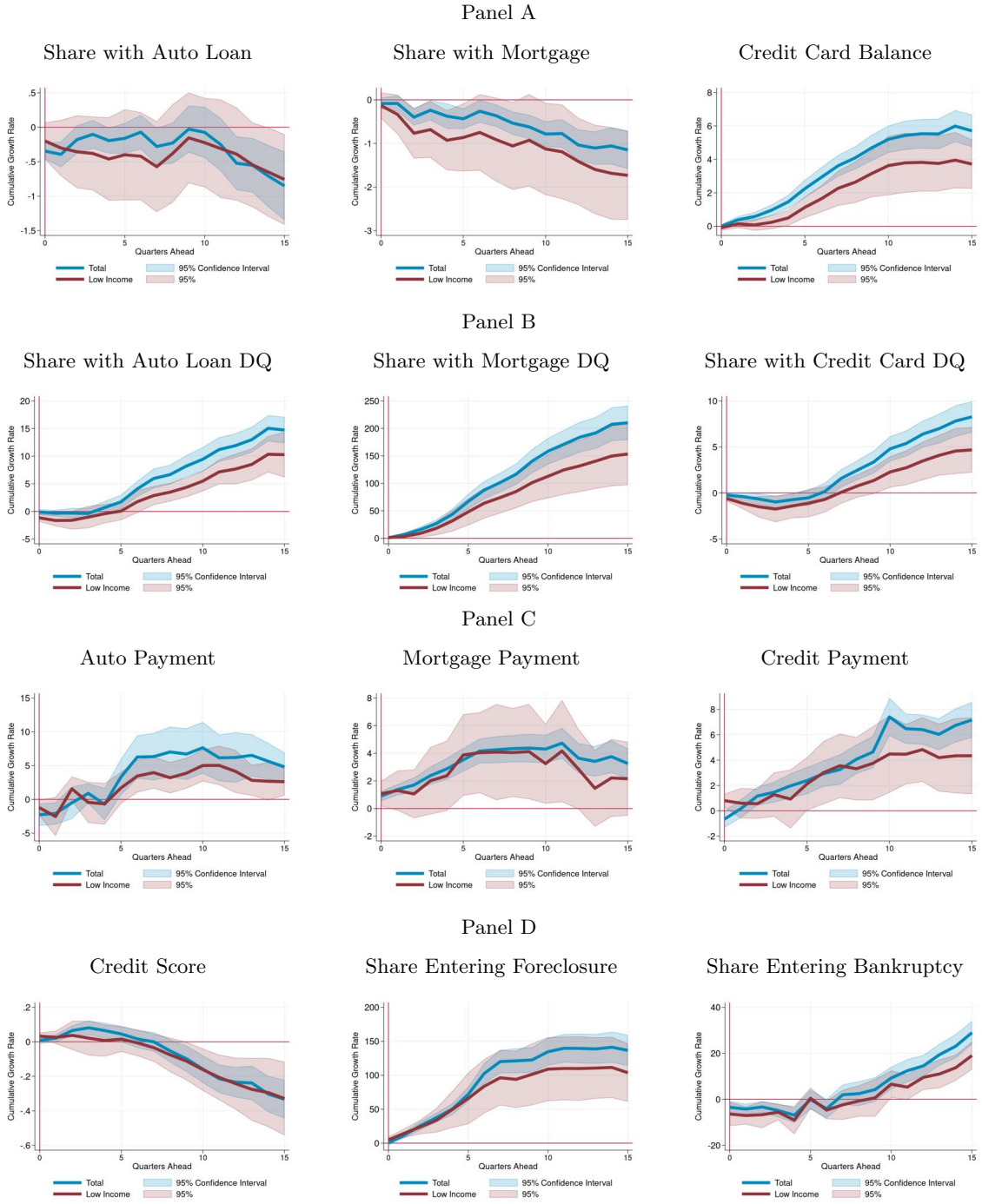
Note: Each line plots the three-quarter smoothed impulse response functions of the growth of the dependent variable, using equation (5). Solid blue line indicates impulse response for ARM DTI, and dotted blue line indicates impulse response for FRM DTI. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure 9A: Household Debt Outcomes: Heterogeneity by Race/Ethnicity



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. Higher-Share Black counties are counties in the top quartile of weighted distribution of the share of Black residents.

Figure 9B: Household Debt Outcomes: Heterogeneity by Income



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. Low-income counties are counties in the bottom quartile of the weighted distribution of median income.

Figure 9C: Household Debt Outcomes: Heterogeneity by Credit Score

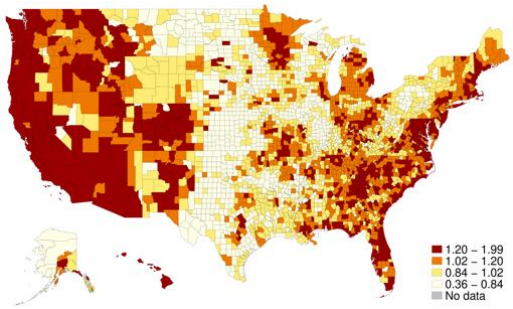


Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. High subprime counties are counties in the top quartile of the weighted distribution of share of subprime residents.

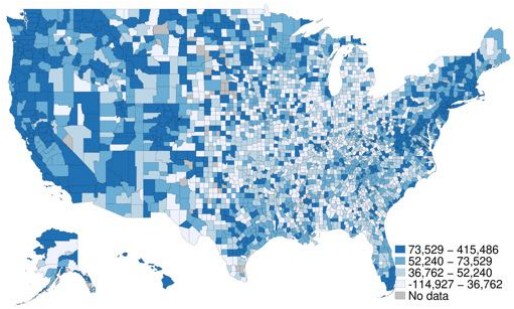
Appendix A: Outcomes and Robustness

Figure A.1: Maps of Equity, DTI, and Mortgage Types

Panel A: DTI

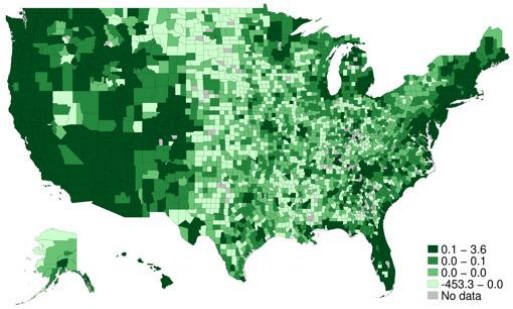


Panel B: Home Equity

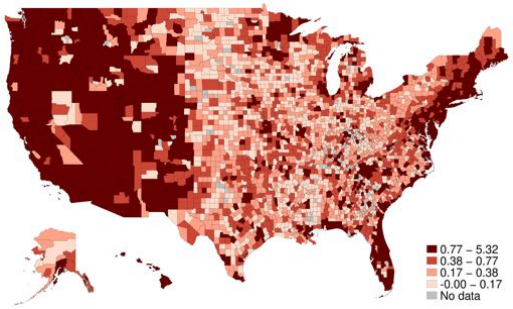


Panel C: DTI

ARM DTI

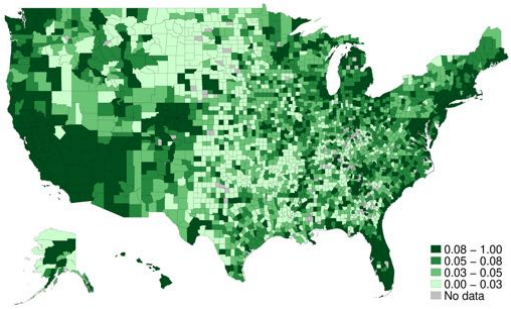


FRM DTI



Panel D: % Share of Mortgage Debt

ARM Mortgage Share



FRM Mortgage Share

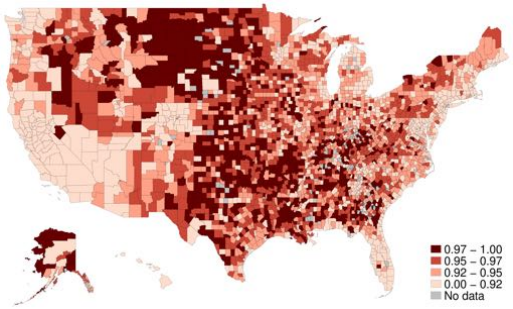


Figure A.2: ARM DTI vs. FRM DTI balance.

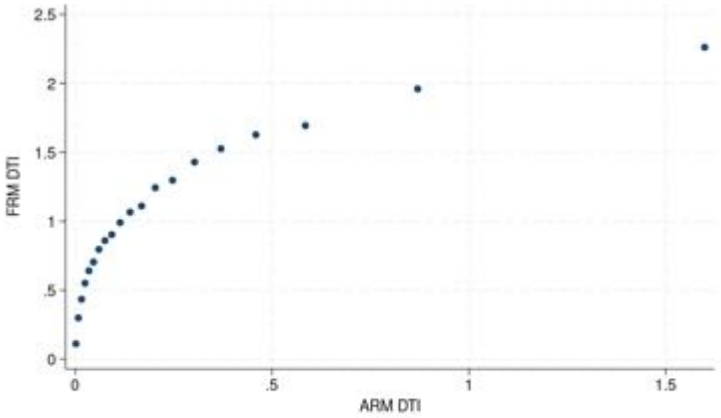
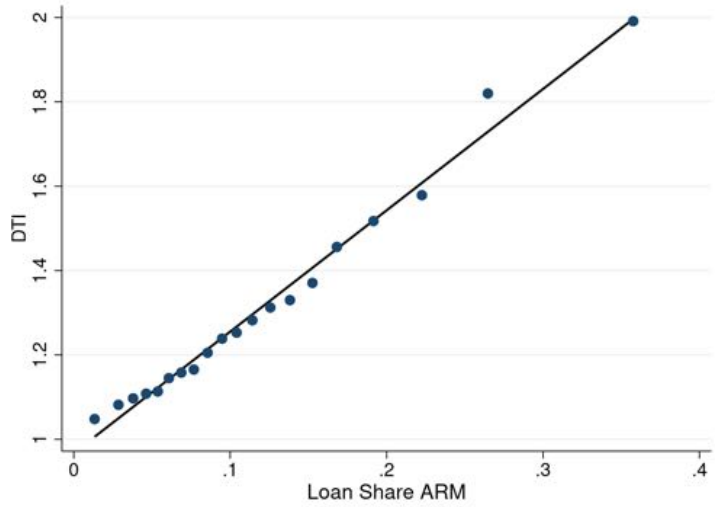


Figure A.3: DTI vs. Loan Share ARM



[8]Correlation coefficient 0.68; regression coefficient 0.64.

Figure A.4: National-Level ARM Share

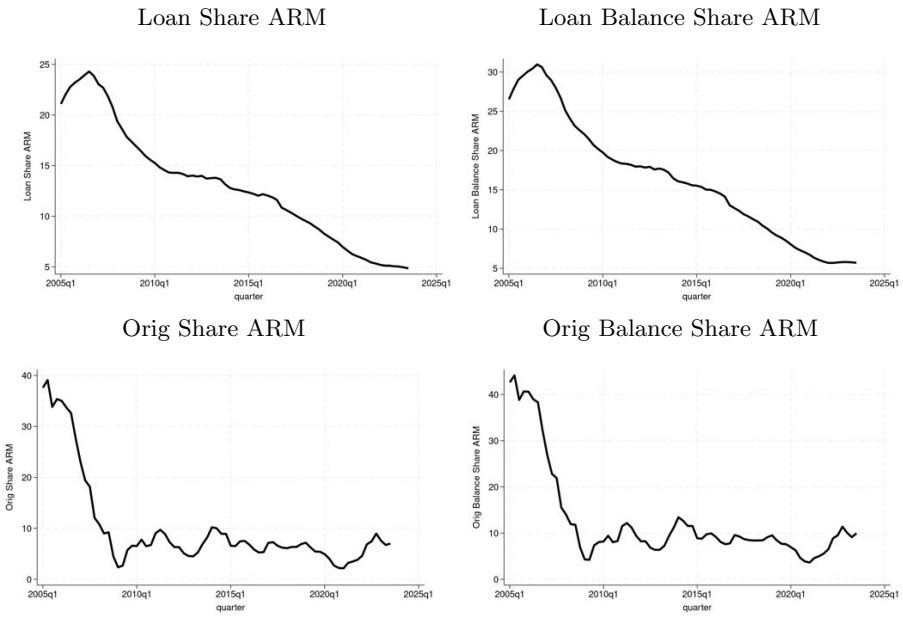


Figure A.5: County-Level ARM Share, weighted by number of borrowers

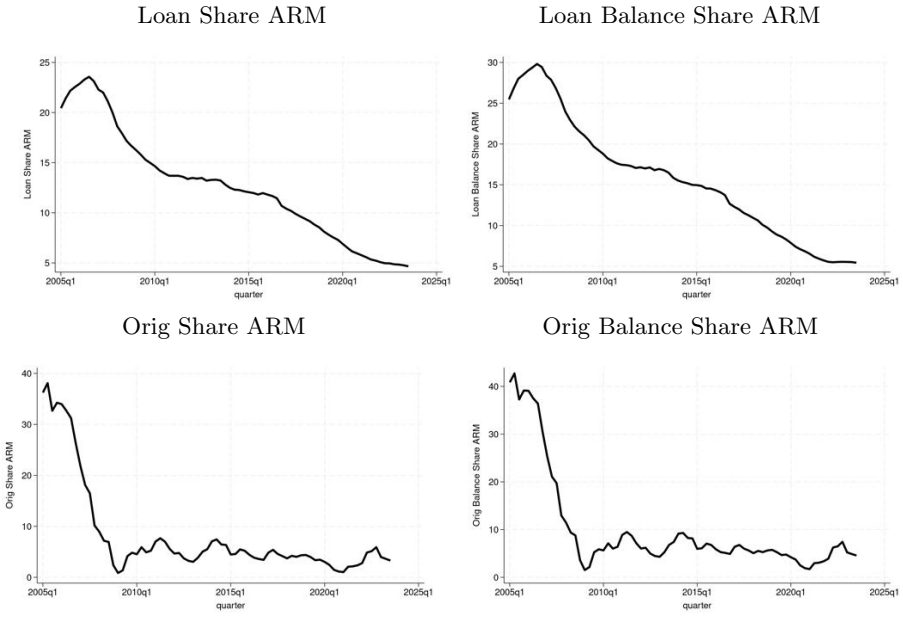
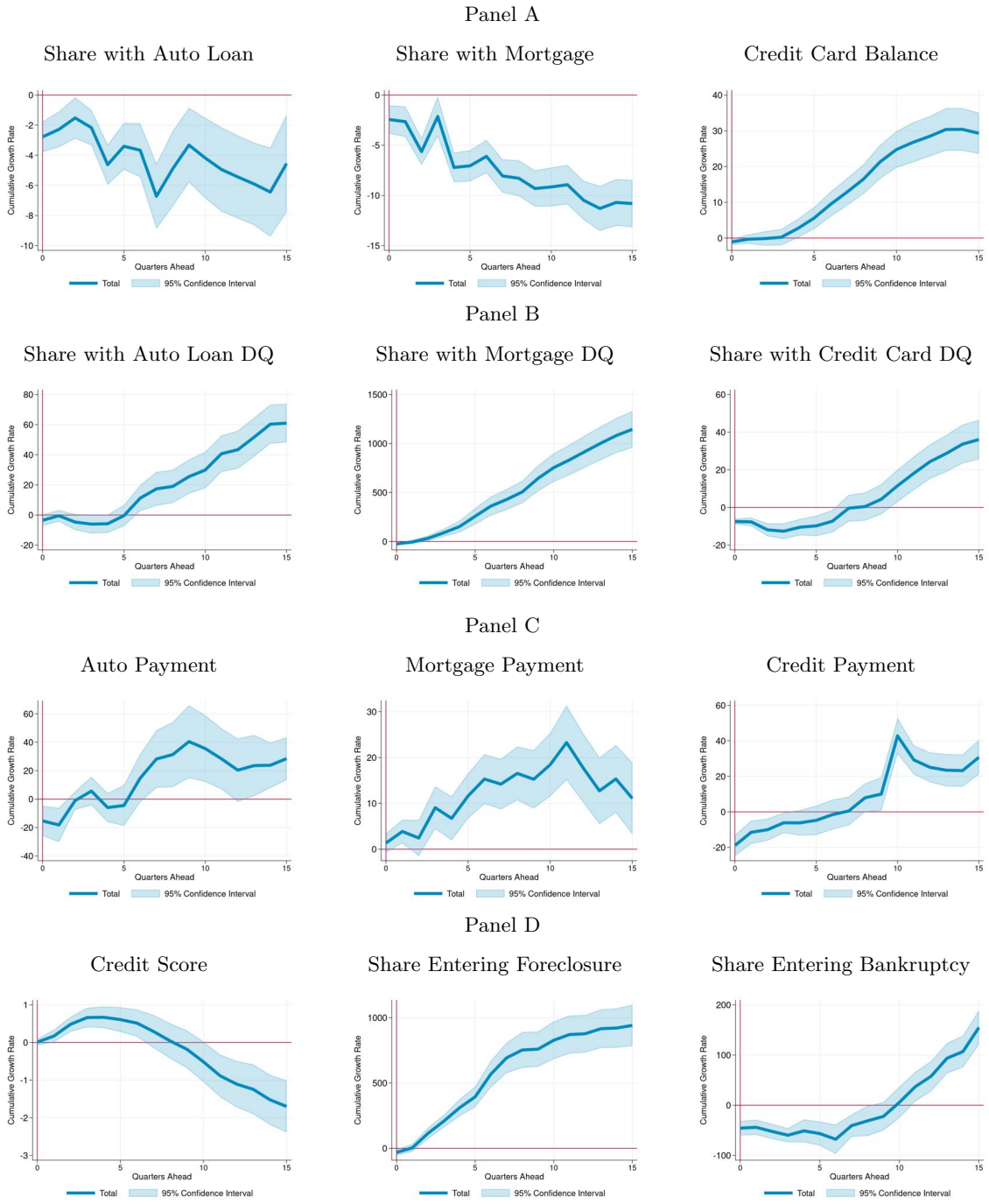


Figure A.6: Household Debt Outcomes, Dollar Share of Delinquencies and Balances



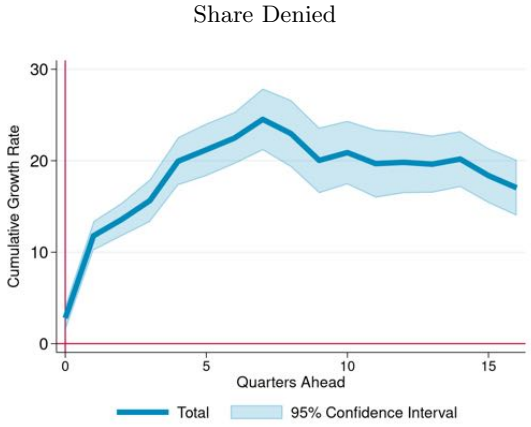
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.7A: Household Debt Outcomes, Monetary Policy Shocks from Jarociński and Karadi (2020)



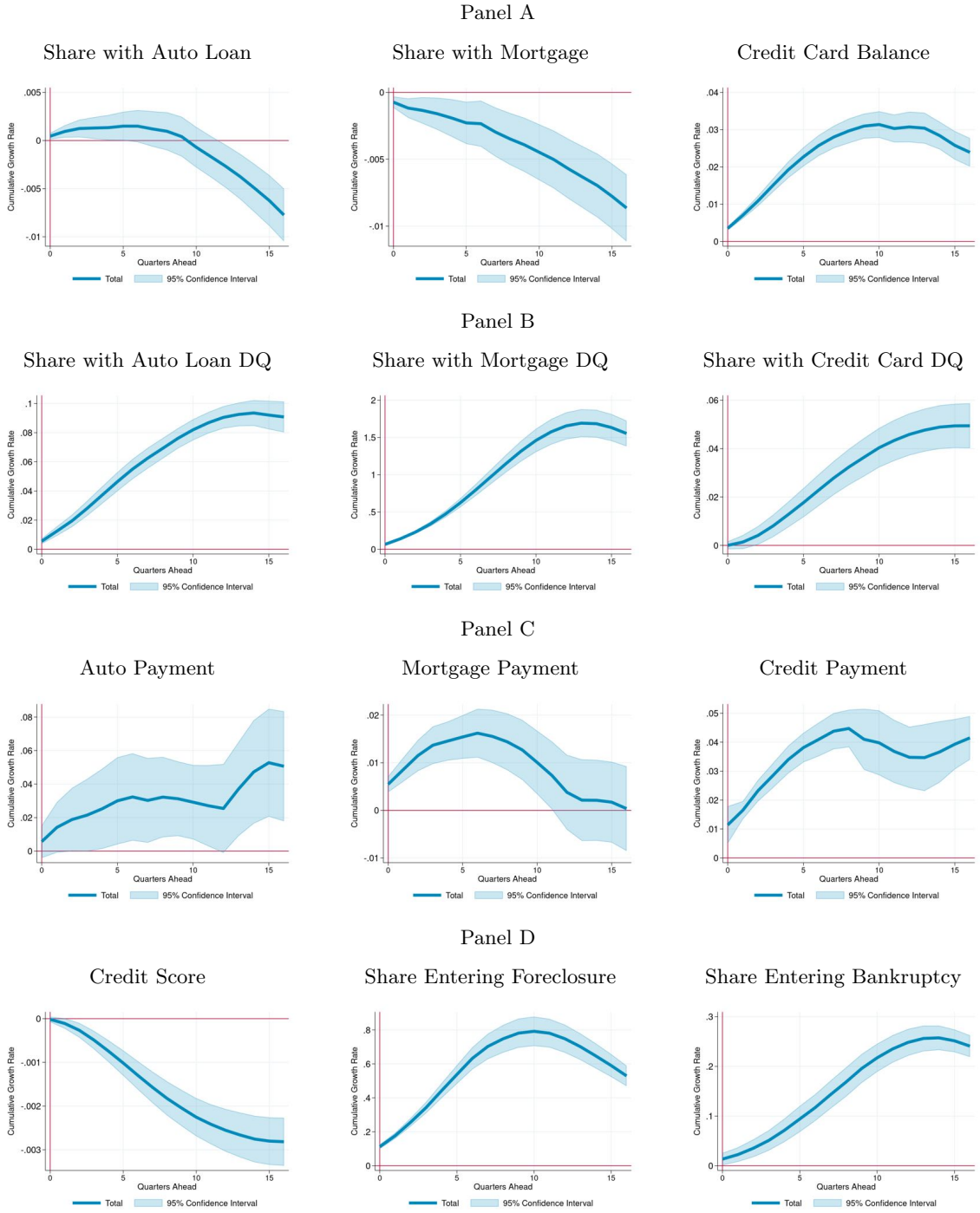
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.7B: Mortgage Outcomes, Using Monetary Policy Shocks from Jarociński and Karadi (2020)



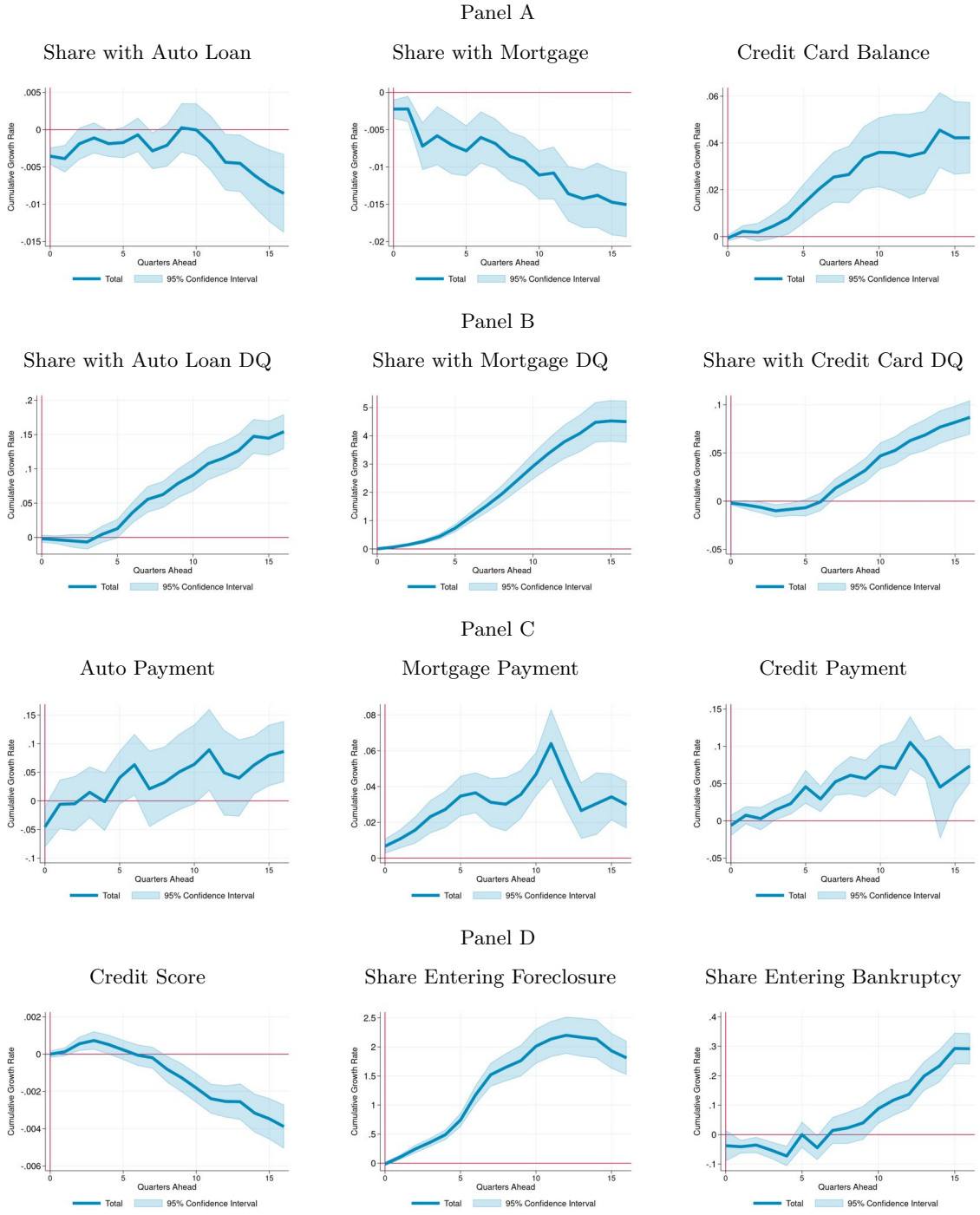
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1–2022Q4.

Figure A.8: Household Debt Outcomes - One Lag



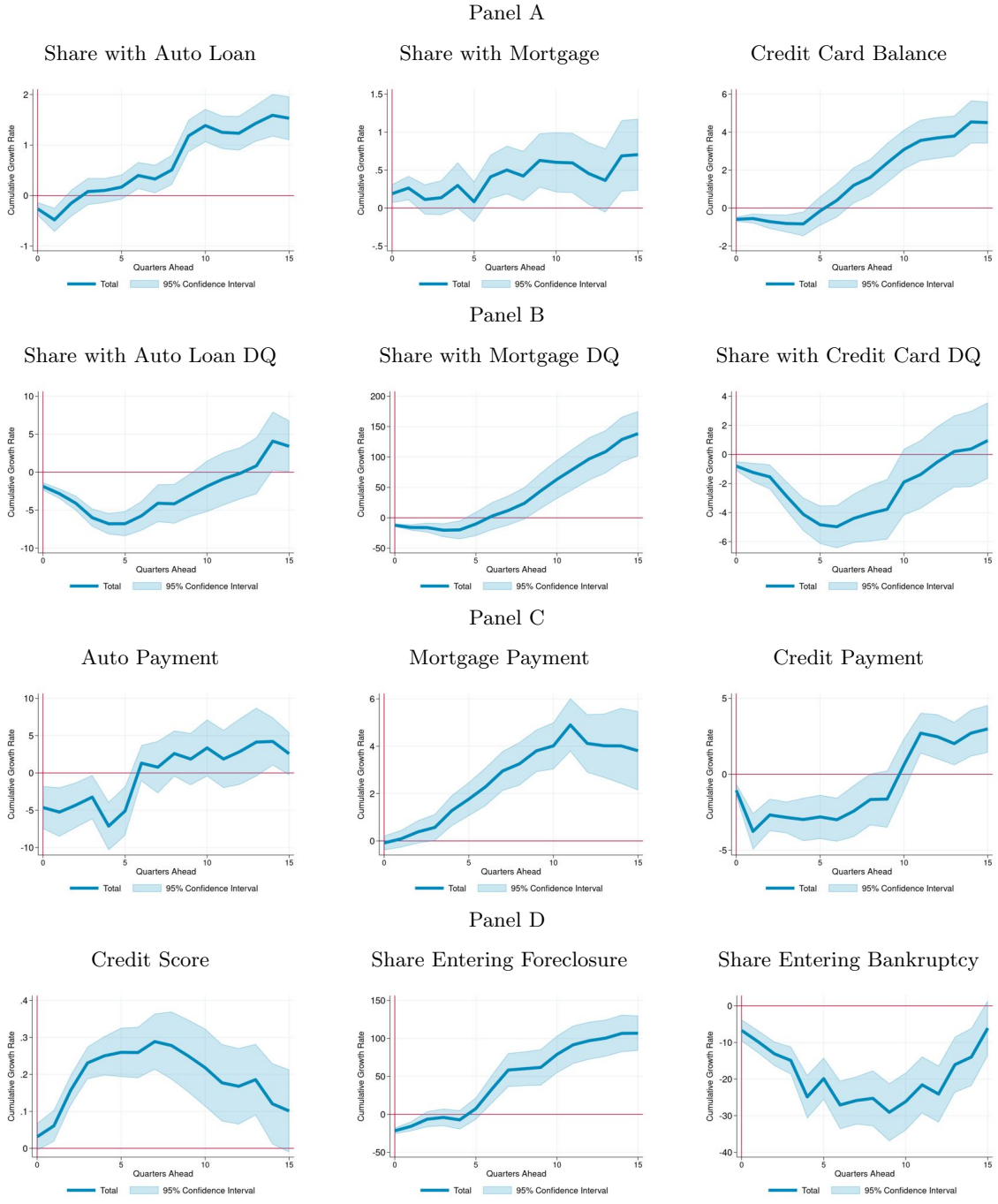
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), replacing $J = 8$ with $J = 1$. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.9: Household Debt Outcomes, Controlling for EPOP lags



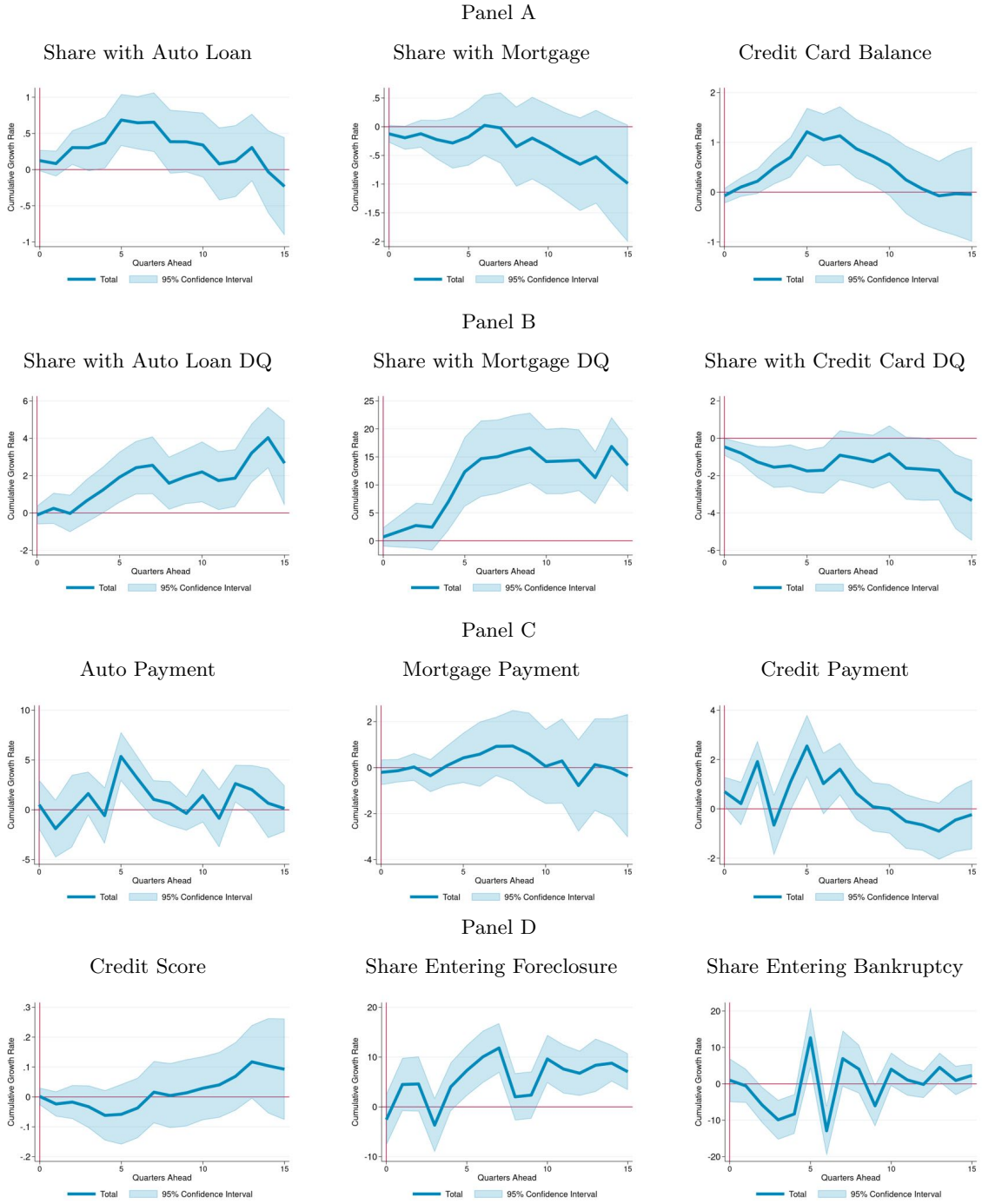
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), controlling for lags of the employment-to-population ratio (EPOP). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.10: Household Debt Outcomes - Use FFR Differences



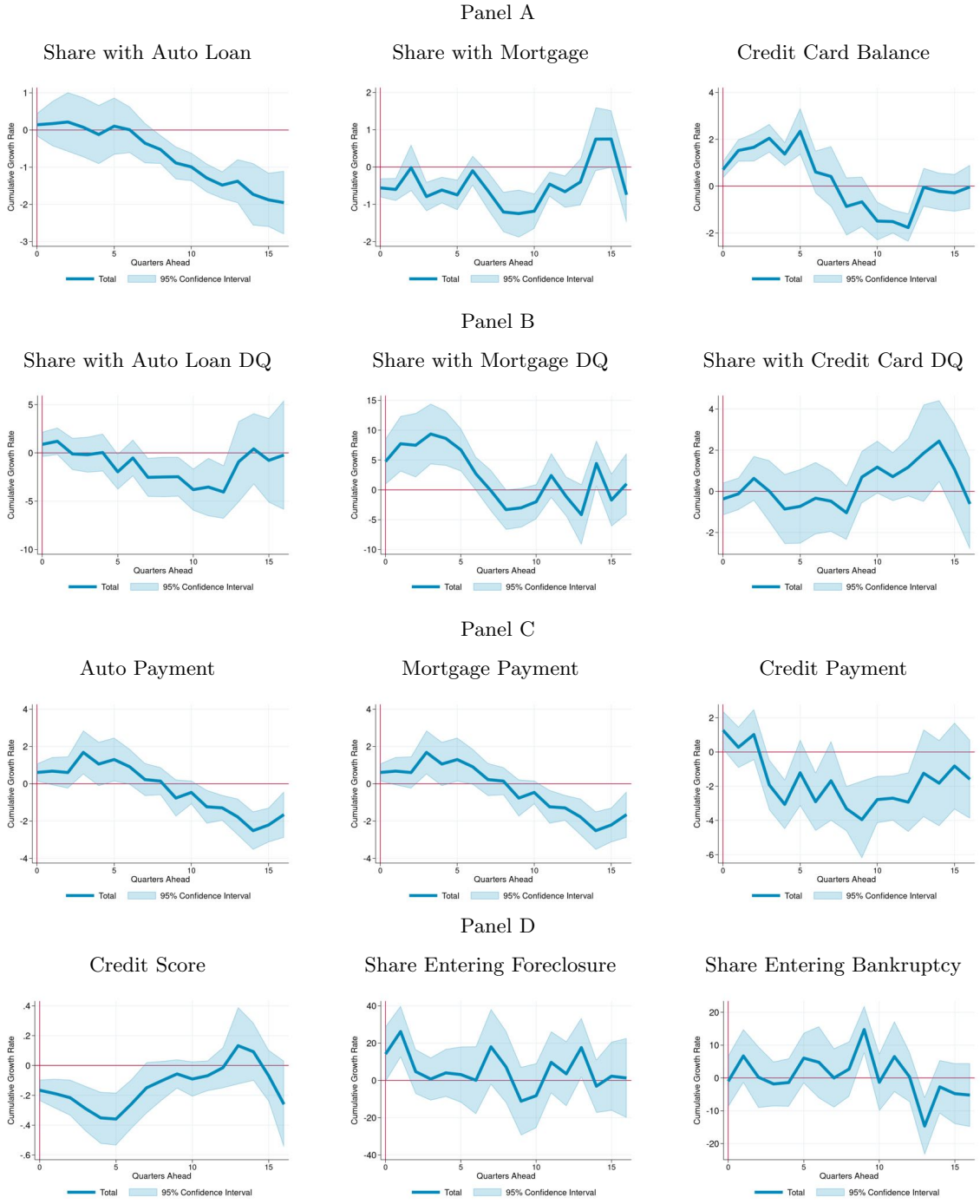
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), replacing FFR by its first difference. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.11: Household Debt Outcomes, Controlling for ACS Covariates



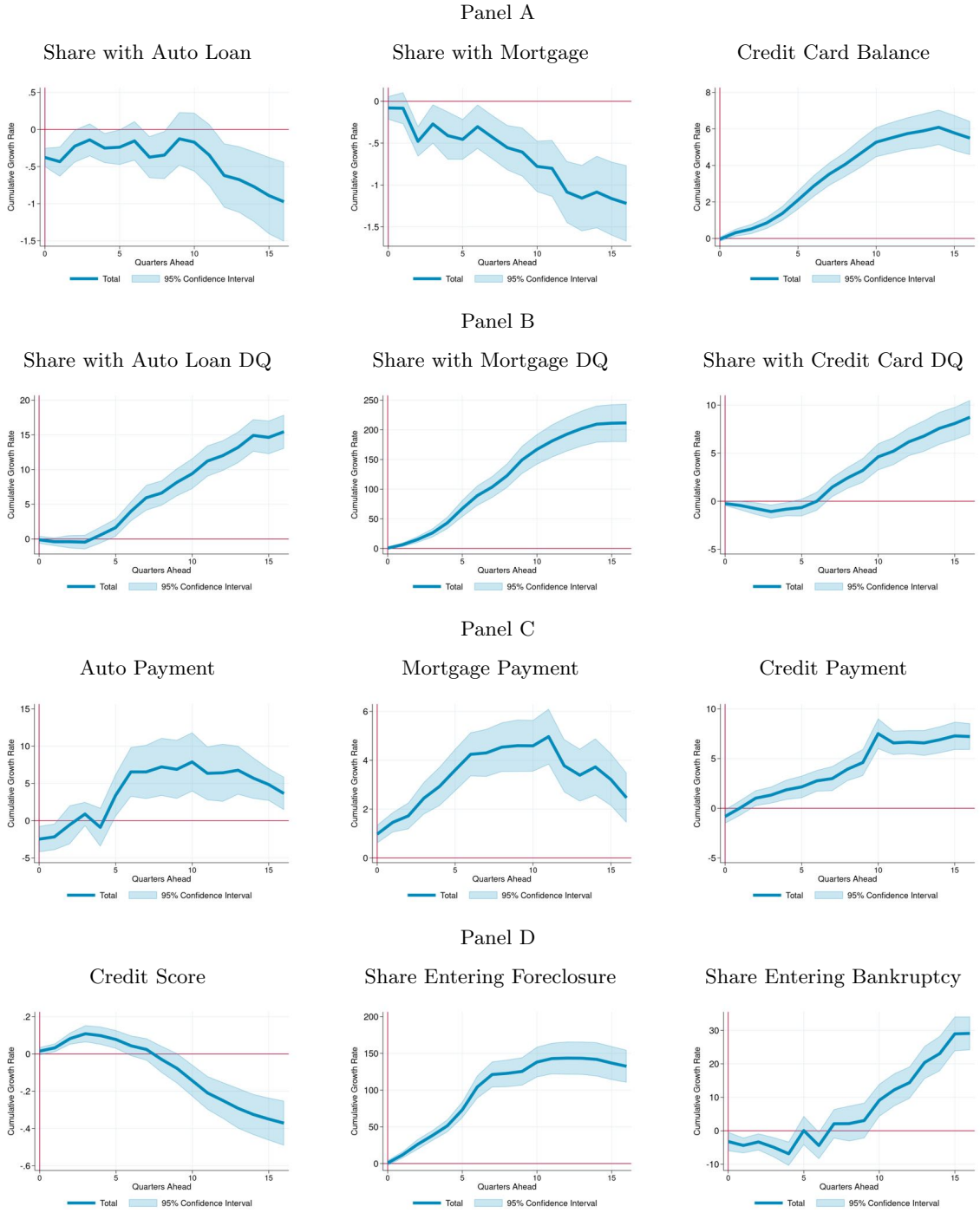
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), adding county-level controls from the ACS: race, age, unemployment rate, average wage, and share of industries such as manufacturing and construction. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.12: Household Debt Outcomes, 2015-Present



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 2015Q1-2023Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

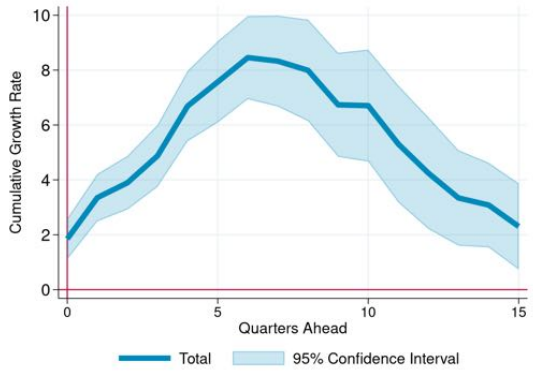
Figure A.13A: Household Debt Outcomes, pre-COVID (1999q1 - 2019q4)



Note: Each line plots the impulse response functions of the growth of the dependent variable. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2019Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.13B: Mortgage Outcomes, Pre-COVID (1999q1 - 2019q4)

Share Denied



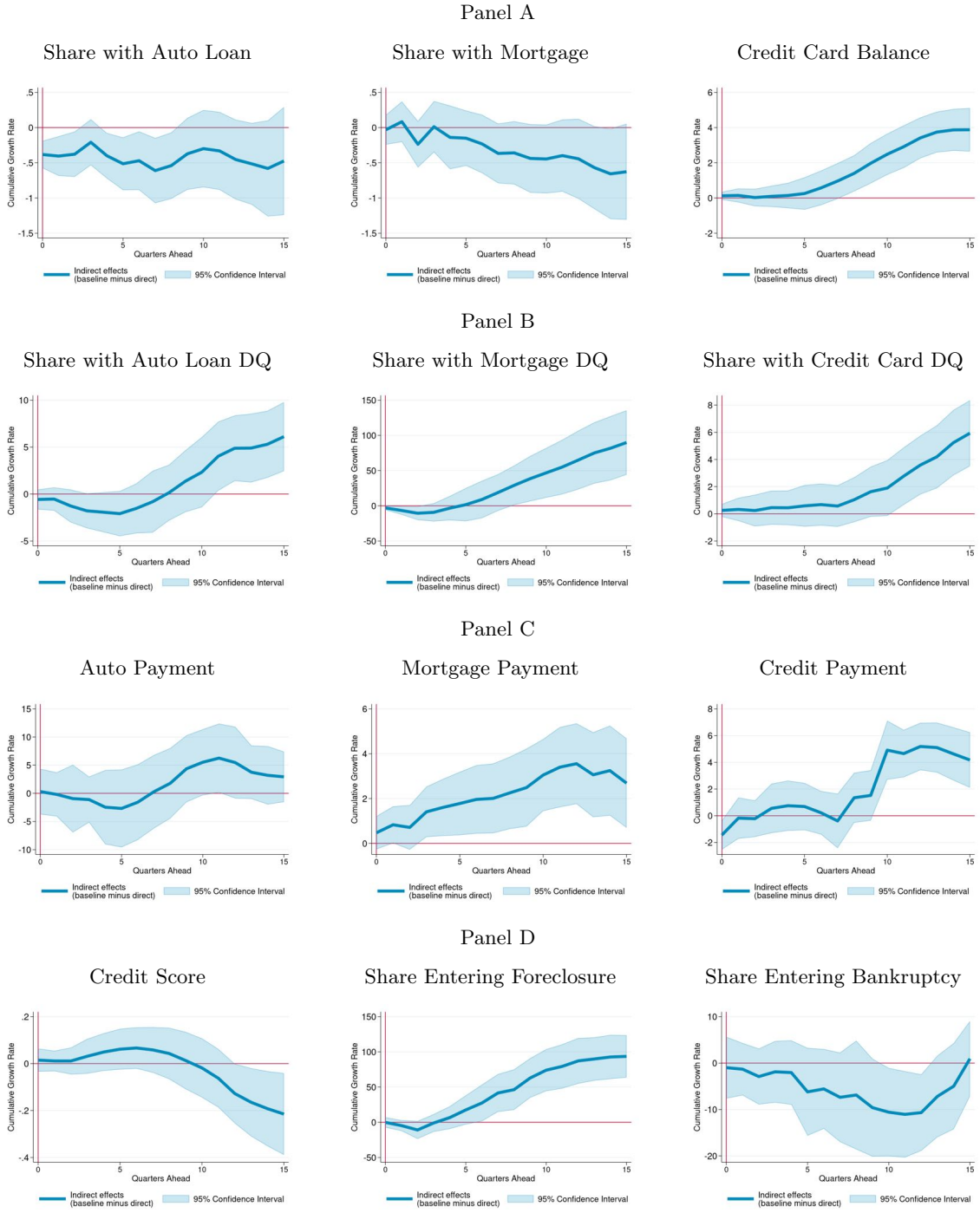
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1–2019Q4.

Figure A.14: Household Debt Outcomes, Difference Between Positive and Negative Shocks



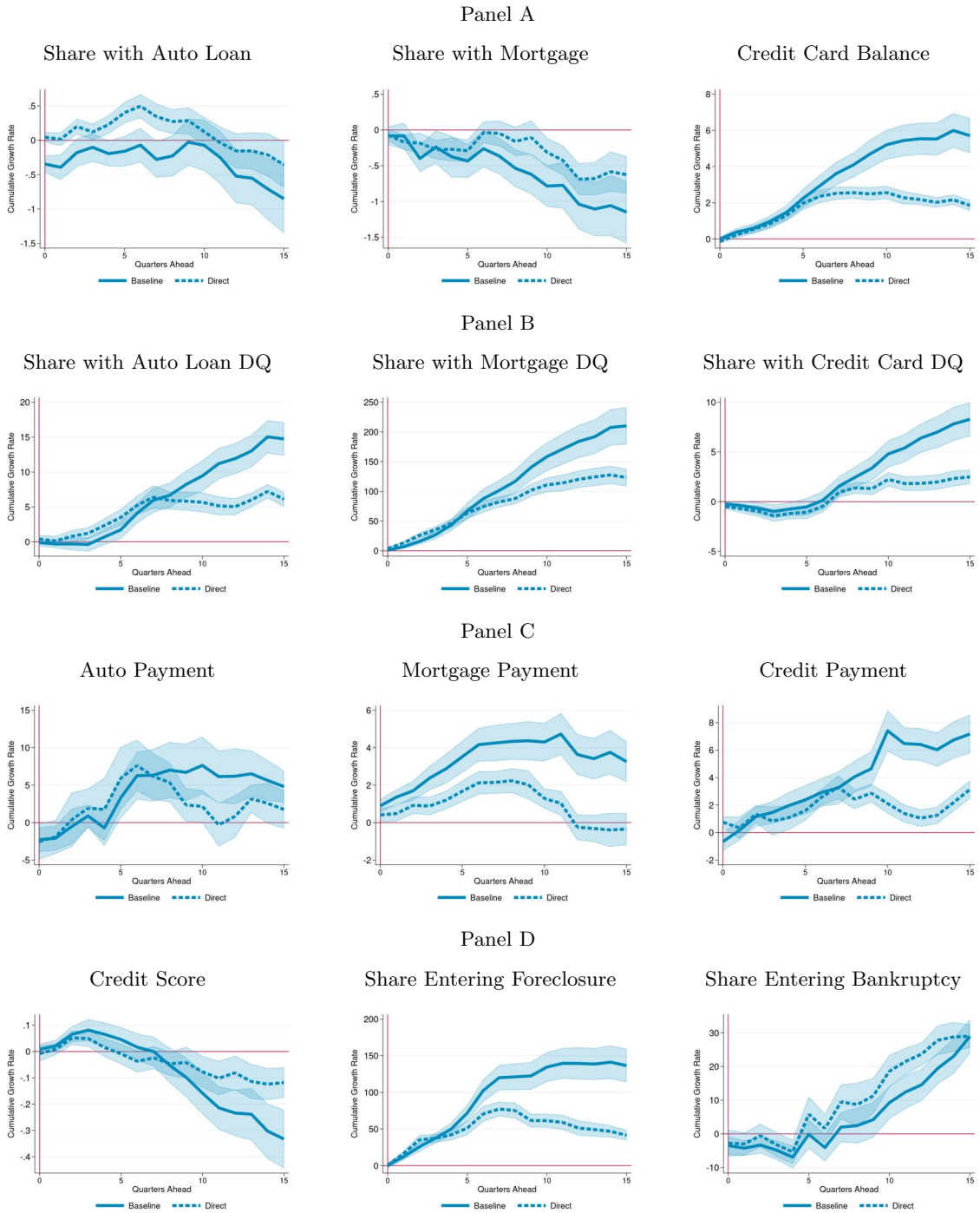
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates the difference in impulse response for positive FFR shocks and negative FFR shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.15: Household Debt Outcomes: indirect effects, using Bartik labor-demand shocks



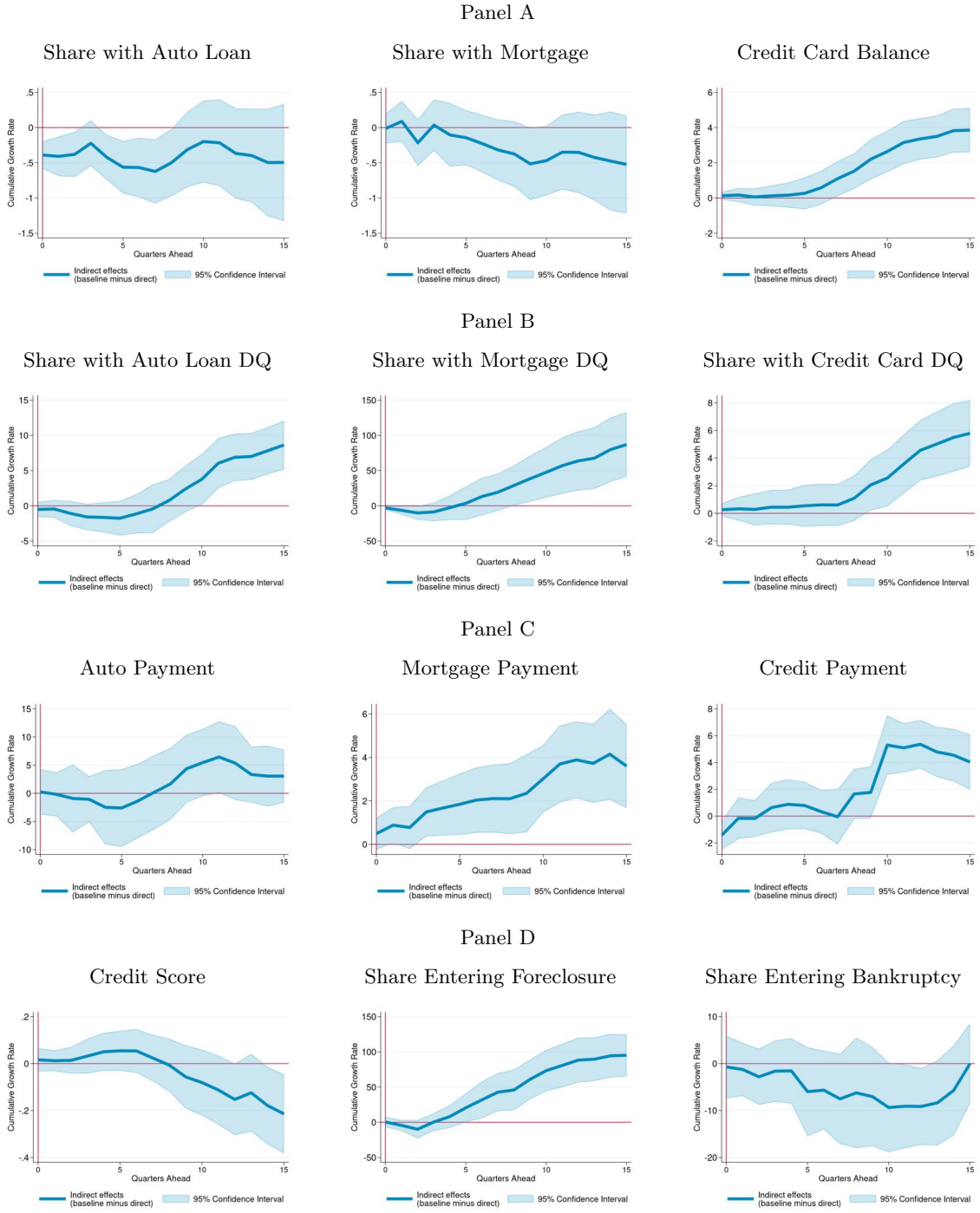
Note: The line plots the impulse response functions of the growth of the dependent variable using equation (3), using Bartik labor-demand shocks (total effect minus that from the Bartik labor-demand shocks) to isolate the indirect effect. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.16: Household Debt Outcomes: total and direct effects, employment growth leads & controls



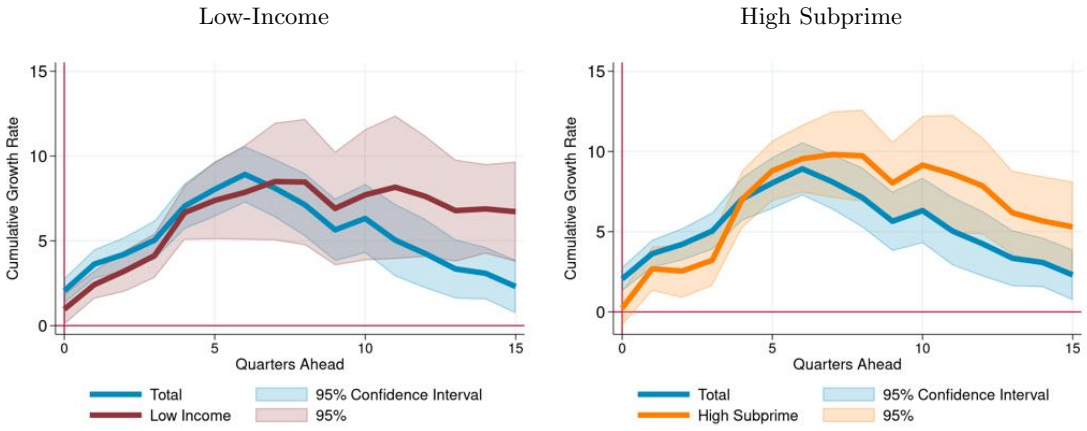
Note: The solid line shows the baseline (total) impulse response, while the dashed line shows the direct impulse response after controlling for leads of employment growth in a specification analogous to equation (3). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.17: Household Debt Outcomes: indirect effects, employment growth leads & controls



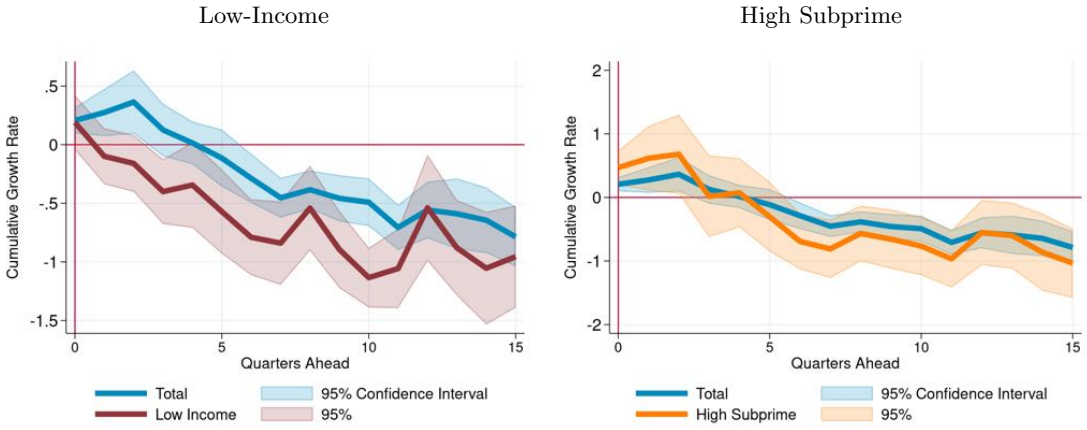
Note: The line plots the impulse response functions of the growth of the dependent variable using equation (3), using a EPOP leads (total effect minus that from the EPOP Leads) to isolate the indirect effect. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.18: Share Denied (by Heterogeneity)



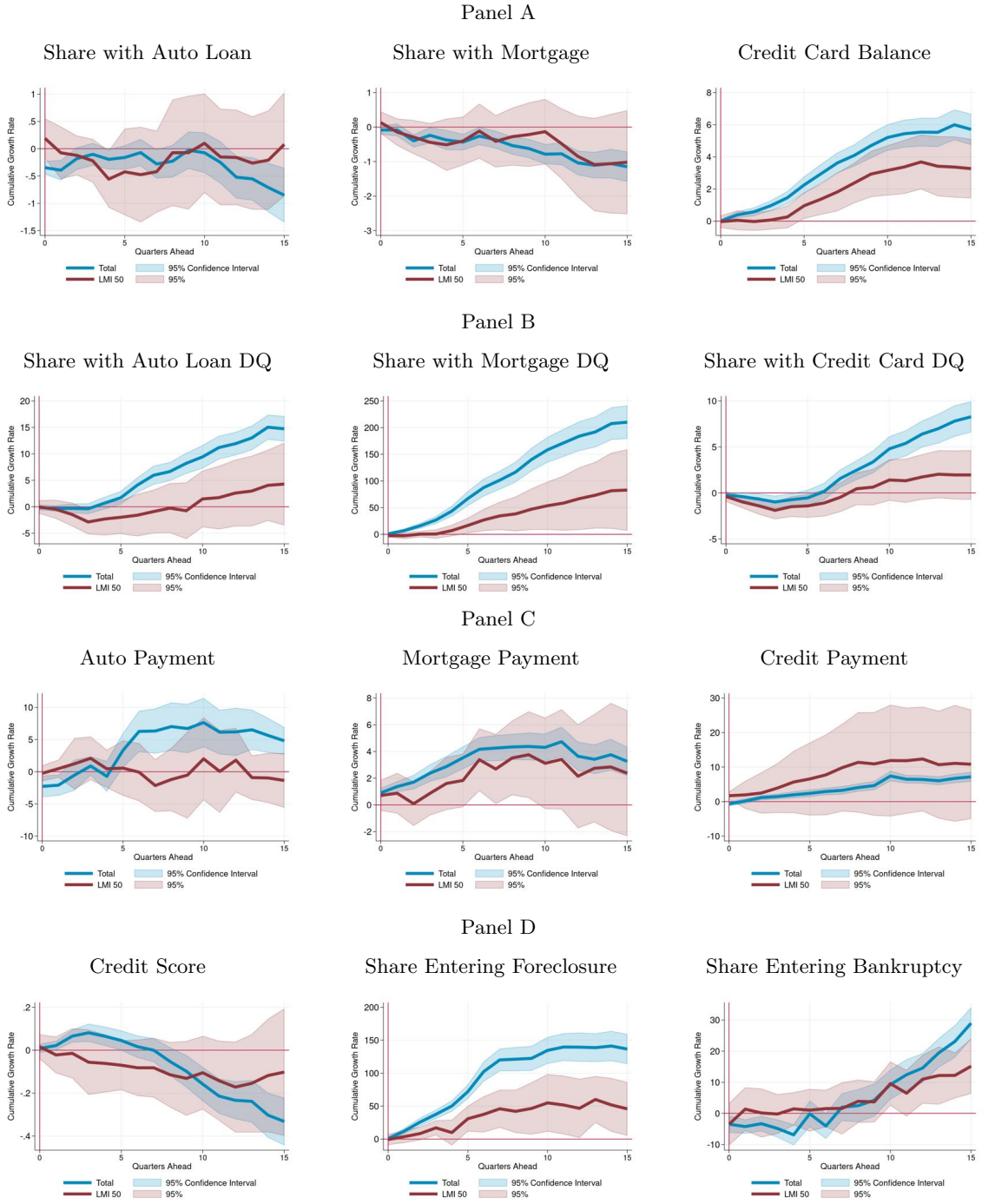
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. Low-income counties are counties in the bottom quartile of the weighted distribution of median income. High subprime counties are counties in the top quartile of the weighted distribution of share of subprime residents.

Figure A.19: Total Employment (by Heterogeneity)



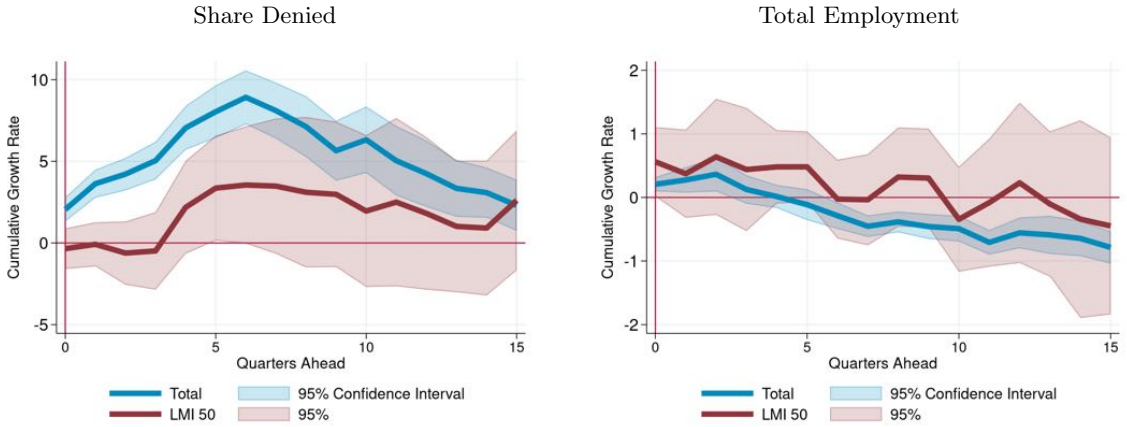
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Low-income counties are counties in the bottom quartile of the weighted distribution of median income. High subprime counties are counties in the top quartile of the weighted distribution of share of subprime residents.

Figure A.20: Household Debt Outcomes: Heterogeneity by LMI 50



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. LMI 50 counties are counties with at least half of its population living in Census-designated LMI (Low-to-Moderate Income) tracts.

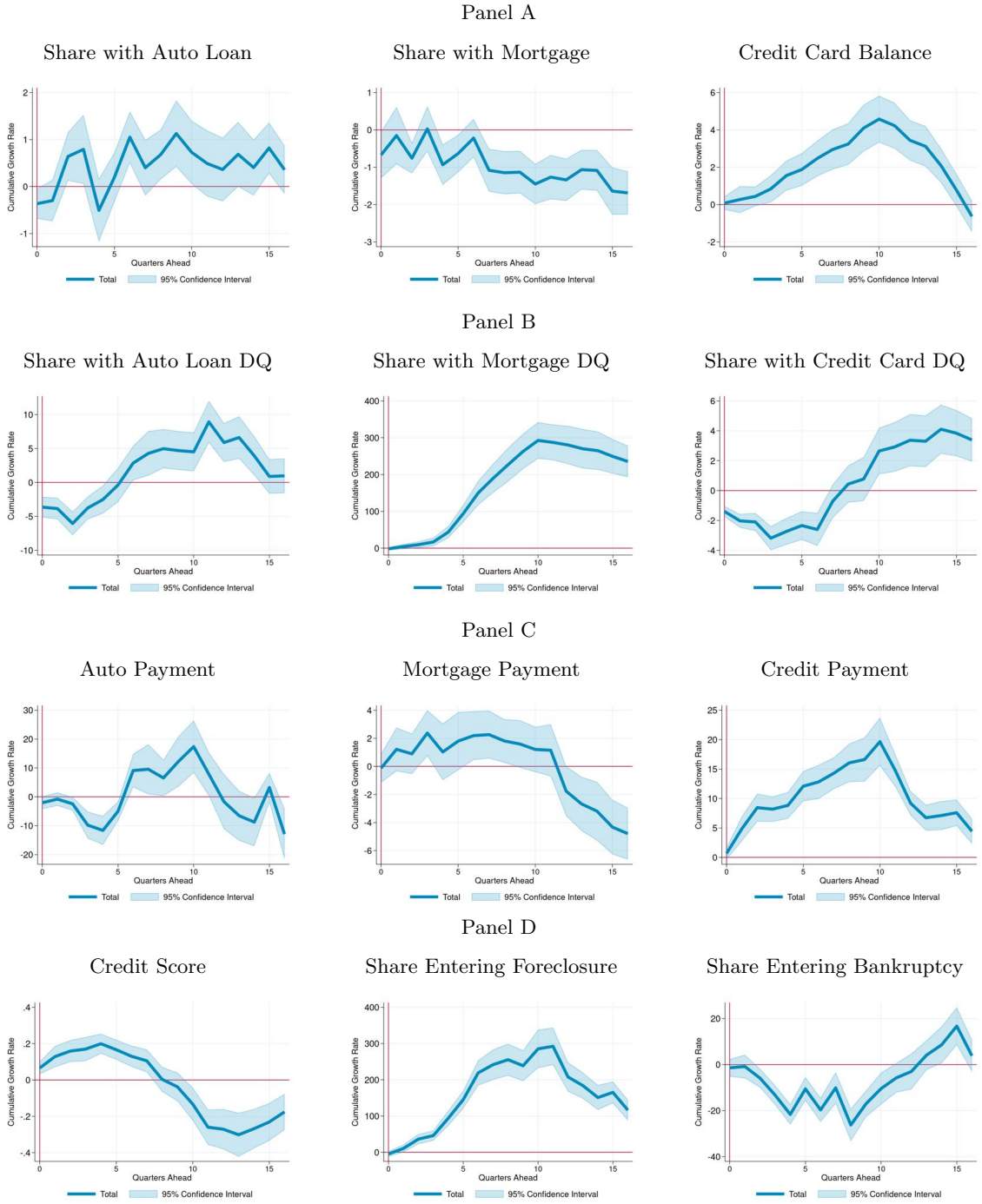
Figure A.21: Mortgage & Employment Outcomes, By LMI Status



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Mortgage application denials are from confidential Home Mortgage Disclosure Act data for range 1999Q1–2019Q4. LMI 50 counties are counties with at least half of their population living in Census-designated LMI (Low-to-Moderate Income) tracts.

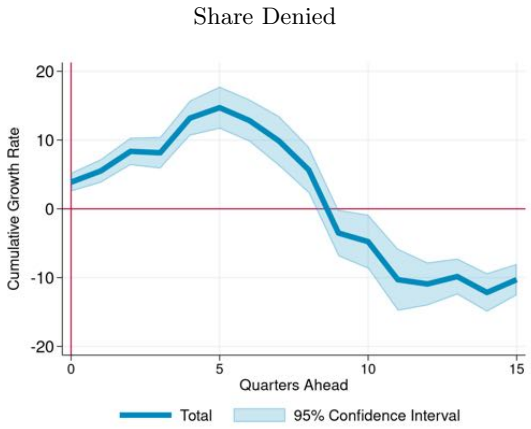
Appendix B: Nakamura and Steinsson (NS) Shocks

Figure B.1: Household Debt Outcomes, 1999-2008, using NS shocks



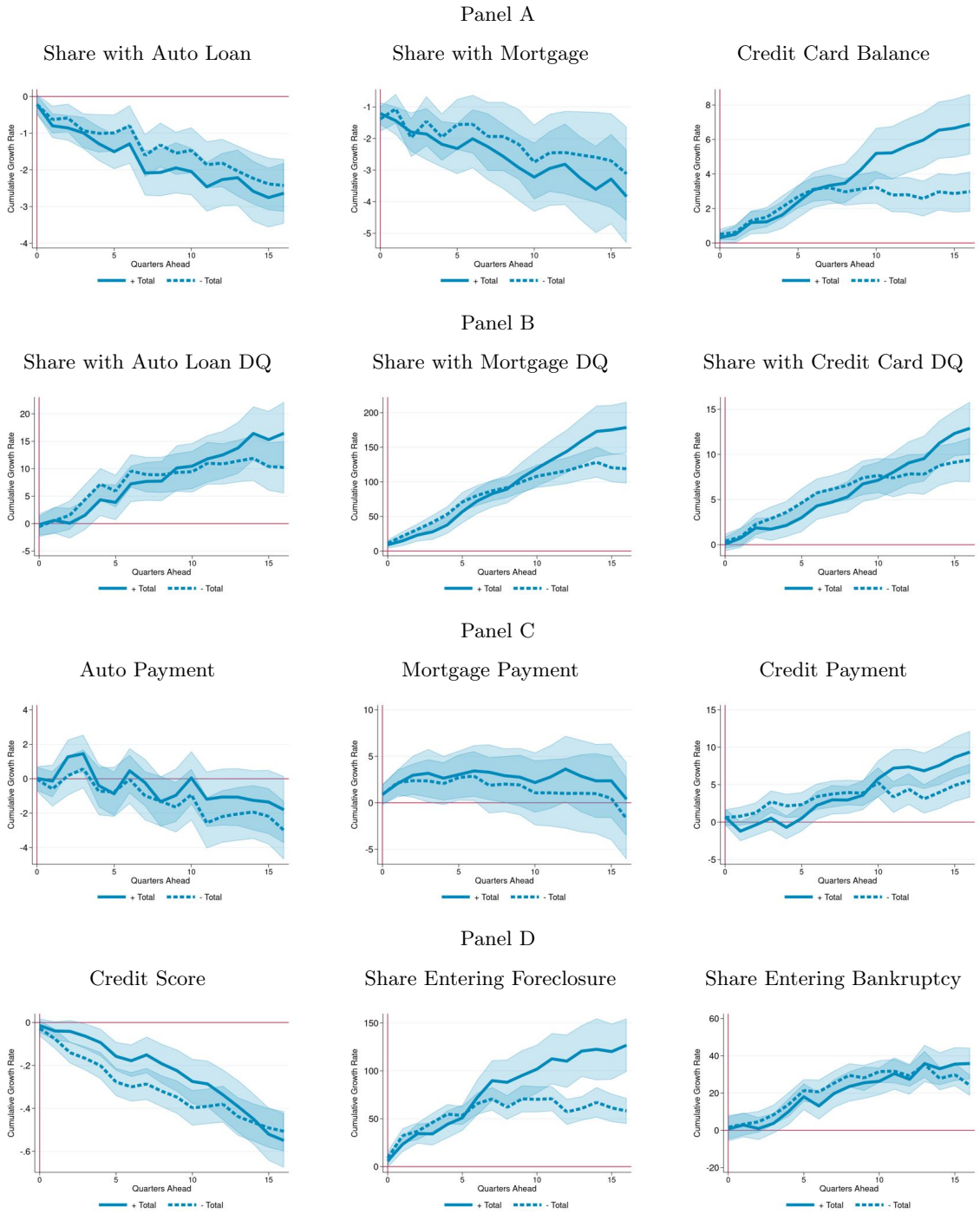
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2008q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure B.2: Mortgage Outcomes, 1999-2008 NS



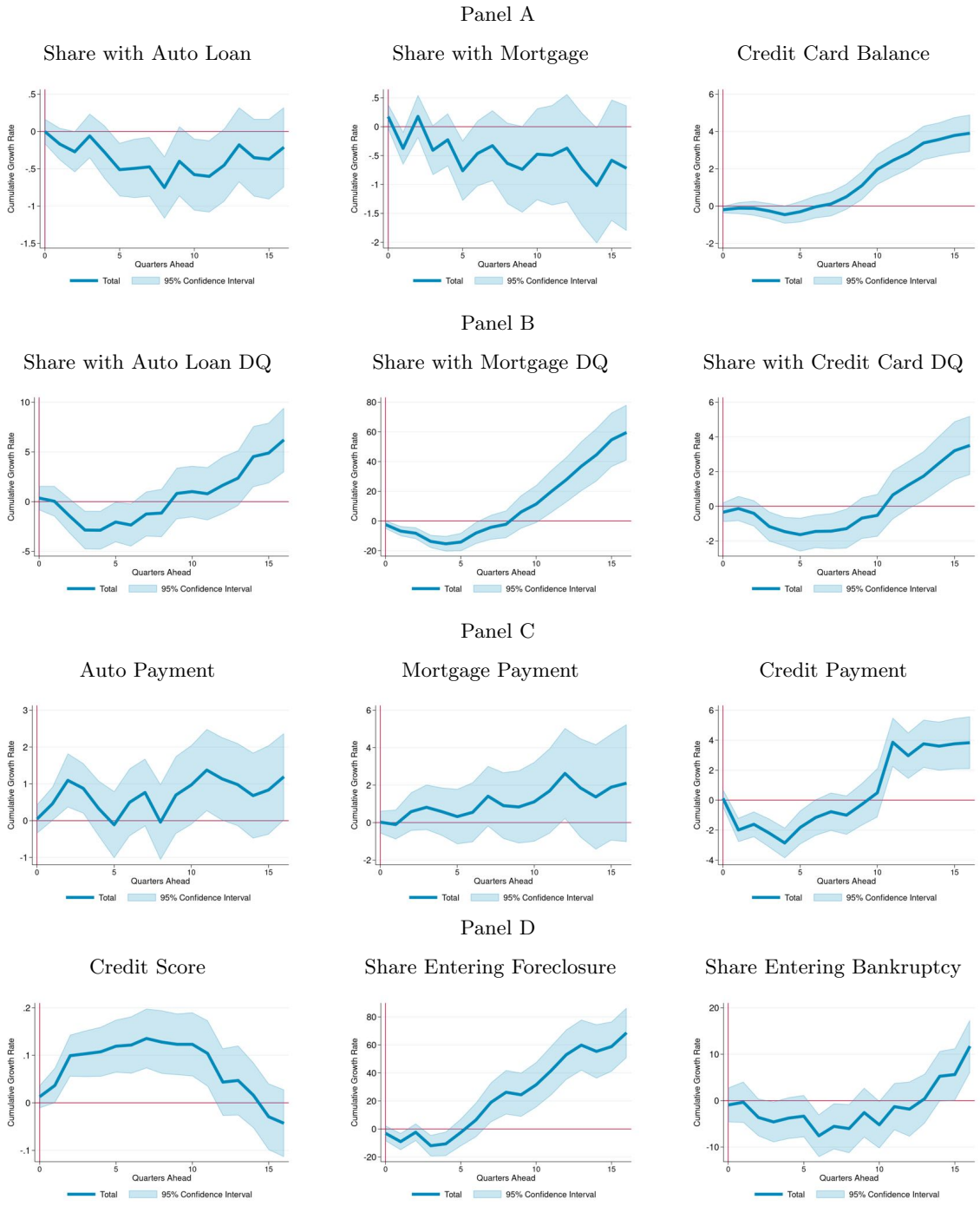
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1–2019Q4.

Figure B.3A: Household Debt Outcomes, Allowing for Asymmetric Effects of Positive and Negative Shocks, Using NS Shocks



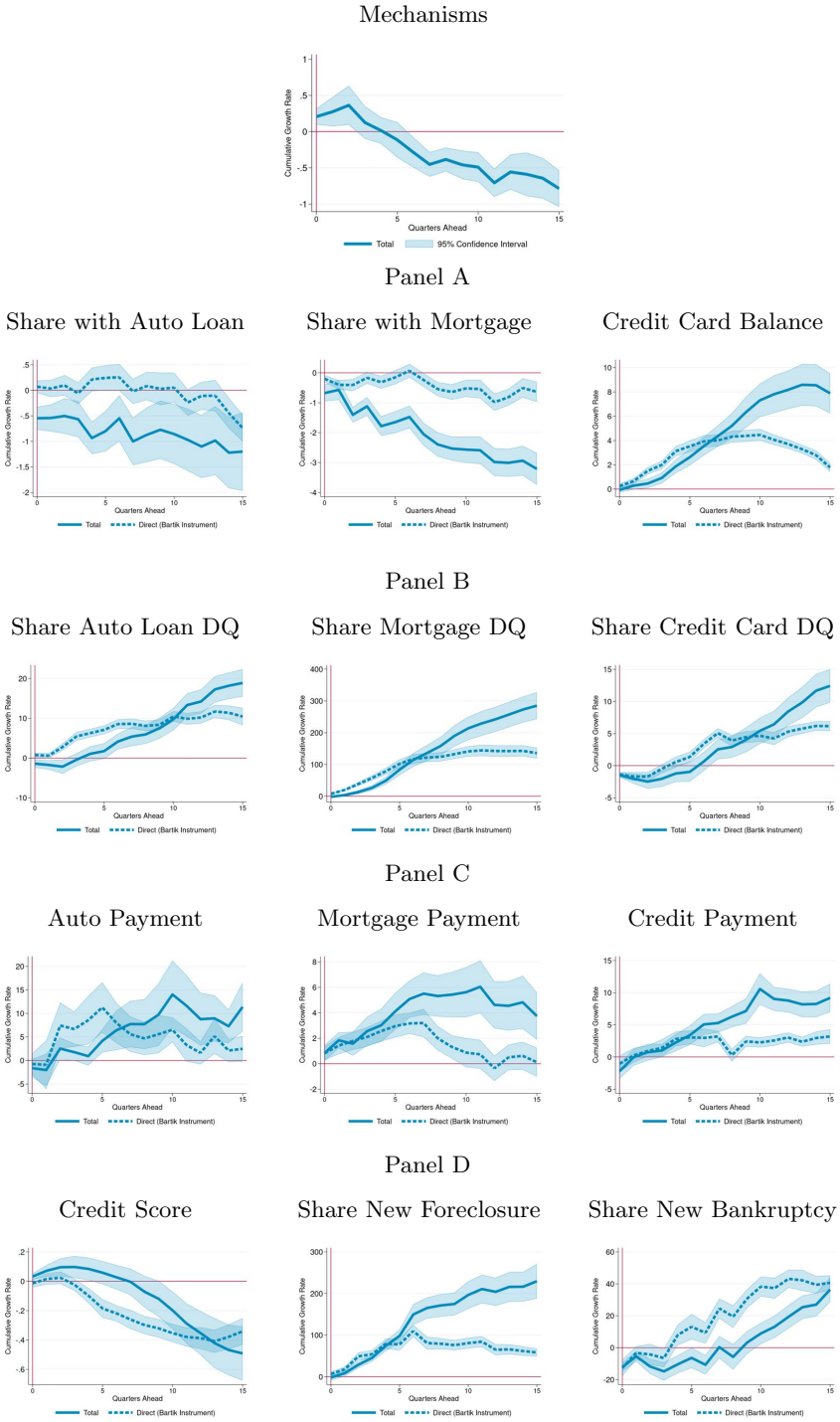
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates impulse response for positive NS shocks and dashed line for negative NS shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure B.3B: Household Debt Outcomes, Difference Between Positive and Negative Shocks, using NS shocks



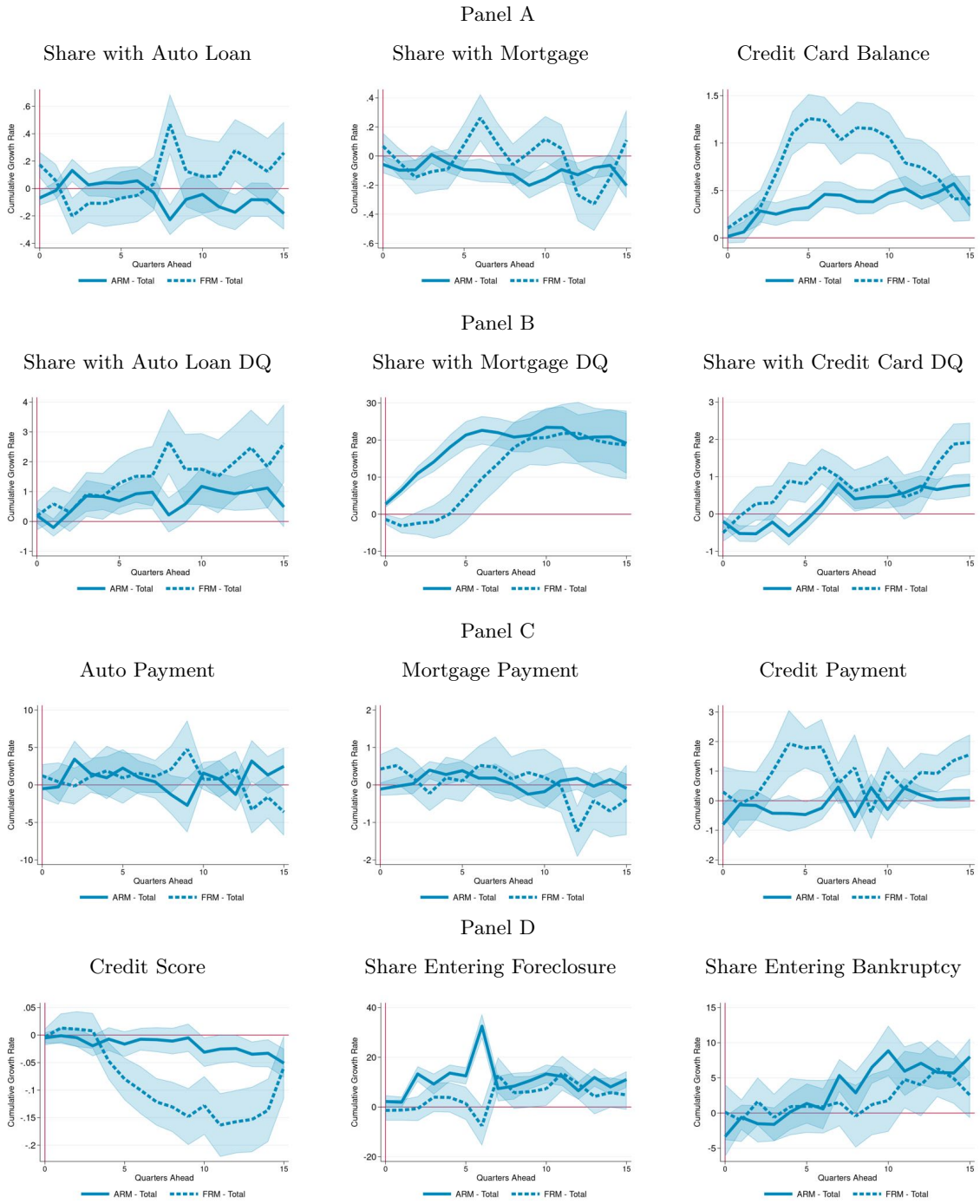
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates the difference in impulse response for positive NS shocks and negative NS shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure B.4: Household Debt Outcomes (Direct, Indirect Effects with Bartik), NS Shocks



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (3), using Bartik labor-demand shocks to separate direct and indirect effects. The solid blue line indicates the total impulse response, while the dashed blue line indicates the impulse response when using the Bartik labor-demand shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure B.5: Household Debt Outcomes, ARM vs. FRM Horse Race, Using Monetary Policy Shocks from Nakamura and Steinsson (2018). **ARM and FRM DTI are standardized**



Note: Each line plots the 3-quarter smoothed impulse response functions of the growth of the dependent variable using equation (5). Solid blue line indicates impulse response for ARM DTI, and dotted blue line indicates impulse response for FRM DTI. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Appendix C: Tables

Table C.1: Date Ranges of Outcome Datasets

Dataset Name	Data Type	Date Range
CCP	Consumer Credit	1999Q1-2023Q4
McDash	Mortgages	2005Q1-2023Q3
HMDA	Mortgage Applications	1999Q1-2022Q4
QWI	Employment	2005Q1-2022Q4

Table C.2: Summary Statistics - Explanatory Variables

Variable	**Mean**	**SD**	**25th Percentile**	**75th Percentile**
			Federal Funds Rate	
FFR	1.913	2.003	0.143	3.478
Δ FFR	0.006	0.467	-0.027	0.213
			Nakamura-Steinsson Shocks	
FFR NS	-5.018	4.502	-8.552	-1.464
Δ FFR NS	-0.007	1.071	-0.428	0.514
			Measures of Credit Market Exposure	
DTI	1.041	0.411	0.809	1.223
ARM DTI	0.065	2.752	0.005	0.061
FRM DTI	0.569	0.620	0.161	0.757

Table C.3: Summary Statistics - Outcome Variables

Variable	**Mean**	**SD**	**25th**	**75th**
Raw Variables				
Share Holding Auto Loan (%)	32.790	6.505	29.144	37.224
Share Holding Mortgage (%)	31.347	7.853	26.442	36.504
Average Credit Card Balance	3,591.467	863.417	3,000.676	4,136.967
Share Holding Delinquent Auto Loan (%)	2.171	1.318	1.208	2.854
Share Holding Delinquent Mortgage (%)	0.648	0.745	0.211	0.779
Share Holding Delinquent Credit (%)	6.705	2.296	5.065	8.004
Average Auto Loan Payments	142.748	187.091	107.179	163.666
Average Mortgage Payments	317.741	157.489	211.615	407.172
Average Credit Card Payments	226.073	253.427	140.079	274.961
Average Credit Score	642.539	34.856	617.139	669.349
Share New Foreclosure (%)	0.083	0.107	0.013	0.115
Share New Bankruptcy (%)	0.161	0.146	0.053	0.233
Orig Share Refinances	54.132	16.025	43.195	65.920
Avg Monthly Payment	1,260.590	500.576	883.464	1,539.891
Share Apps Originated (%)	58.457	7.977	53.522	63.933
Share Apps Denied (%)	19.983	6.366	15.593	23.529
Total Employment	50,777.306	183,508.70	3,483	27,951
Growth Variables				
Share Holding Auto Loan (%)	1.734	5.661	-1.769	4.197
Share Holding Mortgage (%)	0.040	5.053	-2.405	1.585
Average Credit Card Balance	2.029	8.267	-3.094	6.710
Share Holding Delinquent Auto Loan (%)	6.450	22.256	-5.666	14.800
Share Holding Delinquent Mortgage (%)	5.230	55.227	-26.079	19.415
Share Holding Delinquent Credit (%)	2.366	13.549	-5.900	9.519
Average Auto Loan Payments	4.548	15.273	-0.434	7.250
Average Mortgage Payments	3.805	15.371	-1.829	7.703
Average Credit Card Payments	4.121	19.345	-5.708	12.674
Average Credit Score	0.047	0.798	-0.409	0.461
Share New Foreclosure (%)	5.195	87.610	-45.935	29.182
Share New Bankruptcy (%)	7.136	78.314	-34.199	27.984
Orig Share Refinances	-2.176	34.202	-21.047	13.486
Avg Monthly Payment	0.525	5.419	-2.843	3.814
Share Apps Originated (%)	0.558	9.888	-5.155	5.689
Share Apps Denied (%)	2.492	25.116	-14.567	15.126
Total Employment	0.324	5.281	-2.322	3.070

Table C.4: Summary Statistics by Demographic Characteristic - Raw Variables

Variable	**All**	**Higher Share Subprime**	**Low-income**	**LMI 50**
Share Holding Auto Loan (%)	32.790	33.437	31.594	28.721
	(6.505)	(6.238)	(7.177)	(8.571)
Share Holding Mortgage (%)	31.347	25.106	23.668	23.446
	(7.853)	(6.237)	(6.335)	(8.207)
Average Credit Card Balance	3,591.467	3,126.450	2,820.920	3,161.074
	(863.417)	(807.232)	(694.052)	(796.027)
Share Holding Delinquent Auto Loan (%)	2.171	3.408	2.638	2.793
	(1.318)	(1.549)	(1.639)	(1.848)
Share Holding Delinquent Mortgage (%)	0.648	0.686	0.543	0.665
	(0.745)	(0.839)	(0.638)	(0.707)
Share Holding Delinquent Credit (%)	6.705	8.588	7.418	8.373
	(2.296)	(2.683)	(2.828)	(2.954)
Average Credit Score	642.539	599.827	613.450	602.941
	(34.856)	(21.290)	(33.292)	(34.331)
Share New Foreclosure (%)	0.083	0.086	0.069	0.080
	(0.107)	(0.111)	(0.102)	(0.099)
Share New Bankruptcy (%)	0.161	0.180	0.169	0.174
	(0.146)	(0.168)	(0.180)	(0.168)
Average Mortgage Payments	317.741	228.899	182.025	238.639
	(157.489)	(126.572)	(129.890)	(123.915)
Average Credit Card Payments	226.073	204.836	155.177	207.119
	(253.427)	(111.322)	(153.797)	(119.856)
Average Auto Loan Payments	142.748	158.783	139.394	132.890
	(187.091)	(173.906)	(300.814)	(237.639)

Table C.5: Summary Statistics by Demographic Characteristic - Growth Variables

Variable	**All**	**Higher Share Subprime**	**Low-income**	**LMI 50**
Share Holding Auto Loan (%)	2.332	2.550	3.214	2.684
	(8.750)	(8.250)	(9.856)	(8.524)
Share Holding Mortgage (%)	-0.340	0.221	1.025	-0.281
	(6.749)	(8.164)	(10.081)	(8.006)
Average Credit Card Balance	1.711	1.901	1.760	1.526
	(11.619)	(12.681)	(13.350)	(12.359)
Share Holding Delinquent Auto Loan (%)	10.284	10.214	12.641	10.995
	(32.194)	(28.943)	(40.893)	(31.346)
Share Holding Delinquent Mortgage (%)	17.383	12.854	15.807	11.793
	(126.195)	(108.967)	(114.966)	(102.450)
Share Holding Delinquent Credit (%)	1.949	2.362	2.813	1.805
	(18.862)	(21.945)	(24.429)	(21.602)
Average Credit Score	-0.005	-0.020	-0.130	0.007
	(1.102)	(1.374)	(1.409)	(1.539)
Share New Foreclosure (%)	9.660	5.950	3.754	3.081
	(129.681)	(125.749)	(136.024)	(117.288)
Share New Bankruptcy (%)	-1.599	-1.834	-0.822	-3.406
	(82.100)	(84.799)	(102.969)	(80.188)
Average Mortgage Payments	4.868	5.755	6.813	5.176
	(20.200)	(23.773)	(29.456)	(22.309)
Average Credit Card Payments	3.495	3.663	3.583	3.661
	(21.539)	(22.313)	(26.479)	(22.550)
Average Auto Loan Payments	5.111	5.443	6.129	5.050
	(19.484)	(18.653)	(20.209)	(19.217)
Share Apps Denied (%)	0.089	-0.695	-1.225	-0.610
	(23.559)	(20.643)	(21.431)	(21.225)

Table C.6: Project Summary Statistics Expanded only one period out

Variable	Mean	SD	25th	75th	5th	95th	min	max
Cumulative Growth Variables G1								
Share Holding Auto Loan (%)	1.405	4.640	-1.463	3.584	-4.684	9.370	-17.171	30.375
Share Holding Mortgage (%)	-0.067	3.974	-2.047	1.376	-4.898	5.722	-23.063	41.209
Average Credit Card Balance	1.654	7.214	-2.938	6.043	-10.306	13.776	-24.881	34.989
Share Holding Delinquent Auto Loan (%)	5.103	18.561	-4.718	12.516	-16.671	30.829	-100.000	243.617
Share Holding Delinquent Mortgage (%)	2.554	48.391	-23.949	16.565	-45.702	86.938	-100.000	369.916
Share Holding Delinquent Credit (%)	1.770	11.455	-5.231	8.021	-14.950	20.981	-57.071	92.082
Average Auto Payments	2.820	13.023	-0.629	5.899	-11.087	16.328	-48.312	74.986
Average Mortgage Payments	2.811	12.509	-1.860	6.411	-9.943	15.705	-57.216	141.519
Average Credit Payments	2.917	15.942	-5.663	10.603	-19.633	24.861	-58.459	121.365
Average Credit Score	0.033	0.675	-0.351	0.382	-0.959	1.179	-3.415	3.479
Share New Foreclosure (%)	7.232	109.571	-45.962	29.212	-100.000	184.927	-100.000	690.106
Share new Bankruptcy (%)	2.691	78.951	-34.145	23.131	-100.000	118.450	-100.000	680.531
Orig Share Refinances	2.808	30.873	-14.290	15.969	-38.782	51.934	-100.000	375.468
Avg Monthly Payment	0.358	3.118	-1.413	2.106	-4.629	5.251	-12.345	16.800
Share Apps Originated (%)	0.348	8.222	-4.226	4.495	-11.218	13.092	-45.192	82.955
Share Apps Denied (%)	1.486	18.601	-10.056	11.207	-25.512	31.843	-60.544	151.235
Total Employment	-0.000	2.924	-1.132	1.488	-4.930	3.953	-13.227	13.363

Table C.7: Project Summary Statistics - cumulative growth expanded all periods out

Variable	Mean	SD	25th	75th	5th	95th	min	max
Growth Variables								
Share Holding Auto Loan (%)	3.911	10.042	-2.514	8.728	-9.096	22.028	-25.876	80.542
Share Holding Mortgage (%)	-0.563	7.941	-4.280	2.140	-11.077	10.424	-31.813	125.731
Average Credit Card Balance	2.851	13.521	-5.484	10.805	-19.382	25.601	-45.371	82.593
Share Holding Delinquent Auto Loan (%)	16.478	36.763	-5.163	30.089	-24.974	78.754	-100.000	552.800
Share Holding Delinquent Mortgage (%)	23.536	152.436	-47.414	28.741	-73.746	264.655	-100.000	1,092.053
Share Holding Delinquent Credit (%)	3.058	22.159	-10.829	14.172	-28.177	42.413	-80.981	203.469
Average Auto Payments	8.691	21.692	0.065	15.173	-16.659	40.020	-63.865	154.922
Average Mortgage Payments	7.949	23.183	-2.873	14.592	-18.104	42.071	-63.915	399.495
Average Credit Payments	5.540	24.410	-8.179	15.449	-24.800	44.108	-75.332	229.055
Average Credit Score	-0.004	1.297	-0.718	0.642	-1.994	2.213	-7.346	6.259
Share New Foreclosure (%)	7.894	140.561	-70.123	25.754	-100.000	290.580	-100.000	703.913
Share new Bankruptcy (%)	-8.127	81.589	-55.327	14.151	-100.000	129.946	-100.000	684.888
Orig Share Refinances	0.518	43.019	-25.779	19.590	-61.261	75.487	-100.000	400.000
Avg Monthly Payment	-0.024	9.667	-4.893	4.931	-16.997	15.277	-33.649	52.750
Share Apps Originated (%)	1.679	12.463	-5.284	7.511	-17.153	22.773	-46.429	105.189
Share Apps Denied (%)	0.508	28.711	-17.897	13.188	-37.710	52.731	-66.897	161.538
Total Employment	-0.216	4.721	-2.495	2.476	-8.778	6.707	-23.234	25.565