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. Among higher-share Black, low-income, and higher-share subprime counties, those that are more leveraged see an even greater decrease in the growth of auto and mortgage borrowing. Furthermore, we find that the effects of monetary policy are asymmetric – monetary contraction has larger (absolute) effects than monetary expansion. 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.

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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.

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, we use 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.

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.

We investigate the mechanisms underlying the effects we find. First, 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. Second, we inquire whether the effects of FFR changes on household financial health are mediated by local employment changes. We find that there are substantial direct effects of FFR changes that are not mediated by employment, but after a year to a year and a half, most of the effects of FFR changes are results of the generally negative emloyment response. Third, we investigate whether the stronger impacts in the high leverage areas are driven by the predominance of a certain kind of debt in those areas or whether the effects are symmetric across various kinds of debt. In other words, we investigate whether the transmission of monetary policy is more effective in the presence of a certain kind of debt.We find that monetary policy transmission is markedly stronger in the presence of ARM debt – geographies and time periods that have a preponderance of ARM debt are much more impacted by monetary policy.

Finally, we investigate the heterogeneities 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 in 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 fall more rapidly in response. In contrast, these effects are muted in low-income counties, likely because of their lower connection to formal financial borrowing mechanisms.

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, 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. Using loan-level McDash, we leverage data on mortgages refinancing as distinct from purchase mortgages as well as data on scheduled payments. McDash also helps us separately identify the type of mortgage: 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. Figure A.1 Panel A shows the distribution of DTI across the country while Figure A.1 Panel B shows the distribution of ARM DTI and FRM DTI across the country. We find that there is a 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. Figure A.1 Panel C presents the distribution of ARM mortgage share and FRM mortgage share. We immediately see a difference in their spatial distribution. ARM mortgage shares are highest in the Northeast, Florida, the Southwest, and the Pacific, whereas FRM DTI mortgage 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, likely because the areas with higher balances are typically higher income areas. 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. 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 investigate heterogeneity across different types of counties, we consider several categorizations of them. We refer to counties that fall in the top quartile of the weighted distribution of share of Black residents as higher-share Black counties. Examples of 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 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 data ranges of our various data are 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, we build on Bergman, Matsa and Weber (2022; hereafter BMW) and 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 $DTI_{c,t}$ 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, $J = 8$ (8 quarters or two years) and $H = 16$ (we 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 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 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 $\beta_1^{(h)} < 0$.

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 $DTI_{c,t}$ 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, 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.[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

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

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 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 following Holm, Paul and Tischbirek (2021) and controlling for leads of employment growth between period t and period $t+h$, measured by the standard Bartik interaction of county employment sectoral shares and national employment sectoral growth rates in order to avoid the potential endogeneity of employment in this regression[2]. The coefficients β_h in the below specification trace out the direct effect of FFR shocks

[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.

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

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

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

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:

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

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

4 Results

4.1 Baseline Results for Credit Outcomes with DTI as Exposure Measure

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 in counties with higher DTI, after an FFR increase, people reduce borrowing growth 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, 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. This means that after a 100 bps increase in the FFR, counties that have a 1 standard deviation higher DTI (about 40 percentage points) had credit card balances growing by 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 up to nearly four years after the FFR increase. The magnitude of this differential is also sizable, implying that a 1 standard deviation increase in DTI implies an increase in the growth rate in the share of delinquent borrowers for auto loans of almost a third of the average rate of growth. 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 about 1.5-2 percentage point increases 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 of each of these measures. The positive impulse response is intuitive as 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 be coming from mortgage holders paying off their mortgage, or becoming delinquent on the mortgage. The decline in the growth rate of mortgage balances around this time after the FFR shock in Panel A is consistent with the paying-off mechanism being important.

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, with the differential growth rate going to zero two to three years after the FFR shock. Credit score growth declines with leverage. Four years after the shock, the impulse response function converges to zero.

We further investigate mortgage-related outcomes in Figure 2. Using data from McDash, a loan-level database of mortgage servicing, we can more precisely see how FFR increases affect household finances for the largest loans that households normally take out. The first chart of Figure 2, Panel A shows the higher a county's DTI, the stronger FFR increases lower the growth rate of the share of mortgage loans that are refinances, which is expected as refinancing when rates increase involve increasing the monthly payment relative to what it would have been before the rate increase. Panel B shows the same is true for the share of mortgage dollars that are refinances.

We also can shed light on another dimension of financial health: the rate at which mortgage denials differentially change by county DTI after an FFR increase. To do so, we use data from

[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 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.

the confidential Home Mortgage Disclosure Act (HMDA) database on mortgage originations and denials. Panel C of Figure 2 presents the results. We see that the growth rate of the share of mortgage 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 would expect to see a 2% higher rise in the share of applications leading to denials, 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 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[5]. 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

[4]Symmetric dynamics happen with the share of mortgage applications ending in originations

[5]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.

the announcement, and if asset markets are reasonably efficient, neither set of shocks should be correlated with macroeconomic variables known previously[6].

Figure 3 replicates Figure 1 (our baseline results for credit outcomes) using the running sum of NS shocks in place of FFR in specification (1)[7]. 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 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 balances, all types of delinquencies, new foreclosures and bankruptcies. The magnitudes of the impulse responses at their peak are somewhat (e.g. 1-1.5 times) larger than the magnitudes of our baseline estimates. Figure 4 presents the same results for the mortgage outcomes in Figure 2. The trends are again very similar, with the impacts on the refinance share being much more clearly negative and with the magnitudes being somewhat larger[8].

4.3 Robustness Checks

Besides using the monetary surprise measures, we perform additional robustness checks in the Appendix. Figure A.8 replicates Figure 1 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 similar to the baseline. Figure A.9 replicates Figure 1 including a lagged dependent variable on the right hand-side of equation (1), following Coibion et al. 2017. Figure A.10 shows results for the summary household debt results replacing FFR 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 results for different sub-periods of our sample. It is clear that our results

[6]We have also considered shocks from Jarocinski and Karadi 2019 and provide a version of our baseline estimates using them in Appendix Figure A.7A. Our conclusions are qualitatively unchanged.

[7]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 (and more generally, $DTI_{c,t}$ and its lags) as covariates in equation (1)

[8]We also investigate the robustness of our results to using shocks from Jarocinski and Karadi 2019 in Appendix Tables A.7A and A.7B, with analogous 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 documents that our results are not driven by the Covid-19 pandemic.

4.4 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 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 (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.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 that of an expansionary policy (Appendix Figure B.1).

4.5 Direct and Indirect Effects of FFR Changes

It is interesting and important to 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 differentially declining 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 decrease employment growth, which may be a channel through which they affect financial health[9] . 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 employment growth 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 employment 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.). If the Bartik instruments are valid instruments for 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 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 tamp down, 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 (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, peaking at about six quarters after the FFR shock. Appendix Figure B.2 presents the results when shocks from Nakamura and Steinsson 2018 are used in place of the FFR. The qualitative and quantitative pattern

[9] Appendix Figure B.2 finds a similar effect on employment when FFR shocks are replaced with monetary surprise shocks from Nakamura and Steinsson 2018.

of the direct and indirect effects is the same, with all effects being slightly larger in magnitude. Our finding of the direct effect of interest rate increases being prominent early on, but then slowly giving way to indirect effects operating through the labor market is consistent with similar findings by Holm, Paul and Tischbirek (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. It is interesting 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. Redefining the DTI variable in our baseline equation (1) to reflect the ratios of different types of debt to income, we do not find heterogeneous effects of interest rate changes by the ratio of credit card or auto loan debt to income, but we do find it by the ratio of either fixed rate mortgage (FRM) or adjustable-rate mortgage (ARM) debt to income, 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 to income or places with high ratios of ARM 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 to income ratios experience impulse responses of FFR shocks relative to counties with smaller ARM to income ratios in ways that are qualitatively similar to our baseline estimates. In contrast, holding ARM to income ratios fixed, counties with greater FRM to income ratios do not experience particularly strong or consistent impulse responses to FFR shocks relative to counties with smaller FRM ratios. Our results are consistent with ARM debt being an important driver of the responses to FFR shocks. This is consistent with the fact that higher DTI areas have higher ARM shares (Figure A.3), further supporting the fact 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 during two subsamples: 1999-2008 and 2015-2023. The ARM share was considerably higher during the earlier time period than later, as seen in Figures A.4 and A.5. We exclude 2009-2015 because of two factors (a) interest rate was close to zero lower bound and hence had very little variation (b) dropping this period gives us two periods that are discretely 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 above mechanism - 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 impacted by monetary policy.

A natural question that arises is why areas with higher presence of ARM debt experience stronger pass-through of monetary policy while that is not the case in areas with higher presence of credit card debt. While both are types of floating debt, size of credit card debt is considerably smaller than mortgage debt for households, indicating that rate changes result in meaningfully larger interest charges for mortgage debt, leading to stronger transmission through ARM debt. Second, credit card interest rate is markedly higher than mortgage rate. Hence a certain change in interest rate is much more meaningful for mortgage rates than 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 more of a barrier to the origination of new auto, mortgage and credit card debt to a greater extent 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 overall. 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 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, foreclosure and bankruptcy to FFR changes as a function of county leverage in panels C and D are similar for higher-share Black counties to the baseline.

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 decline in growth in balances with

an FFR increase, but growth in delinquencies remains similar to the baseline.[10]

5 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 the effects of monetary contraction on the inequalities are greater than, rather than symmetric to, the effects of monetary expansion, and that after about six quarters, these effects are mediated by the response of employment. Exploring mechanisms, we find that the transmission of monetary policy is much more effective in time periods and geographies that are characterized by higher ARM shares. Transmission is much less effective in the presence of 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 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.

[10] Appendix Figure A.16 shows heterogeneity results for mortgage outcomes. We note in particular that the impulse response for growth in refinances is less negative for higher-share Black counties than it is for counties overall in the first year, although the difference is not significant. This is consistent with Black mortgage holders facing greater frictions in refinancing when it is advantageous for them to do so in low interest rate environments (Gerardi, Willen and Zhang 2023). Furthermore, low-income, higher-share Black counties and higher-share subprime counties 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.17. The impulse responses for low-income, higher-share Black and higher-share subprime counties are generally negative but occasionally statistically insignificant from zero.

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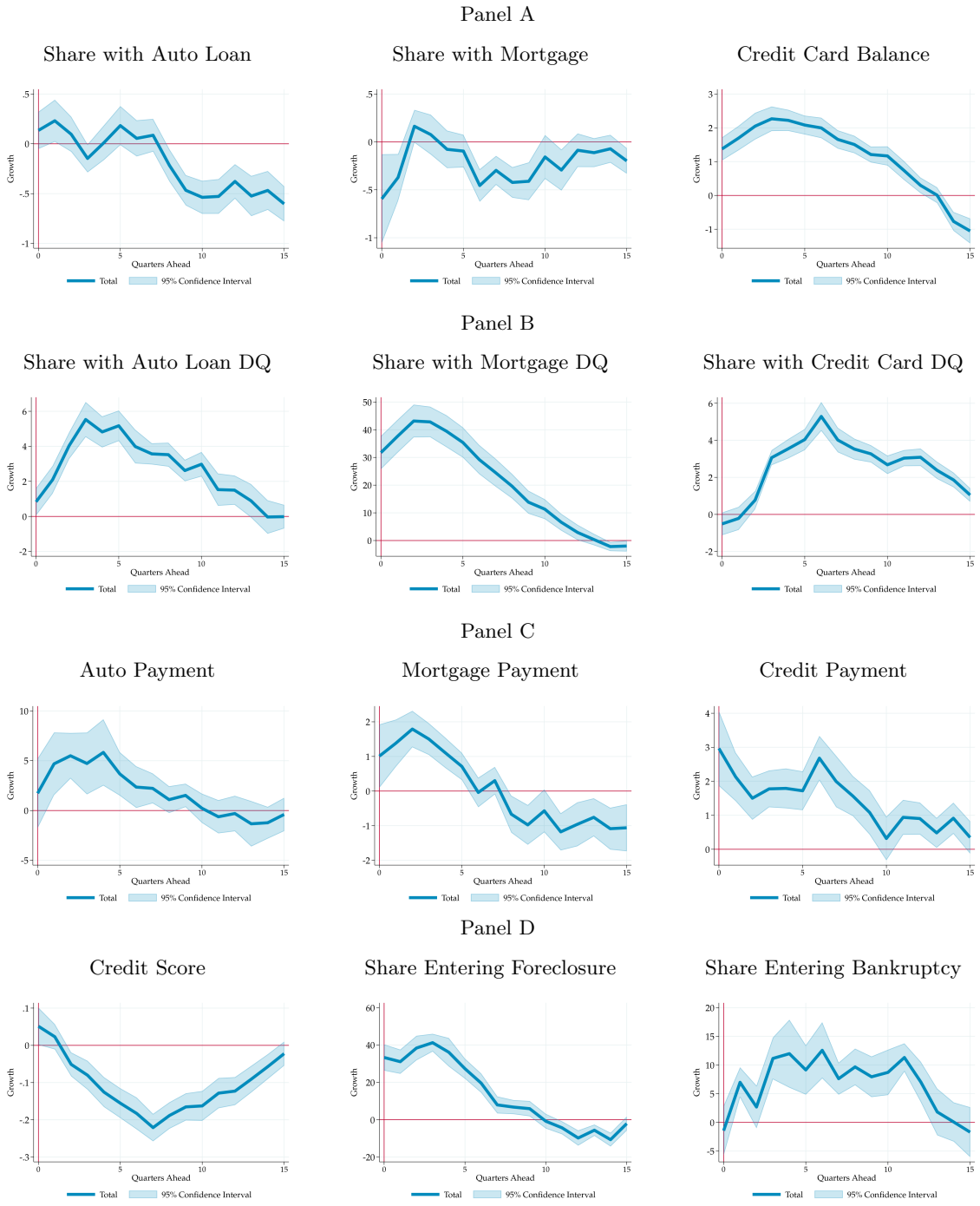
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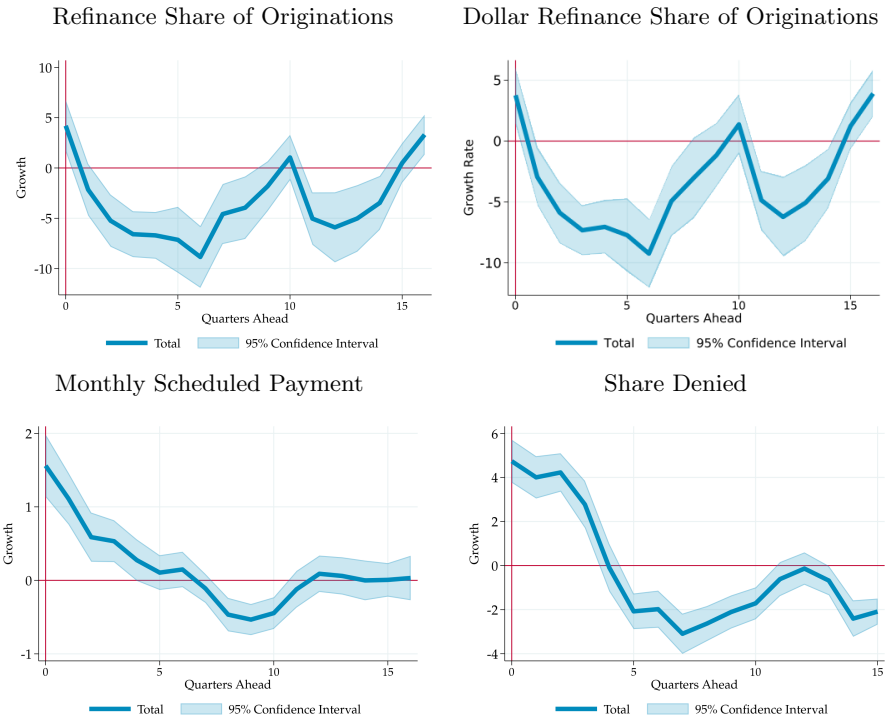
Figures

Figure 1: Household Debt Outcomes



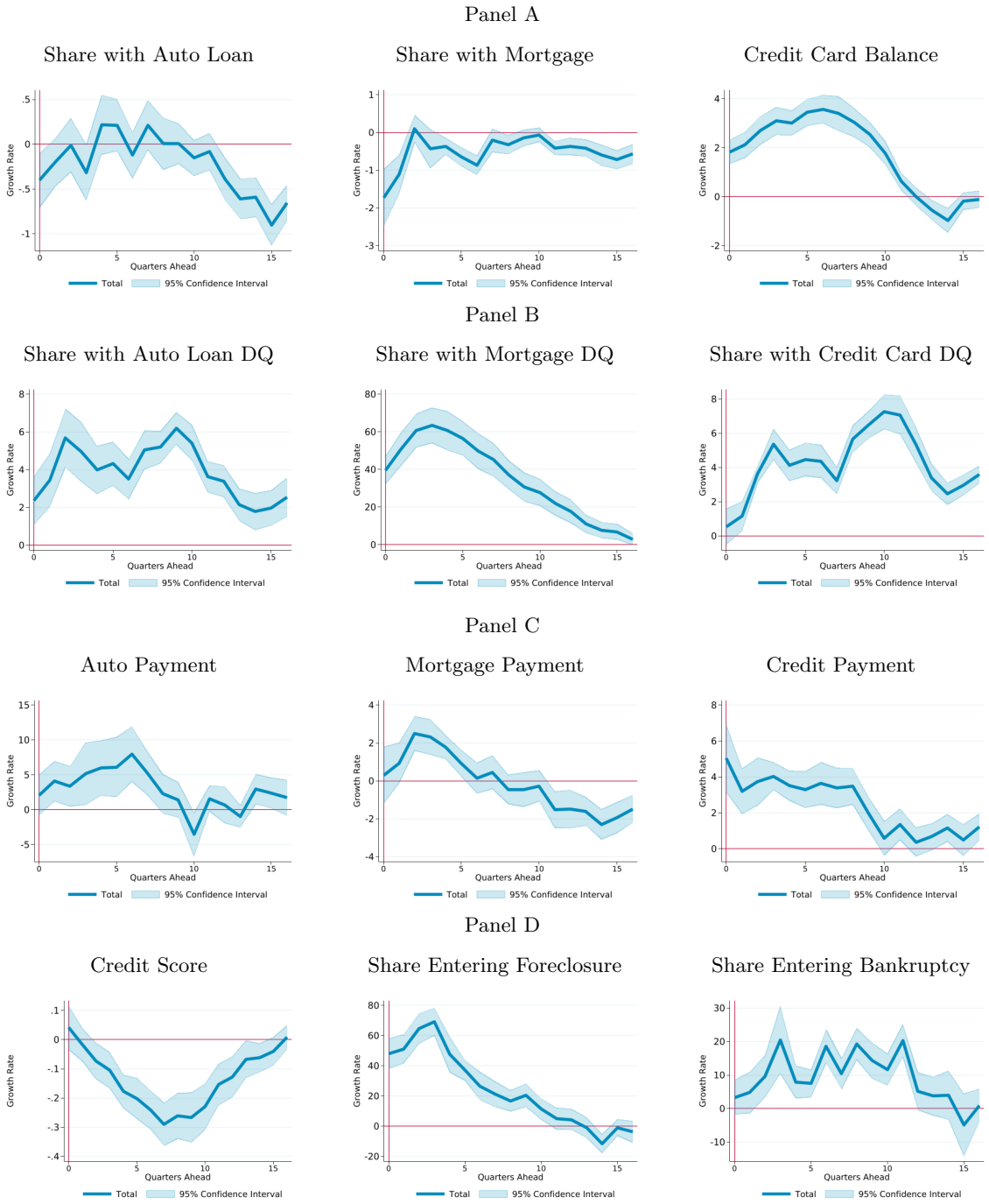
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure 2: Mortgage Outcomes



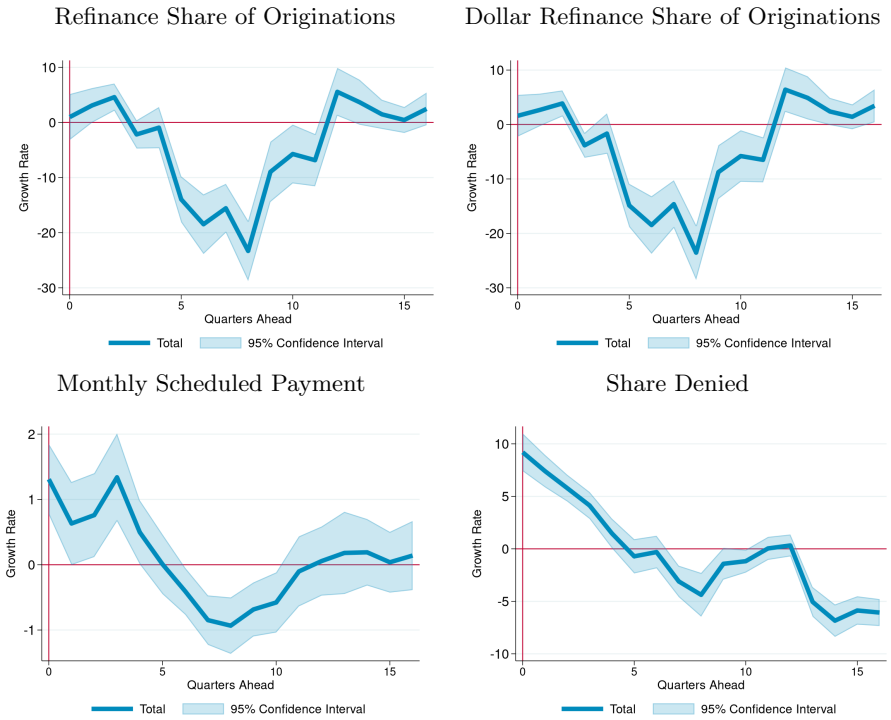
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on refinances and payment from Black Knight McDash for range 2005Q1 – 2023Q3. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2022Q4.

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



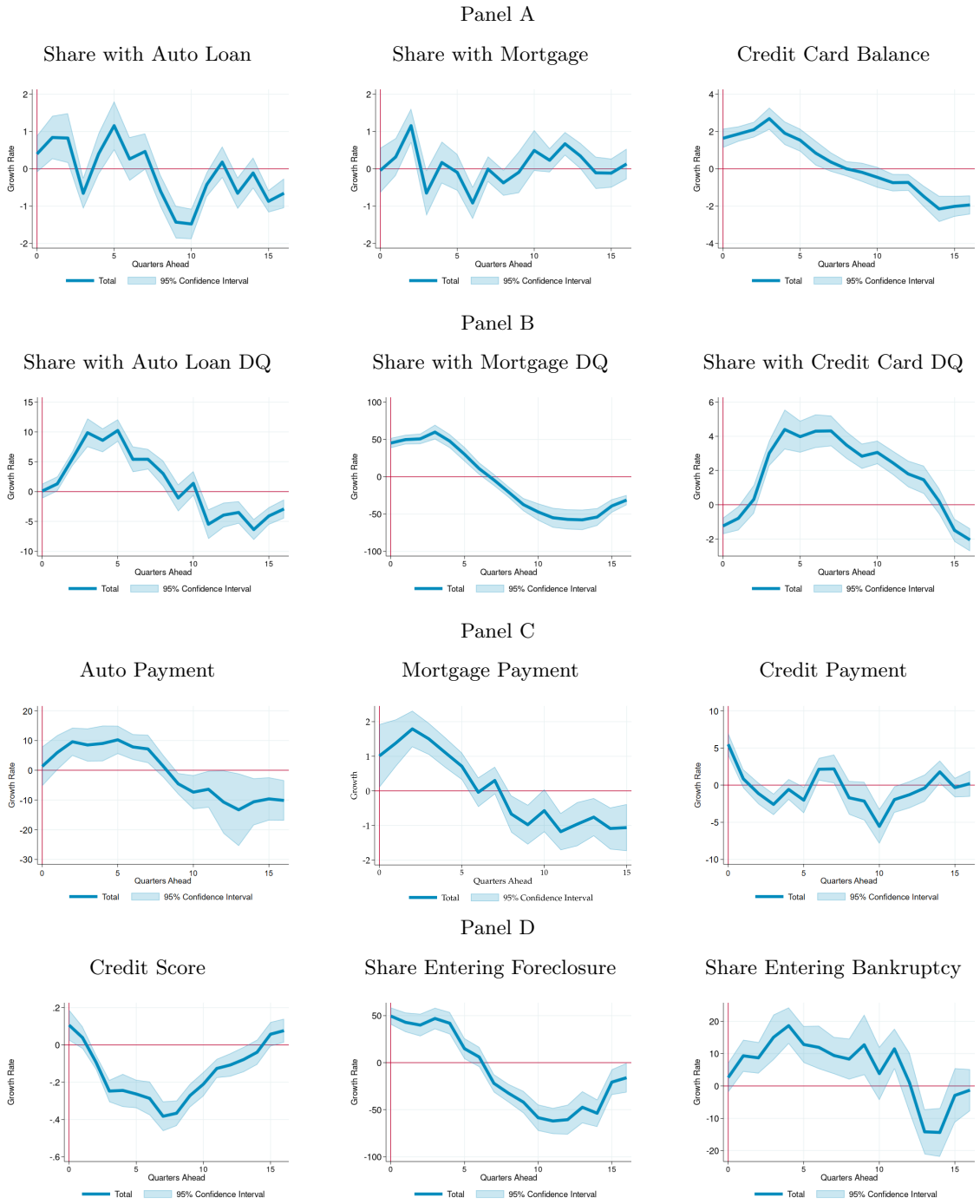
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2022Q3. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

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



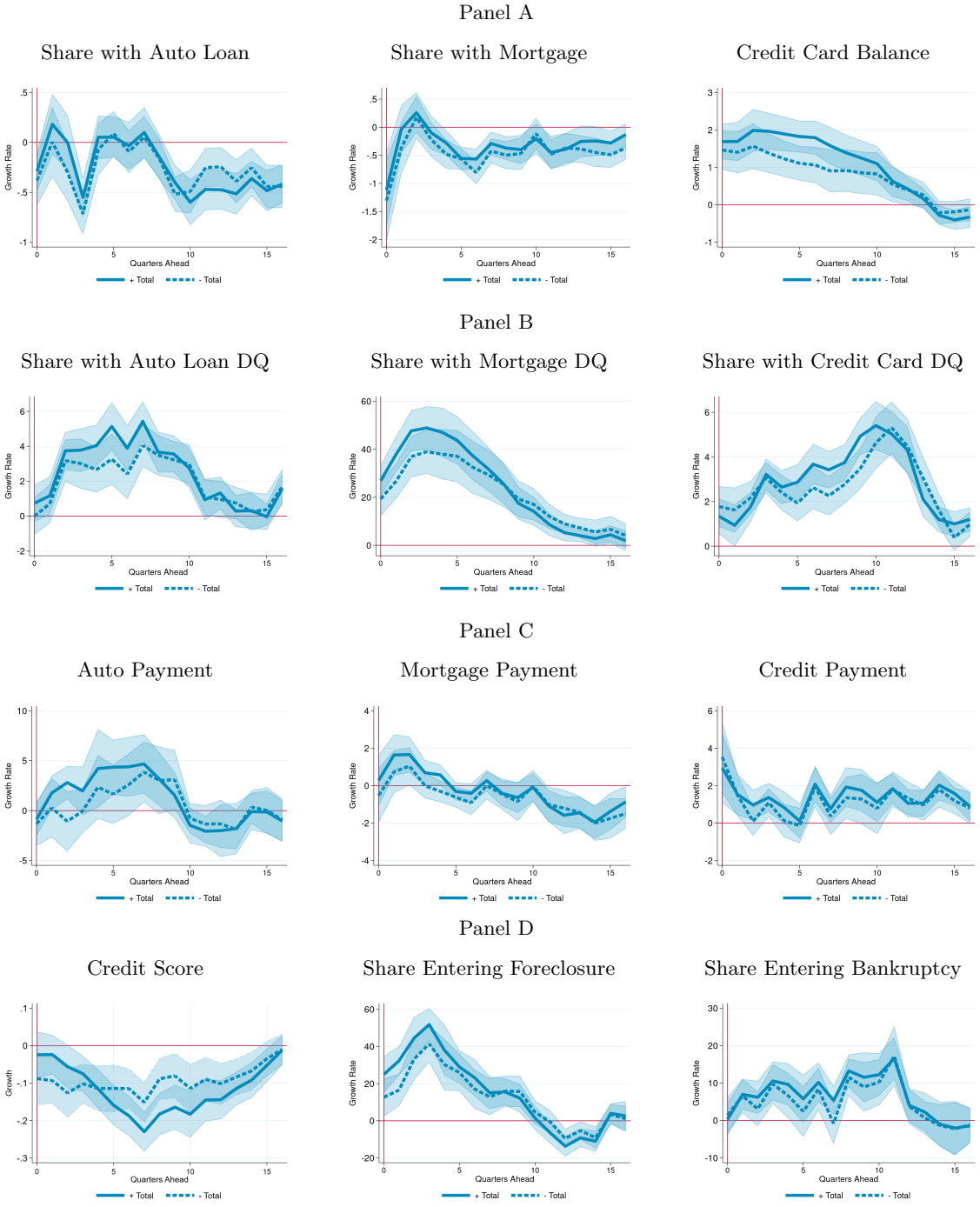
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on refinances and payment from Black Knight McDash for range 2005Q1 – 2023Q3. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2022Q4.

Figure 5: Household Debt Outcomes, 1999-2008



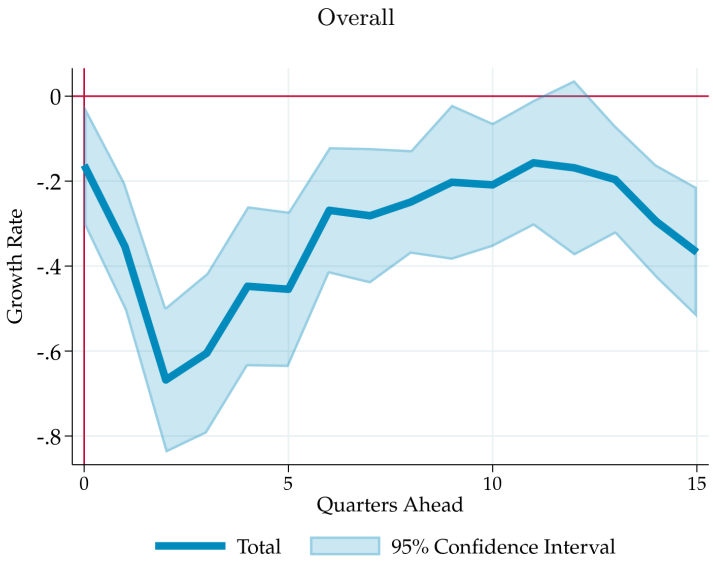
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2008q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

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



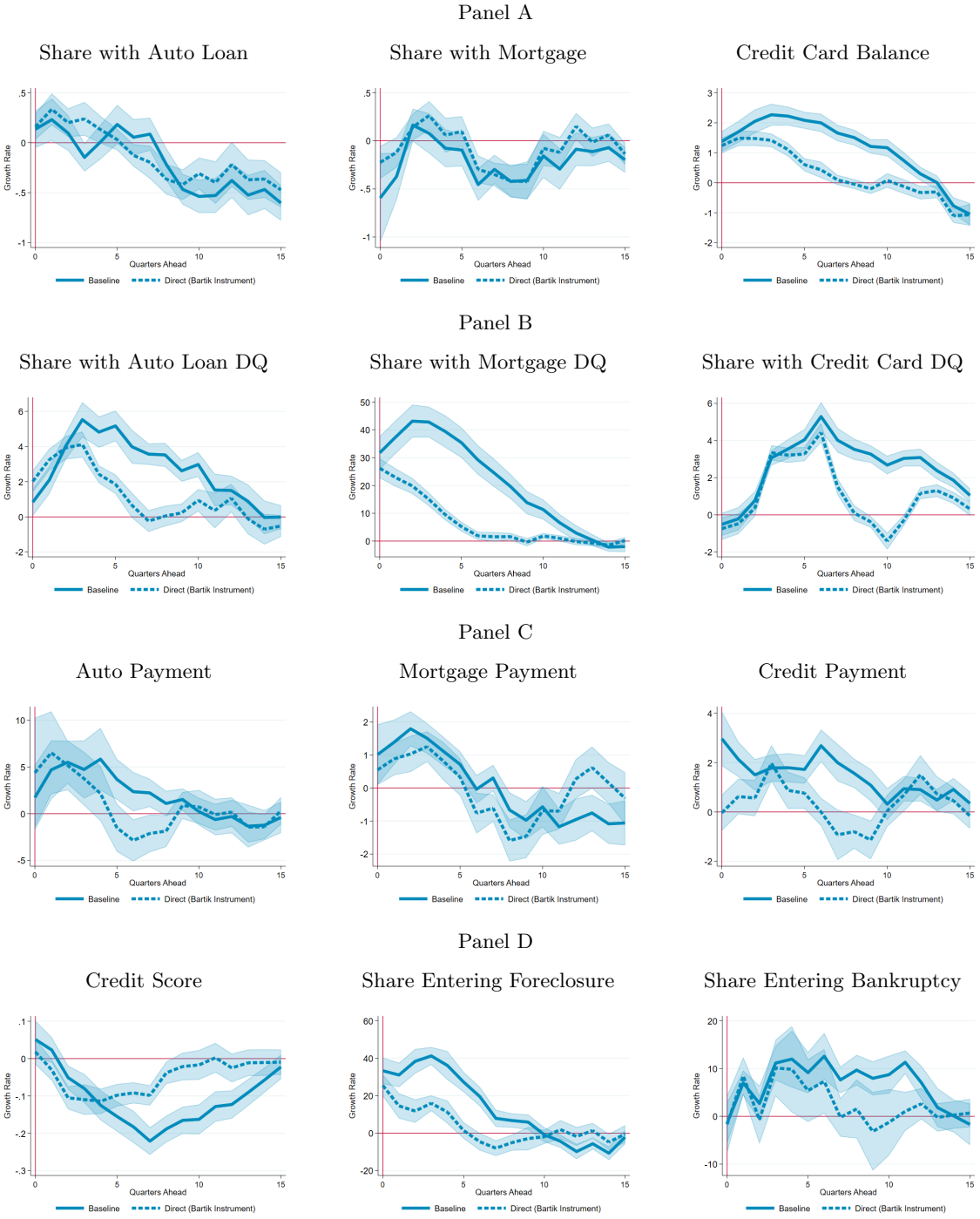
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates impulse response for positive FFR shocks and dashed line for negative FFR shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county.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

Figure 7A: Total Employment, DTI as Exposure Measure



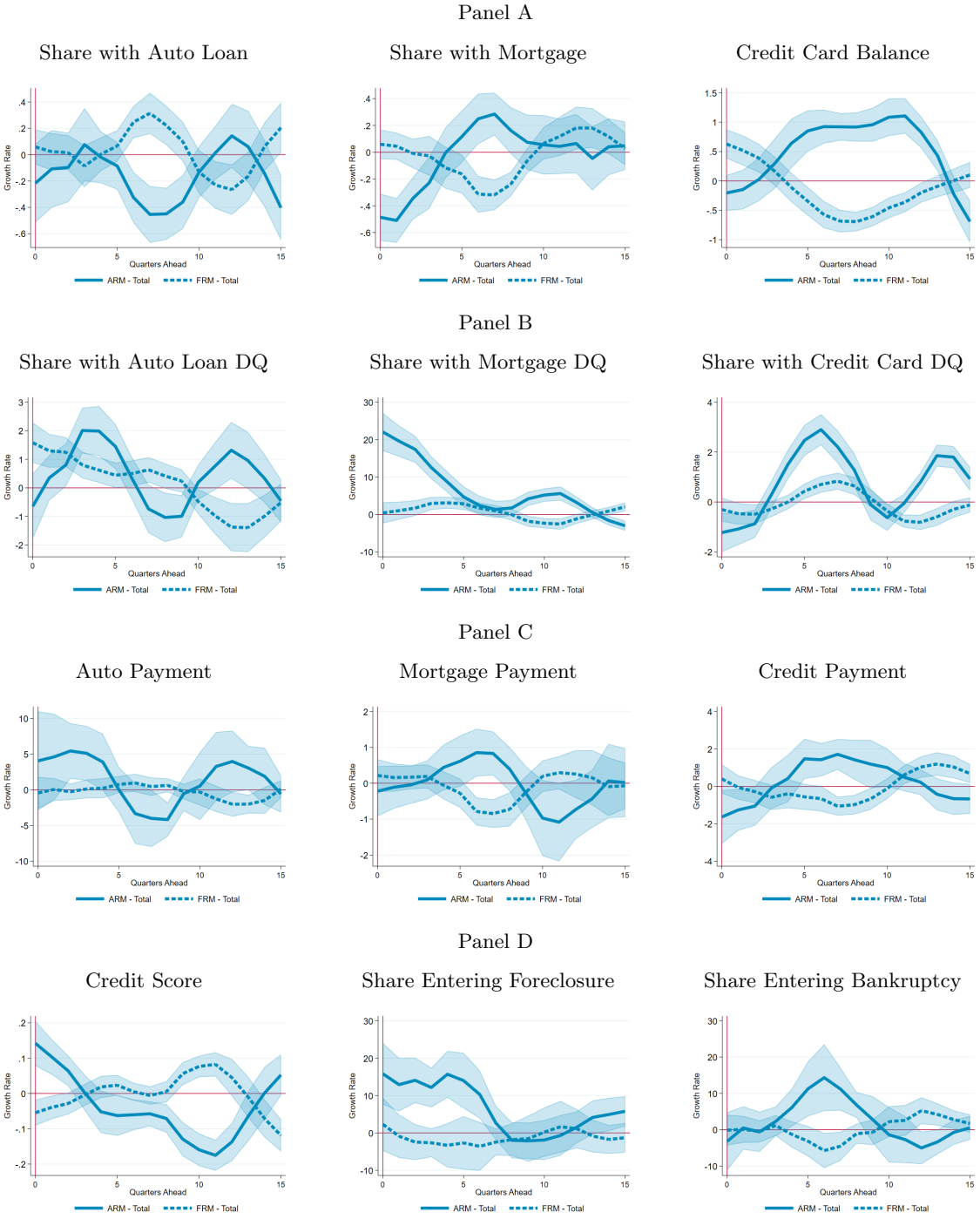
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the U.S. Census Bureau Quarterly Workforce Indicators.

Figure 7B: Household Debt Outcomes (Total vs. Direct Effects using Bartik Instrument)



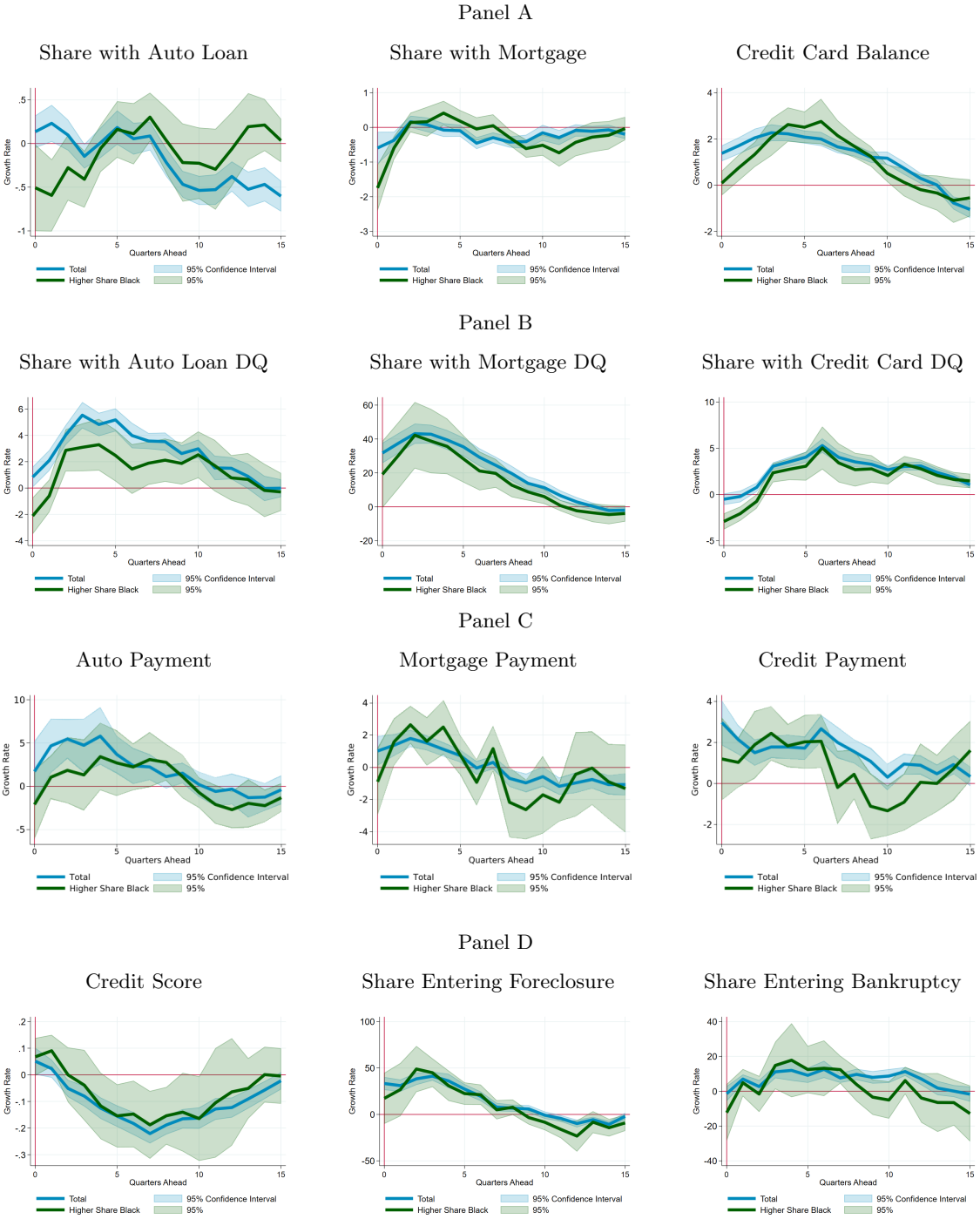
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (3), which uses a Bartik instrument to isolate the direct effect. The solid blue line indicates the total impulse response, while the dashed blue line indicates the impulse response when using the Bartik instrument. 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



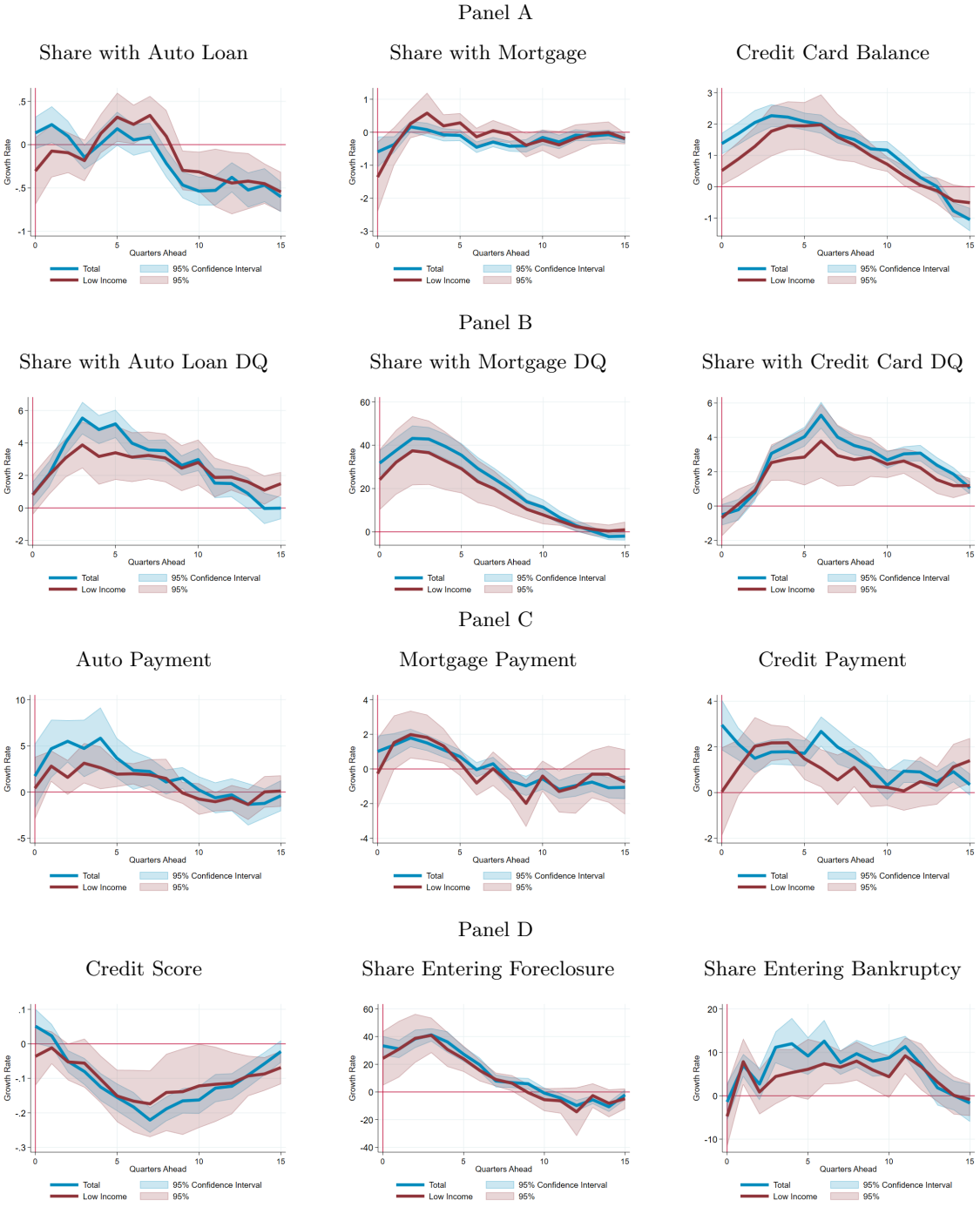
Note: Each line plots the three-quarter smoothed impulse response functions of the growth of the dependent variable, using equation (5). Solid blue line indicates impulse response for ARM DTI, and dotted blue line indicates impulse response for FRM DTI. Shaded regions indicate 95% confidence intervals. 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

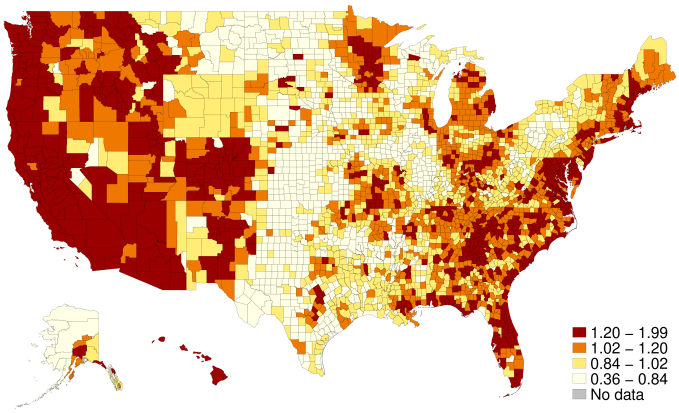


Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. High subprime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

Appendix A: Outcomes and Robustness

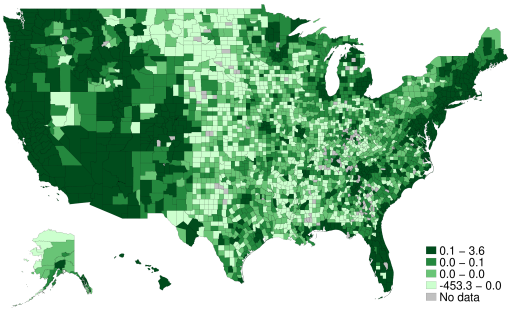
Figure A.1: Maps of DTI and Mortgage Types

Panel A: DTI (Total Debt)

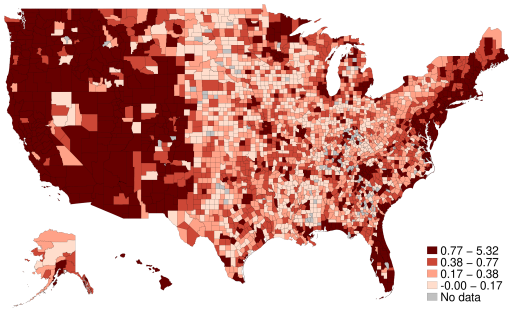


Panel B: DTI

ARM DTI

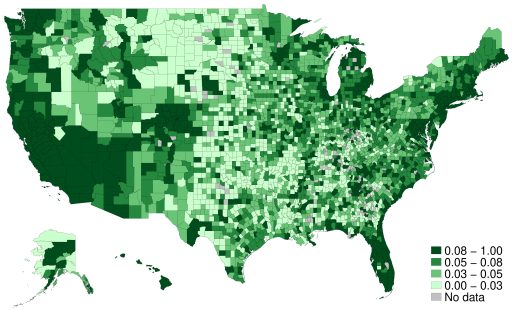


FRM DTI



Panel C: % Share of Mortgage Debt

ARM Mortgage Share



FRM Mortgage Share

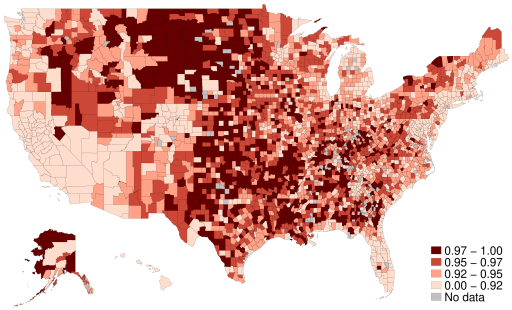


Figure A.2: ARM DTI vs. FRM DTI balance.[11]

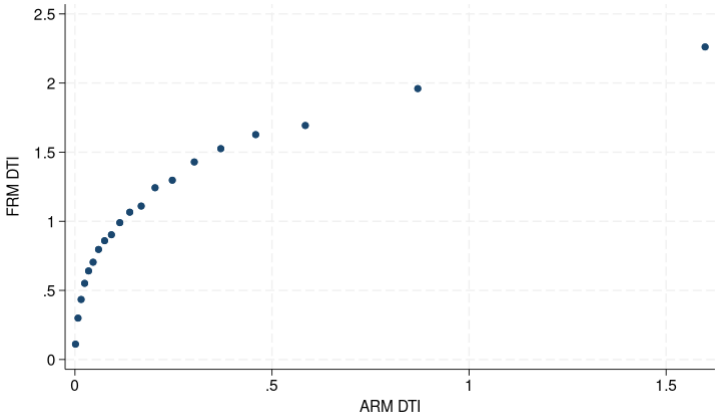
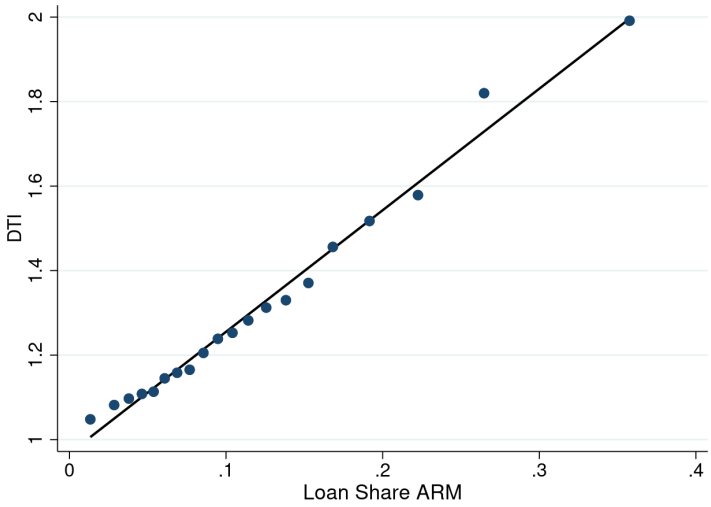


Figure A.3: DTI vs. Loan Share ARM



[11] Correlation coefficient 0.68; regression coefficient 0.64.

Figure A.4: National-Level ARM Share

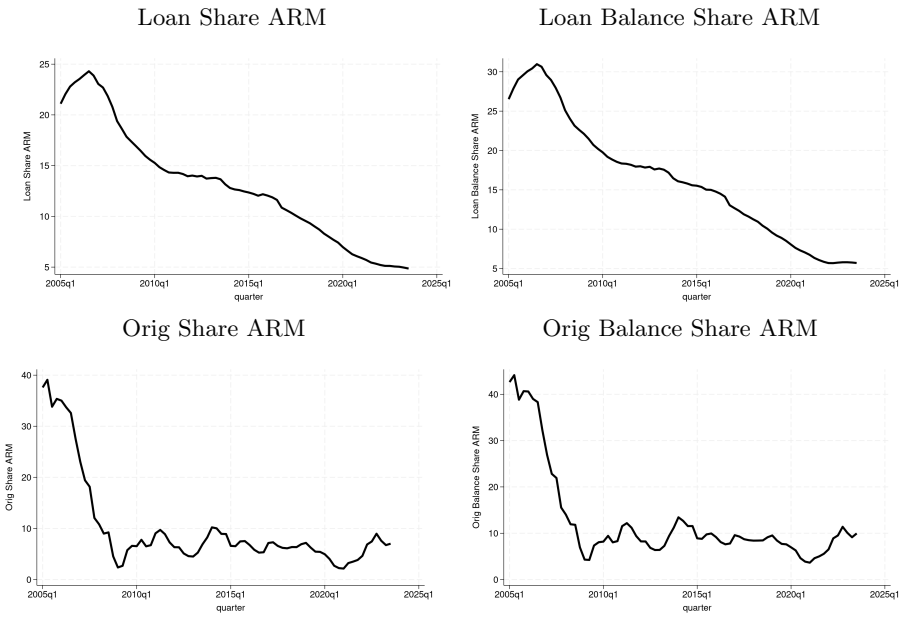


Figure A.5: County-Level ARM Share, weighted by number of borrowers

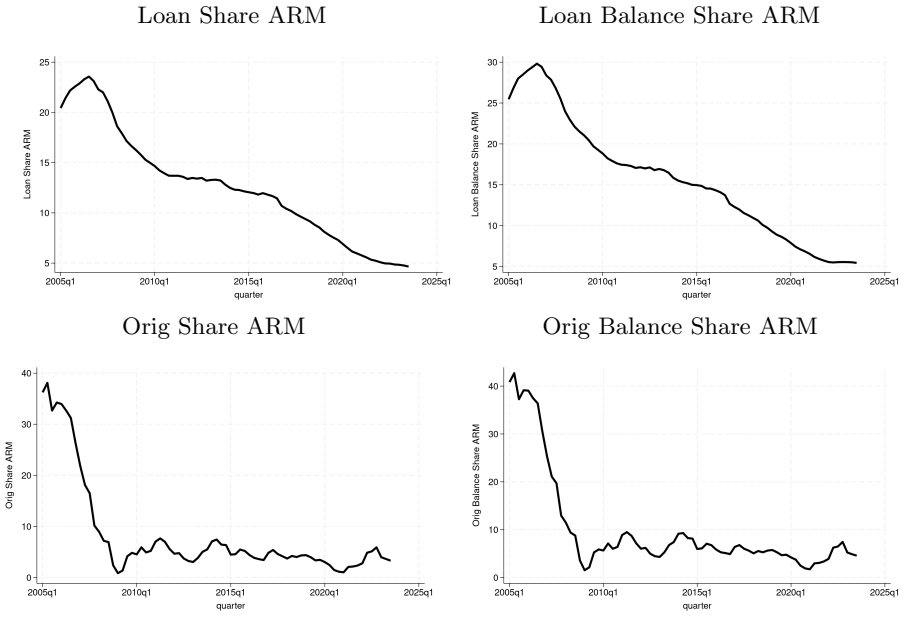
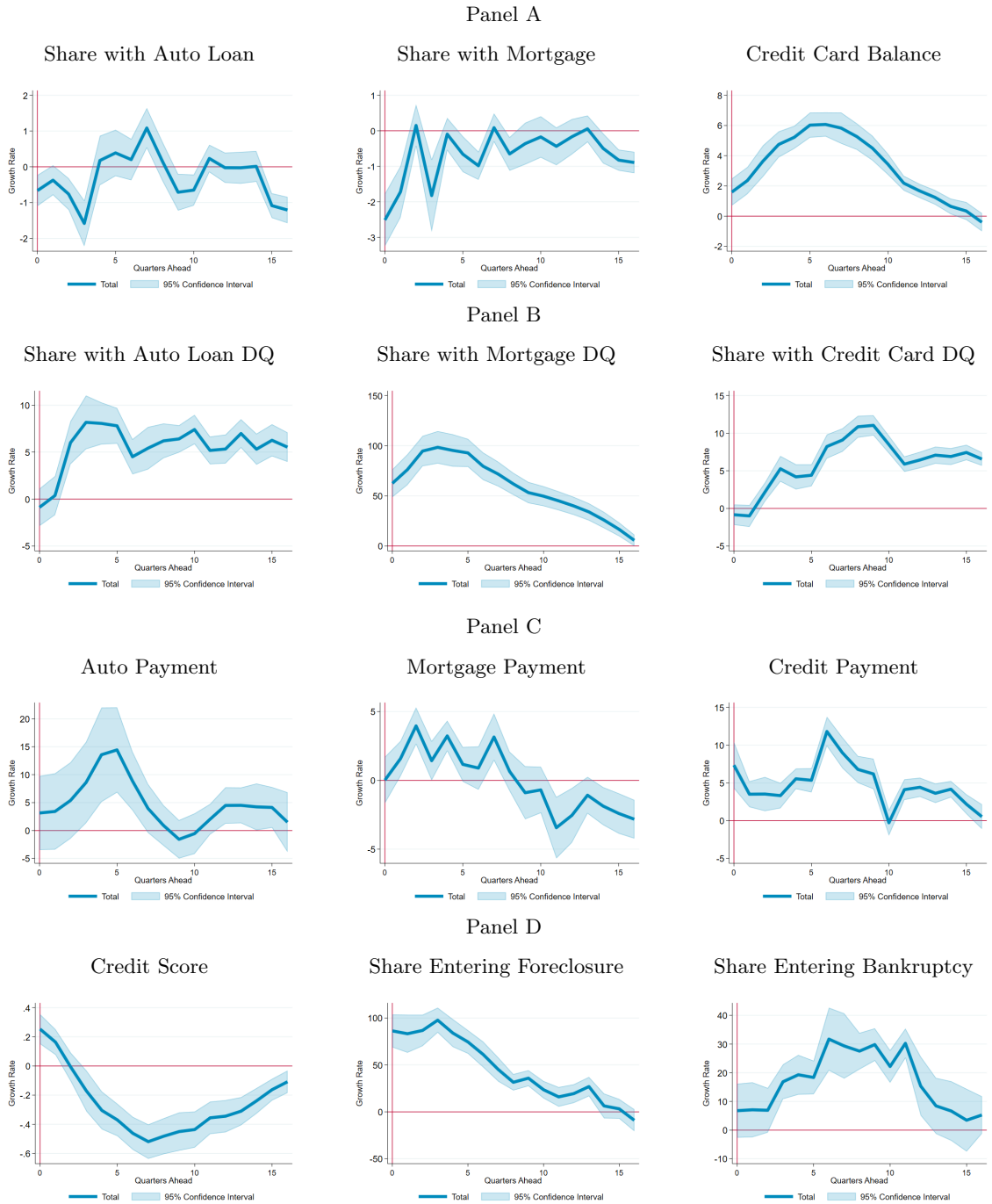


Figure A.6: Household Debt Outcomes, Dollar Share of Delinquencies and Balances



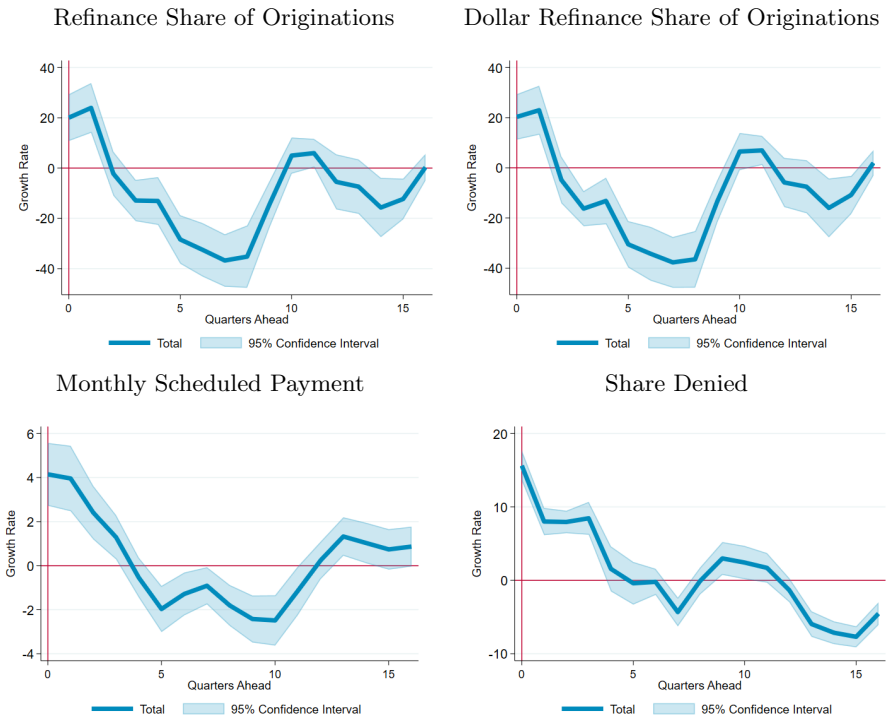
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.7A: Household Debt Outcomes, Monetary Policy Shocks from Jarocinski and Karadi (2019)



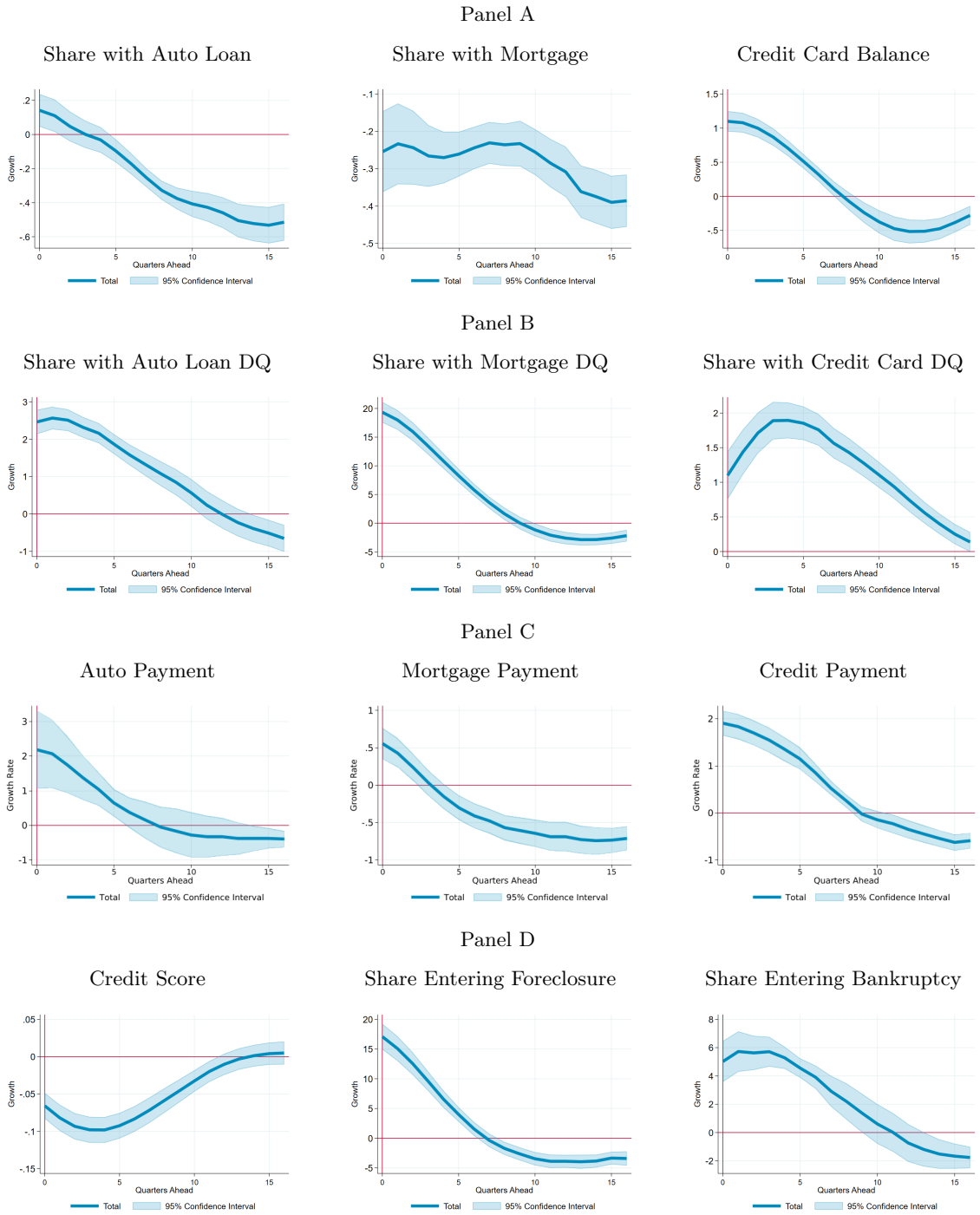
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2023Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

Figure A.7B: Mortgage Outcomes, Using Monetary Policy Shocks from Jarocinski and Karadi (2019)



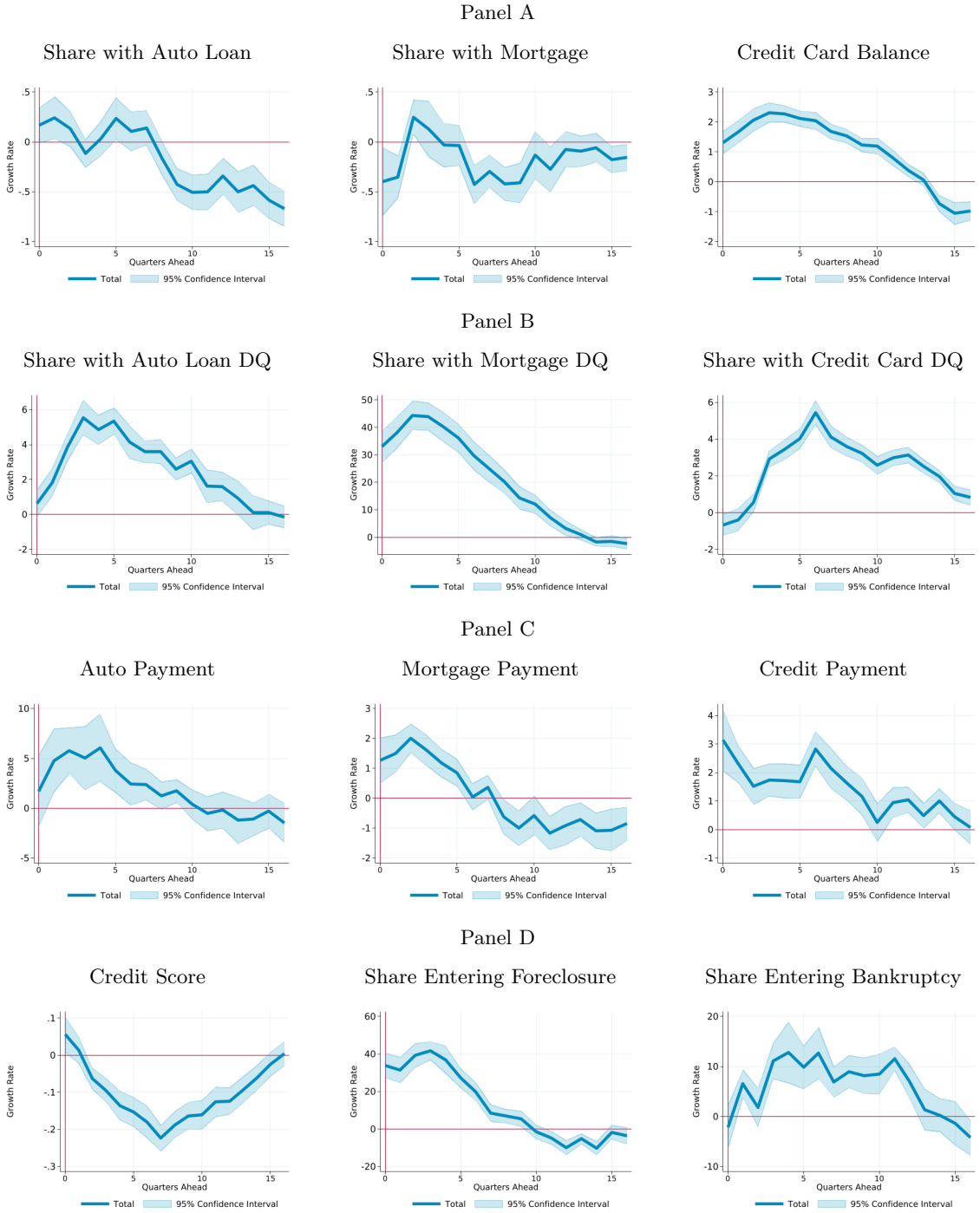
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on refinances and payment from Black Knight McDash for range 2005Q1 – 2023Q3. Data on mortgage application denials from confidential Home Mortgage Disclosure Act data for range 1999Q1 – 2022Q4.

Figure A.8: Household Debt Outcomes - One Lag



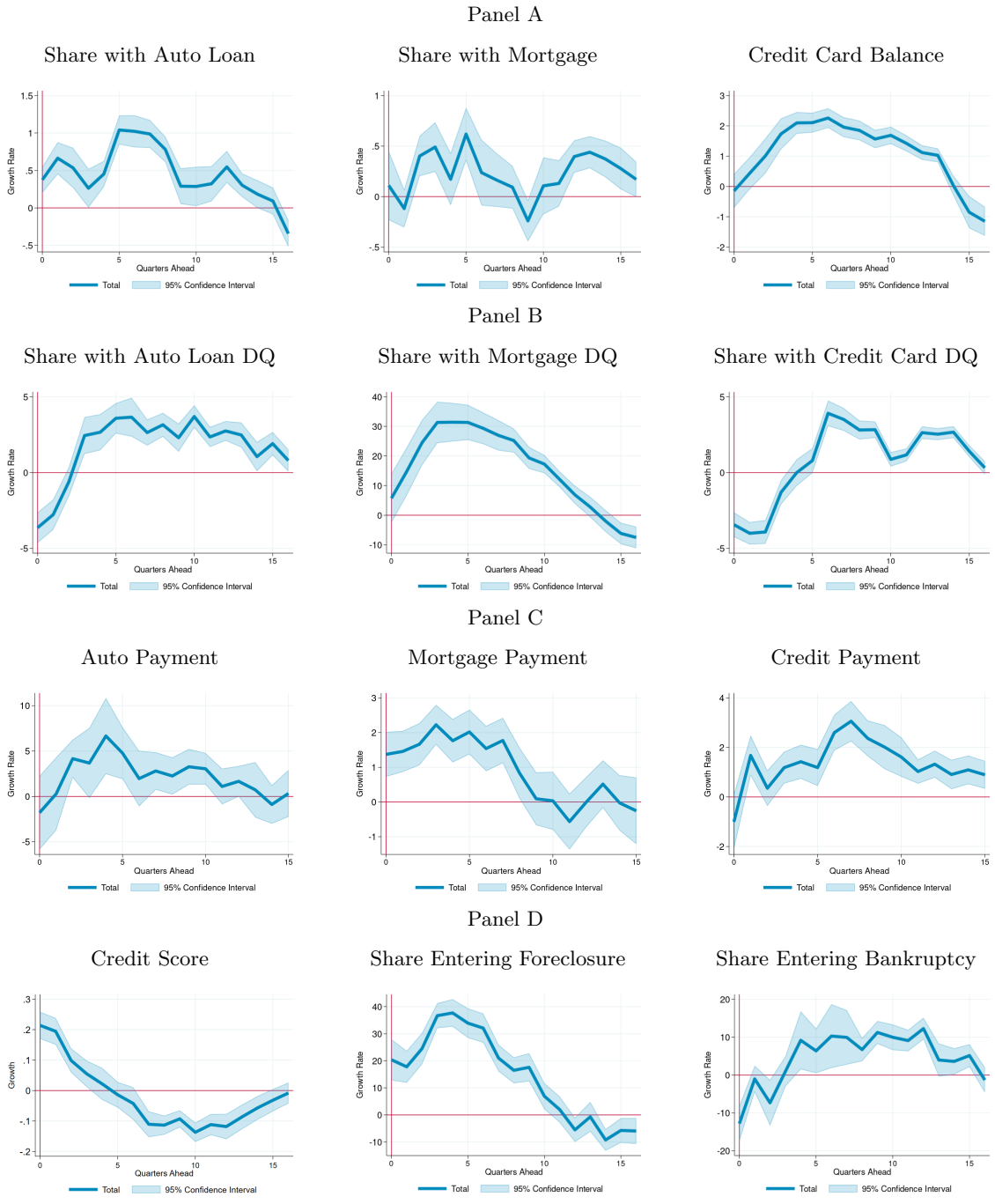
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), replacing $J = 8$ with $J = 1$. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax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

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



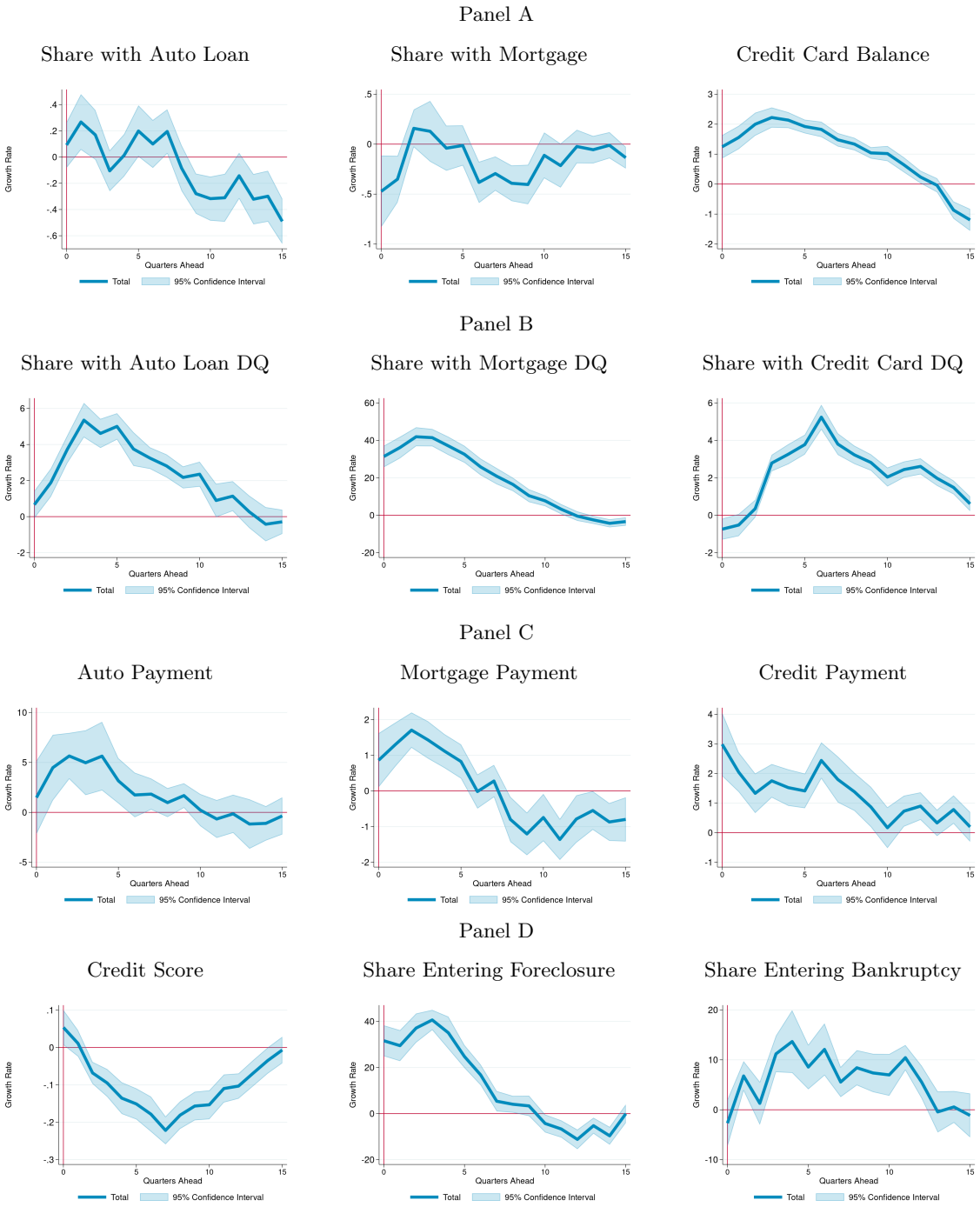
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), adding a lagged dependent variable. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax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



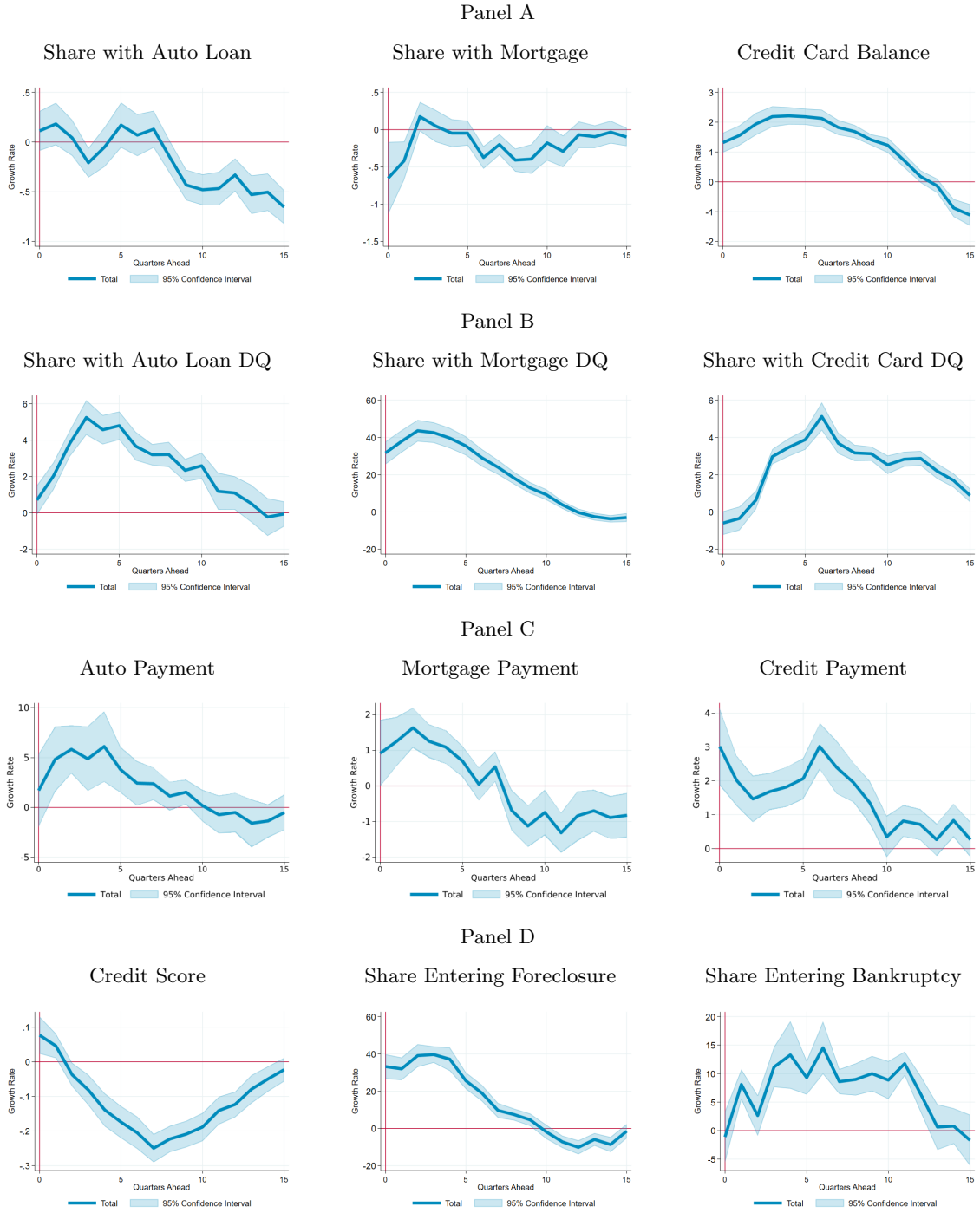
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1), adding county-level controls from the ACS: race, age, unemployment rate, average wage, and share of industries such as manufacturing and construction. Shaded regions indicate 95% confidence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

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



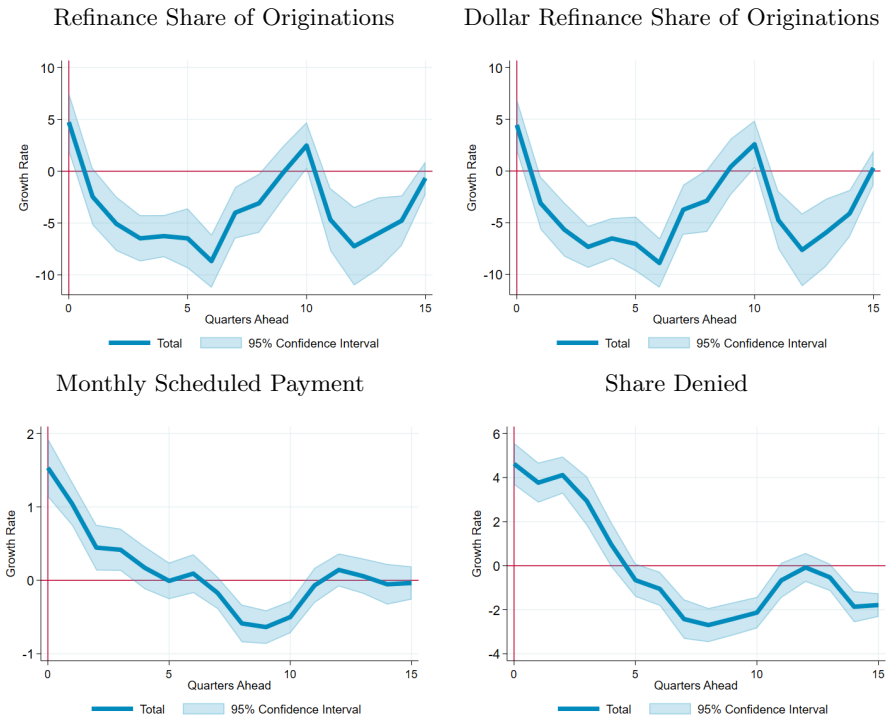
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 2015Q1-2023Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

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



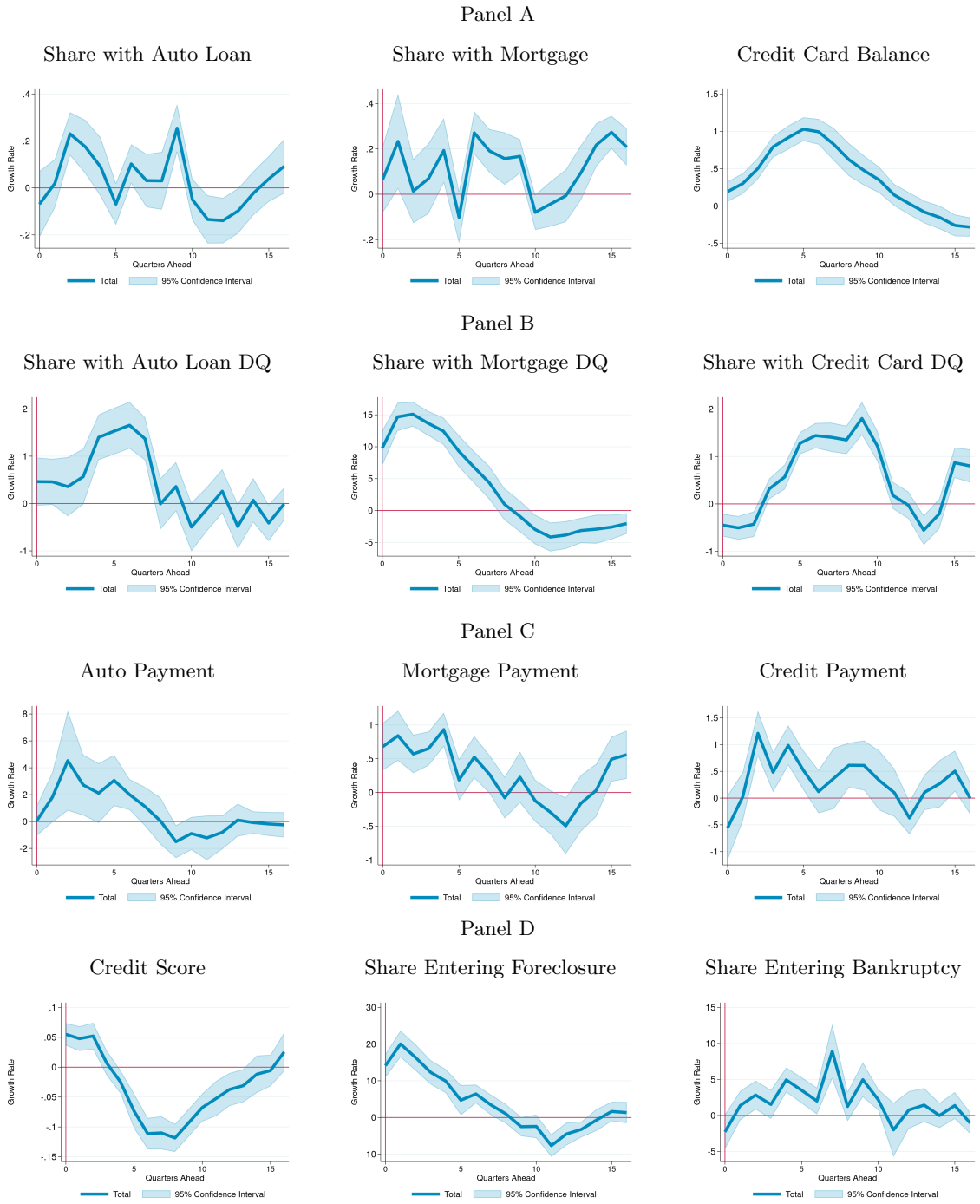
Note: Each line plots the impulse response functions of the growth of the dependent variable. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999Q1-2019Q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel.

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



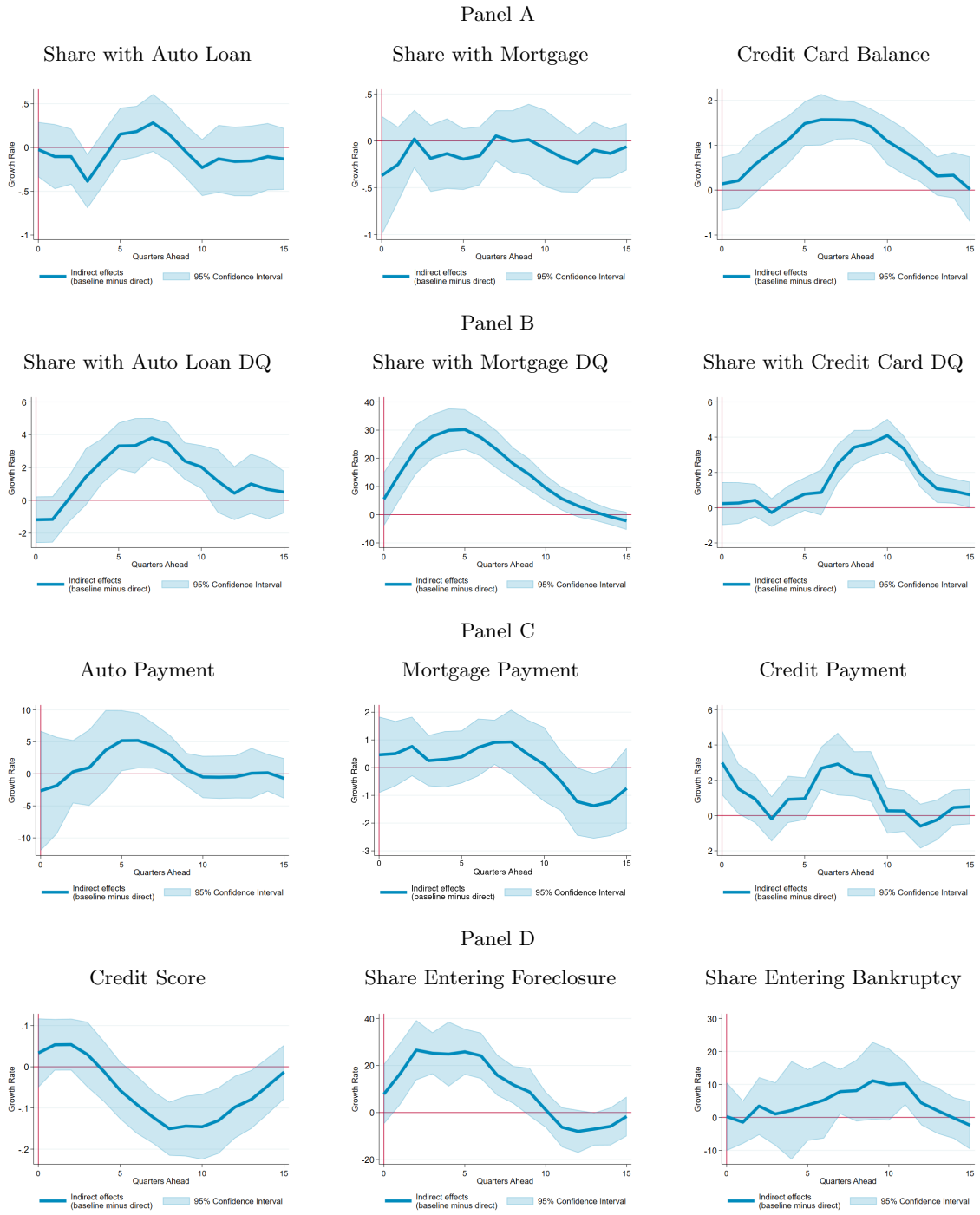
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Data on refinances and payment from Black Knight McDash for range 2005Q1 – 2019Q4. Data on mortgage application 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.

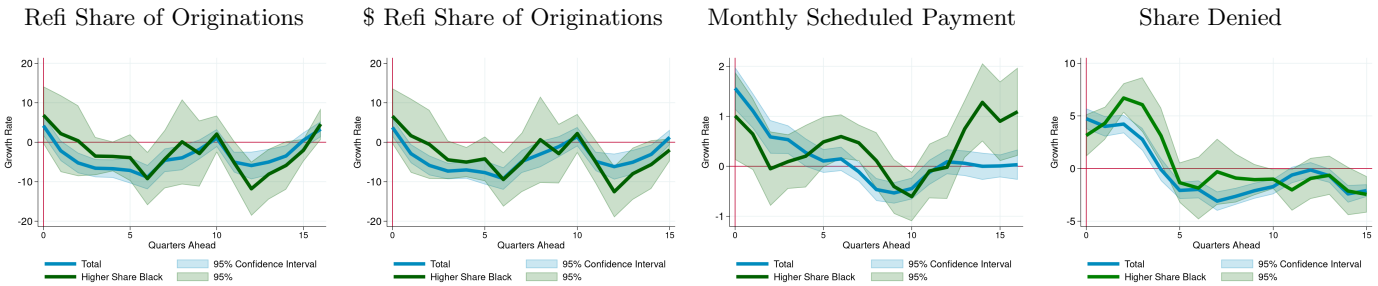
Figure A.15: Household Debt Outcomes: indirect effects, using Bartik instrument



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

Figure A.16: Mortgage Outcomes (By Heterogeneity)

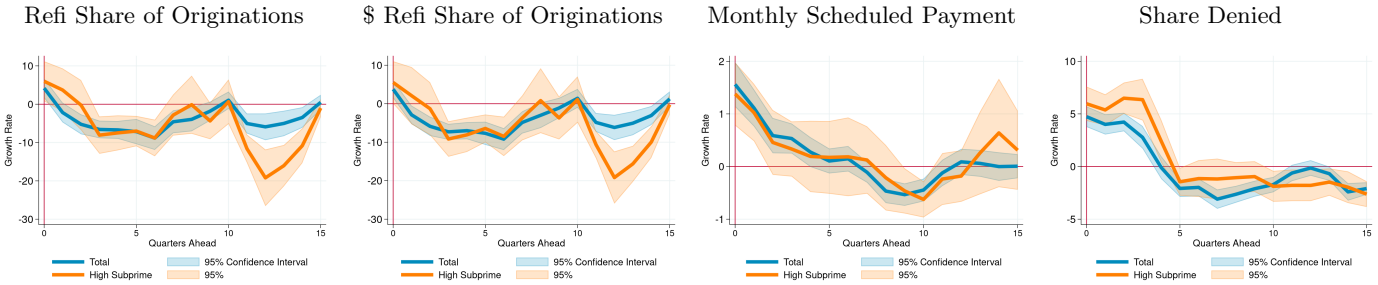
Panel A: Race



Panel B: Income

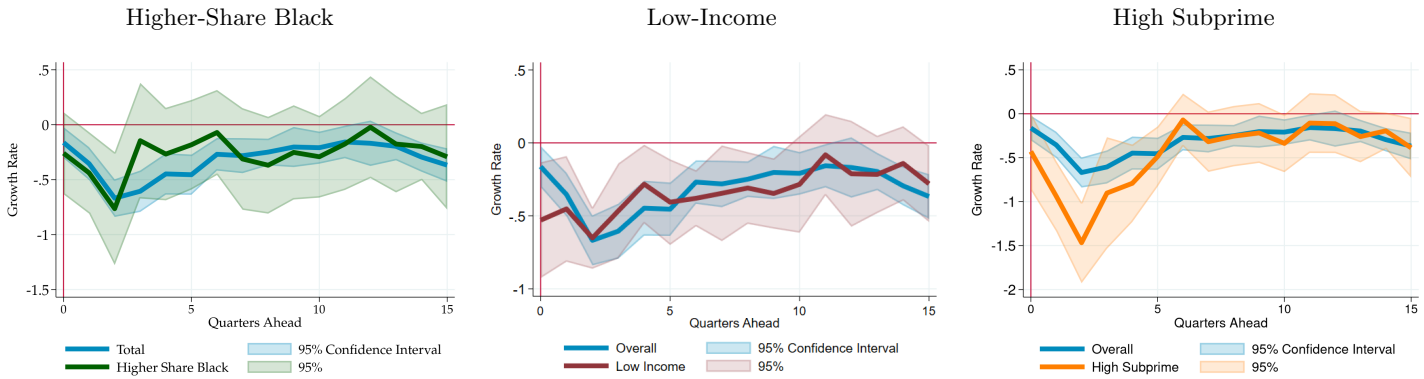


Panel C: Credit Score



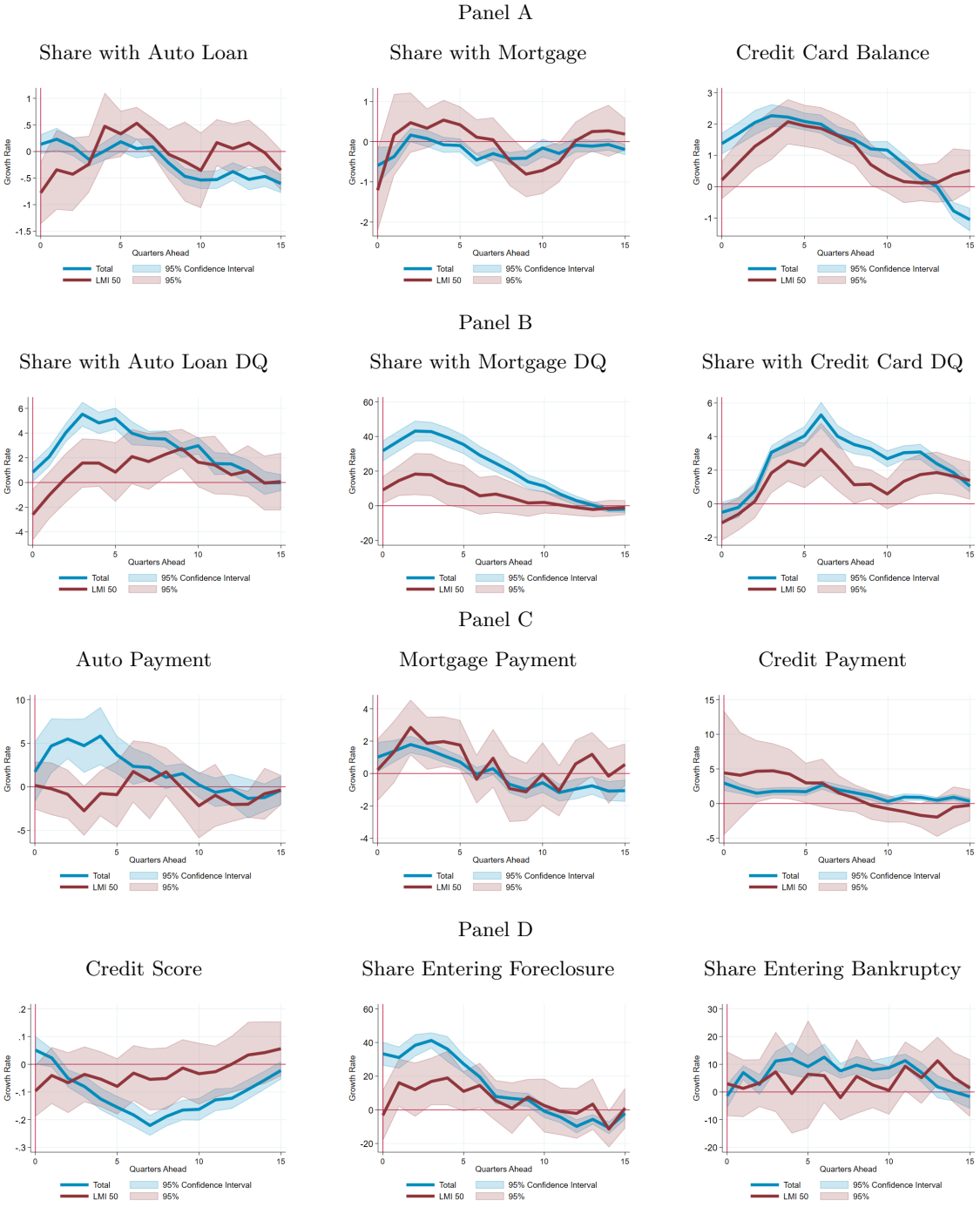
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (1). Shaded regions indicate 95% confidence intervals. Standard errors clustered by county. Time frame is 1999q1-2023q4. Dependent variables from the FRBNY/Equifax Consumer Credit Panel. Higher-Share Black counties are counties in the top quartile of weighted distribution of the share of Black residents. 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.17: 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. 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. 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.18: 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.

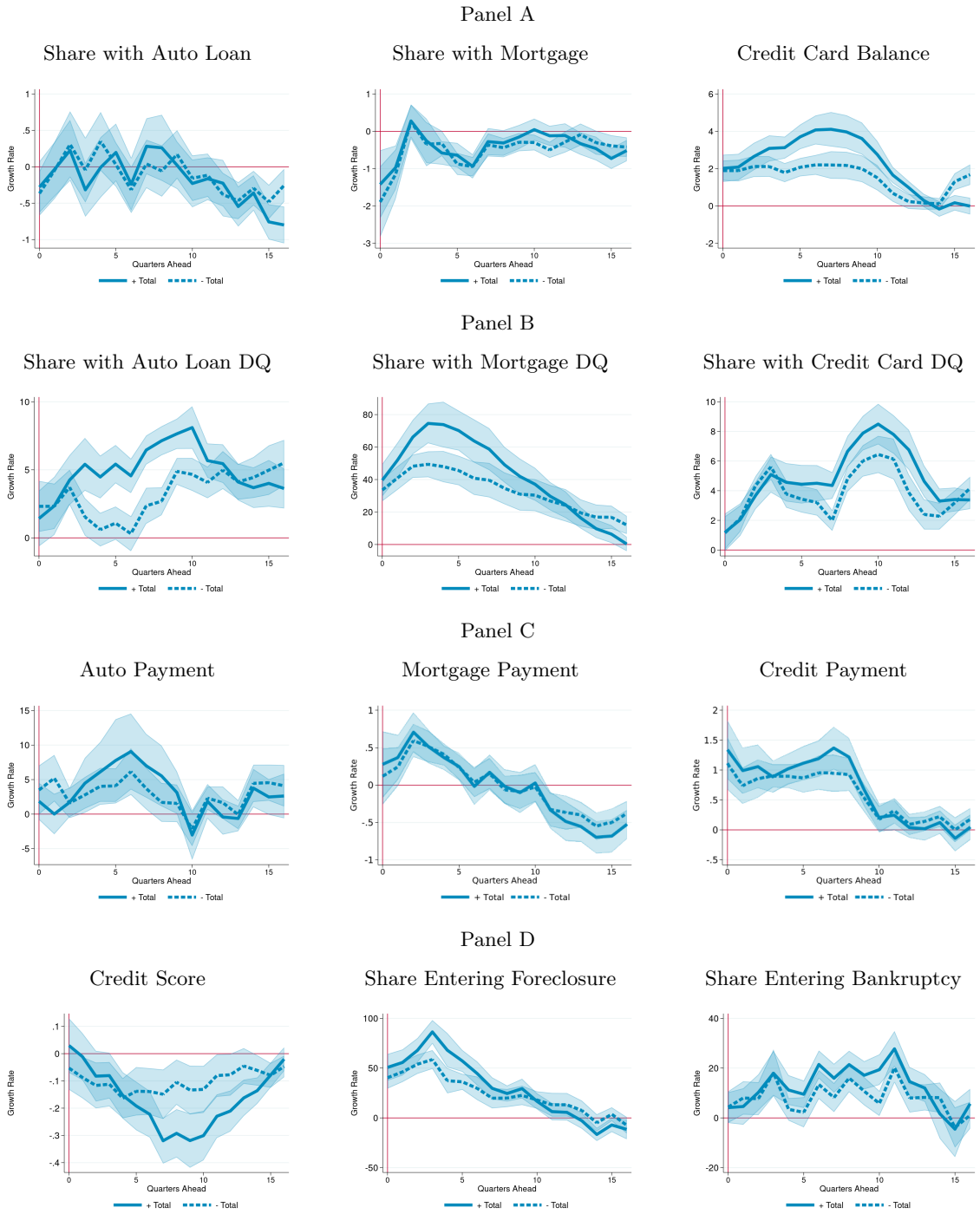
Figure A.19: Mortgage & Employment Outcomes, By LMI Status



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. 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 – 2019Q4. LMI 50 counties are counties with at least half of its population living in Census-designated LMI (Low-to-Moderate Income) tracts.

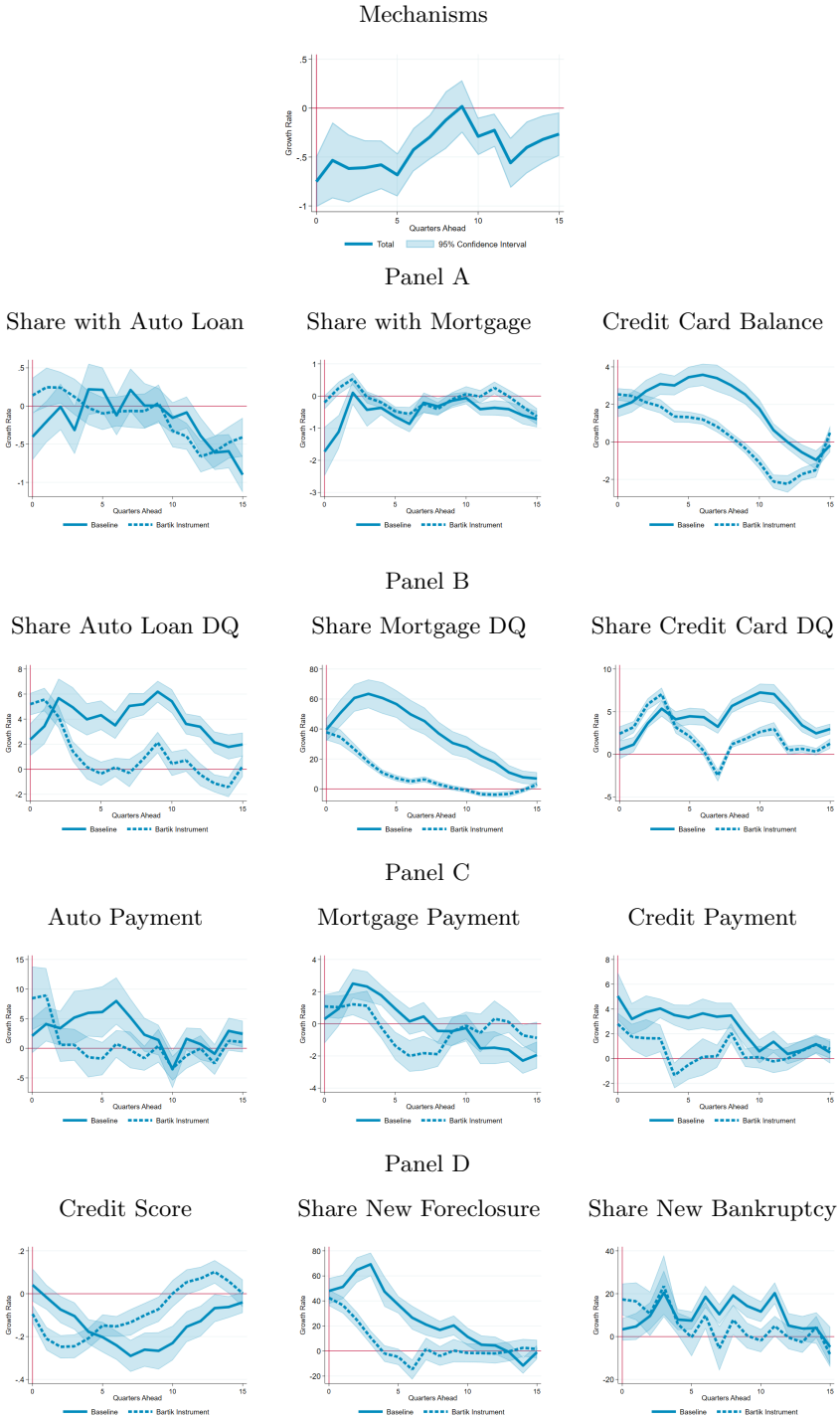
Appendix B: Nakamura and Steinsson (NS) Shocks

Figure B.1: Household Debt Outcomes, Allowing for Asymmetric Effects of Positive and Negative Shocks, Using NS Shocks



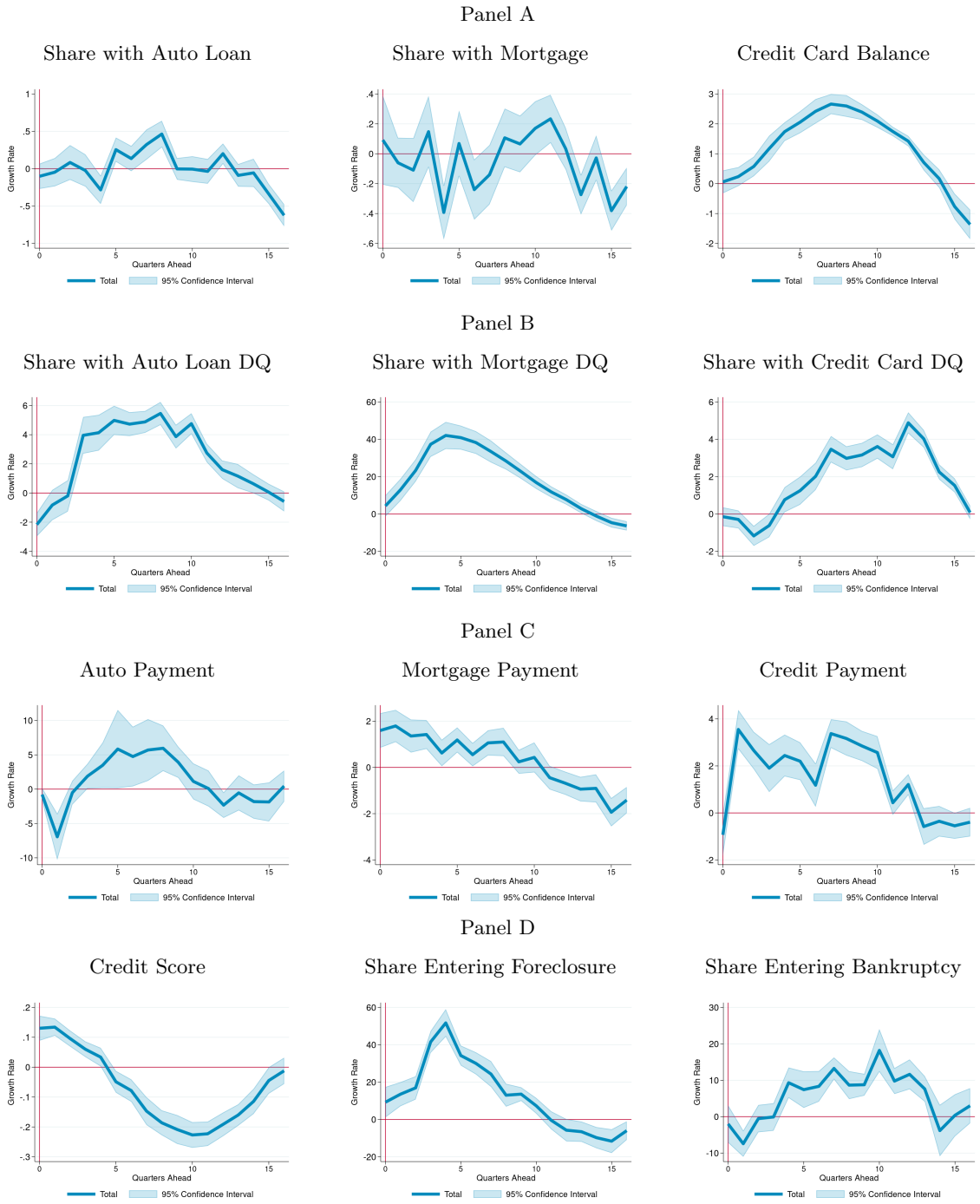
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates impulse response for positive NS shocks and dashed line for negative NS shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county.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

Figure B.2: Household Debt Outcomes (Direct, Indirect Effects with Bartik), NS Shocks



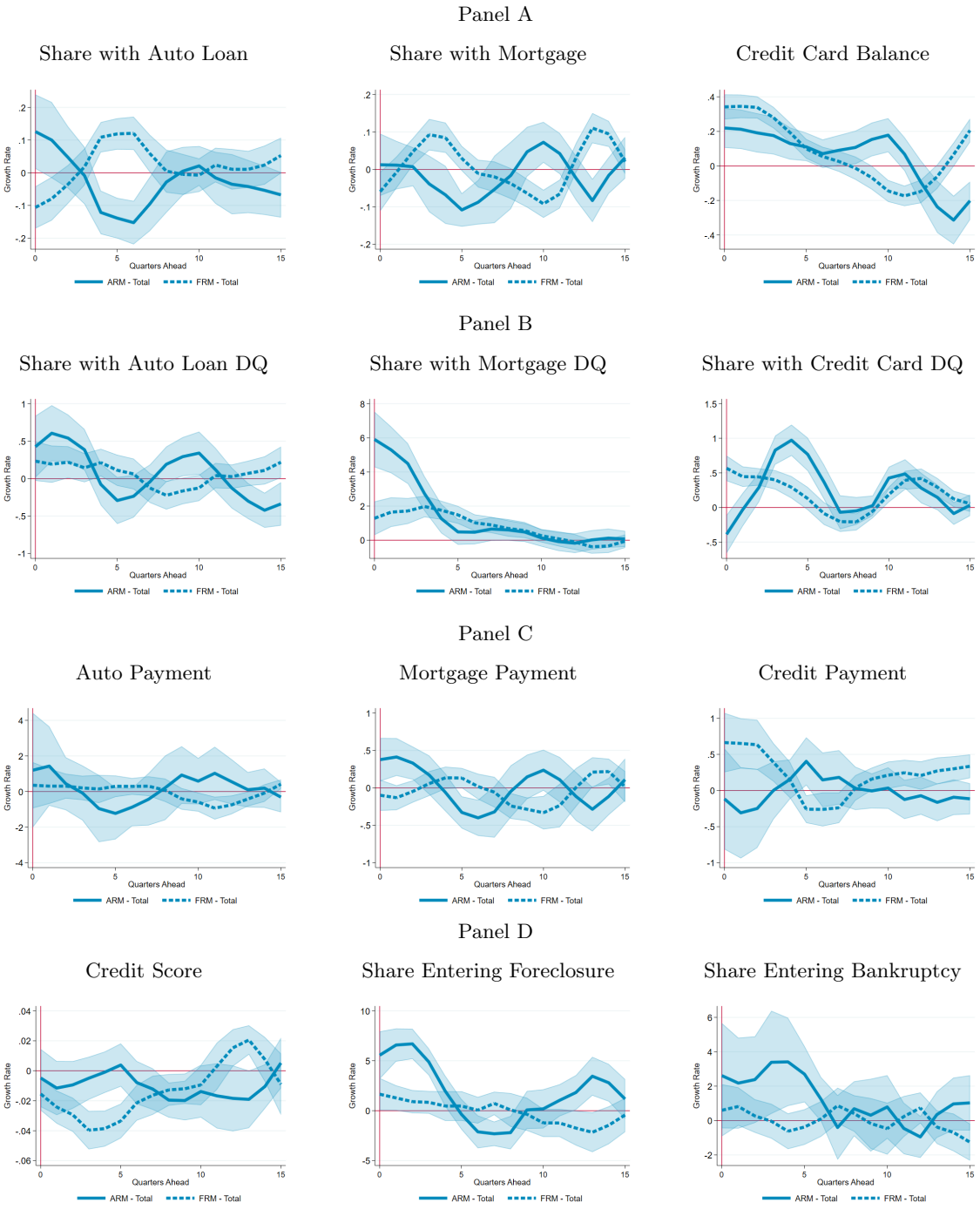
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (3), using a Bartik instrument to separate direct and indirect effects. The solid blue line indicates the total impulse response, while the dashed blue line indicates the impulse response when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

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



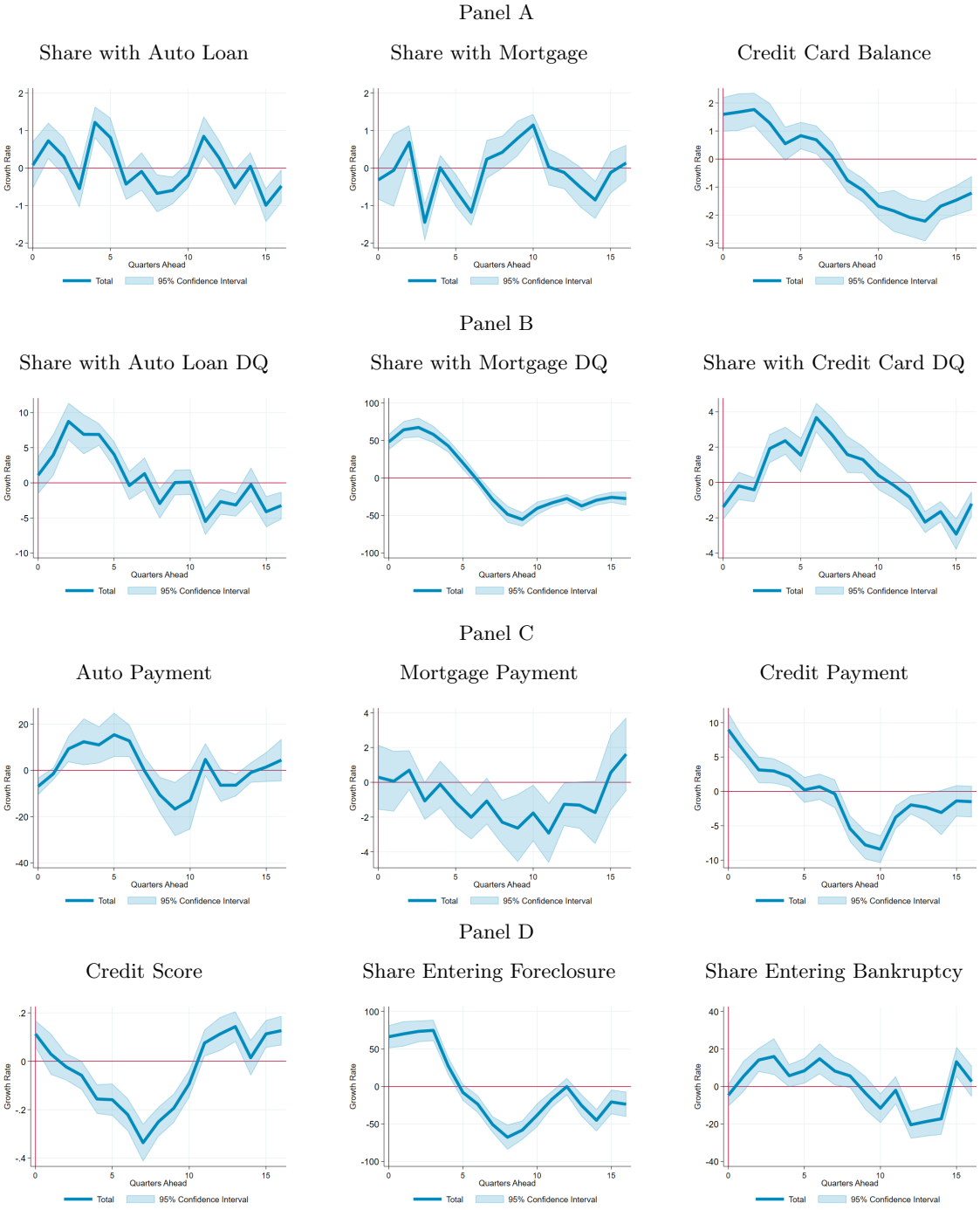
Note: Each line plots the impulse response functions of the growth of the dependent variable using equation (2). Solid line indicates the difference in impulse response for positive FFR shocks and negative FFR shocks. Shaded regions indicate 95% confidence intervals. Standard errors clustered by county.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

Figure B.4: Household Debt Outcomes, ARM vs. FRM Horse Race, Using Monetary Policy Shocks from Nakamura and Steinsson (2018)



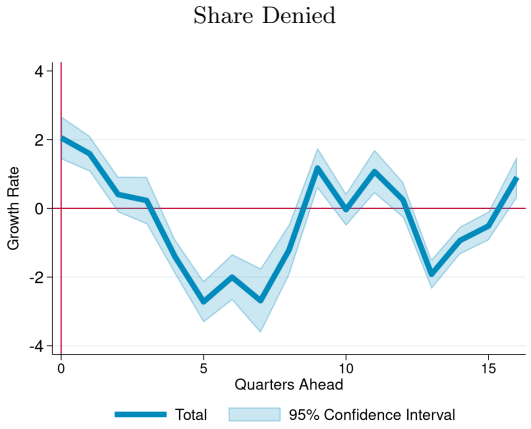
Note: Each line plots the 3-quarter smoothed impulse response functions of the growth of the dependent variable using equation (5). Solid blue line indicates impulse response for ARM DTI, and dotted blue line indicates impulse response for FRM DTI. Shaded regions indicate 95% confidence intervals. Standard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

Figure B.5A: 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.5B: 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 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 – 2019Q4. High subprime counties are counties in the top quartile of the weighted distribution of share of subprime residents.

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 Black**	**Higher Share Subprime**	**Low-income**
Share Holding Auto Loan (%)	32.790	31.227	33.437	31.594
	(6.505)	(6.982)	(6.238)	(7.177)
Share Holding Mortgage (%)	31.347	26.874	25.106	23.668
	(7.853)	(7.108)	(6.237)	(6.335)
Average Credit Card Balance	3,591.467	3,400.079	3,126.450	2,820.920
	(863.417)	(793.993)	(807.232)	(694.052)
Share Holding Delinquent Auto Loan (%)	2.171	2.890	3.408	2.638
	(1.318)	(1.438)	(1.549)	(1.639)
Share Holding Delinquent Mortgage (%)	0.648	0.699	0.686	0.543
	(0.745)	(0.750)	(0.839)	(0.638)
Share Holding Delinquent Credit (%)	6.705	8.211	8.588	7.418
	(2.296)	(2.585)	(2.683)	(2.828)
Average Credit Score	642.539	612.400	599.827	613.450
	(34.856)	(28.479)	(21.290)	(33.292)
Share New Foreclosure (%)	0.083	0.085	0.086	0.069
	(0.107)	(0.092)	(0.111)	(0.102)
Share New Bankruptcy (%)	0.161	0.181	0.180	0.169
	(0.146)	(0.153)	(0.168)	(0.180)
Average Mortgage Payments	317.741	270.330	228.899	182.025
	(157.489)	(122.743)	(126.572)	(129.890)
Average Credit Card Payments	226.073	228.934	204.836	155.177
	(253.427)	(106.077)	(111.322)	(153.797)
Average Auto Loan Payments	142.748	143.459	158.783	139.394
	(187.091)	(127.168)	(173.906)	(300.814)
Orig Share Refinances	54.132	49.468	46.208	49.388
	(16.025)	(14.505)	(14.162)	(15.945)
Avg Monthly Payment	1,260.590	1,098.378	977.834	912.111
	(500.576)	(360.263)	(287.494)	(347.332)
Share Apps Originated (%)	58.457	54.988	53.404	53.640
	(7.977)	(7.697)	(7.806)	(9.058)
Share Apps Denied (%)	19.983	22.680	24.096	24.694
	(6.366)	(6.495)	(6.930)	(7.417)

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

Variable	**All**	**Higher Share Black**	**Higher Share Subprime**	**Low-income**
Share Holding Auto Loan (%)	1.734	1.844	1.958	2.383
	(5.661)	(5.139)	(5.556)	(6.797)
Share Holding Mortgage (%)	0.040	0.075	0.500	1.067
	(5.053)	(4.950)	(5.915)	(7.603)
Average Credit Card Balance	2.029	2.067	2.225	2.125
	(8.267)	(8.200)	(8.952)	(9.675)
Share Holding Delinquent Auto Loan (%)	6.450	6.151	6.507	8.170
	(22.256)	(18.046)	(20.927)	(29.613)
Share Holding Delinquent Mortgage (%)	5.230	3.003	5.039	7.828
	(55.227)	(46.632)	(54.588)	(65.435)
Share Holding Delinquent Credit (%)	2.366	2.476	2.710	3.234
	(13.549)	(13.502)	(14.922)	(17.984)
Average Credit Score	0.047	0.048	0.049	-0.016
	(0.798)	(0.919)	(1.003)	(1.078)
Share New Foreclosure (%)	5.195	5.044	2.773	-4.119
	(87.610)	(82.998)	(86.353)	(93.484)
Share New Bankruptcy (%)	7.136	7.538	7.249	4.604
	(78.314)	(74.692)	(82.263)	(90.290)
Average Mortgage Payments	3.805	3.841	4.731	5.750
	(15.371)	(14.895)	(18.930)	(24.172)
Average Credit Card Payments	4.121	4.052	4.272	4.598
	(19.345)	(17.861)	(20.725)	(25.003)
Average Auto Loan Payments	4.548	4.490	4.672	5.144
	(15.273)	(14.330)	(14.695)	(15.723)
Orig Share Refinances	-2.176	-1.386	0.201	2.178
	(34.202)	(32.892)	(36.895)	(44.654)
Avg Monthly Payment	0.525	0.436	0.643	0.710
	(5.419)	(5.260)	(5.768)	(6.045)
Share Apps Originated (%)	0.558	0.677	0.841	1.383
	(9.888)	(10.355)	(11.415)	(13.153)
Share Apps Denied (%)	2.492	1.994	1.606	1.704
	(25.116)	(22.931)	(22.288)	(24.940)