# Effects of Changes in the Federal Funds Rate on Household Finance

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**Abstract**

We use local projection techniques along with cross-sectional heterogeneity to estimate plausibly causal effects of monetary policy on household finance. Comparing counties that differ by leverage, we find that monetary tightening increases the growth of adverse household debt outcomes, including delinquencies, foreclosures and bankruptcies, and decreases the growth of credit scores and of borrowing to purchase houses and cars in more leveraged counties compared to less leveraged ones. Exploring mechanisms, 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/long term following a rate change, while contribution of the house price channel is small. Moreover, we find that the transmission of monetary policy is considerably more effective in areas and time periods that have higher Adjustable Rate Mortgage Debt (ARM)-to-income ratios. Furthermore, we find that the effects of monetary policy are asymmetric – monetary contraction has larger (absolute) effects than monetary expansion.

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# 1 Introduction

An important aspect of the effects of monetary shocks is their effects on household financial health. Many households in the bottom and middle of the U.S. income distribution carry considerable debt (Mian, Straub and Sufi 2020) and rely on debt financing to purchase important assets such as homes and cars. The interest rate that households pay to originate and service this debt is closely related to the Federal Funds Rate. In this paper, we estimate effects of monetary shocks on household debt in areas that are more exposed to monetary policy shocks versus those that are less exposed. Our measure of exposure to monetary shocks is the ratio of debt to income.

We find that following a monetary tightening, areas with high leverage (a high debt-to-income ratio, DTI) see a decline in the growth rate of borrowing to finance houses or cars, which for most families are investment goods, relative to less leveraged areas. However, more leveraged areas see a substantial relative rise in the growth rate of credit card balances and of their mortgage payments. They also see relative growth in the fraction of mortgage applications that are denied, and a relative decrease in growth in mortgages that are refinances. Finally, they see a relative decline in financial health - relative falls in 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.

Estimating the effects of monetary shocks is difficult because monetary policy is not set at random, but rather it reacts to economic conditions. Therefore, a naïve

regression of outcomes on monetary policy shocks risks assigns the effects of economic conditions to monetary policy. We implement several empirical approaches to avoid this problem. First, our differential approach allows the use of panel data and county-level outcomes, largely ruling out the potential of reverse causation from outcomes to monetary policy by controlling for all national-level time-varying variables. Second, by focusing on differential effects between areas with high and low leverage we rule out confounding factors that do not operate differentially across high versus low exposure areas. Third, we control for a long series of monetary policy lags, further capturing economic conditions preceding a monetary policy shock and the past path of monetary policy, thus isolating the effect of the monetary shock. Fourth, we provide alternative estimates using measures of monetary shocks derived from high-frequency changes in interest rate futures and asset prices around Federal Open Market Committee (FOMC) announcements, thus capturing plausibly exogenous monetary policy shocks. Fifth, we conduct a battery of robustness checks by controlling for differential effects of monetary policy shocks across areas that differ by other characteristics, such as demographics and economic composition, as well as by controlling for differential effects of other macroeconomic variables such as GDP across high- and low-leveraged areas.

We investigate the mechanisms underlying the effects we find. First, we inquire whether the effects of FFR changes on household financial health are mediated by endogenous economic variables, such as local employment or house price changes. By controlling for the leads of these potential mediators, instrumented with a design inspired by Bartik (1991). We find that there are substantial direct effects of FFR

changes within the first year of an interest rate change, but after a year to a year and a half, most of the effects of FFR changes are results of the generally negative employment response, with changes in house prices playing a complementary but minor role. Second, we investigate whether the stronger impacts in the high leverage areas are driven by the predominance of specific kinds of debt in those areas or whether the effects are symmetric across various kinds of debt. We find that monetary policy transmission is markedly stronger in the presence of mortgage debt, and especially ARM debt – geographies and time periods that have a preponderance of mortgage and ARM debt are much more affected by monetary policy. Finally, we ask whether the (generally adverse) differential effects of monetary contraction on household debt, spending and employment are symmetric in magnitudes to (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.

The rest of the paper is organized as follows. Section 2 describes the data. Section 3 discusses the empirical strategy, including our approach to testing mechanisms. Section 4 presents the baseline results. Section 5 discusses potential threats to our identification strategy and our analyses to verify that our results are robust to these concerns. Section 6 explores the mechanisms for our findings. Section 7 concludes.

# 2 Data

Table C.1 provides a list of all the data sources that we use and the time ranges for which they are available. 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, which is constructed using data from mortgage servicers, which are financial institutions that collect payments from borrowers. McDash covers 60 to 80 percent of the mortgage market. McDash allows us to classify mortgages by type: fixed rate mortgage versus adjustable rate mortgage. This differentiation is important as it helps us explore whether our results are driven by areas with higher adjustable mortgage shares or areas with higher fixed rate mortgage shares thus giving us important insights on the transmission mechanism of monetary policy. Additionally, McDash reports home value at mortgage origination for each property. We use home value at origination along with zipcode level home price indices from Corelogic to compute estimate home values of each property for the period 2005-2023 (Haughwout et al 2018).

We use confidential Home Mortgage Disclosure Act (HMDA) data to analyze mortgage application denials. The HMDA covers 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 our analysis for all data used here.

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 explore the role of house price variation, we use county-level house price indexes from Zillow, which provides information for counties containing 97.1% of the U.S. population.

# 3 Empirical Strategy

To identify the effects of monetary policy shocks on household debt in more exposed areas relative to less, we estimate a local projections model on county-by-quarter level 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. 2022; Holm, Paul and Tischbirek 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 uses *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. The lags enable us to capture the effects of current monetary policy innovations while controlling for the past path of monetary policy. This is important because of the strong serial correlation in monetary policy across time (Coibion et al. 2017). To that end, we control for lags of the interaction of monetary policy and county-level exposure. Controlling for monetary policy lags also allows us to better align the persistence of our outcomes and our regressors.

We aim to recover how the effect of monetary policy varies across more and less financially exposed areas, which requires less stringent identification assumptions compared to 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 $DTI_c$ be the debt-to-income ratio associated with county $c$ in 1999 that is indicative of the exposure of county $c$ to the monetary policy shock.[1] Additionally, let $G_{c,t+h} = (y_{c,t+h} - y_{c,t}) / y_{c,t}$

[1]We use a time-constant measure of DTI to avoid its potential endogeneity over the course of the sample period. Figure A.1 shows that $DTI_c$ is very persistent over time. Allowing $DTI_c$ to vary over time generates results that are qualitatively similar to the baseline, as shown in Figure A.13, with differences in magnitude coming from differences in scaling by the standard deviation of time-constant vs. time-varying DTI.

be the cumulative growth rate of an outcome of interest $y$ in county $c$ at period $t+h$. 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):

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{p=1}^{4} \rho_p^{(h)} y_{c,t-p} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} \times DTI_c + \varepsilon_{c,t+h} \qquad (1)$$

For our baseline estimates, $J = 8$ (8 quarters or two years) and $H = 16$ (we look up to four years ahead). We also include 4 lags of the outcome variable, spanning a full year before period $t$. 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 us alleviate omitted variable bias, and also helps 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 re-

gardless of the value of $DTI_c$. 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 $DTI_c$, and vice versa for $\beta_1^{(h)} < 0$.

Our approach is most similar to Bergman, Matsa and Weber (2022; hereafter BMW) in that we use a heterogeneity parameter, $DTI_c$, to index the exposure of county $c$ to exposure from the Federal Funds rate, $FFR_t$, which varies only across time and is otherwise constant across counties. However, our specification builds considerably on that of BMW by considering cumulative rather than year-on-year changes in the dependent variables, controlling for the lagged dependent variable, and controlling for the effects of lagged Federal Funds rates on the outcome.

## 3.1 Threats to Identification

A concern may be 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 as 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 year 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 $DTI_c$ as we control for $DTI_c$ directly. Finally,

the fact that $FFR_{t-1}$ enters in an interaction with $DTI_c$ implies that any potential confounder needs to be a nationwide macroeconomic variable that affects different counties differently based on that county's value of $DTI_c$.

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, Jarocinski and Karadi 2020, Nakamura and Steinsson 2018, Miranda-Agrippino and Ricco 2019, etc.). 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, 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.[2] We also perform more direct tests of the idea that the effects of the FFR are not masking other macroeconomic variables by including interaction terms between $DTI_c$ and nationwide GDP growth in Equation (1).

[2]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.

A related concern may be that even if we are isolating exogenous monetary policy variation in FFR or through the use of surprise shocks, the debt-to-income ratio may be capturing alternative sources of variation across counties that may be correlated with trends in household outcomes in response to interest rate shocks. In other words, heterogeneity in different places' response to interest rate shocks may not have anything to do with indebtedness. To address this concern, we perform robustness checks in which we include interactions of the FFR with other county-level variables, such as county demographic composition, to show that controlling for alternative spatial trends in the effect of the FFR on household financial outcomes does not change our estimates of its effect as a function of the debt-to-income ratio. We also present evidence that county demographic composition does not change in response to FFR shocks over time at the county level.

## 3.2 Testing Mechanisms

In order to better understand the mechanisms behind our findings, we decompose the overall effect of FFR shocks on outcomes into direct effects of FFR changes and indirect effects operating through resulting changes to employment or house prices. We do this by (a) following Holm, Paul and Tischbirek (2021) and controlling for leads of employment or house price growth between period $t$ and period $t + h$ and (b) by replacing the leads of employment growth by standard Bartik labor demand shocks interacting county employment sectoral shares with national employment sectoral growth rates (Notowidigdo et al. 2020) in order to avoid the potential endogeneity

of employment growth leads in this regression[3]. The specification for (b) is below, while (a) uses employment growth in place of the Bartik labor demand shocks in the regression below. The coefficients $\beta_h$ in the below specification trace out the direct effect of FFR shocks, netting out the indirect labor market effects.

$$\begin{aligned} G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{p=1}^{4} \rho_p^{(h)} y_{c,t-p} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} \times DTI_c \\ + \sum_{j=1}^{h} \delta_j^{(h)} B_{c,t+j} + \varepsilon_{c,t+h} \end{aligned} \tag{2}$$

$$B_{c,t} = \sum_{k=1}^{K} \varphi_{i,k,t-\tau} \left( \frac{v_{-i,k,t} - v_{-i,k,t-\tau}}{v_{-i,k,t-\tau}} \right) \tag{3}$$

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 $i$.

Furthermore, we investigate whether our baseline results are driven by particular kinds of debt; in particular, mortgage debt or ARM debt. To do so, we include interactions of the FFR with both the mortgage debt to income ratio and the non-mortgage debt to income ratio in the regression. We denote $DTI_c^M$ as the mortgage DTI and $DTI_c^N$ as the non-mortgage DTI for county $c$ in 1999 in the following specification:

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

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{p=1}^{4} \rho_p^{(h)} y_{c,t-p} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} \times DTI_c^M + \sum_{j=1}^{J} \tilde{\beta}_j^{(h)} FFR_{t-j} \times DTI_c^N + \varepsilon_{c,t+h} \tag{4}$$

where $\left\{\beta_1^{(h)}\right\}_{h=0}^{H}$ traces out the impulse response function for FFR as a function of mortgage DTI controlling for non-mortgage DTI. A similar equation allows us to look at the impulse response function of FFR as a function of ARM DTI controlling for non-ARM DTI.

Finally, we 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

$$G_{c,t+h} = \alpha_c^{(h)} + \lambda_t^{(h)} + \sum_{p=1}^{4} \rho_p^{(h)} y_{c,t-p} + \sum_{j=1}^{J} \beta_j^{(h)} FFR_{t-j} DTI_c + \sum_{j=1}^{J} \tilde{\beta}_j^{(h)} z_t FFR_{t-j} DTI_c + \varepsilon_{c,t+h} + \sum_{j=1}^{J} \bar{\beta}_j^{(h)} z_t DTI_c \tag{5}$$

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 for any time horizon $h$.

# 4 Main Results

## 4.1 Baseline Results

Figure 2 presents estimates of $\beta_1^{(h)}$, the interaction coefficient on FFR and the county's debt-to-income ratio at time $t$ on the cumulative growth rate in the outcome at time $t+h$. For ease of interpretation, we multiply each coefficient by the standard deviation of DTI in 1999 and also by 0.1 so that the y-axis shows the differential effect of a 100 bps increase in the Federal Funds Rate between two counties with a small (0.1 SD) difference in DTI. Standard deviations of all right-hand side variables that are used in this standardization are provided in Table 3. Panel A shows the impulse responses for auto loan origination balances, mortgage loan origination balances and overall credit card balances. We see that in counties with higher DTI, after an FFR increase, people reduce borrowing for investment-type goods, such as cars and houses. In contrast, we see that 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, four years after a 100 bps FFR increase, the value of $\beta_1^{(h)}$ for credit card balances is around 0.35 and is statistically significantly different from zero. This means that after a 100 bps increase in the FFR, counties that have a 0.1 standard deviation higher DTI (about 2 percentage points) saw 0.35 percentage points higher growth of credit card balances four years later. This is 2% of the standard deviation of cumulative growth of credit card balances after four years. It is also about 7% of the mean of the cumulative growth of credit card balances after four years. [4]

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 up to nearly four years after the FFR increase. The magnitude of this differential is also sizable, implying that a 0.1 standard deviation increase in DTI implies an increase in the cumulative growth rate in the share of delinquent borrowers for auto loans of 2% of its standard deviation. For mortgage loans and credit card loans, these differentials are similar in magnitude relative to their standard deviations. FFR increases result in considerable additional financial stress in areas already experiencing high levels of borrowing relative to income.[5]

[4] We will continue reporting effects in terms of standard deviations of the dependent variable – the cumulative growth of the outcome variable – rather than the mean of the dependent variable because standard deviations of cumulative growth are expected to grow over time as random shocks cumulate, whereas means of cumulative growth do not have an expected temporal pattern to follow.

[5] Appendix Figure A.2 presents impulse response functions for the outcomes in panel B, as well as the credit balance figure in panel A, in which we use the dollar shares of balances and delinquencies. The results are qualitatively similar.

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. Counties with a 0.1 standard deviation higher DTI experience increases in cumulative payment growth equal to about 1%-2% of their standard deviations following a 100 bps FFR increase. The positive impulse response is intuitive as the interest rates on the payments increase with FFR increases.

Finally, Panel D focuses on credit scores, new foreclosures and new bankruptcies as summary measures of households' financial conditions. Both new foreclosures and new bankruptcies grow at a faster rate with a county's leverage. Credit score growth declines with leverage. Counties with a 0.1 standard deviation higher DTI see cumulative growth of foreclosures and bankruptcies higher by 2%-3% of their standard deviations, and cumulative growth of credit scores lower by 1% of its standard deviation four years after the interest rate increase.

We also can shed light on another dimension of financial health: the rate at which mortgage approvals and denials differentially change by county DTI after an FFR increase. To do so, we use data from the confidential Home Mortgage Disclosure Act (HMDA) database on mortgage originations and denials. Panel A of Figure 3 presents the results. We see that the growth rate of the share of mortgage applications resulting in originations falls, and the share resulting in denials rises as a function of county DTI, with the effect size being large – a county with a 0.1 standard deviation higher DTI would expect to see a rise in the cumulative growth of the share of applications leading to denials equal to 1.4% of its standard deviation

after four years. Most of the effect takes place during the first year after the FFR increase, with the shares of approvals and denials leveling off subsequently. This is unlike the effects on the household financial outcomes in Figure 2, which tend to grow consistently over time, and is likely caused by mortgage denials being largely affected through the direct effect of rising interest rates making mortgage applications fail specific lending tests. Finally, in Figure 3 we consider the effects of FFR changes on financial outcomes associated with student loans. We see that, in more vs. less leveraged counties, student loans originations and balances fall, while delinquencies rise and payments rise with an approximately one-year delay. These results are consistent with student loans being investment-type loans like auto and mortgage loans.

Overall, we find significant differential effects of interest rate increases on household financial health outcomes for more vs. less leveraged counties. These effects are quantitatively moderate, ranging from 1% to 3% of a standard deviation of the cumulative growth of the outcome and typically from 2% to 21% of the mean of the cumulative growth of the outcome for a 100 bps increase in the FFR between two counties differing by 0.1 standard deviation of the DTI (about 2 percentage points) after four years. Interest rate changes have a noticeably stronger effect on financial health in more leveraged counties relative to less leveraged ones.

## 4.2 Mechanisms

We now explore the mechanisms for the core results we have discussed in the previous subsection. First, we show that while interest rate increases have a substantial direct effect in the first year after they take place, their subsequent effects are mostly mediated by their negative effect on employment. Another indirect channel, house prices, plays a minor role. Second, we provide evidence that the composition of debt matters for the effect of interest rates – mortgage, and in particular ARM debt, has the most pronounced effects on outcomes. Finally, we see that the effects of FFR increases and the effects of FFR decreases are asymmetric, with the former having a greater magnitude than the latter.

### 4.2.1 Direct and Indirect Effects of FFR Changes

It is interesting and important to examine what fraction of the baseline effects measured in Figure 2 comes from the direct effects of interest rate changes and what fraction is mediated by the effect of interest rates on other economic variables. We consider two potential sources of indirect effects: the county employment rate and county house price index. First, we present evidence that more leveraged counties experience a greater decline in employment and in house prices following an FFR increase than less leveraged counties do. Panel A of Figure 4 presents estimates of the impulse response function for the growth of the employment rate. We see that the employment rate falls in the long run as the interest rate rises, declining by 1%

of its cumulative growth standard deviation with a 100 bps FFR increase in a 0.1 standard deviation more leveraged county. Panel B shows the impulse response for Zillow's Home Value Index (ZHVI), a measure of house prices. We see that house prices immediately and consistently decline with interest rates in more relative to less leveraged counties. In the last two panels of Figure 4 we obtain similar results using alternative measures of house value or equity. Therefore, interest rate increases decrease employment growth and house prices, both of which may be channels through which they affect financial health Following Holm, Paul and Tischbirek (2021), we can estimate the direct effects of interest rate changes by running our baseline regression (1), additionally controlling for the leads of the mediating variables between horizon $t$ and horizon $t+h$, and taking the resulting impulse response coefficients as the direct effects. The indirect effects would be 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 both employment growth and house price growth may be endogenous. We overcome this concern by replacing the leads of employment growth with the standard Bartik instruments for them, specifically the weighted average of national-level employment growth across industries where the weights are county-level industry employment shares (Bartik 1991 etc.), and the leads of house price growth with the county's share of owner-occupied homes in 2000 interacted with cumulative national house price growth. If the Bartik instruments are valid, 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 holding the employment and housing channels constant.

Figure 5 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 (2) with employment as the only mediating channel. 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 decline, while indirect effects, coming through changes to local employment growth, increase. By the third year after a monetary shock, the direct effect of interest rate increases has leveled off, and nearly all of the additional effect on financial outcomes is mediated by leads of local employment growth. Appendix Figure A.3 presents estimates of the indirect effects (the component of the overall effect that is mediated by changes to employment growth). We see indirect effects following similar patterns to the direct effects, but shifted over in time, starting at about six quarters after the FFR shock.

Figure A.5 reproduces Figure 5 but now adds an additional estimate of direct effects that accounts for potential indirect effects through either employment growth or house price growth. These impulse response functions are very similar to the original direct effects netting out employment growth alone, suggesting that house price growth plays a minor role in explaining indirect effects of interest rate increases once employment growth has been accounted for.

### 4.2.2 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. It is interesting and important to ask whether all kinds of leverage matter equally in the transmission of monetary policy or whether some types of debt are more responsive for transmitting monetary policy than others. Specific candidates for the types of debt driving our results would include mortgage debt, which is the largest source of household debt, and particularly adjustable-rate mortgage (ARM) debt, the rate of which can vary with the FFR. In Figure A.8, we investigate the potential heterogeneity of our effects by type of debt by estimating specification 4, in which we interact FFR with both the mortgage and non-mortgage DTIs, and plotting the coefficients on the interaction with the mortgage DTI. The differential effects of a 100 bps FFR increase on areas differing by 0.1 standard deviation of mortgage DTI are qualitatively and quantitatively similar to the baseline. If we drill down further, and estimate specification 4 using ARM and non-ARM DTIs (measured in 2005, the first year that data on the fraction ARM and FRM becomes available in McDash) instead of mortgage and non-mortgage DTIs, we see that much of the effect of mortgage debt comes specifically from ARM debt. Figure A.9 plots the coefficients on FFR X ARM DTI (once again rescaled by the standard deviation of ARM DTI); they are qualitatively similar to the baseline and quantitatively slightly larger for some variables

(e.g. mortgage delinquencies).[6]

### 4.2.3 Are the Effects of FFR Changes Symmetric?

It is interesting to examine whether the effects of FFR shocks are symmetric for FFR increases and FFR decreases. Figure 8 replicates Figure 2, allowing the coefficients on the FFR interaction and its lags to be different depending on whether that quarter's change in the FFR was positive or negative (see specification 5). 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 (Barnichon and Matthes 2015) that finds stronger effects of contraction on output and prices. Most of these differences are statistically significant as can be seen from Appendix Figure A.10. These differential effects are particularly noticeable for credit card balances, mortgage delinquencies and the credit score.

[6]In Appendix Figures 7, we estimate variants of equation 4, but controlling not just for the DTI of all debt that is not mortgage debt or not ARM debt, respectively, but also for other types of DTIs including credit, HELOC, auto and student DTIs, as well as fixed-rate mortgage (FRM) DTI in Our results from Figures 7 and A.8 continue to hold.

# 5 Threats to Identification and Robustness

## 5.1 Results Using Monetary Surprise Measures

As we discussed in the Empirical Strategy section, there are many reasons to be concerned that the Federal Funds Rate is an endogenous measure of monetary policy shocks. The most serious of these reasons are addressed by our inclusion of time fixed effects (thus capturing any correlation between monetary policy and macroeconomic aggregates), our use of a county-level analysis to rule out reverse causality, and our focus on differential effects of FFR changes across more and less exposed counties rather than the main effect of these changes. However, some reasons for concern about endogeneity remain, specifically that FFR shocks may be correlated with other macroeconomic variables that affect counties heterogeneously based on their debt-to-income ratio (a candidate variable may be expectations).

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[7]. We consider the shocks calculated in Nakamura and Steinsson (2018; NS), which are based on a first principal component of Fed Funds Rate futures of various maturities. These shocks try to measure the information revealed through the FOMC decision on interest rates, as well as through the

[7]Bergman, Matsa and Weber (2022) 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.

announcement, and if asset markets are reasonably efficient, neither set of shocks should be correlated with macroeconomic variables known previously[8].

Figure 9 replicates Figure 2 (our baseline results for credit outcomes) using the running sum of NS shocks in place of FFR in specification (1)[9]. We rescale the running sum of the NS shocks to make its standard deviation the same as that of the Federal Funds Rate, for easier comparison across results. The trends of the impulse responses in Figure 9 closely track our baseline results in Figure 2. In particular, in the long run, more leveraged counties have more negative growth of auto and mortgage origination balances as well as of credit scores, and substantially more positive growth of credit balances, all types of delinquencies, new foreclosures and bankruptcies. There are a few differences from our baseline results in Figure 2 in the short run (the year after the FFR increase), with more leveraged counties seeing higher auto origination balances, lower auto and credit card delinquencies and lower propensity for bankruptcy. It is likely that these differences reflect consumers purchasing cars preemptively before anticipated further rises in interest rates as well as the fixed cost of bankruptcy. Another source of these differences could be that the employment rate is estimated to rise in the first year following an increase in the NS running sum, leading to a larger indirect effect and more positive credit outcomes. The magnitudes of the impulse responses after four years (comparable to Figure 2)

[8]We have also considered shocks from Jarocinski and Karadi 2019 and provide a version of our baseline estimates using them in Appendix Figure B.1 and Figure B.2. Our conclusions are qualitatively unchanged.

[9]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 $DTI_c$ and its lags (and more generally, $DTI_c$ and its lags) as covariates in equation (1)

are generally similar to the baseline. Figure 10 presents the same results for the mortgage outcomes in Figure 3. The trends are again very similar.[10].

The rest of the figures in Appendix B replicate all of the figures in the main body of our paper using shocks from Nakamura and Steinsson 2018 in place of the FFR. In particular, Appendix Figure B.4 presents the 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, but some short-run indirect effects are consistent with FFR increases improving financial health in the short run by increasing employment. The direct effect of FFR increases on financial health in the first few quarters worsens financial health, as does the overall effect.

## 5.2 Robustness to Differential National Economic Trends

Besides using the monetary policy surprise measures, we can also verify directly that allowing national macroeconomic indicators other than FFR to affect household financial outcomes as a function of county DTI does not change our results. Specifically, in Appendix Figure A.11 we augment our baseline specification with interactions of county-level DTI in 1999 with U.S. GDP. The impulse responses remain substantially similar to the baseline.

[10]We also investigate the robustness of our results to using shocks from Jarocinski and Karadi 2019 in Appendix Tables B.1 and B.2, with analogous results.

## 5.3 Robustness to DTI

An additional source of potential failure of identification may be that counties with different DTIs may also differ along other dimensions that may affect how their financial health responds to an interest rate shock. Table 4 presents a regression of DTI on county demographic and economic characteristics. We see that counties with higher DTI have a relatively smaller fraction of young children, a relatively larger fraction of people in middle age (36-54), a smaller share of people who are white and lower average wages. It may be the case that an increase in FFR is not really mediated by DTI, but by one of these correlates, e.g. by the presence of a large fraction of the population that is purchasing homes using financing.

We directly include interactions between FFR and these characteristics as additional controls in specification 1. If DTI is really reflecting demographic characteristics of the different counties, which are mediating the effects of interest rate changes, we would expect the impulse responses that we estimated in Figure 2 to change. However, we see in Figure 11 that when we include these additional interactions, our estimates of the impulse responses are qualitatively and quantitatively similar to the baseline. Thus, it is unlikely that DTI is masking for alternative characteristics that are mediating the effects of the FFR.

## 5.4 Other Robustness Checks

In addition to our analysis of exogeneity of the variation in FFR and DTI, we perform several additional specification checks in Appendix A. Figure A.16 replicates Figure 2 but using only one FFR lag on the right hand-side of equation (1) instead of 8 lags (as do BMW and Holm, Paul and Tischbirek 2021). The results are qualitatively similar to the baseline, although the magnitudes are smaller. Thus, our extension of the dynamics of the model adds value in highlighting that the effects of interest rate changes are much more sensitive to the underlying leverage of an area. Figure A.15 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. Figure A.14 includes lags of the employment to population ratio as controls; our results remain nearly identical. Figure A.12 presents our results restricting to the time period 1999-2008 (this restriction applies to all values of t+h in specification 1, so values after 2009 do not play a role in identifying any points on the impulse response functions). Thus, we restrict the sample away from several important policy changes, including the extended period during which interest rates were at the zero lower bound, as well as the fiscal stimulus during the Great Recession and the CARD Act that may have affected credit card balances and delinquencies. We also restrict the data to a period when a relatively large fraction of mortgages were ARM (see Figure A.6). We find qualitatively similar effects, though some of the impulse response functions (e.g. for mortgage delinquencies) decline rather than hold steady or continue to rise in the longer term.

Figure A.12A excludes the Covid-19 recession and its aftermath from the sample; our results are qualitatively and quantitatively similar to the baseline.

### 5.5 Permutation Tests

A final round of robustness checks that we perform are permutation tests for the FFR and DTI variables. In Figure A.19, we randomly permute the sequence of changes to the FFR, leaving its overall change between 1999 and 2023 to be the same as it was in actuality. We generate a distribution of counterfactual IRFs resulting from running our baseline regression on random permutations of the FFR time path and draw this distribution in gray next to our baseline results. It is clear that if the ordering of FFR changes was statistically unrelated to household financial health, our results would have been radically different, with all the IRFs clustering at zero. In Figure A.20 we randomly permute the cross-sectional distribution of DTI across counties in 1999. Once again, our baseline impulse response functions are very different from the distribution of IRFs we would have obtained if DTI was statistically unrelated to the mechanism through which FFR changes affect household financial health.

## 6 Conclusion

In this paper, we have documented that monetary policy shocks have substantially heterogeneous effects on household debt, employment and spending. Areas with

high leverage (DTI) experience an increase in growth of adverse debt outcomes and a decline in spending and employment growth. We find that this heterogeneity is mediated by the heterogeneity in employment responses to FFR changes in the long run, although there is short-run direct effect of changing interest rates. We find that the effects of monetary contraction on the inequalities are greater than, rather than symmetric to, the effects of monetary expansion. Moreover, household financial health is much more sensitive to monetary policy in areas that are characterized by higher ARM shares. 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 than other kinds of floating debt. On the other hand, fixed rate mortgage debt is not affected by changes in interest rate unless it is the end of the mortgage term. Hence 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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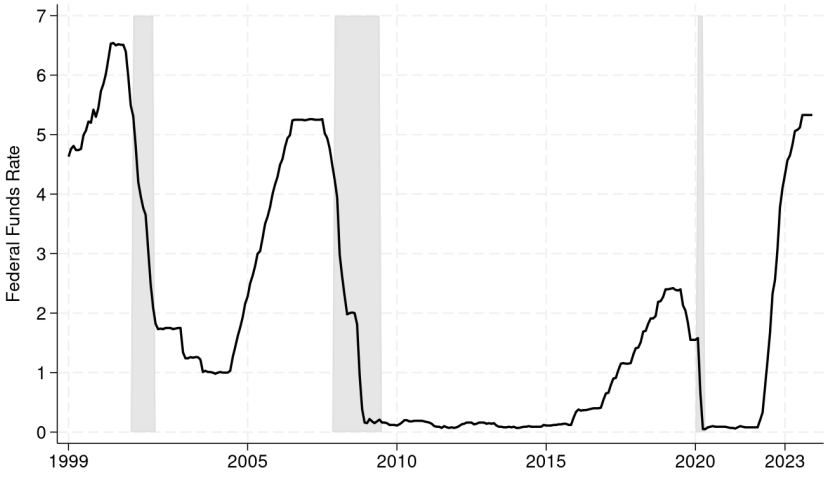
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# Figures

Figure 1: Federal Funds Rate Changes

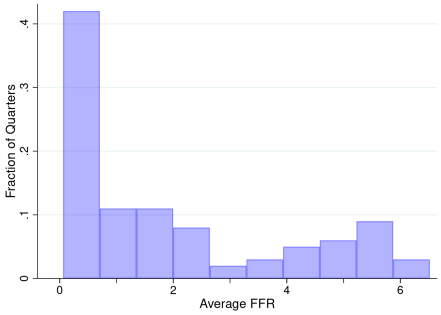
Panel A

FFR Time Series

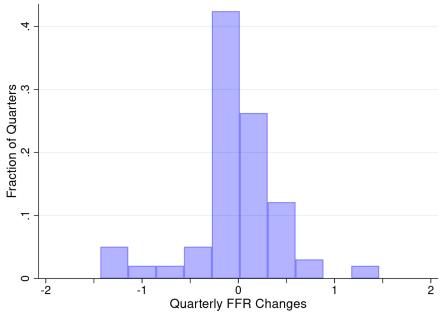



Panel B

FFR Levels Distribution

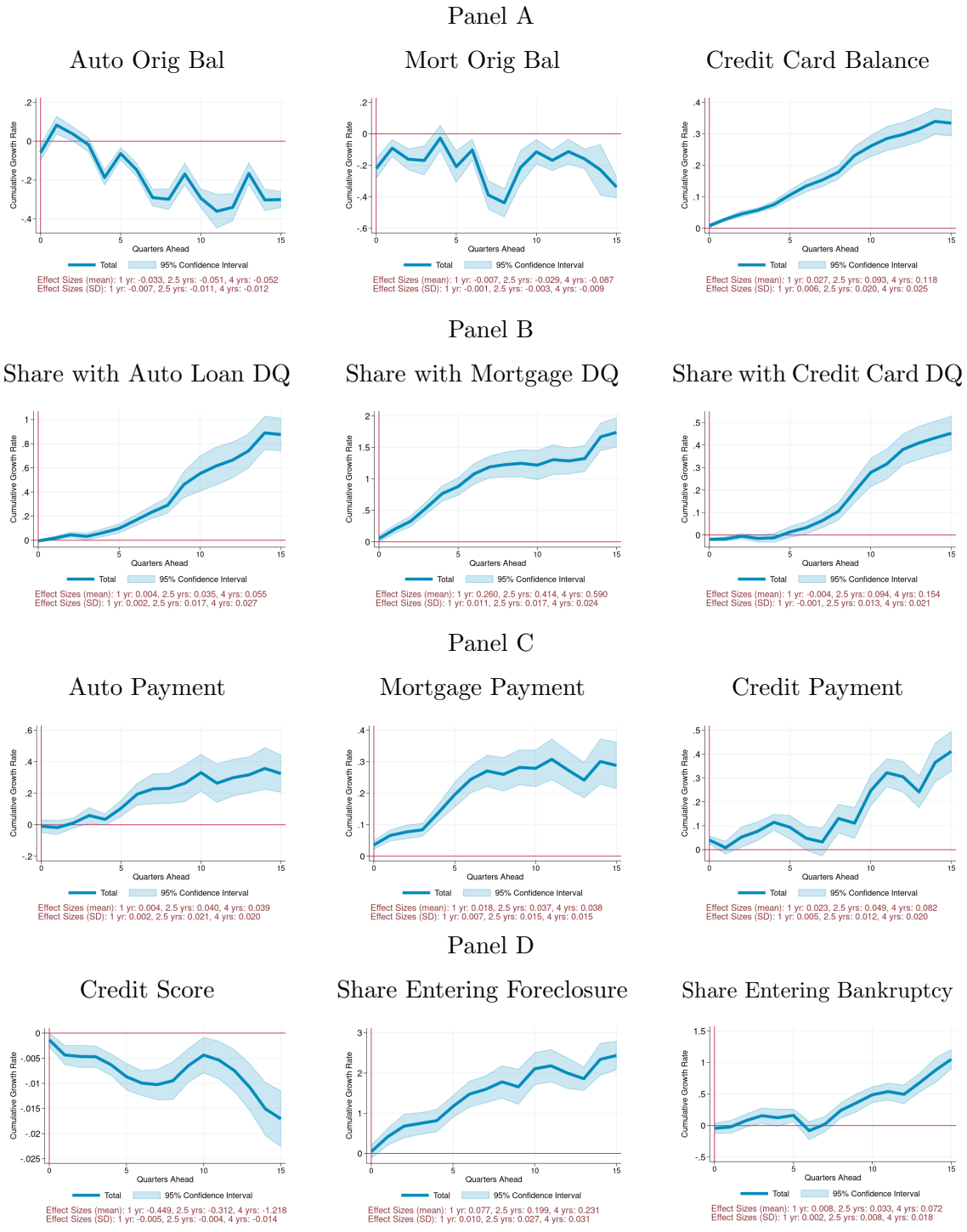



FFR Change Distribution



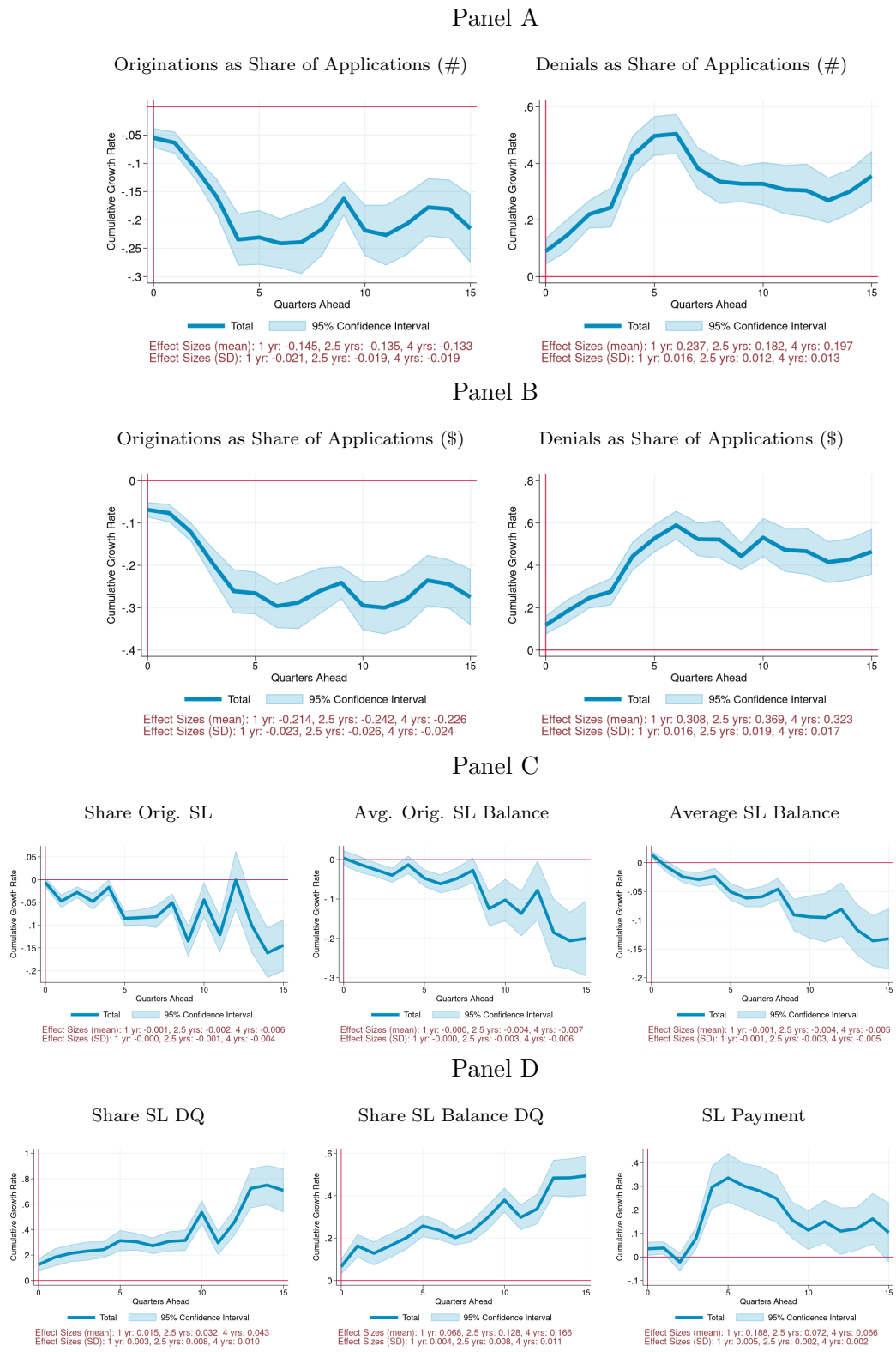

Note: 61% of quarters had an absolute change in FFR of $\leq 0.25$, 57% had positive FFR changes, 41% had negative FFR changes, and 2% had no change.

Figure 2: Effects of FFR Change by Leverage: Household Financial 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 3: Effects of FFR Change by Leverage: More Mortgage Outcomes (HMDA) & Student Loans (CCP)

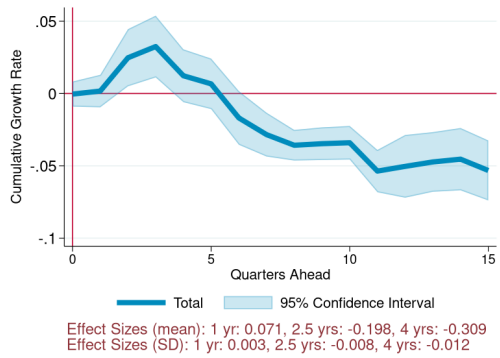



Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Dependent variables on mortgage applications from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2023Q4. Student loan variables come from the FRBNY/Equifax Consumer Credit Panel.

Figure 4: Effects of FFR Change by Leverage: Employment and House Prices

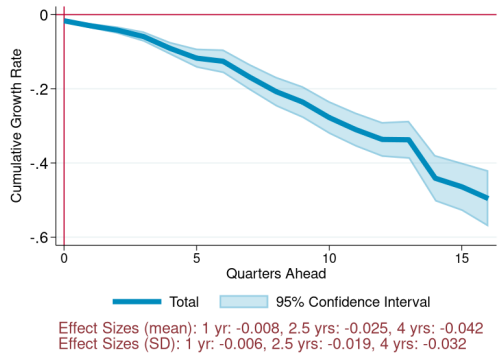
Panel A: Employment

Total Employment

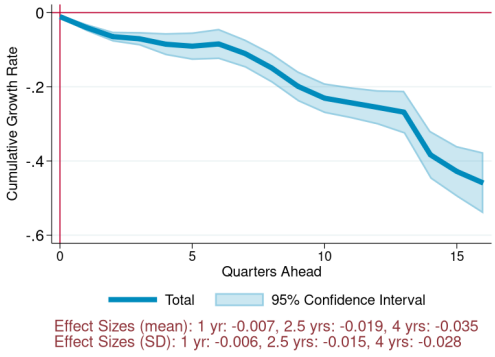



Panel B: Home Prices

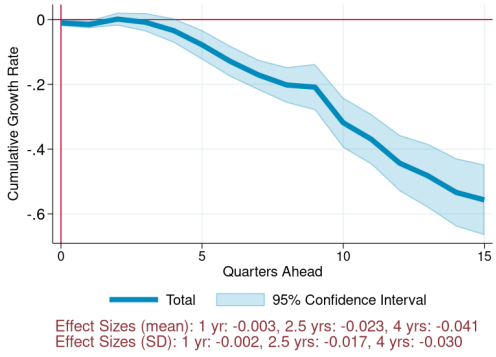
HPI




ZHVI

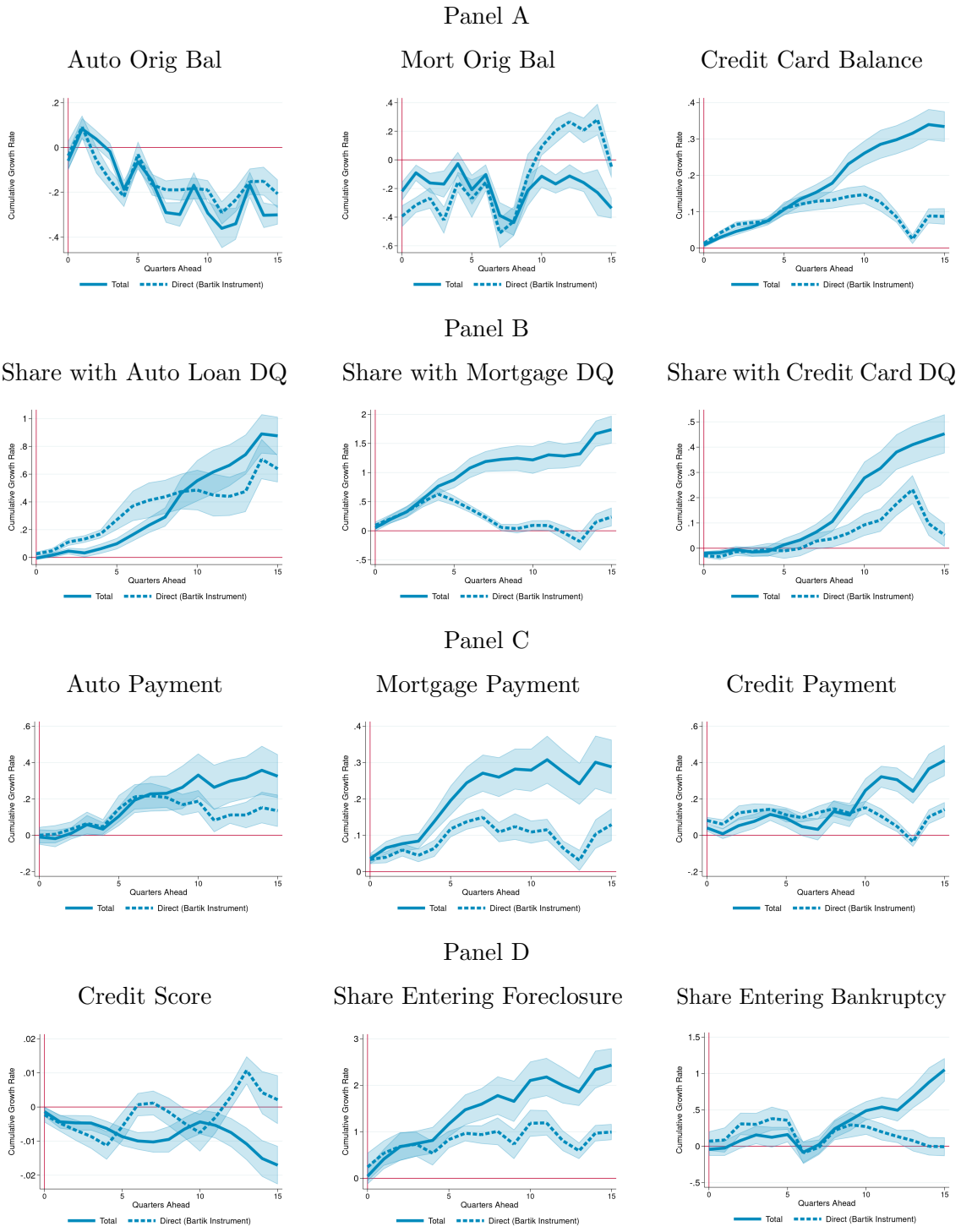



Equity



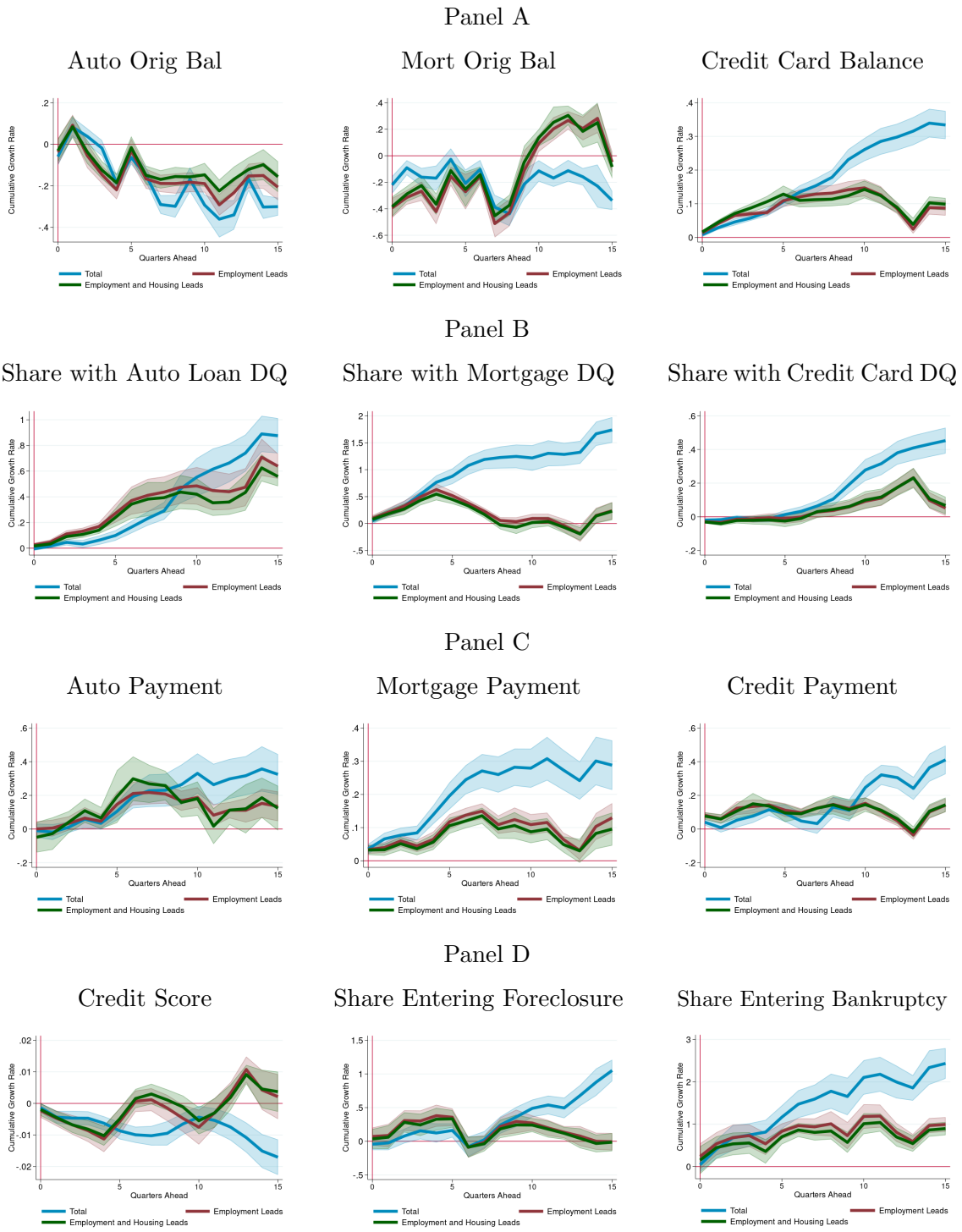

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (5). ZHVI refers to the county-level Zillow Home Value Index, HPI refers to the county-level House Price Index, and Equity is calculated at the county level as the product of the total dwelling units and the ZHVI, less the total mortgage debt. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from Zillow, CoreLogic, and the Consumer Credit Panel.

Figure 5: Mechanisms: Borrowing Costs & Labor Market Effects (Using Bartik Shocks to Instrument for Employment Rate)



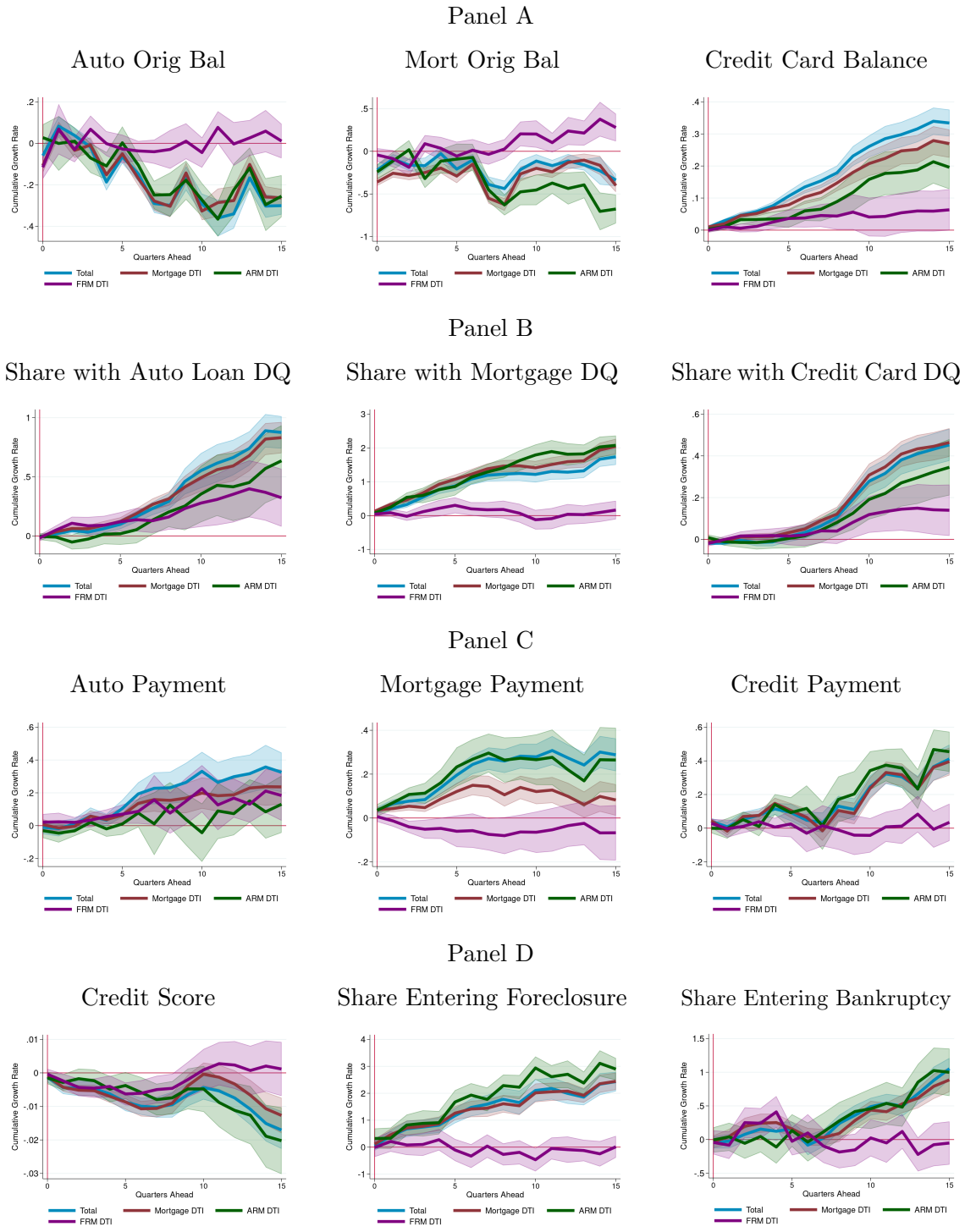

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2), controlling for labor market effects using a Bartik instrument to separate direct and indirect effects. The solid blue line indicates the baseline result (Figure 2), while the dashed blue line indicates the impulse response when using the Bartik instrument. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure 6: Mechanisms: Borrowing Costs. Labor Market Effects & Housing Market Effects (Using Employment and Home Price Bartik Shocks to Instrument for Employment Rate and House Prices)



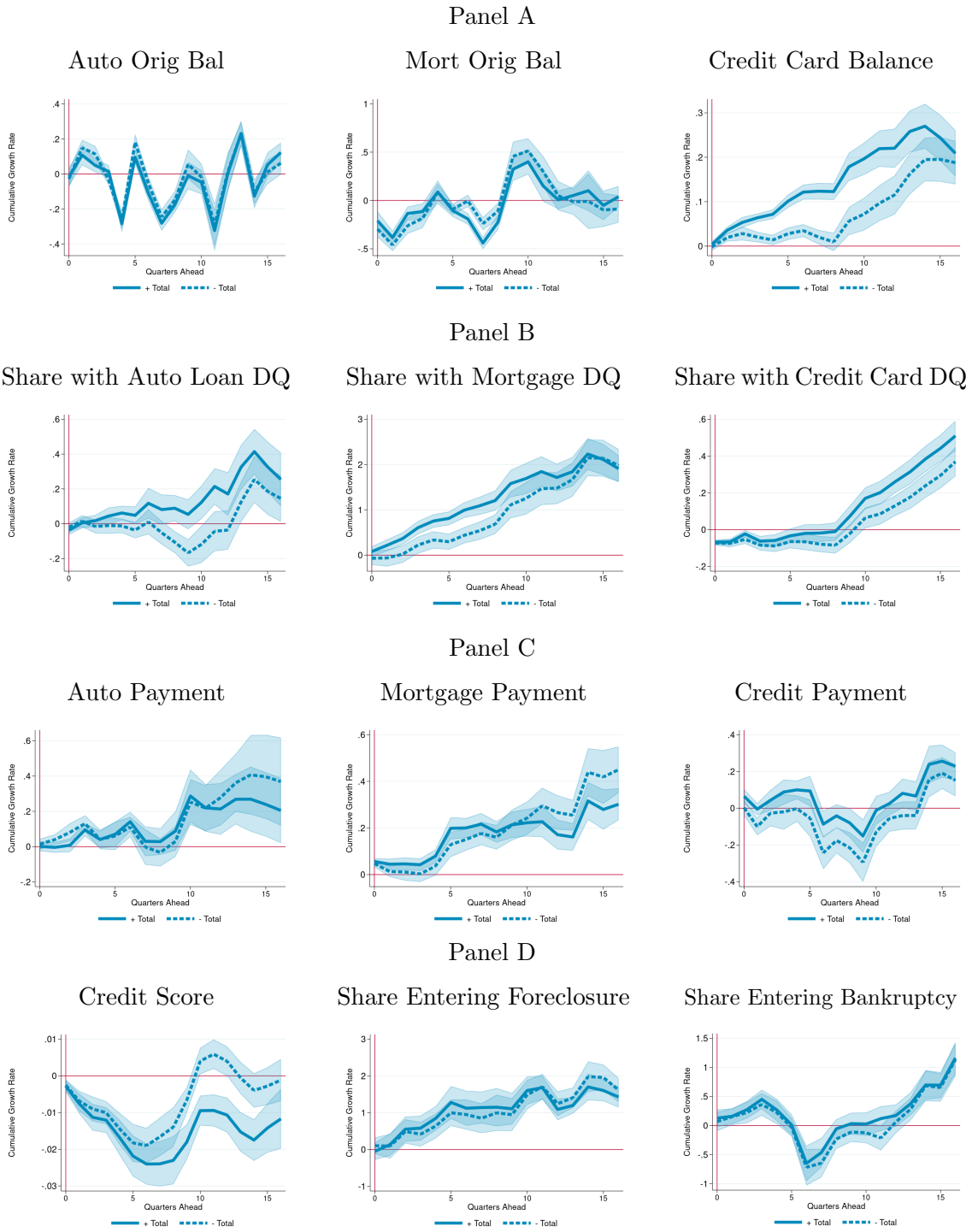

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). The blue line indicates the total impulse response, the red line indicates the impulse response when using the Bartik employment instrument, and the green line indicates the impulse response when using the Bartik housing instrument alongside the employment instrument. 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 7: Mechanisms: Which DTI Drives Results?



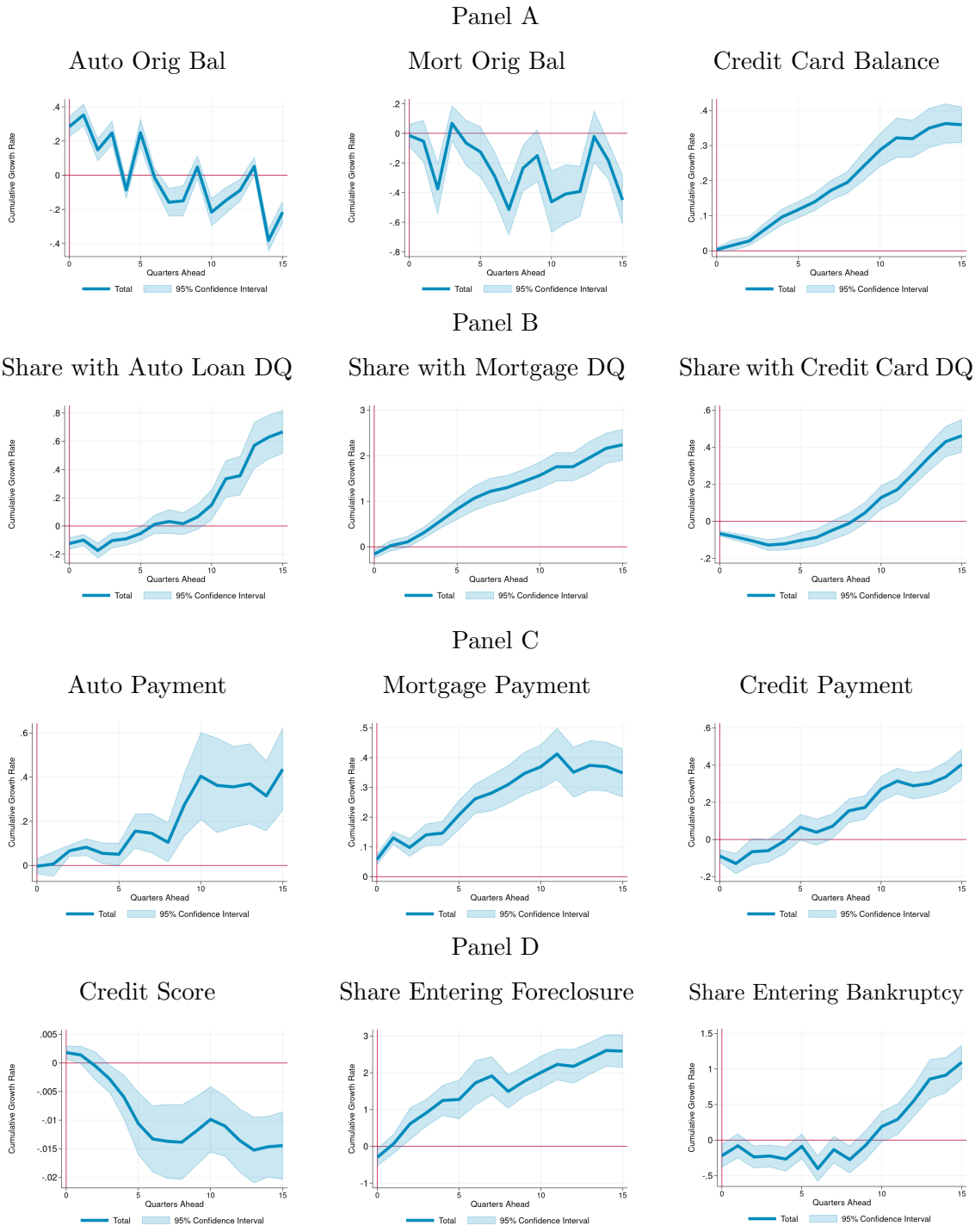

Note: Each line plots the impulse response functions of the growth of the dependent variable, using equation (4). The blue line plots the total (baseline results) effect, the red line plots the impulse response of FFR × Mortgage DTI, while also controlling for FFR × non-mortgage DTI, and the green line plots the impulse response of FFR × ARM DTI, while also controlling for FFR × non-ARM DTI. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure 8: Effects of FFR Change by Leverage: Asymmetric Effects of Positive and Negative Shocks



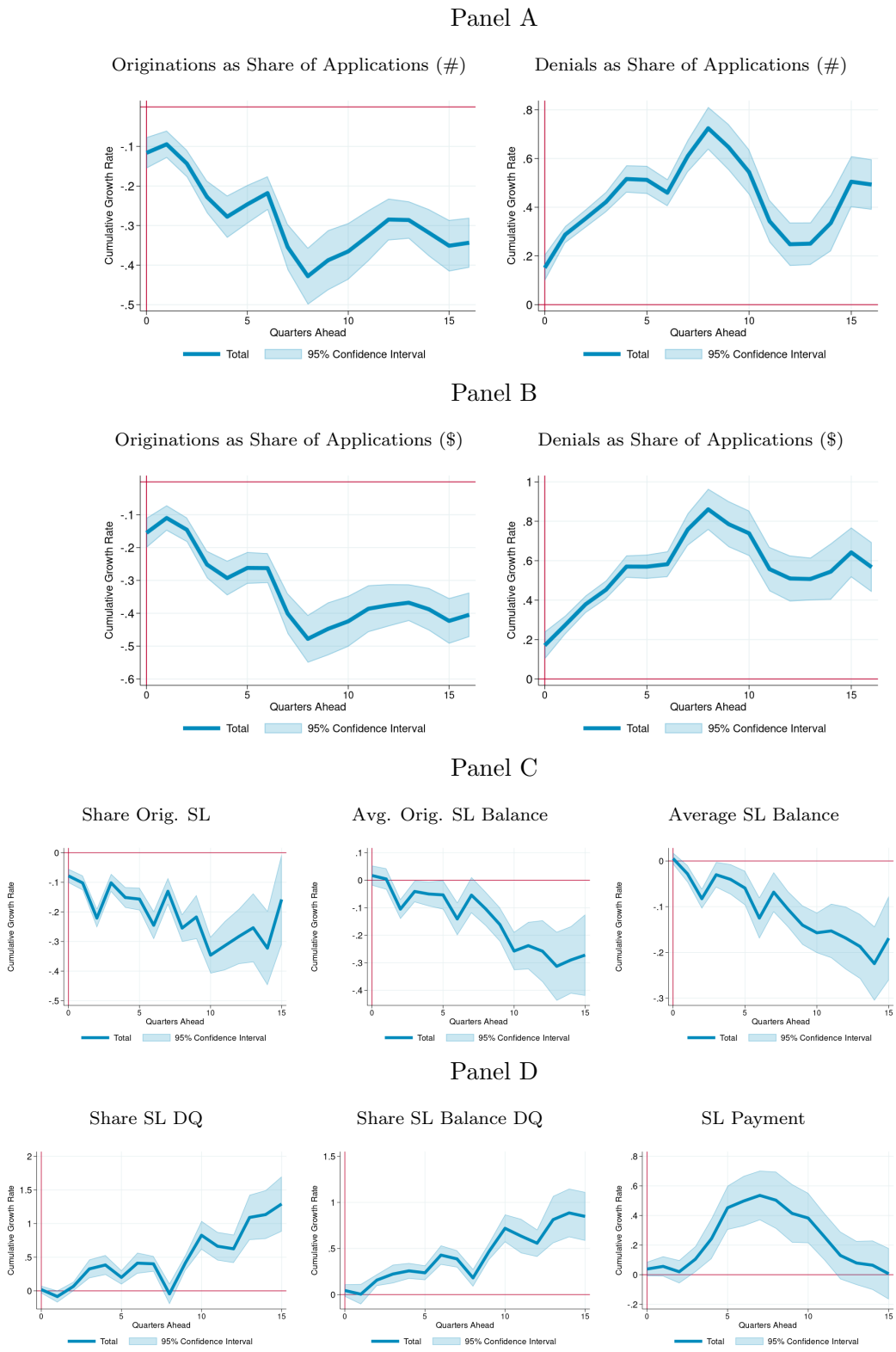

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (5). Solid line indicates impulse response for positive FFR shocks and dashed line for negative FFR shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 9: Effects of NS Shocks by Leverage: Household Financial 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 1999Q1-2022Q3. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 10: Effects of NS Shocks by Leverage: More Mortgage Outcomes (HMDA) & Student Loans (CCP)




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 refinances and payment from Black Knight McDash for range 2005Q1 – 2023Q3. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2023Q4. Student loan variables come from the FRBNY/Equifax Consumer Credit Panel.

Figure 11: Effects of FFR Change by Leverage: Household Financial Outcomes, Controlling for FFR-by-Demographic Interactions




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1) with . Shaded regions indicate 95% confidence intervals. The dashed lines represents the main baseline result (Figure 2). The Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. Demographic variables from 2000 Census.

# Tables

Table 1: Outcome Summary Statistics

| Variable | Mean | SD | 25th | Median | 75th |
|---|---|---|---|---|---|
| | | Levels | | | |
| Auto Orig Bal | 512.511 | 183.893 | 389.891 | 497.125 | 613.399 |
| Mort Orig Bal | 2,575.820 | 1,716.201 | 1,385.221 | 2,182.652 | 3,325.156 |
| Average CC Bal | 3,591.467 | 863.417 | 3,000.676 | 3,610.452 | 4,136.967 |
| Share DQ Auto (%) | 2.171 | 1.318 | 1.208 | 1.921 | 2.854 |
| Share DQ Mort (%) | 0.648 | 0.745 | 0.211 | 0.386 | 0.779 |
| Share DQ Credit (%) | 6.705 | 2.296 | 5.065 | 6.420 | 8.004 |
| Average Auto Payment | 142.748 | 187.091 | 107.179 | 134.325 | 163.666 |
| Average Mortgage Payment | 317.741 | 157.489 | 211.615 | 295.666 | 407.172 |
| Average Credit Payment | 226.073 | 253.427 | 140.079 | 195.163 | 274.961 |
| Average Credit Score | 642.539 | 34.856 | 617.139 | 644.740 | 669.349 |
| Share New Foreclosure (%) | 0.083 | 0.107 | 0.013 | 0.052 | 0.115 |
| Share New Bankruptcy (%) | 0.161 | 0.146 | 0.053 | 0.126 | 0.23 |
| Share Apps Orig (%) | 58.457 | 7.977 | 53.522 | 59.071 | 63.933 |
| Share Apps Denied (%) | 19.983 | 6.366 | 15.593 | 19.251 | 23.529 |
| Orig Bal Share | 61.037 | 8.070 | 56.421 | 61.967 | 66.551 |
| Deny Bal Share | 17.083 | 6.045 | 12.848 | 16.241 | 20.267 |
| Total Employment | 0.469 | 0.052 | 0.440 | 0.473 | 0.504 |
| ZHVI | 167,152.826 | 129,375.215 | 97,578.562 | 134,951.531 | 194,645.266 |
| HPI | 158.407 | 55.687 | 119.189 | 143.769 | 181.031 |
| Equity | 1.083e+10 | 5.111e+10 | 685997504 | 1752799360 | 5610516480 |
| Credit Bal DQ (%) | 8.769 | 3.264 | 6.542 | 8.343 | 10.351 |
| Auto Bal DQ (%) | 3.438 | 1.988 | 1.995 | 3.070 | 4.490 |
| Mortgage Bal DQ (%) | 2.375 | 3.210 | 0.623 | 1.223 | 2.761 |
| Share Credit Bal DQ | 8.858 | 3.269 | 6.611 | 8.343 | 10.502 |
| Share Originating SL (%) | 16.914 | 8.741 | 9.412 | 16.834 | 22.949 |
| Avg Orig. SL Bal | 1,382.075 | 789.554 | 727.403 | 1,343.072 | 1,902.830 |
| Average SL Bal | 4,427.136 | 2,376.474 | 2,473.271 | 4,279.971 | 6,086.678 |
| Share SL DQ (%) | 8.481 | 4.937 | 5.359 | 4.595 | 11.455 |
| Share SL Bal DQ (%) | 5.487 | 4.403 | 1.974 | 8.358 | 7.981 |
| Average SL Payment | 44.777 | 35.781 | 20.506 | 37.172 | 57.143 |

Table 2: Outcome Summary Statistics

| **Variable** | **Mean** | **SD** | **25th** | **Median** | **75th** |
|---|---|---|---|---|---|
| Cumulative Growth | | | | | |
| Auto Orig Bal | 5.727 | 25.471 | -10.746 | 5.106 | 20.919 |
| Mort Orig Bal | 3.882 | 39.217 | -24.061 | -1.089 | 26.717 |
| Average CC Bal | 2.819 | 13.112 | -5.484 | -0.892 | 10.805 |
| Share DQ Auto (%) | 15.983 | 32.938 | -5.163 | 3.362 | 30.089 |
| Share DQ Mort (%) | 2.944 | 72.250 | -47.414 | 9.003 | 28.741 |
| Share DQ Credit (%) | 2.946 | 21.376 | -10.829 | 1.602 | 14.172 |
| Average Auto Payment | 8.377 | 15.985 | 0.065 | 6.380 | 15.173 |
| Average Mortgage Payment | 7.505 | 18.775 | -2.873 | 4.927 | 14.592 |
| Average Credit Payment | 5.036 | 20.884 | -8.179 | 2.819 | 15.449 |
| Average Credit Score | -0.014 | 1.223 | -0.718 | -0.011 | 0.642 |
| Share New Foreclosure (%) | -10.549 | 79.331 | -70.123 | -29.237 | 25.754 |
| Share New Bankruptcy (%) | -14.665 | 57.559 | -55.327 | -23.437 | 14.151 |
| Share Apps Orig (%) | 1.618 | 11.241 | -5.209 | 1.056 | 7.541 |
| Share Apps Denied (%) | -1.801 | 23.559 | -18.436 | -4.542 | 12.107 |
| Orig Bal Share | 1.218 | 11.556 | -5.273 | 1.048 | 7.161 |
| Deny Bal Share | -1.437 | 27.228 | -20.516 | -6.263 | 12.675 |
| Total Employment | -0.172 | 4.359 | -2.495 | 0.270 | 2.476 |
| ZHVI | 12.349 | 15.420 | 2.130 | 65.754 | 19.954 |
| HPI | 11.176 | 14.661 | 1.766 | 8.086 | 18.016 |
| Equity | 13.719 | 18.737 | 1.480 | 9.833 | 22.975 |
| Credit Bal DQ (%) | 7.168 | 29.946 | -12.390 | 3.307 | 19.383 |
| Auto Bal DQ (%) | 15.093 | 45.173 | -12.637 | 6.392 | 30.809 |
| Mortgage Bal DQ (%) | 9.955 | 90.388 | -50.667 | -17.243 | 34.602 |
| Credit Bal DQ (%) | 7.168 | 29.957 | -11.568 | 3.307 | 21.245 |
| Share Originating SL (%) | 25.593 | 36.120 | 1.155 | 12.305 | 39.019 |
| Avg Orig. SL Bal | 27.027 | 36.294 | 4.090 | 14.809 | 39.373 |
| Average SL Bal | 26.480 | 28.020 | 7.223 | 16.349 | 36.848 |
| Share SL DQ (% ) | 16.562 | 69.768 | -26.012 | 10.268 | 53.393 |
| Share SL Bal DQ (%) | -2.972 | 45.354 | -27.444 | -2.573 | 19.870 |
| Average SL Payment | 1.580 | 63.211 | -41.531 | -6.762 | 20.995 |

Table 3: Summary Statistics - Shocks & Exposure Measures

| **Variable** | **Mean** | **SD** | **25th Percentile** | **Median** | **75th Percentile** |
|---|---|---|---|---|---|
| | | | Federal Funds Rate | | |
| FFR | 1.913 | 2.003 | 0.143 | 1.225 | 3.497 |
| Δ FFR | 0.006 | 0.467 | -0.027 | 0.010 | 1.463 |
| | | | Nakamura-Steinsson Shocks | | |
| FFR NS | -5.018 | 4.503 | -8.552 | -6.479 | -1.464 |
| Δ FFR NS | -0.007 | 1.071 | -0.428 | 0.121 | 0.514 |
| | | | Debt-to-Income Ratio | | |
| DTI | 0.699 | 0.216 | 0.537 | 0.693 | 0.842 |
| | | | Measures of Credit Market Exposure at Base | | |
| ARM DTI | 0.083 | 0.124 | 0.008 | 0.029 | 0.092 |
| FRM DTI | 0.413 | 0.404 | 0.111 | 0.264 | 0.585 |
| HELOC DTI | 0.009 | 0.009 | 0.002 | 0.007 | 0.015 |
| Student Loan DTI | 0.028 | 0.014 | 0.018 | 0.024 | 0.035 |
| Auto DTI | 0.079 | 0.031 | 0.055 | 0.074 | 0.099 |
| Credit Card DTI | 0.090 | 0.021 | 0.074 | 0.088 | 0.104 |

Table 4: How Does DTI Vary by Demographics?

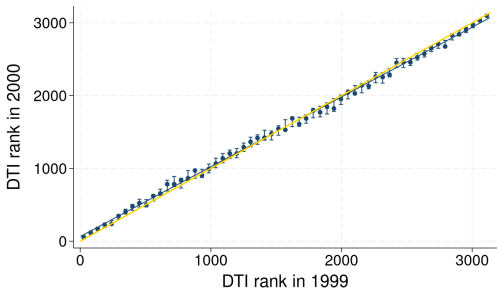
| | (1)<br>Fixed DTI |
|---|---|
| Avg. wage | -0.0000131 |
| | (-1.07) |
| Prop. Under 25 | 0.913*** |
| | (5.97) |
| Prop. 25-54 | 1.800*** |
| | (9.26) |
| Prop. College Degree | 0.369*** |
| | (4.56) |
| Share Construction | 0.630*** |
| | (3.71) |
| Share Education, Health | -0.156* |
| | (-2.25) |
| Share Leisure, Hospitality | 0.699*** |
| | (5.34) |
| Share Manufacturing | 0.0578 |
| | (1.34) |
| Unemployment | 1.799*** |
| | (4.15) |
| Share White | -0.192*** |
| | (-6.97) |
| Share Hispanic | -0.128** |
| | (-3.10) |
| Equity (Thousands) | 4.69e-10 |
| | (1.41) |
| Urbanicity | 0.0768*** |
| | (7.16) |
| Observations | 3060 |

$t$ statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

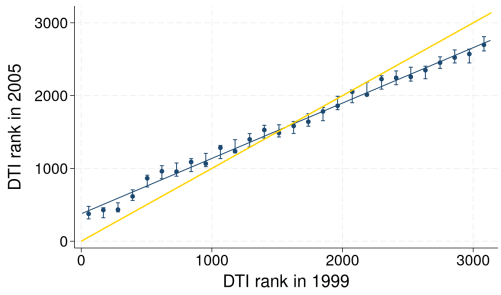
# Appendix A: Outcomes and Robustness

Figure A.1: Binned Scatters of DTIs in various years versus base year (1999)

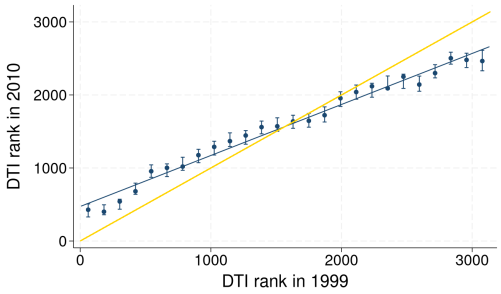
Corr: 0.96



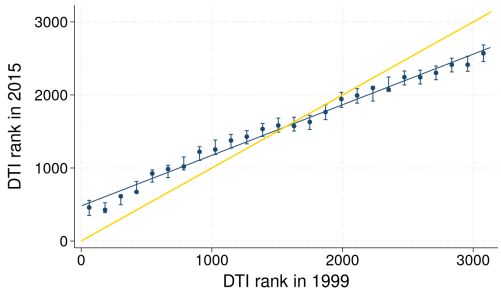

Corr: 0.76




Corr: 0.70




Corr: 0.69




Corr: 0.60

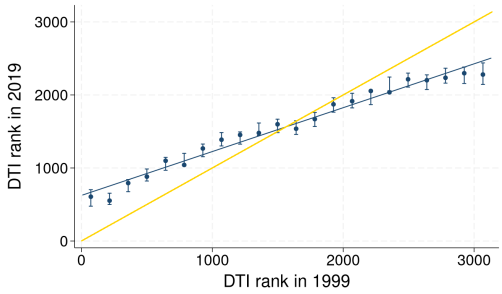
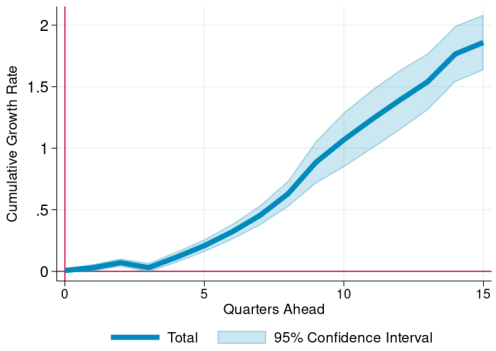


Figure A.2: Effects of FFR Change by Leverage: Dollar Share of Balances and Delinquencies

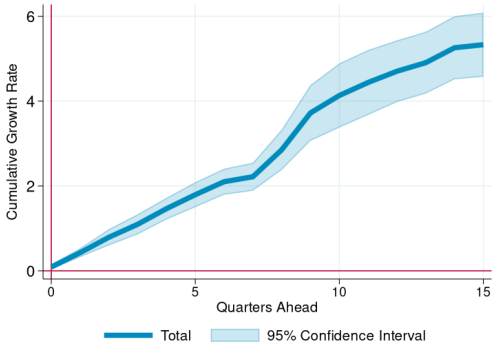
Panel A

Share ($) with Auto Loan DQ

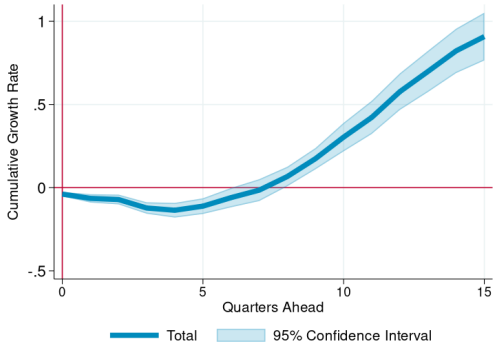



Panel B

Share ($) with Mortgage DQ




Share ($) with Credit Card DQ



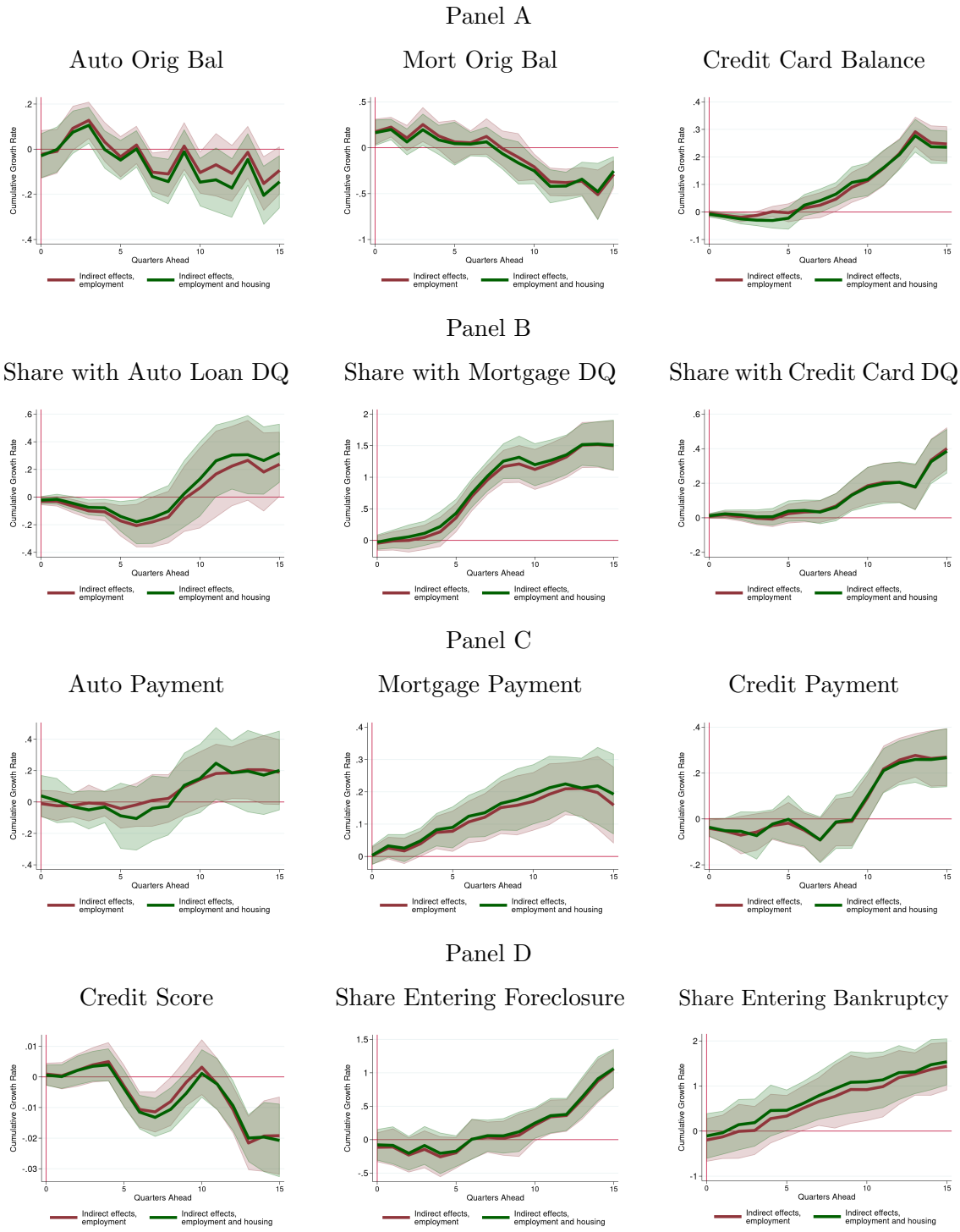

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.3: Mechanisms: Indirect Effects Due to the Labor Market (Using Bartik Shocks)



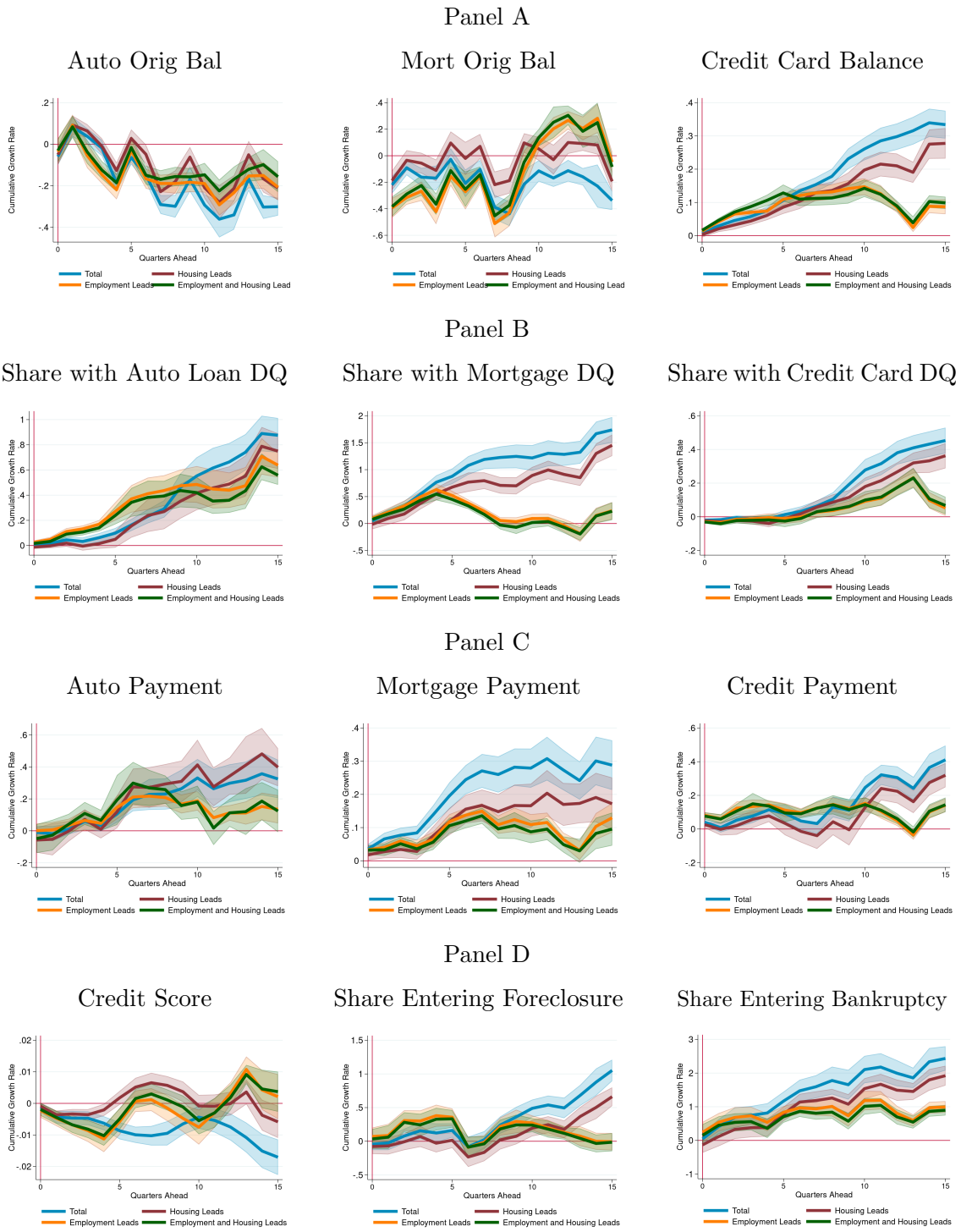

Note: The line plots the impulse response functions of the indirect effect on the growth of the dependent variable using equation (2). That is, the line is the difference between the impulse response of the total effect in our baseline results (Figure 2) minus the impulse response after controlling for the labor market mechanisms using the Bartik instrument (Figure 5) in order to isolate the indirect effect. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.4: Mechanisms: Indirect Effects Due to the Labor and Housing Markets




Note: The lines plots the impulse response functions of the indirect effect on the growth of the dependent variable using equation (2). That is, the red line is the difference between the impulse response of the total effect in our baseline results (Figure 2) minus the impulse response after controlling for the labor market mechanisms using the Bartik instrument (Figure 5) in order to isolate the indirect effect. The blue line the difference between the impulse response of the total effect in our baseline results (Figure 2) minus the impulse response after controlling for the labor market mechanisms and housing market mechanisms using the Bartik instruments (Figure A.5). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.5: Mechanisms: Borrowing Costs. Labor Market Effects & Housing Market Effects (Using Employment and Home Price Bartik Shocks to Instrument for Employment Rate and House Prices)




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). The blue line indicates the total impulse response, the red line indicates the impulse response when using the Bartik employment instrument, and the green line indicates the impulse response when using the Bartik housing instrument. 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.6: National-Level ARM Share

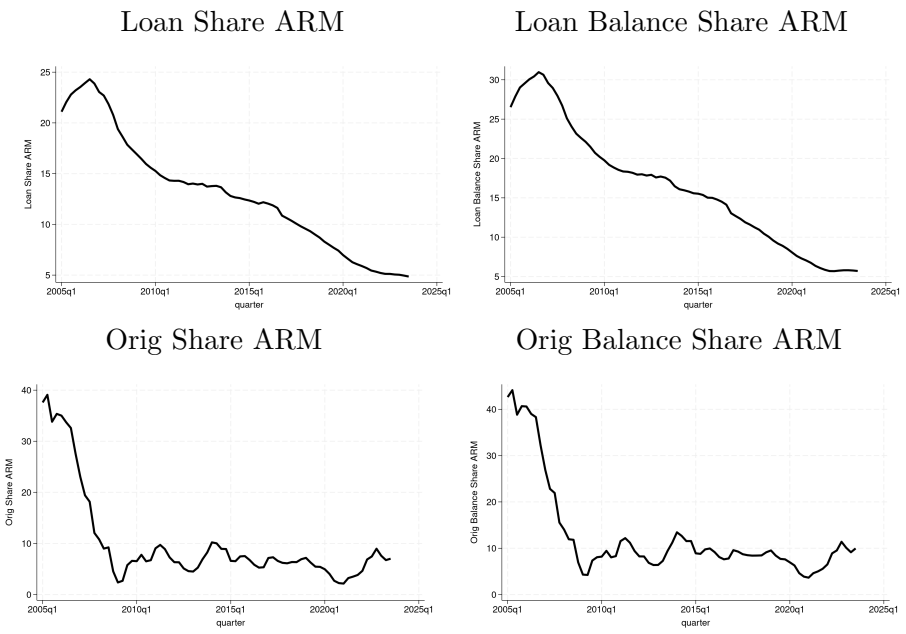
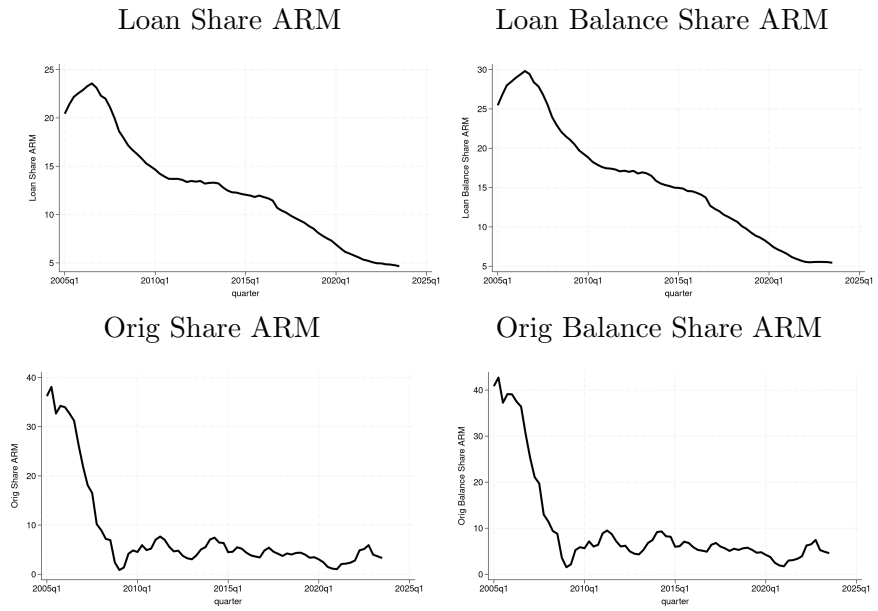
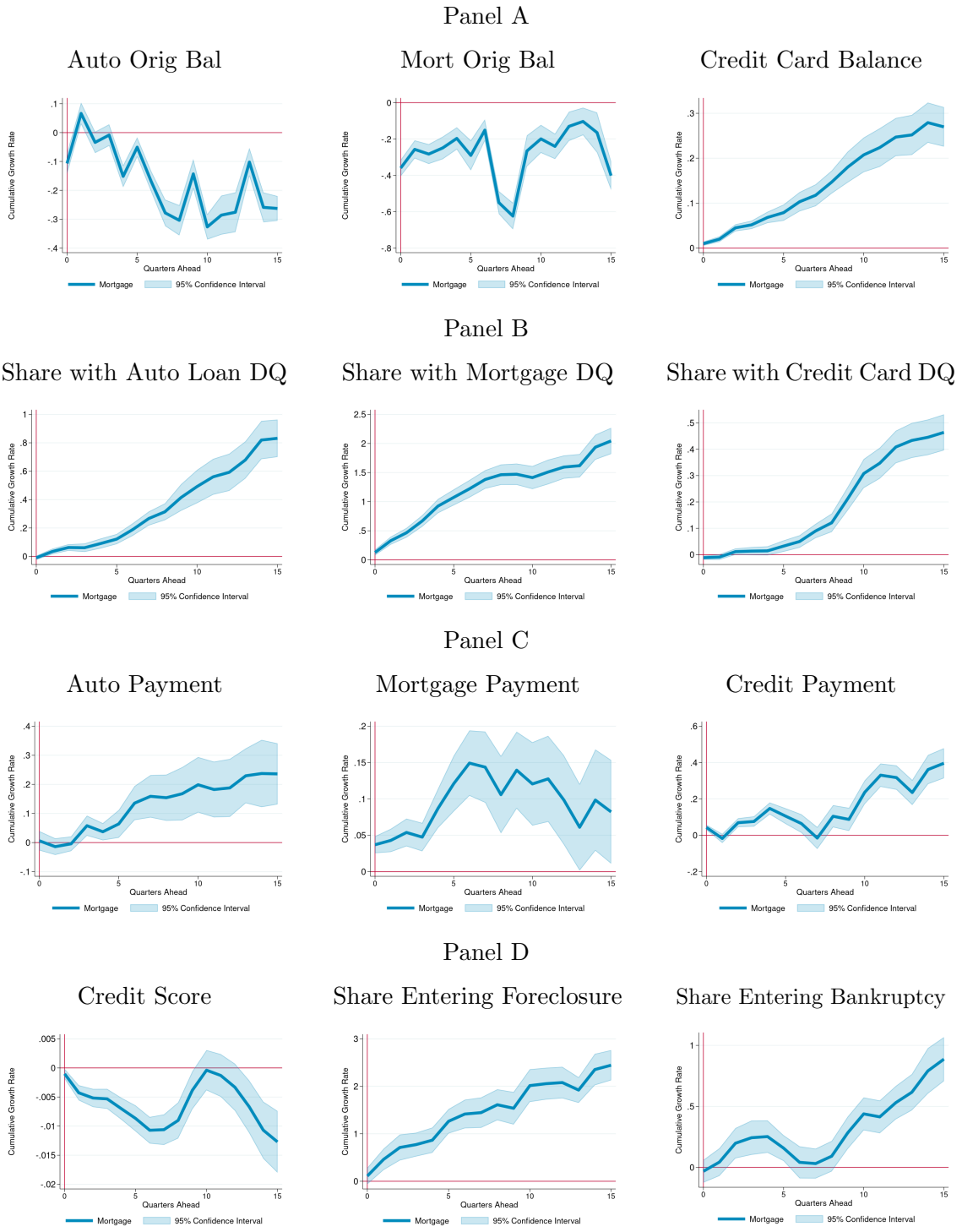


Figure A.7: County-Level ARM Share, weighted by number of borrowers




Note: Each line plots the total share of adjustable-rate mortgages as the share of number of home loans, the dollar share of the total balance of home loans, the share of the number of originations of loans, and the dollar share of the originations of home loans in McDash from 2005Q1-2023Q4

Figure A.8: Mechanisms: Mortgage DTI as Exposure Measure—Controlling for Component and Non-Mortgage $DTIs \times FFR$



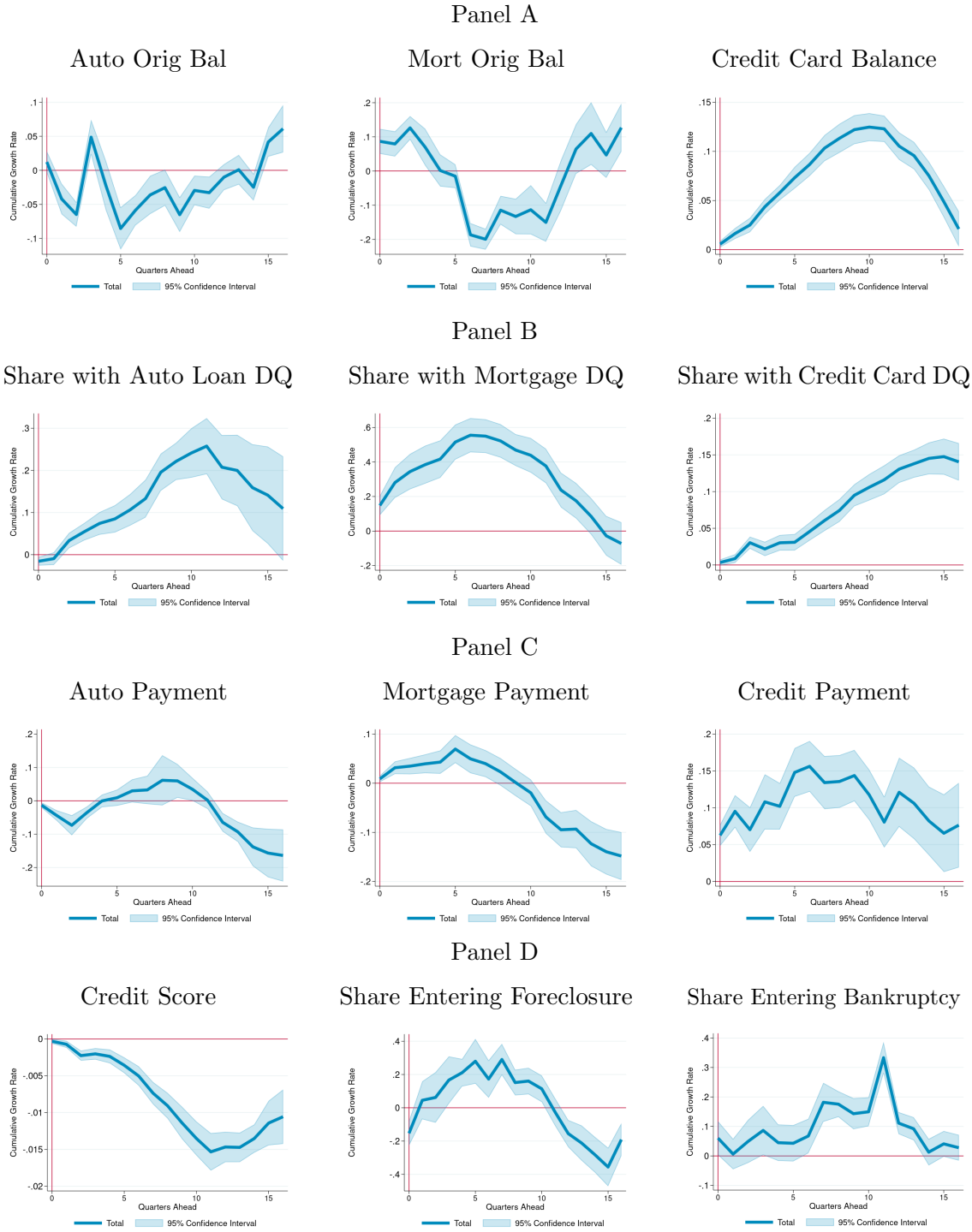

Note: Each line plots the impulse response functions of the growth of the dependent variable, using equation (4), with mortgage DTI as the exposure measure. This specification also controls for four other types of DTI: HELOC DTI, student loan DTI, auto DTI, and credit DTI. The Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.9: Mechanisms: ARM DTI as Exposure Measure—Controlling for Component and non-ARM $DTIs \times FFR$



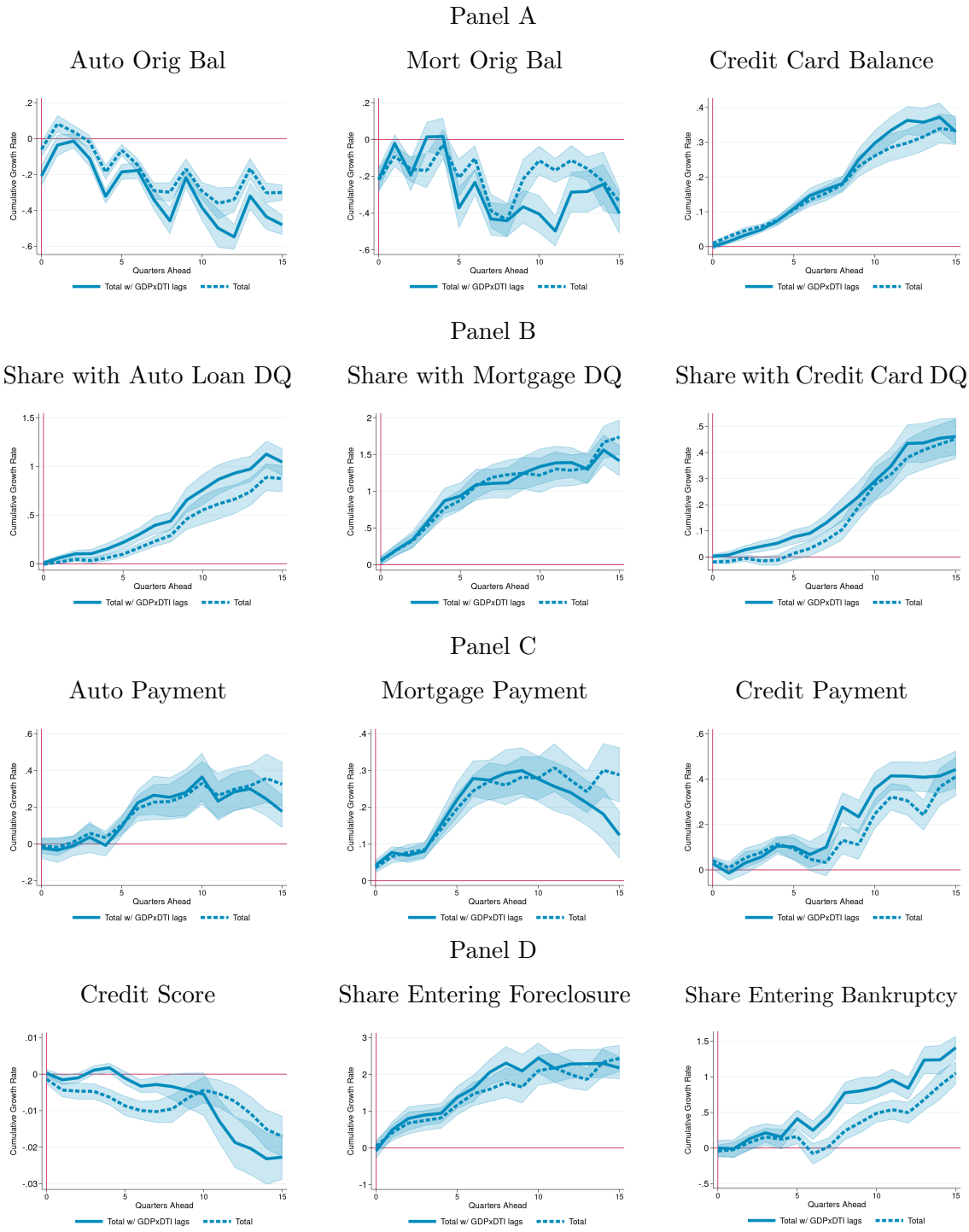

Note: Each line plots the impulse response functions of the growth of the dependent variable, using equation (4), with ARM DTI as the exposure measure. This specification also controls for five other types of DTI: HELOC DTI, student loan DTI, auto DTI, credit DTI, and FRM DTI. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.10: Effects of FFR Change by Leverage: Household Financial Health, Positive vs. Negative Shocks Differences



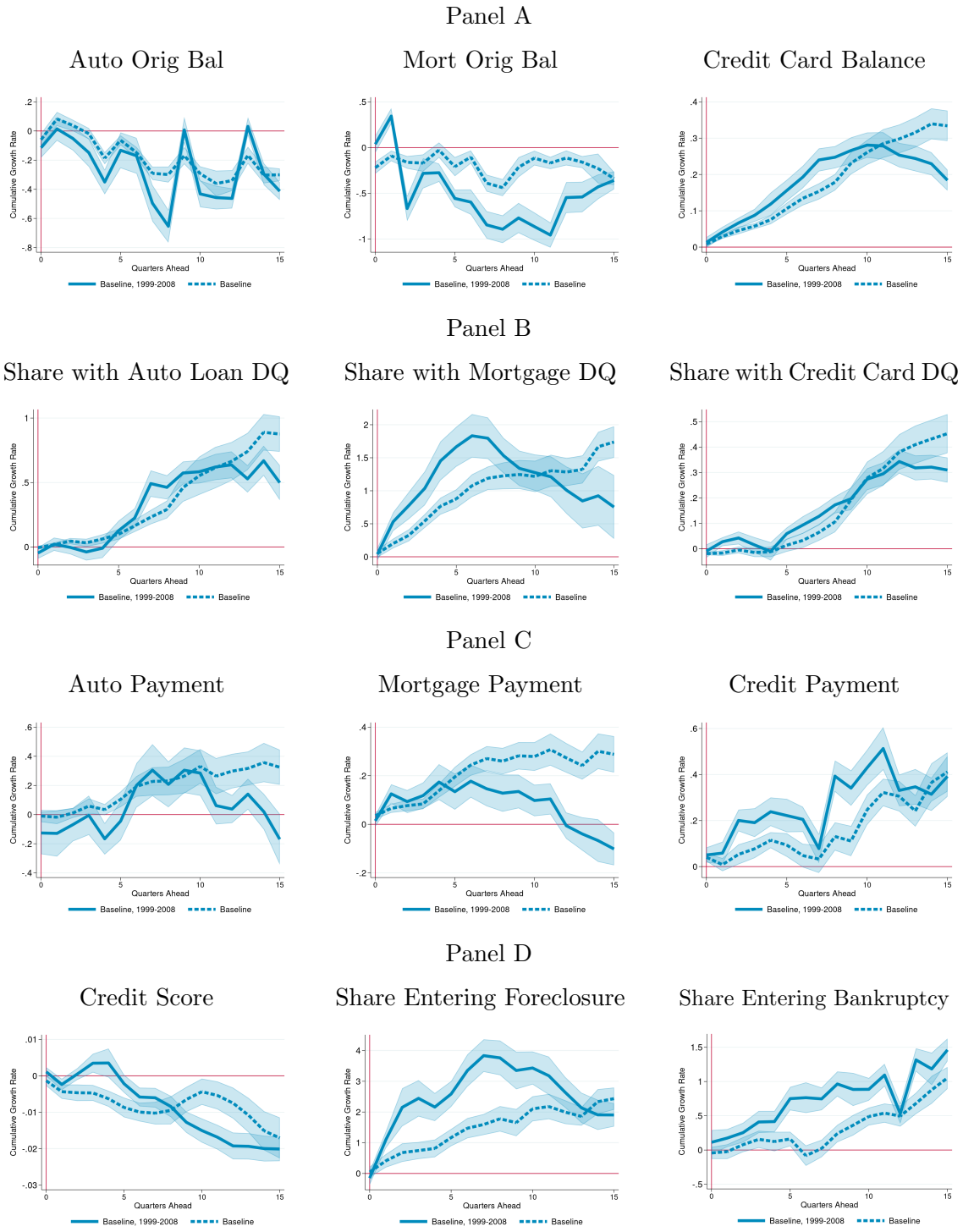

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (5). The solid line indicates the difference in impulse response for positive FFR shocks and the negative FFR shocks (Figure 8) represented by $\tilde{\beta}_j^{(h)}$. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.11: Effects of FFR Change by Leverage: Controlling for GDP × DTI Lags




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). The solid lines represents controlling for $J = 8$ GDP × DTI lags. The dashed line represents the main baseline result (2) 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.12: Is 1999-2008 Driving Results?

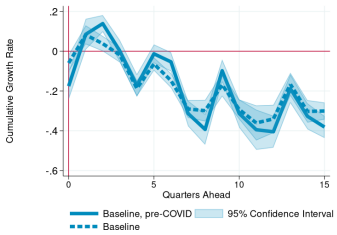



Note: Each line plots the impulse response functions of the growth of the dependent variable, using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2008Q4.

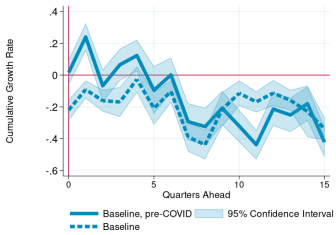
Figure A.12A: Effects of FFR Change by Leverage: Pre-COVID (1999q1 - 2019q4)
Household Financial Outcomes

Panel A

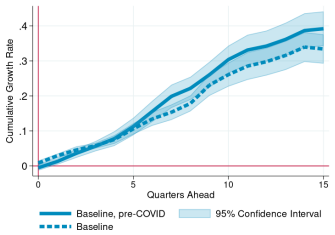
Auto Orig Bal




Mort Orig Bal

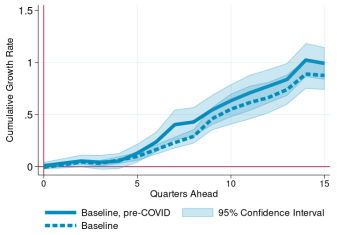



Credit Card Balance

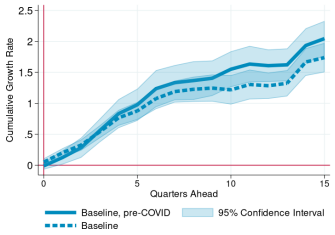



Panel B

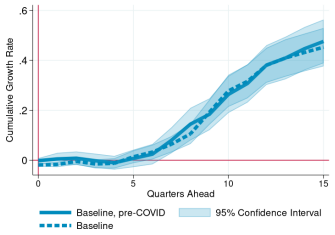
Share with Auto Loan DQ




Share with Mortgage DQ

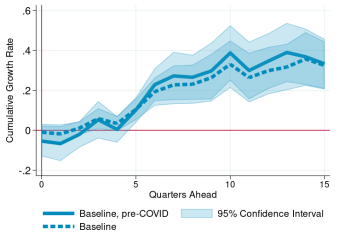



Share with Credit Card DQ

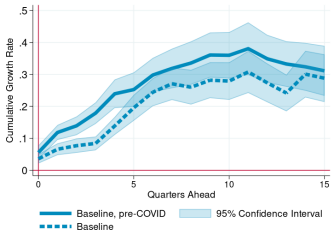



Panel C

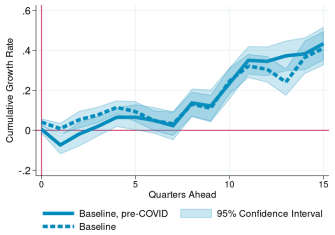
Auto Payment




Mortgage Payment

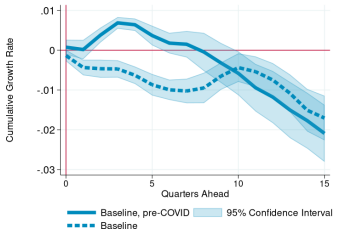



Credit Payment

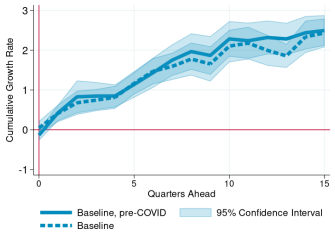



Panel D

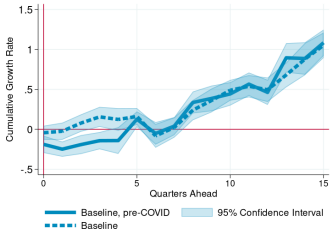
Credit Score




Share Entering Foreclosure

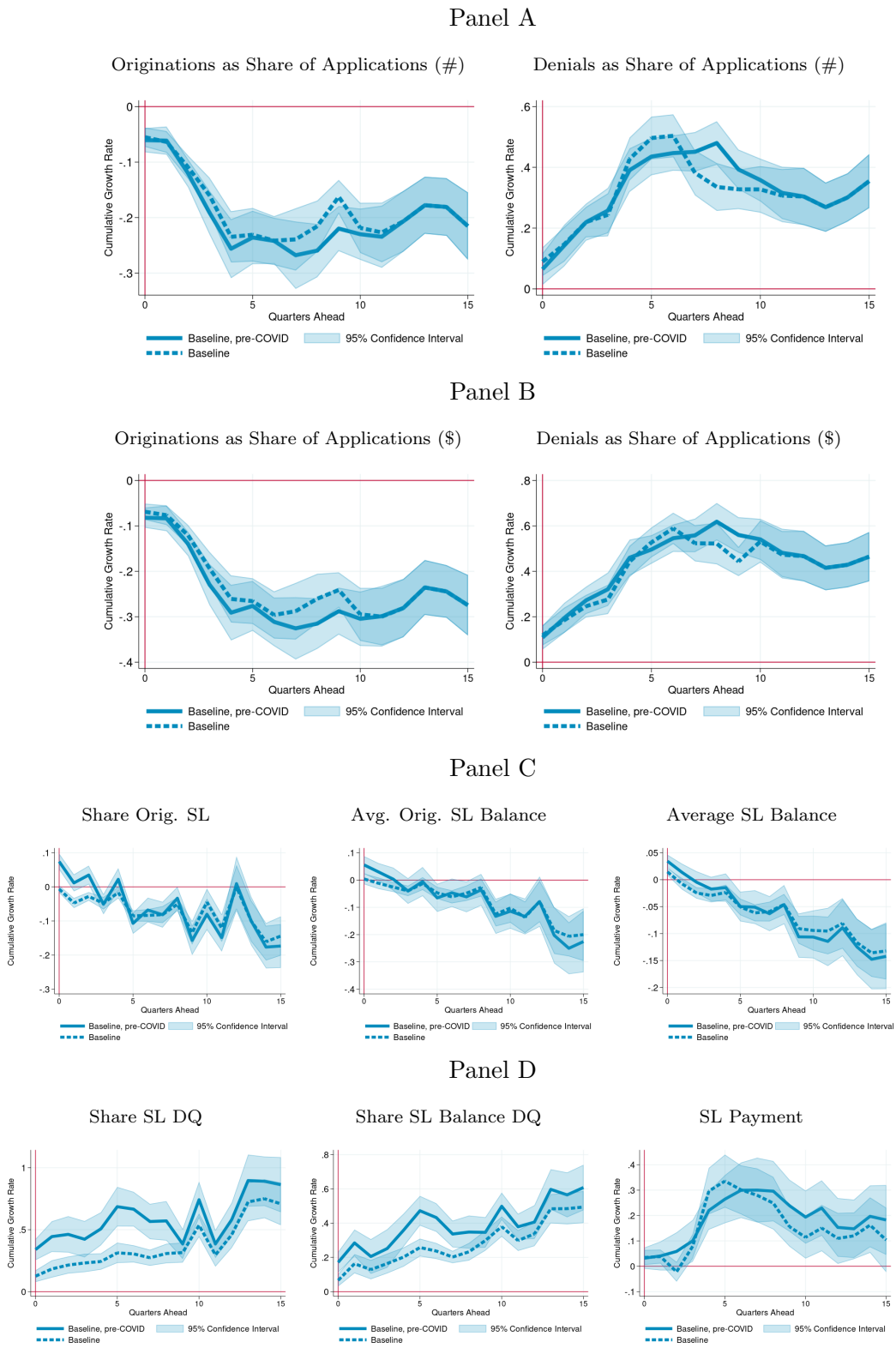



Share Entering Bankruptcy



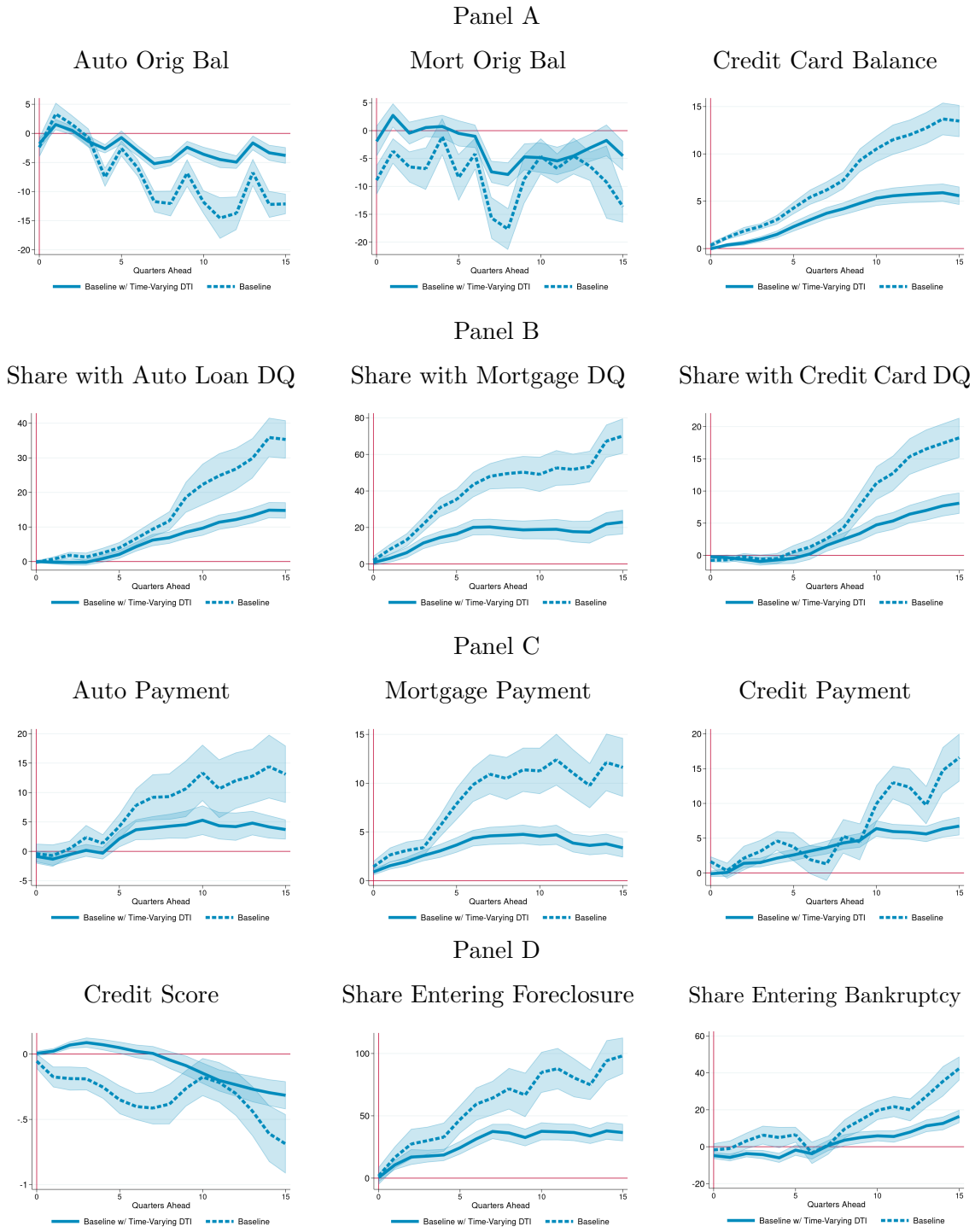

Note: Each line plots the impulse response functions of the growth of the dependent variable. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. The solid lines represents the result with time limited to the 2019Q4. The dashed line represents the main baseline result (2). Time frame is 1999Q1-2019Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.12B: Effects of FFR Change by Leverage: Pre-COVID (1999q1 - 2019q4)
More Mortgage Outcomes (HMDA) & Student Loans (CCP)



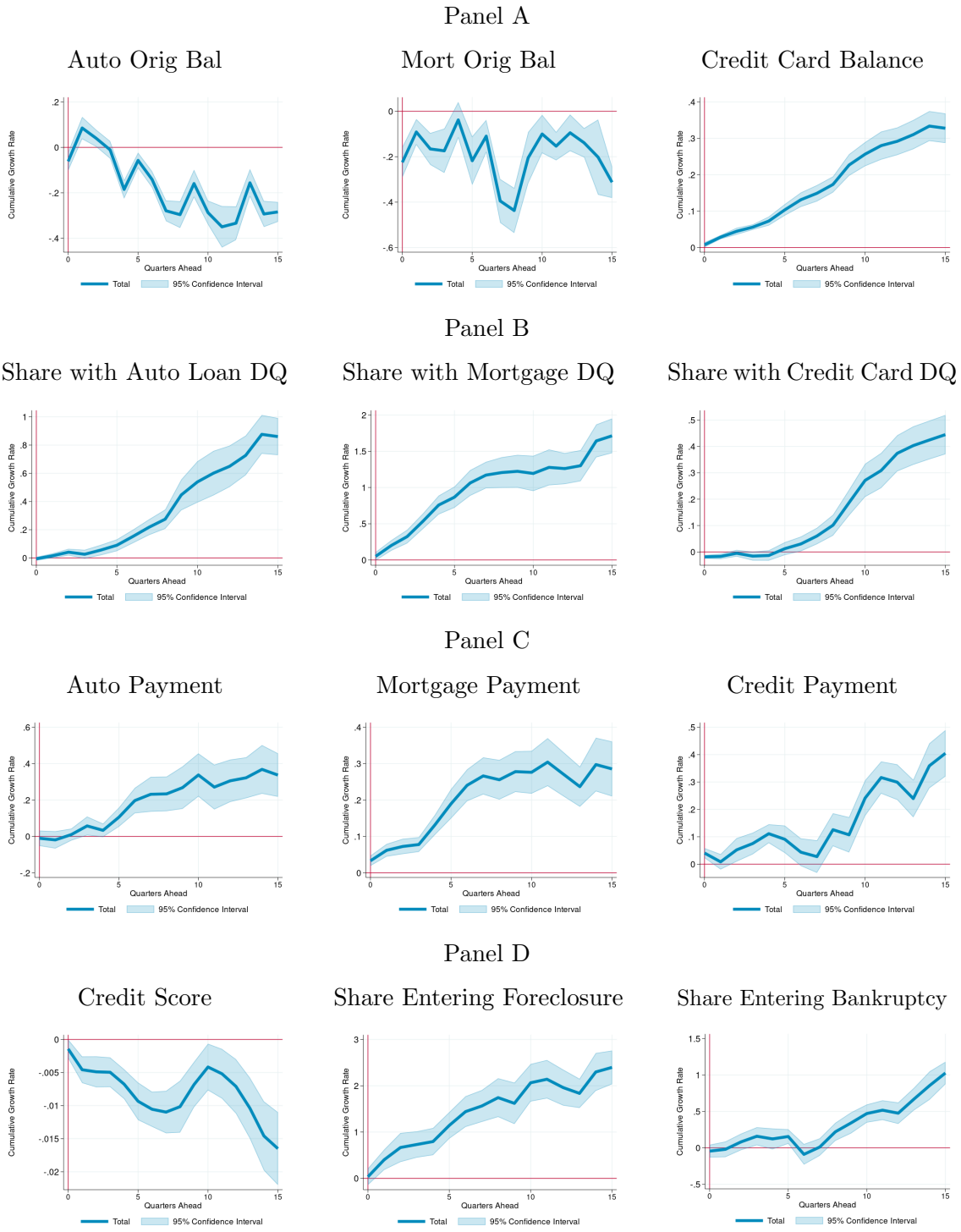

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. The solid lines represents the result with time limited to the 2019Q4. The dashed line represents the main baseline result (2). Standard errors clustered by county. Dependent variables on mortgage applications from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2019Q4. Student loan variables come from the FRBNY/Equifax Consumer Credit Panel

Figure A.13: Effects of FFR Change by Leverage: With Time-Varying DTI



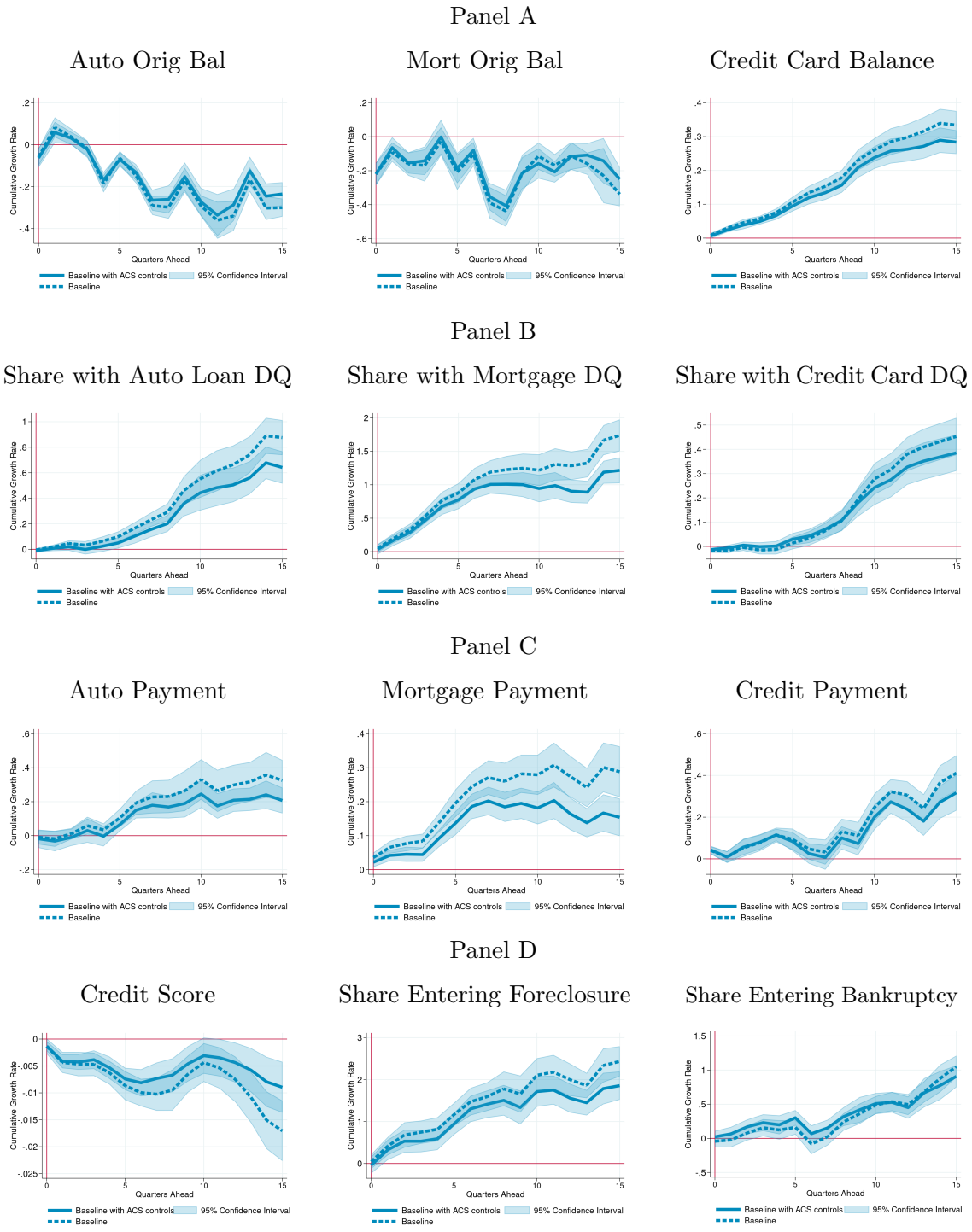

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), with time-varying DTI as the exposure measure. Shaded regions indicate 95% confidence intervals. The solid lines represents the result with time-varying DTI. The dashed line represents the main baseline result (2). Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.14: Effects of FFR Change by Leverage: Controlling for EPOP Lags



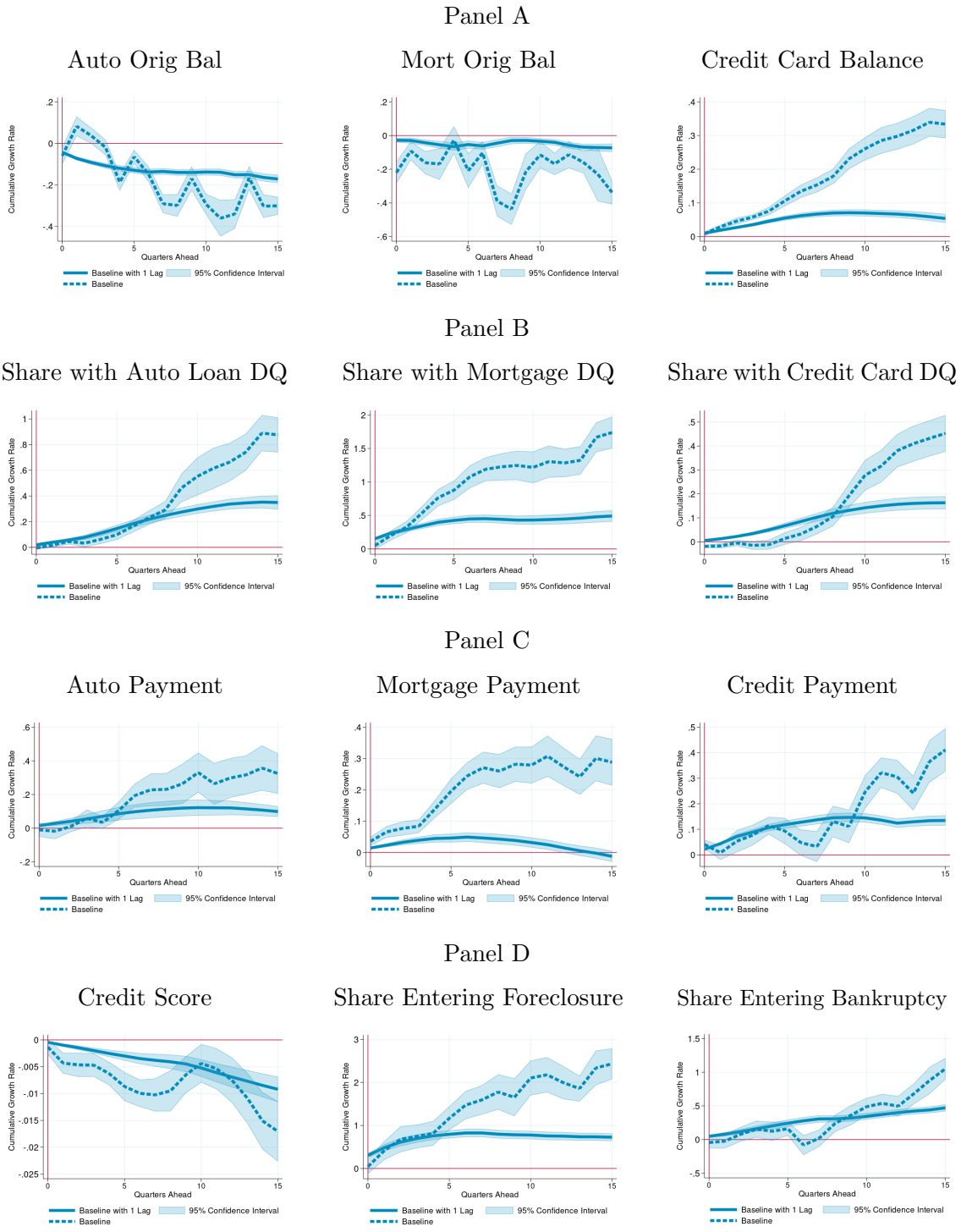

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). The solid lines represents the result adding $J = 8$ lags of EPOP. The dashed line represents the main baseline result (2). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.15: Effects of FFR Change by Leverage: Demographic Controls



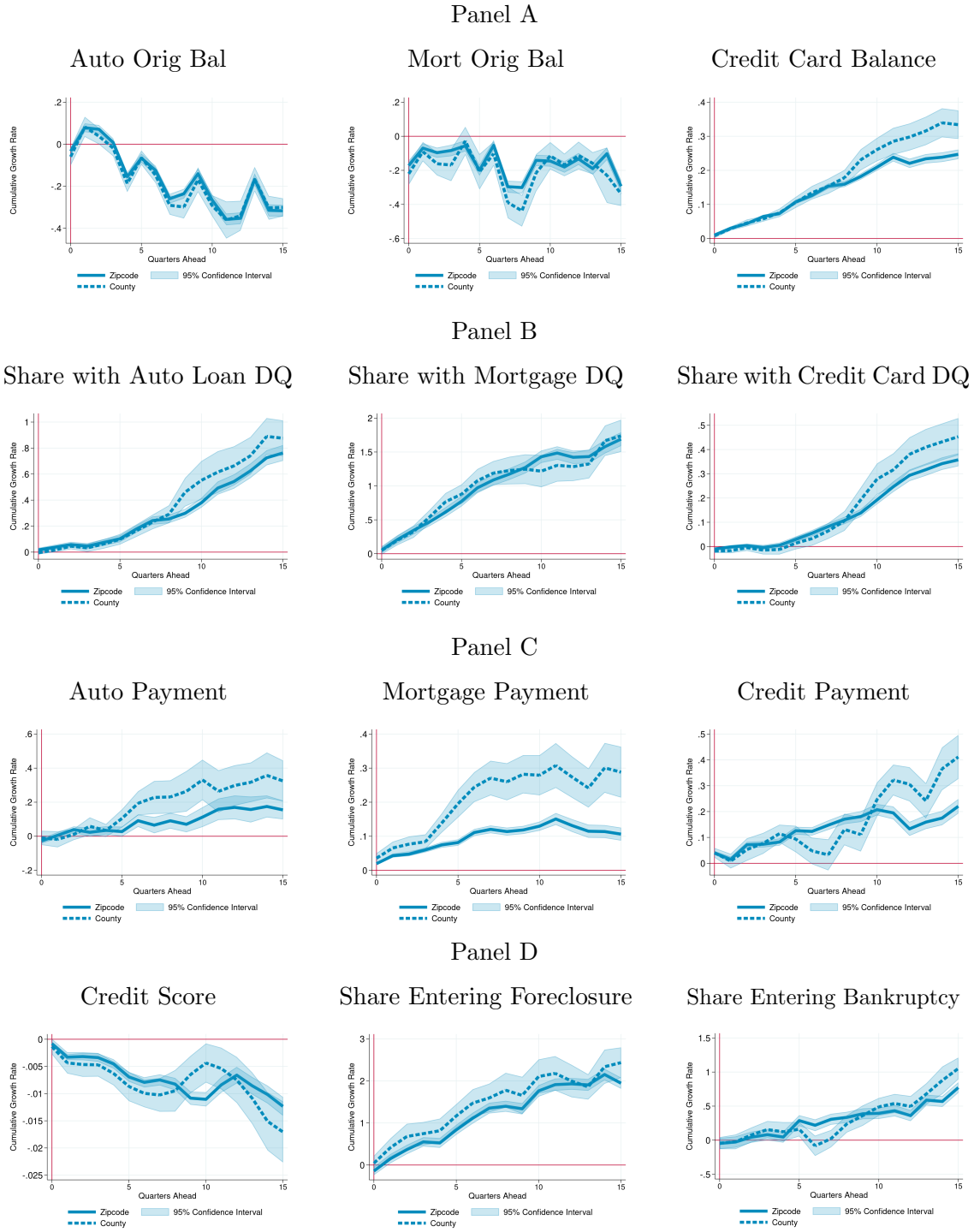

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), adding county-level controls from the ACS: age, race, education, from the ACS and Census. The solid line represents the result with these controls. The dashed line represents the baseline main results (Figure 2). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure A.16: Effects of FFR Change by Leverage: With One Lag of FFR




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.17: Effects of FFR Change by Leverage: Household Financial Outcomes Zipcode v. County




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/zip. Time frame is 1999q1-2023q4. The solid lines represents the results at the zipcode level, and the dashed line at the baseline county level results (2 and the Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.18: Effects of FFR Change by Leverage (Debt to Credit Limit as Leverage): Household Financial Outcomes at Individual Level




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.19: Effects of FFR Change by Leverage: FFR Falsification Test




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). The blue line is the impulse response function of the baseline result (Figure 2), while the gray lines are the impulse response functions of 100 permutation tests of FFR. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.20: Effects of FFR Change by Leverage: DTI Falsification Test




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). The blue line is the impulse response function of the baseline result (Figure 2), while the gray lines are the impulse response functions of 100 permutation tests of DTI. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

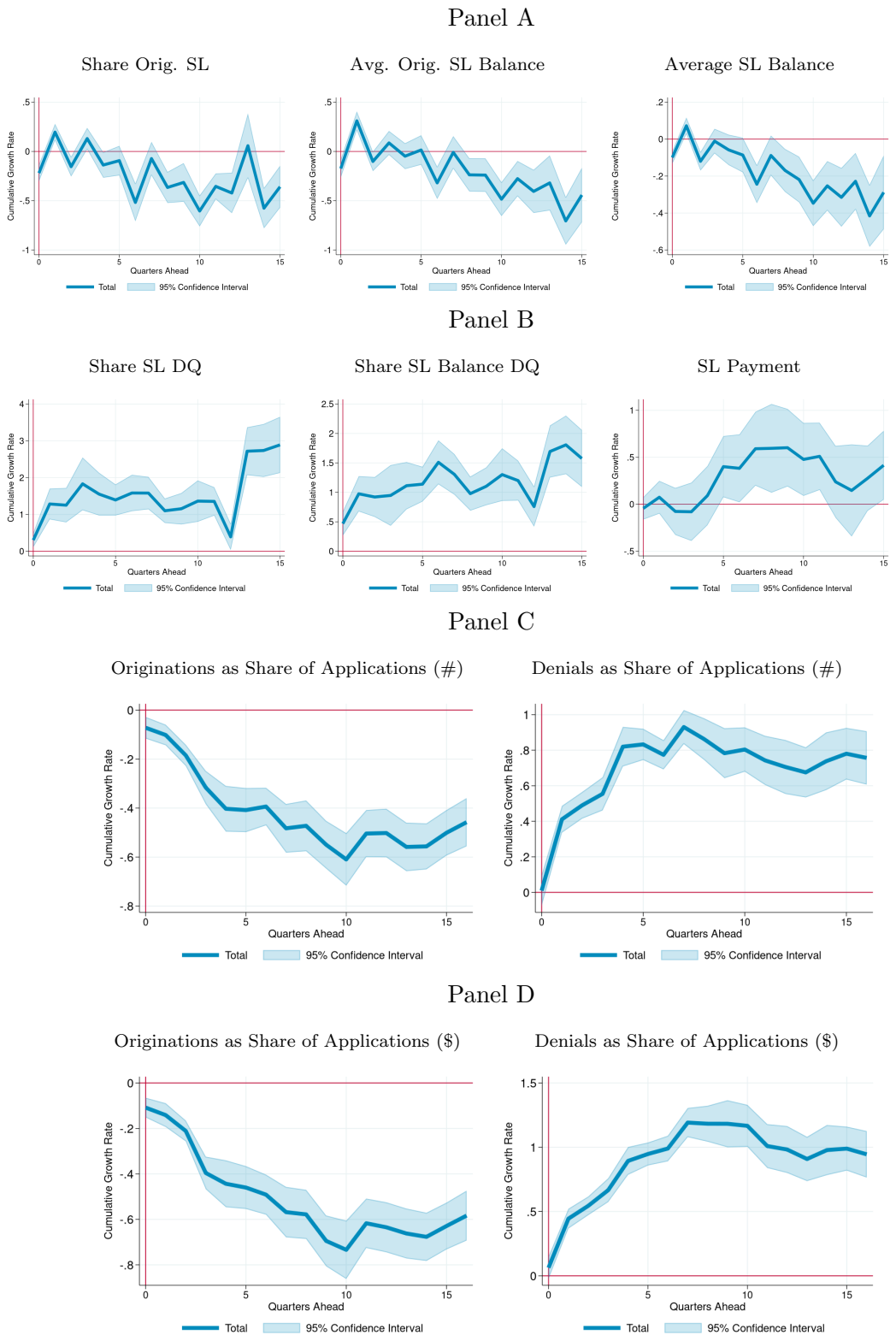
# Appendix B: Surprise Shocks

Figure B.1: Effects of JK Shocks by Leverage: Household Financial Outcomes



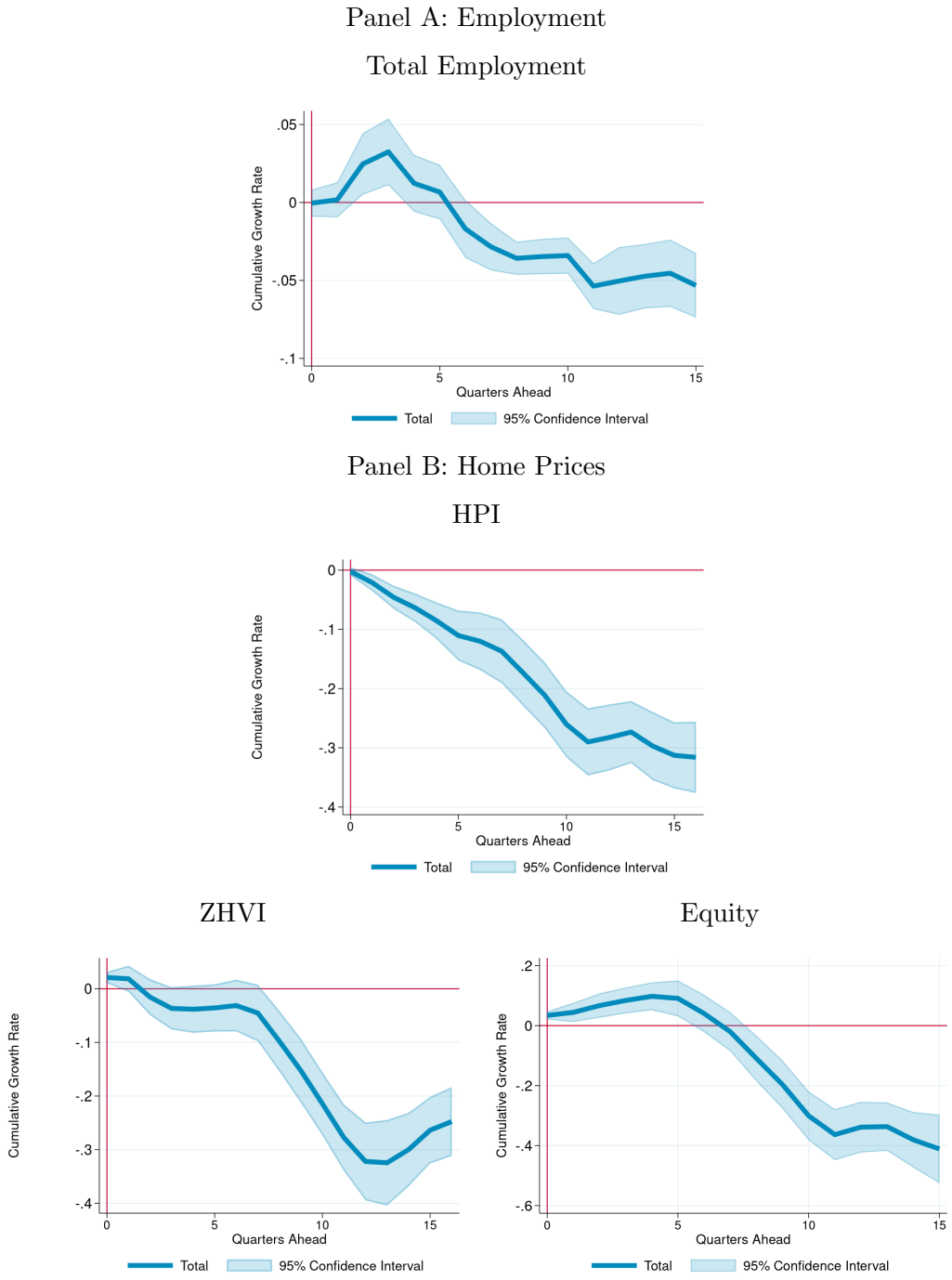

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure B.2: Effects of JK Shocks by Leverage: Student Loans & More Mortgage Outcomes (HMDA)



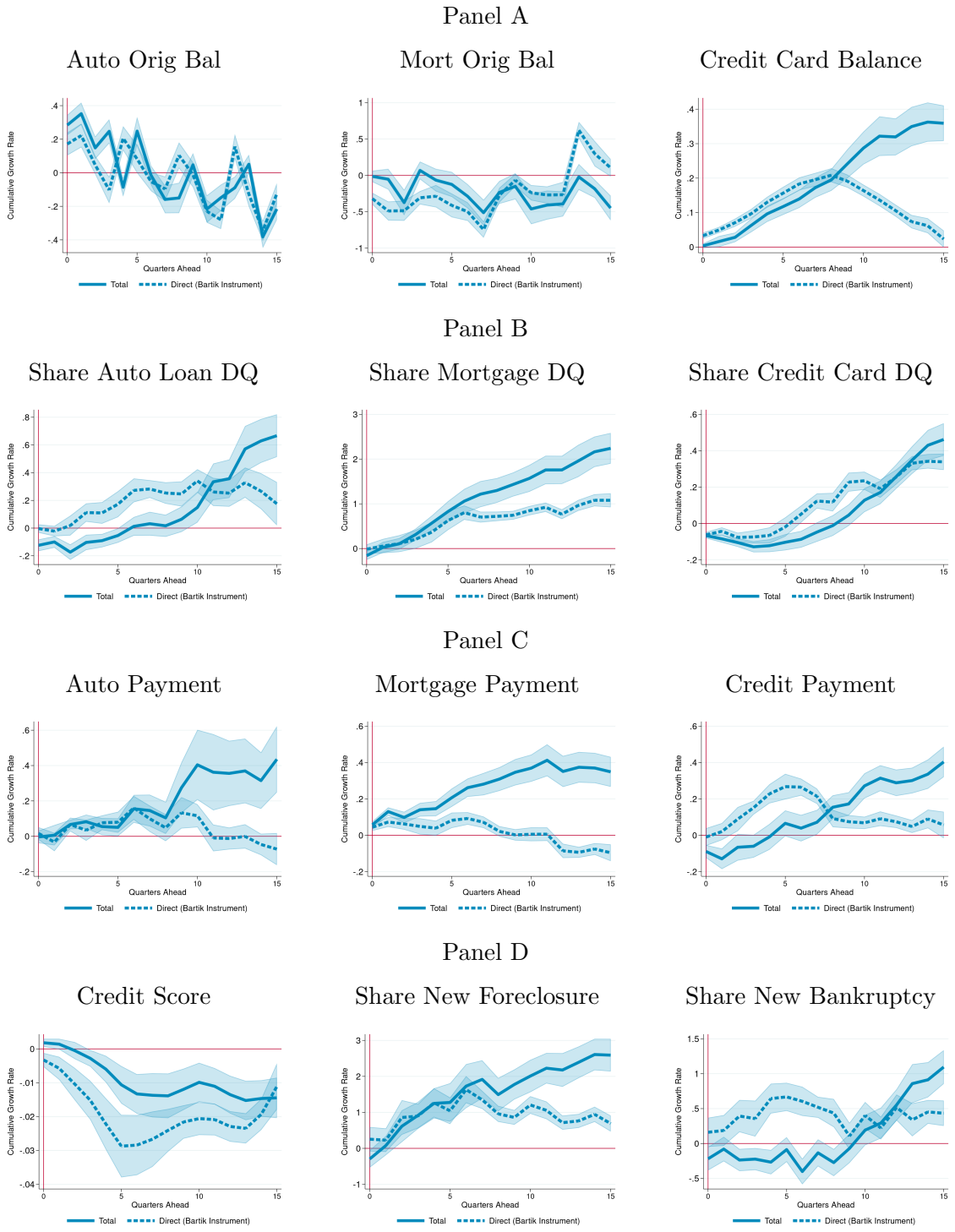

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2023Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure B.3: Effects of NS Shocks by Leverage: Employment and House Prices



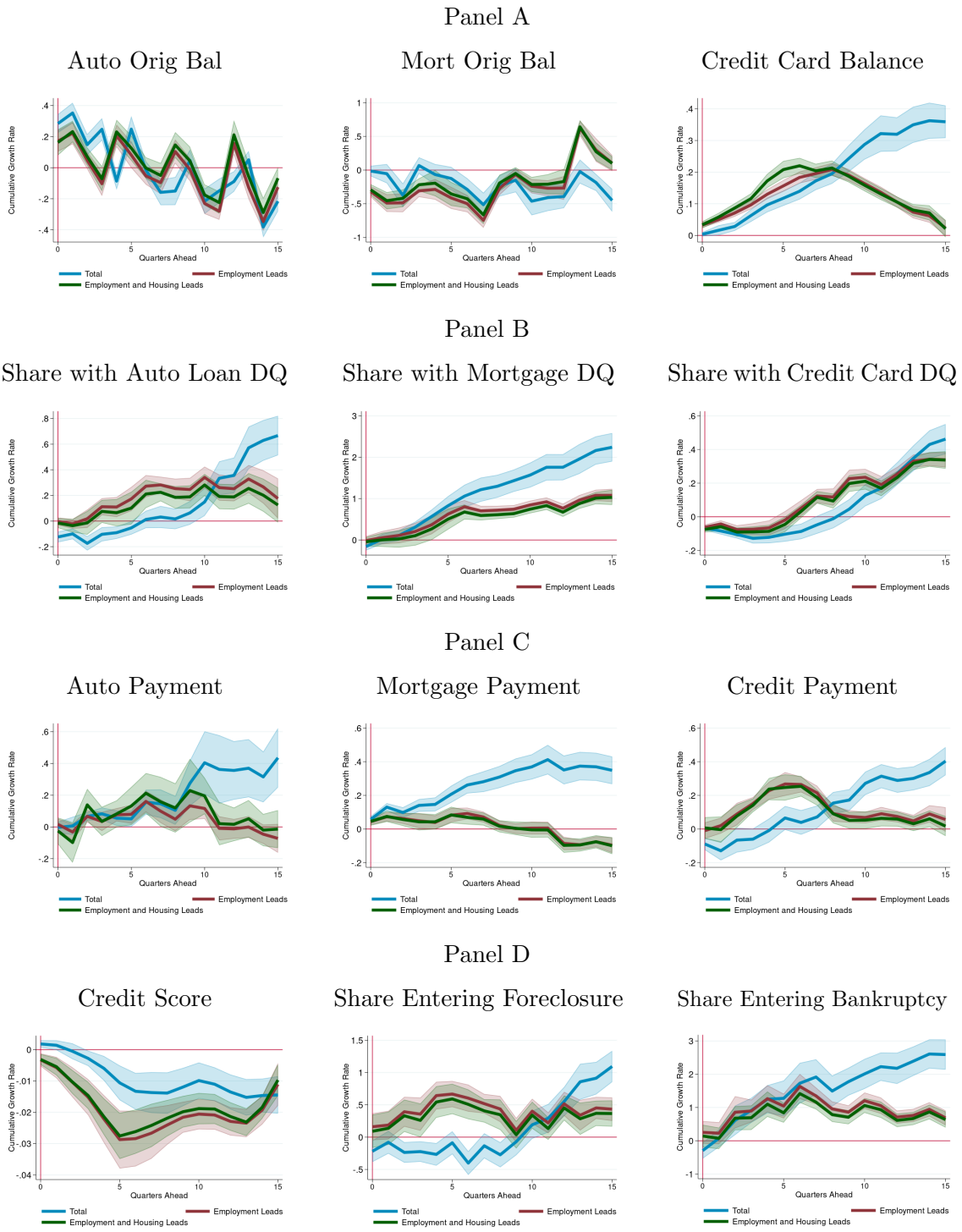

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (5). ZHVI refers to the Zillow Home Value Index, HPI refers to the House Price Index, and Equity is calculated at the county level as the product of the total dwelling units and the ZHVI, less the total mortgage debt. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from Zillow, CoreLogic, and the Consumer Credit Panel.

Figure B.4: Mechanisms: Borrowing Costs & Labor Market Effects (using Bartik Shocks to Instrument for Employment Rate) (NS Shocks)



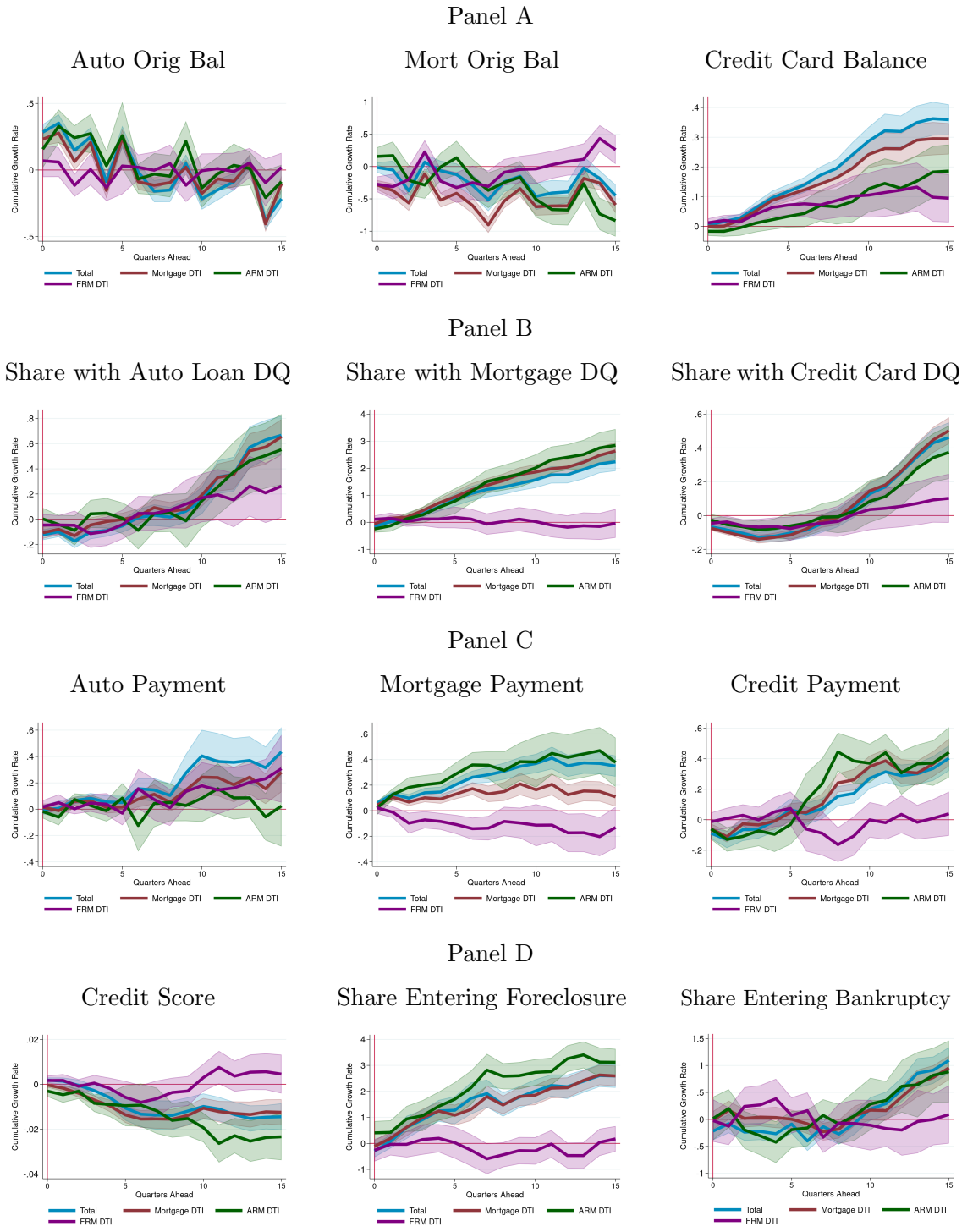

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2), controlling for labor market effects using a Bartik instrument to separate direct and indirect effects. The solid blue line indicates the baseline result with NS shocks (Figure 9), while the dashed blue line indicates the impulse response when using the Bartik instrument. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure B.5: Mechanisms: Borrowing Costs. Labor Market Effects & Housing Market Effects (Using Employment and Home Price Bartik Shocks to Instrument for Employment Rate and House Prices), NS Shocks



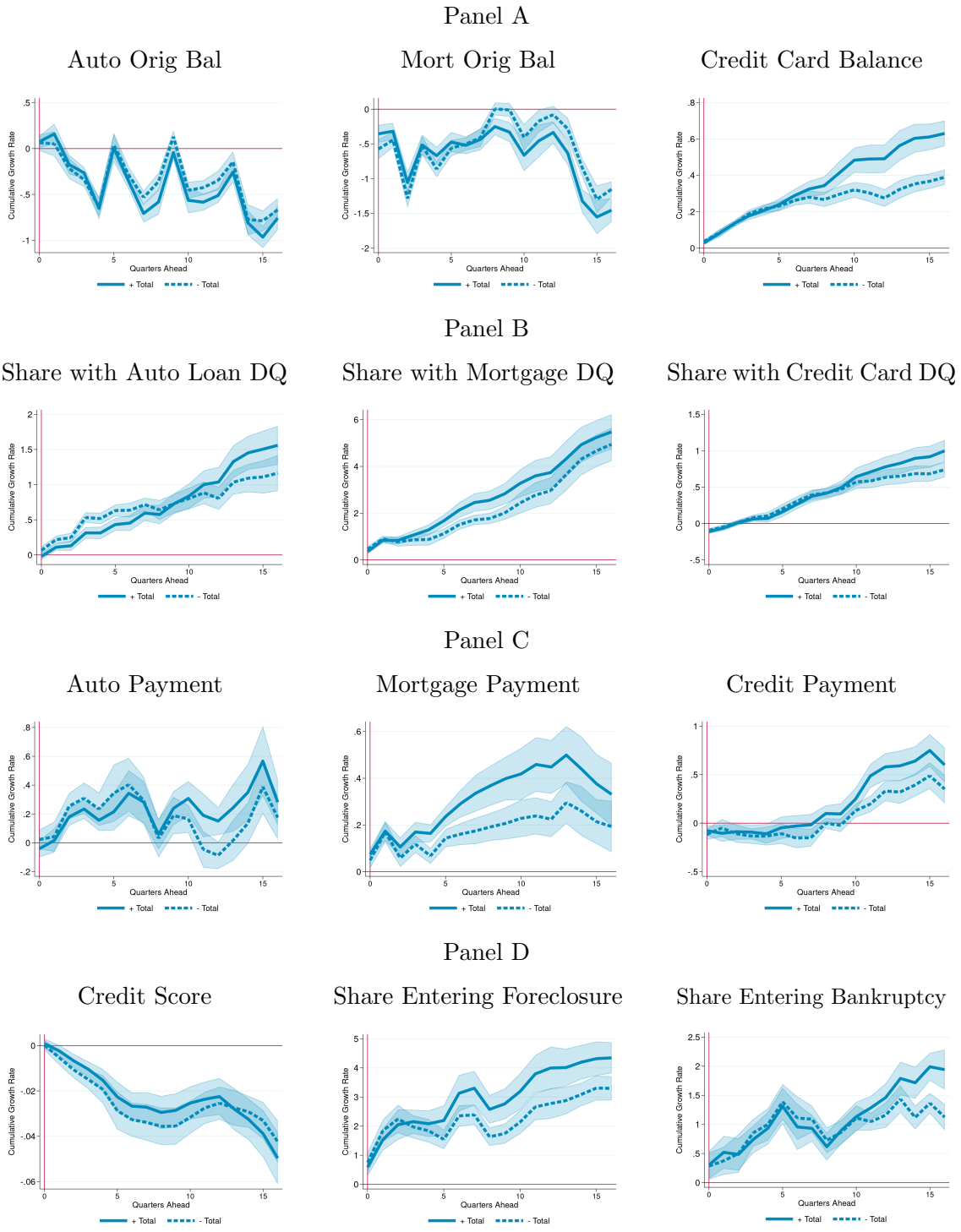

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). The blue line indicates the main baseline result with NS shocks (Figure 9), the red line indicates the impulse response when controlling for the labor market effect using the Bartik employment instrument (Figure B.4), and the green line indicates the impulse response when controlling for the labor market effects and housing market effects using the Bartik housing and Bartik employment instrument. 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.6: Mechanisms: Which DTI Drives Results?



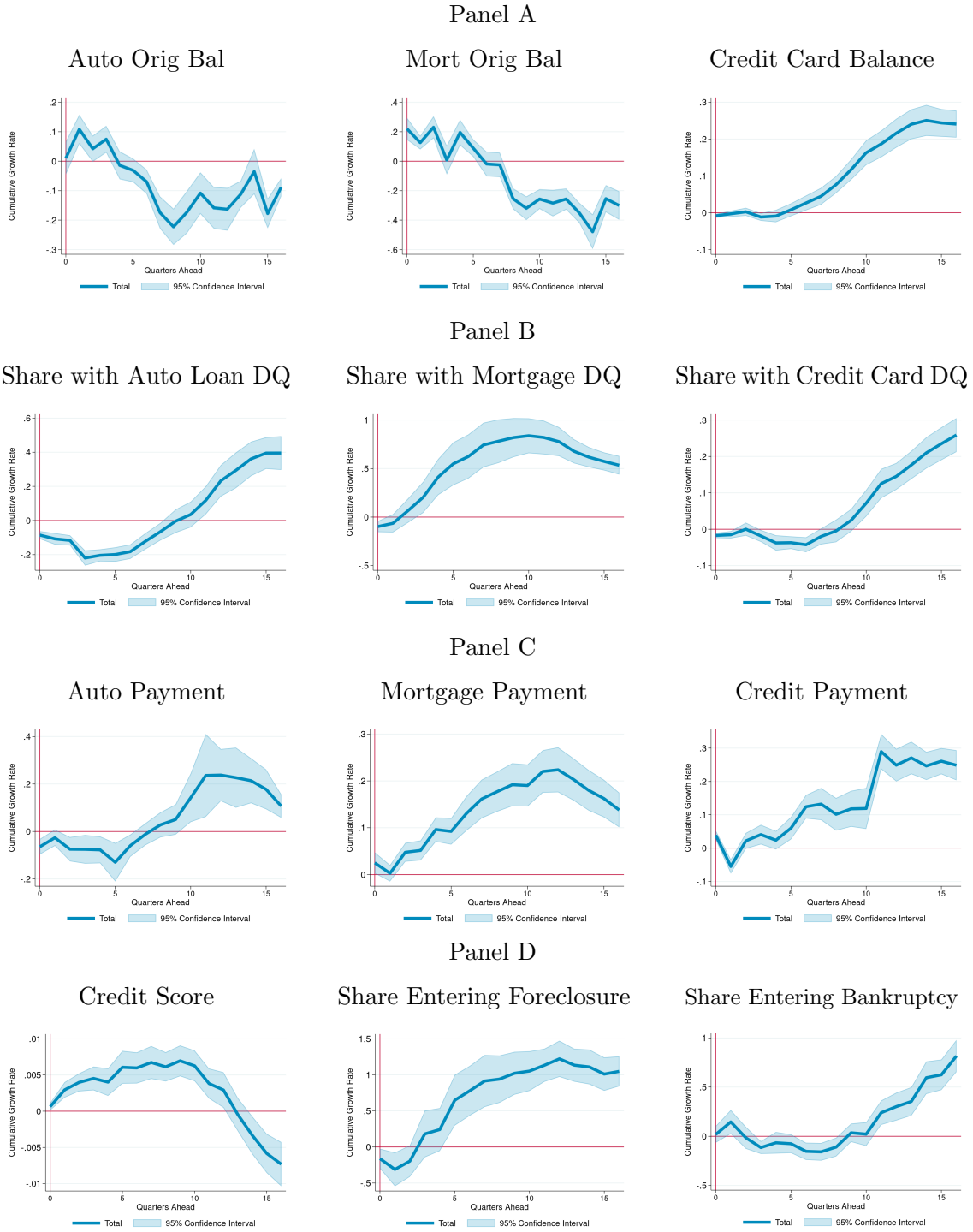

Note: Each line plots the impulse response functions of the growth of the dependent variable, using equation (4). The blue line plots the total (baseline results) effect, the red line plots the impulse response of FFR × Mortgage DTI, while also controlling for FFR × non-mortgage DTI, and the green line plots the impulse response of FFR × ARM DTI, while also controlling for FFR × non-ARM DTI. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure B.7: Effects of NS Shocks by Leverage: Asymmetric Effects of Positive and Negative Shocks



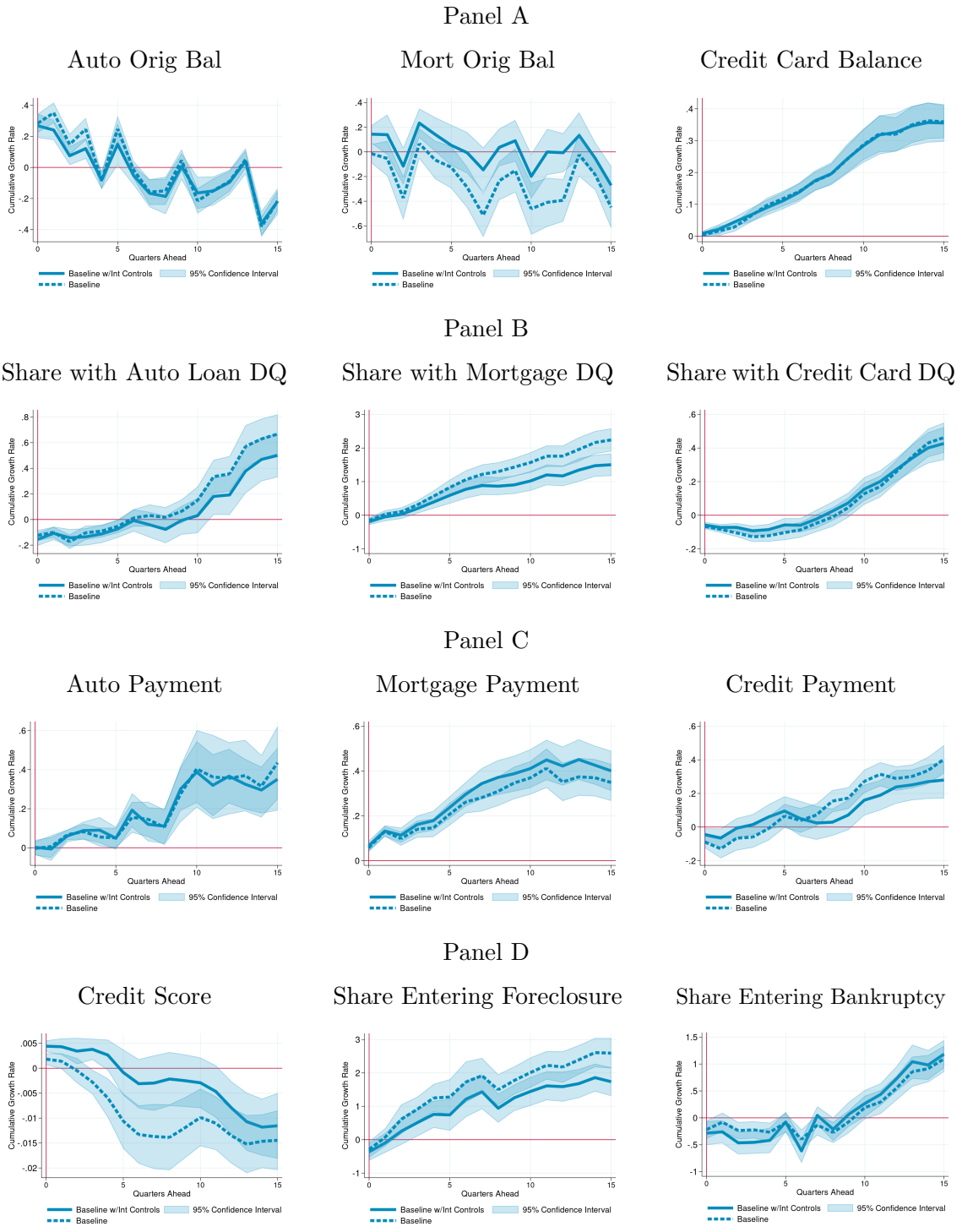

Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (5). Solid line indicates impulse response for positive NS shocks and dashed line for negative NS shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure B.8: Effects of NS Shocks by Leverage: Difference Between Positive and Negative Shocks




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (5). The solid line indicates the difference in impulse response for positive NS shocks and the negative NS shocks (Figure B.7) represented by $\tilde{\beta}_j^{(h)}$. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4.

Figure B.9: Effects of NS Shocks by Leverage: Household Financial Outcomes, Controlling for FFR-by-Demographic Interactions




Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Solid line includes $NS \times Demographics$ terms, dashed line is from Figure 9. 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. Demographic variables from 2000 Census.

# Appendix C: Supplementary Tables

Table C.1: Date Ranges of Outcome Datasets

| Dataset Name | Data Type | Date Range |
|---|---|---|
| CCP | Consumer Credit | 1999Q1-2023Q4 |
| HMDA | Mortgage Applications | 1999Q1-2022Q4 |
| QWI | Employment | 1999Q1-2022Q4 |
| Zillow | House Prices/Rents | 1999Q1-2023Q4 |
| CoreLogic | House Prices | 1991Q1-2023Q4 |

Table C.2: Demographic Summary Statistics

| **Variable** | **Mean** | **SD** | **25th** | **Median** | **75th** |
|---|---|---|---|---|---|
| Levels | | | | | |
| Med. HH Income | 46,065.525 | 14,083.468 | 36,398.000 | 43,524.000 | 52,842.517 |
| Prop. College Degree | 0.202 | 0.097 | 0.136 | 0.178 | 0.242 |
| Share White | 0.782 | 0.198 | 0.670 | 0.856 | 0.939 |
| Share under 25 | 0.323 | 0.055 | 0.294 | 0.320 | 0.349 |
| Share prime age | 0.380 | 0.049 | 0.356 | 0.381 | 0.407 |
| EPOP | 0.469 | 0.052 | 0.440 | 0.473 | 0.504 |
| Unemployment | 0.027 | 0.013 | 0.018 | 0.024 | 0.032 |
| ZHVI | 167,152.826 | 129,375.215 | 97,578.562 | 134,951.531 | 194,645.266 |
| Equity (Millions) | 10,830.681 | 51,114.360 | 685.997 | 1,752.799 | 5,610.517 |
| Urbanicity | 0.335 | 0.472 | 0.000 | 0.000 | 1.000 |
| Cumulative Growth | | | | | |
| Med. HH Income | 7.143 | 6.497 | 2.346 | 5.812 | 10.707 |
| Prop. College Degree | 5.078 | 7.930 | 0.721 | 3.874 | 8.657 |
| Share White | -1.214 | 1.505 | -1.687 | -0.730 | -0.262 |
| Share under 25 | -1.248 | 2.824 | -2.580 | -1.126 | -0.109 |
| Share 25-54 | -1.731 | 2.508 | -2.973 | -1.418 | -0.335 |
| EPOP | -0.172 | 4.359 | -2.495 | 0.270 | 2.476 |
| Unemployment | 1.841 | 35.781 | -23.570 | -7.169 | 18.134 |
| Equity | 13.719 | 18.737 | 1.480 | 9.833 | 22.975 |
| Urbanicity | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |