Household Wealth and Local Labor Markets: Which Asset Classes Matter?

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ABSTRACT

Household wealth effects are likely to be heterogeneous between asset classes due to concentrated asset ownership between investor groups with plausibly different marginal propensities to consume. However, wealth effect estimates between asset classes across studies often cannot be directly compared. To allow for this comparison I construct a new data set on U.S. household asset and debt positions at the county-level and estimate wealth effects on local labor market outcomes simultaneously for majority of asset classes. This holistic setup also reveals the quantitative importance of my approach relative to a single asset case that may be prone to endogeneity. I find evidence of large (opposite signed) wealth effects from local house price shocks and mortgage rate shocks, and small positive effects from stock market wealth shocks on per capita payroll and employment, but no cleanly identified effects from bond market or deposit wealth shocks. House price and mortgage effects operate primarily via the construction sector while stock market effects also via the non-tradable sector. A model with heterogeneous agents motivates the empirical analysis.

JEL classification: E21, E24, E44, G21, G51, J23

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I. Introduction

In traditional macroeconomic models with a representative agent, a return on wealth generates a proportional consumption response. The additional consumption generated by the increase in household wealth subsequently supports local labor markets by encouraging firms to increase wages and employment. However, these models also imply that it does not matter *which* asset in agent's portfolio generates this wealth increase; the consumption response is the same for all assets following a wealth increase of similar magnitude.

The more recent literature has challenged this assumption and indeed a new generation of macro-models try to incorporate heterogeneity in wealth effects (Kaplan, Moll, and Violante, 2018). The usual explanation for the mechanism that generate this effect has two components. First, there is substantial heterogeneity in asset ownership between individuals with different levels of wealth and income. Second, the marginal propensity to consume (MPC) is found to vary based on wealth and income.[1] The correlation between asset concentration and MPC heterogeneity together imply that average consumption responses from wealth shocks may be very different between asset classes depending on the investor base. Said differently, it seems plausible that, say, the stock market –where the participation rate is low and majority of wealth is concentrated in the hands of the richest households– and the housing market –where participation rate is relatively high and wealth is much more dispersed– should trigger differing consumption responses because the richest households have lower MPCs than the lower income households.

Though the possibility of this type of heterogeneity is often acknowledged by prior empirical studies attempting to measure the wealth effects, the focus is usually fully devoted to a single asset class and other asset wealth—if at all—is controlled with imprecise proxies. This poses a two-fold problem: First, the risk of omitted variable bias is potentially large since one form of asset may capture the wealth effects of another if these other assets are not properly controlled for in the research design. Second, given that the studies differ in their sample size, identification techniques, geographic coverage and, most importantly, even the assumed underlying economic model, it is often hard to draw conclusions about how the wealth effects between asset classes compare quantitatively. Indeed, Cooper and Dynan (2016) argue that more evidence about the wealth effect of different assets is much needed.

In this paper, I study how shocks to household balance sheets impact the local labor market outcomes

[1] This is confirmed by empirical estimates from wide variety of studies e.g. Blundell, Pistaferri, and Preston (2008), Broda and Parker (2014), Kreiner, Dreyer Lassen, and Leth-Petersen (2019), Jappelli and Pistaferri (2014), Fisher et al. (2020) and Baker et al. (2021), report that households with little liquid wealth and without high past income react particularly strongly to an economic stimulus. Also simulations such as Carroll, Slacalek, et al. (2017) match the evidence. For more discussion on MPCs see e.g. Kaplan and Violante (2022).

through wealth effects and try to answer the two problems posed above by *simultaneously* incorporating the most important household asset classes to the analysis.[2] I use the same methodology with Bartik (1991) instrumental variable identification technique for all asset classes and a new extensive quarterly panel data set on household wealth at the county level from the United States (US), which ensures that we can compare the obtained wealth effects across assets. I will next give a short introduction to the main themes and results in this paper.

The recent empirical literature around wealth effects has mostly moved from using time series data to using cross-sectional and panel data. The benefit of having a large cross-section is that it enables to better address the so-called leading indicator channel that may create endogeneity in the research design. Specifically, household wealth is a leading indicator of labor markets if it responds to news about anticipated local labor market outcomes. If this is the case, simply regressing labor market outcomes on wealth shocks captures not only the wealth effects but also the effects of this reverse causality. However, with a large heterogeneous cross-section, we can find potential instruments to local wealth shocks that are exogenous to local labor market outcomes. I follow Chodorow-Reich, Nenov, and Simsek (2021) (CRNS henceforth) and estimate the causal responses of employment and payroll (per capita) from wealth shocks using Bartik (1991) instrumental variable identification technique that uses the fact that most national level wealth shocks can be used as an instrument for local county level shocks. The identifying assumption that (national) shocks are exogenous has been recently formulated by Borusyak, Hull, and Jaravel (2022).

One of the main contributions in this paper is that I am also using a new detailed data set that I have collected and constructed from various public sources using best practices from prior work. This new county-level dataset contains household wealth and return series for most of the relevant asset classes that households own: stocks, residential housing, bonds, and deposits. I also collect data on total household debt and the cost of this debt (based on mortgage rates) to acquire an estimate of the effect of household debt shocks since debt can be counted as a negative asset. These series are consistently available for all asset classes from 1999 onwards. Overall, this new dataset allows me to better study the heterogeneity in wealth effects, which is the main topic of interest in this paper.

The analysis is conducted as follows. I start by motivating my empirical strategy with a simple partial equilibrium model that spells out the conditions that can create heterogeneous responses between different types of wealth shocks. Specifically, the model's economy, which can be thought to represent

[2] The largest household asset categories that are excluded from the analysis are non-corporate business assets and defined pension entitlements that correspond to 9% (11.9 trillion USD) and 12% (16.1 trillion USD) respectively of the total household gross assets in 2019Q4 (Source: https://www.federalreserve.gov/releases/z1/dataviz/z1/balance_sheet/chart/#series:assets). The robustness checks suggest excluding these asset classes do not affect the main results in this paper.

a single county, is populated by three groups of capital holders –savers, intermediaries, and borrowers– who have heterogeneous time discount parameters and who invest in different assets. Savers save in fixed income securities (bonds and deposits) and the stock market. Intermediaries, take savers' deposits and allocate them –with their net wealth– to borrowers who then invest these funds in the housing market. Therefore, the borrowers are best thought of as middle-class homeowners and/or mortgagors, intermediaries as bankers and savers as rich individuals. The wealth shocks are thought to be determined at the national or global level and are thus assumed to be exogenous to the model's closed economy. Since risks are less than perfectly shared and the agents differ in their wealth levels and their respective marginal propensities to consume, the wealth shocks between assets can generate effects with very different magnitudes. The fluctuating demand in product markets then directly translates into a representative firm adjusting its demand for labor, creating the link between wealth shocks and aggregate payroll. I then take this partial equilibrium model to the data and estimate the (plausibly) causal responses of employment and payroll from wealth shocks using "the local projection-instrumental variable" (LP-IV) technique (Jordà, 2005; Ramey, 2016; Stock and Watson, 2018) with Bartik (1991) IV identification and a rich set of confounders including but not restricted to county and state $\times$ time fixed effects and multiple lags of both outcome variables and past shocks of instruments.

The main results from the empirical analysis can be summarized as follows. First, incorporating all forms of wealth into the analysis reveals considerable heterogeneity between the wealth effects among asset classes. Some assets matter a lot and others not at all. Housing wealth shocks are found to be the most important ones, which aligns with existing literature.[3] For example, I find that an increase in housing wealth the size of quarterly labor income in quarter t generates approximately 1.5 percentage point higher employment per capita in the quarter $t+7$ relative to a counterfactual scenario where similar increase in housing wealth does not occur. In contrast the stock market and the bond market wealth effects are positive but much smaller. Mortgage rate increases have large negative labor market effects but these are for the most part not very cleanly identified. The deposit wealth shocks have the same problem. Second, the effects seem to originate primarily from the non-tradable or construction sectors that theoretically should be more responsive to wealth shocks. In contrast in the tradable sectors, I find no similar responses. These findings support the argument that the research design is specifically capturing household wealth effects and not changes in firms' cost of capital or collateral lending effects (Adelino, Schoar, and Severino, 2015).

[3]See e.g. Bostic, Gabriel, and Painter (2009), Case, Quigley, and Shiller (2011), Mian and Sufi (2014), Cooper (2013), Guren et al. (2020a), and Aladangady (2017)

Multiple robustness checks show that housing and stock market wealth effects are the most robust to various changes in specification and inclusion of additional covariates. Interestingly, the effects of household debt on employment and payroll are even bigger when measured in non-per capita terms – a finding that seem to arise mostly from the fact that following a relatively large increase in interest payments, there occurs a net migration out of the county. This loss of the working-age population results in lower employment and labor income.

I also show that had we conducted the analysis asset-class-by-asset-class basis while not controlling for other assets we would have gotten quantitatively very different results. This emphasizes the importance of the holistic approach to estimating wealth effects because of the omitted variable concerns in single asset case. Also, by incorporating the empirical strategy within the local projection difference-in-difference (LP-DiD) framework recently introduced by Dube et al. (2022), I show that the main results are robust to the recent econometric critique from using two-way fixed effects regressions in the presence of plausibly heterogeneous treatment effects—for survey see e.g Chaisemartin and D'Haultfoeuille (2022).

Do these results offer new useful information for monetary policymakers and are they consistent with their prior beliefs? At least, what we can infer from central bank communication the heterogeneous-wealth-effects-view may not be as widespread among policymakers as one might think. In particular, based on the textual analysis of Cieslak and Vissing-Jorgensen (2021) members of FOMC or more generally the Federal Reserve (Fed) seem to believe not only in the importance of this transmission channel of wealth shocks but also in this equal importance of asset classes. This fact is illustrated in Figure 1 which shows the counts of economic phrases associated with a given topic that is mentioned with the phrase associated with the stock market or housing market in transcripts of FOMC meetings.

[Figure 1 here]

First, the Fed seems to believe in the overall importance of wealth effects on consumption, by referring directly to consumption in one-third of the time when discussing the stock market or the housing market. Second, in line with the traditional macro models, based on the phrases involving the stock market and the housing market, the Fed seems to put an approximately equal importance of these asset classes in their communication, especially in the context of consumption.

Thus if the different asset class shocks produce different-sized employment and consumption responses due to the wealth effects channel then Fed may erroneously put too much emphasis on worrying about asset markets that may not matter much while partly ignoring the ones that do. However, it should be emphasized that my results do not necessarily imply that, say, housing market wealth shocks are more important than stock market wealth shocks for local labor markets *in general* since there are other channels than just household balance sheet how asset markets can stimulate the real economy. Since the purpose of this paper is to quantify only the wealth effect channel, we can only make claims about the relative magnitudes of wealth shocks in that specific domain of asset market shock transmission. Disentangling the relative sizes of the many different transmission channels is outside the scope of this paper.

Finally, one should note that the results in this paper focus mostly on *micro*-elasticities between wealth effects and local labor market outcomes since micro-level estimates can be more credibly identified. However, I discuss how and under what assumptions we can translate these them into *macro*-elasticities, which are the estimates perhaps most interesting to policymakers and show that under reasonable assumptions the ranking of wealth effects caused by different asset classes persist when translating the micro-estimates to macro-level. I also show that macro-level empirical estimates, though less well-identified, are fully in line with the micro-level results.

The rest of this paper proceeds as follows: Section II briefly reviews the recent related literature. Section III presents the theoretical model. Section IV describes the data sets and construction of main variables. Section V introduces the baseline empirical specification and discusses the conditions for identifying causal estimates. Section VI presents the empirical results and discussion. Section VII concludes.

II. Related literature

Investigating the links between wealth and labor market outcomes or consumption has a long tradition in macroeconomics and finance. Early work by Modigliani (1971) suggested that a dollar increase in wealth (holding fixed labor income) leads to an increase in consumer spending of about five cents. Substantial subsequent academic and policy work has put considerable effort in estimating what these consumption responses for different assets really are. Usually, researchers have picked one form of asset wealth and concentrated on that.

Early papers investigated the impact of wealth shocks using time series data (see e.g. Lettau and Ludvigson (2004) and Carroll, Otsuka, and Slacalek (2011)). More recently there have been attempts to use microdata to answer these questions since it often allows for cleaner identification. An early example is the work by Campbell and Cocco (2007) who studied housing wealth effects on consumption using UK microdata. A closely related branch of literature has also used employment and wages as the dependent variable while focusing on a particular asset class. Mian and Sufi (2014) focus on studying the effects of housing wealth on employment before and after a burst of the housing bubble. Guren et al. (2020a) investigate housing wealth elasticities in the long run. Bhutta and Keys (2016) and Chen, Michaux, and Roussanov (2020) focus more specifically on home equity extraction. Other work on housing wealth effects include Aladangady (2017), Garriga and Hedlund (2020) and Graham and Makridis (2020) while recent theoretical investigations are conducted by Kaplan, Mitman, and Violante (2020) and Berger, Guerrieri, et al. (2018).

Though housing wealth effects have probably received the most attention, other asset classes have also been considered. CRNS investigate the effects of stock market wealth on employment and payroll and contribute to the methodological frontier by introducing new more accurate capitalization approach for equity, which this paper also uses. There are also many papers that have also investigated the effect of mortgage rates and household debt. For example, Di Maggio and Kermani (2017) estimates the effect of credit growth on real economy using local laws against predatory lending, while Di Maggio, Kermani, et al. (2017) find that a decline in mortgage payments induces a significant increase in car purchases. Other notable recent papers studying mortgages and the real economy are at least Greenwald (2018), Keys, Piskorski, Seru, and Yao (2014), Berger, Milbradt, et al. (2021) and Baker (2018). Wealth effects from fixed income wealth –deposits and bond market wealth– have been rather understudied perhaps due to lack of good data. However, the role of deposits may primarily work via mechanisms unrelated to wealth effects. For example, Drechsler, Savov, and Schnabl (2017) provide evidence that when the Fed funds rate rises, banks widen the spreads they charge on deposits, and deposits flow out of the banking system. The withdrawn funds are presumably allocated to other better-yielding assets like treasuries but not necessarily used for increased consumption. In fact, the total effects can even lead to decreases in employment since Drechsler, Savov, and Schnabl (2017) show this reduction in deposits leads to a contraction in bank lending.

Finally, important work on wealth effects has also been conducted with non-US data. A few recent examples include Maggio, Kermani, and Majlesi (2020) and Flodén et al. (2021) who investigate the effects of stock wealth or household debt shocks with adjustable rate mortgages on consumption using Swedish data and Ring (2020) who investigate the effects of wealth shocks of entrepreneurs in Norway.

While most of these studies are interested in same structural parameters, the differences in the empirical methodology, the granularity of the data, or most importantly the assumed data-generating process will often mean that they are in fact estimating slightly different parameters. This means that even if these studies are fully internally consistent within the framework that they use, we cannot quantitatively compare the wealth effects across studies because we are comparing apples with oranges. The contribution of this paper is to study jointly the wealth effects of the most important household wealth classes thus offering the possibility for a systematic comparison between different asset classes.

III. Model

In this section, I derive a simple model that motivates the research design for the empirical analysis. Since the unit of observation in my empirical setup is a county c, the estimates only represent the partial equilibrium responses of wealth shock on wages and employment. Similarly, the economy in the model is best thought to correspond to a single individual county, which I, for simplicity, assume to be an autarky. The agents in the economy are price takers.

In the model three groups of agents operate in the asset markets, whom I call savers (s), intermediaries (i), and borrowers (b). Markets are incomplete and partly segmented in the sense that savers can invest only in equity and bond markets and a risky free asset for which the intermediary sector acts as the counterparty. We can think of the funds that savers invest in this risk-free asset as deposits held at a bank, which the intermediaries manage. The intermediaries on the other hand have the mandate to channel savers' deposits to borrowers who want/can only invest in residential housing markets and borrow from intermediaries with a risk-free rate. We can think of these borrowed funds as mortgage savers acquire to buy houses.[4] Intermediary bankers can also use their own net worth when financing borrower households. [5] Figure 2 summarizes these balance sheet relationships.

[Figure 2 here]

Though clearly, this setup is stylized since in reality households can have simultaneously stocks, fixed income securities, housing wealth, and mortgages in their balance sheet, on a very crude level it is hopefully accurate enough given that also in reality middle classes have portfolios concentrated on housing

[4]For simplicity the model considers housing as a form of financial wealth and thus abstracts from the arguments made for example by Buiter (2010) that housing can be considered not only an asset but also a form of consumption.

[5]There is no restriction in the model that would prevent the borrowers to be the end creditors and savers to be the end debtors. However, for the sake of illustration, we can think of the model parameters to be such that they induce the sort of lending and borrowing patterns described above. As we will see, the empirical results support this interpretation.

and mortgages while the top earners hold fixed income securities and stocks in much bigger proportions (Saez and Zucman, 2016; Kuhn, Schularick, and Steins, 2020). This setup is also in line with recent evidence by Mian, Straub, and Sufi (2020) who show that the rich have been financing a big part of the borrowing of the low-and middle-income people in recent decades. More importantly, this setup ensures that the model stays tractable. Finally, there is also a fourth group called workers who do not participate in the asset markets and only supply labor and consume their wage bill each period. For the most part, we can ignore that this group exists since they do not affect the behavior of the asset market participants.[6]

The model is in continuous time and time evolves from $t = 0$ to infinity. For tractability, I assume all asset participant groups have logarithmic preferences with agent-specific time discount parameter ρ_j where j denotes the type of agent. Agents maximize the expected discounted value of future flow utility, which is given as

$$E_0 \int_{t=0}^{\infty} e^{-\rho_j t} \ln(C_{j,t}) dt$$

I assume that the return for risky forms of capital namely housing, bonds, and stocks are exogenous with respect to our small economy, and their prices are set in the world capital markets. Thus the model completely abstracts from asset pricing considerations with regard to these assets. This means the asset positions of the agents within the county do not affect how those assets are priced in world markets. Return on each risky asset follows an Itô process with drift μ_t^f and volatility σ_t^f. So the return on wealth for asset f is defined as

$$\frac{dV_t^f}{V_t^f} := dR_t^f = \mu_t^f dt + \sigma_t^f dZ_t \tag{1}$$

where V_t^f is the stock of wealth in asset f. Deposits V_t^{Dep} and mortgages V_t^D, are considered to be risk-free and they are in zero net supply. The rates of these assets arise endogenously and risklessness implies $\sigma_t^{Dep} = \sigma_t^D = 0$. However, knowing the functional form of the wealth processes for any asset f is not central for deriving the relationship between wages and wealth shocks in differential form, which is the main goal in this section.

[6]The decision to fix labor supply decisions ensures that the model stays tractable by preventing the wealth shocks endogenously affecting the labor supply and it is often imposed in prior work. For example, assuming households have GHH preferences as done in Guren et al. (2020b), leads to the same outcome. Though, this theoretical part abstracts from labor supply considerations the empirical part of the paper discusses these effects and their implications in detail.

The log utility has the tractable property that the consumption wealth ratio is constant at ρ_j that is for each individual $\frac{C_t^j}{N_t^j} = \rho_j$. This implies that each agent's optimization problem equals one where she maximizes the growth of her net wealth N_t^j given the constraints and consumption rate ρ_j. This assumption simplifies the interpretation of the wealth effects, since a dN_t^j-dollar increase in wealth for agent j leads to $\rho_j dN_t^j$-dollar increase in consumption due to $dC_t^j = \rho_j dN_t^j$, meaning ρ_j can be interpreted as the MPC of agent j. If all agents had homogeneous MPCs—with $\rho_j = \rho$ for all j—then the model would collapse to the representative agent setting where every wealth shock, regardless of its asset class origin, would generate the same consumption response. Thus it is the *combination* of segmented markets and heterogeneous MPCs that underlie the heterogeneity in wealth effects.[7]

Let us start with borrowers' budget constraints. After optimizing their portfolio holdings, borrowers' net wealth $N_t^b = V_t^H - V_t^D$, where V_t^H and V_t^D denote the amount of housing wealth and household debt respectively, evolves as

$$\frac{dN_t^b}{N_t^b} = \frac{V_t^H}{N_t^b} dR_t^H - \frac{V_t^D}{N_t^b} dR_t^D - \rho_b dt \tag{2}$$

The first term on the right-hand side is the long position of the borrowers invested in the housing market, the second term is the short position of borrowers in mortgages and the third term captures how much the agents consume out of their net wealth in each period.

Similar logic applies to other agents. The intermediaries are investing in household debt, V_t^D, issued by borrowers and issuing deposits themselves, V_t^{Dep}. After optimizing their portfolio holdings, the intermediaries' net wealth $N_t^i = V_t^D - V_t^{Dep}$ evolves as

$$\frac{dN_t^i}{N_t^i} = \frac{V_t^D}{N_t^i} dR_t^D - \frac{V_t^{Dep}}{N_t^i} dR_t^{Dep} - \rho_i dt \tag{3}$$

Finally, savers' net wealth is a sum of their deposit, stock market, and bond market holdings $N_t^s = V_t^{Dep} + V_t^E + V_t^B$. After optimizing portfolio weights, the net wealth evolves as

$$\frac{dN_t^s}{N_t^s} = \frac{V_t^{Dep}}{N_t^s} dR_t^{Dep} + \frac{V_t^E}{N_t^i} dR_t^E + \frac{V_t^B}{N_t^i} dR_t^B - \rho_s dt \tag{4}$$

Let us now look at the production side of our economy. I assume there exists a representative firm that produces the consumption good using a technology, A_t, with labor, L_t, as a single input. The production function takes the following form

[7] The log utility simplifies the portfolio choice because the interest rate changes go on to affect the consumption without being distorted by changes in income and substitution effects, which balance each other out. For the purpose of spelling out one potential underlying reasons behind heterogeneous wealth effects, this stylized model is sufficient.

$$Y_t = A_t L_t^\alpha \tag{5}$$

with $0 < \alpha < 1$. The firm maximizes profits $\Pi_t = A_t L_t^\alpha - W_t L_t$ by choosing the labor L_t optimally and taking the wage rate W_t as given. Taking first order condition implies that at the optimum

$$\alpha Y_t = W_t L_t \tag{6}$$

We see that the wage bill is proportional to output.[8] On the other hand good markets clearing implies

$$C_t = Y_t \tag{7}$$

and thus the two conditions above imply $C_t = \frac{1}{\alpha} W_t L_t$.

Now the total consumption in the economy is the weighted sum of the consumption of workers and asset holders. Furthermore, since workers consume all their labor income each period then Equations (6) and (7) imply that their consumption is perfectly correlated with aggregate consumption growth. Formally we get this from using the definition of aggregate consumption growth

$$\frac{dC_t}{C_t} = (1-\alpha) \sum_j \frac{C_{j,t}}{C_t} \frac{dC_t^j}{C_t^j} + \alpha \frac{dC_t}{C_t} \tag{8}$$

where the first term on the right-hand side is the consumption of capital market participants and the second term is the consumption of hand-to-mouth workers. Solving for $\frac{dC_t}{C_t}$, we get

$$\frac{dC_t}{C_t} = \sum_j \frac{C_{j,t}}{C_t} \frac{dC_t^j}{C_t^j} \tag{9}$$

Then we can use the fact that $C_t^j = \rho_j N_t^j$ and $dC_t^j = \rho_j dN_t^j$ to get

$$\frac{dC_t}{C_t} = \sum_j \frac{\rho_j N_{j,t}^j}{C_t} \frac{dN_t^j}{N_t^j} \tag{10}$$

Finally, plugging in individual wealth processes budget constraints introduced above and using the definitions of N_t^j for each agent j –for example, $N_t^i = V_t^D - V_t^{Dep}$– and replacing C_ts and dC_ts by using the fact that $C_t = \frac{1}{\alpha} W_t L_t$ we get

[8] What happens to profits $(1-\alpha)Y_t$ is unimportant in this model but we can for example assume they are paid as dividends to global financial markets or as taxes to the central government.

$$\frac{d(W_t L_t)}{W_t L_t} = \sum_f \beta_f \frac{V_t^f}{W_t L_t} dR_t^f + \sum_f \gamma_f \frac{V_t^f}{W_t L_t} dt \tag{11}$$

where β_f and γ_f are meta parameters that are unique for each asset f. This last step and the expressions for these meta parameters as a function of structural parameters are detailed in Appendix A.

This expression directly links the wealth shocks to total payroll growth and I test its empirical counterpart in Section V. Specifically, the model shows that the growth in total payroll takes a shift-share form where the wealth-to-labor income ratios, $\frac{V_t^f}{W_t L_t}$, are the shares and returns dR_t^f are the shifters. The expression is intuitive: the shock dR_t^f has a bigger impact on total labor income if a big part of the total wealth is held in asset f. We also see by looking at the parameter construction that it is very context dependent what is the sign and magnitude of the response for consumption and wages from return shocks. For example, the housing may generate a much higher consumption response than stocks and bonds if borrowers are much less patient than savers. [9]

IV. Data

In this section, I describe the data sources and how to construct county-level wealth and return series. However, I leave some of the details to the Appendix B and Internet Appendix. A large part of this data work builds on the best practices that previous researchers have come up with in various settings. For example, the work of CRNS and Mian and Sufi (2011) have been important in guiding the construction of stock market and housing wealth estimates respectively.

A. *Stock market wealth and return*

The stock market wealth is constructed following CRNS by capitalizing dividend income from Internal Revenue Services (IRS) Summary of income (SOI) files that are publicly available. This capitalization approach is more detailed than the one used by Saez and Zucman (2016) since it controls for differences in portfolios among households with different wealth and age. Additional details are allocated to Appendix B and Data Appendix of CRNS. Using their procedure I extend the sample to Q2 2019.

[9] Though for simplicity, the illustration above does not explicitly have the price level included, it would be easy to incorporate it by introducing the exogenous inflation process and multiplying both sides of the equation with the price level which would give us a practically identical expression as Eq. 11 but where everything would be in nominal terms. Details are found in the Appendix IA5. Indeed, it is this nominal version of Eq. 11 that I am testing in the empirical section.

Estimating the county stock returns and collecting the related data likewise follows the procedure of CRNS. The stock market return in a county c is given as $r^{E*}_{c,t-1,t} = \alpha_c + r_{t-1,t} + b_{c,t-1}(r^m_{t-1,t} - r_{t-1,t}) + e_{c,t-1,t}$ where $r_{t-1,t}$ is the riskless rate (the interest rate on 3-month Treasury bill) from the end of quarter $t-1$ to the end of quarter t, $r^m_{t-1,t}$ is the net return on market portfolio (total return on CRSP value-weighted portfolio collected from WRDS), $b_{c,t-1}$ is a county-specific portfolio beta obtained from CRNS and $e_{c,t-1,t}$ is an idiosyncratic component of the return.[10] County-specific term α_c captures the differences in portfolio characteristics between counties and the possibility that high stock market wealth areas have systematically better portfolios as noted by Fagereng et al. (2020). Since I do not observe α_c and $e_{c,t-1,t}$, I follow CRNS and use $r^E_{c,t-1,t} = r_{t-1,t} + b_{c,t-1}(r^m_{t-1,t} - r_{t-1,t})$ as a proxy for $r^{E*}_{c,t-1,t}$. Given that my county fixed effects in the specification below absorb the permanent heterogeneity between regions in α_c and idiosyncratic variation in $e_{c,t-1,t}$ has no effect on estimates since it is uncorrelated with our main regressors, this proxy is likely not to generate measurement error. CRNS also test the probability that high-wealth areas have systematically riskier or less risky portfolios beyond the correlation due to age, which would lead to mismeasurement of $b_{c,t-1}$ but show that there is the nearly flat relationship between wealth and beta within age bins. So overall this source of heterogeneity does not appear to be important in practice.

B. Deposits and bond holdings and their rates of return

In this paper, I divide the fixed-income holdings of households into two broad categories: deposits and bond holdings. The capitalization approach works as follows. First I construct the average fixed income yield per county, which is a weighted average of local deposit rate and bond market yield. Second, these county yields are used as capitalization factors for total county interest income reported in IRS-SOI data. Third, I adjust the fixed-income wealth upward to include non-taxable wealth held in pension accounts using portfolio information of different demographic groups from the Survey of Consumer Finances and the county's demographic data from US Census Bureau. Finally, the total fixed-income wealth is allocated to deposit and bond wealth components. For deposit rates, I use quarterly data starting from 1994 based on Federal Financial Institutions Examination Council's (FFIEC) Consolidated Report of Condition and Income (or "Call Reports") that all regulated financial institutions need to file periodically. Since the Call Reports capture essentially all the banks in the US, we can use this data to calculate the rates that

[10]CRNS calculate the betas for each county and year using the relationship between beta and age based on a dataset of Barber and Odean (2000) and measure of the county age-wealth distribution, which is then used to construct wealth and age-weighted average beta for each county based on county demographics.

each bank pay on their deposits. Then these bank-specific deposit rates are allocated to counties based on the county's exposure to each bank based on FDIC Summary of Deposit (SOD) data, which contains information about the size of deposits of each branch in a particular county and which bank operates that branch.

For bond return estimates, $r^B_{t-1,t}$, I use the Barclays Aggregate Bond index obtained from the Thomson Reuters Eikon database. This index is based on bonds with a maturity of five years, which corresponds to my assumption that the bond wealth is held in bonds with a maturity of five years on average. The monthly series is converted to quarterly, which is the sampling frequency used in this study.

C. Household debt and mortgage rates

The county-level household debt data is from Equifax through Fed Enhanced Financial accounts. The Fed reports the debt-to-income ratios. These are calculated using the Bureau of Labor Statistics's (BLS) total annual wage data so I simply multiply each county's quarterly debt-to-income ratios with the same BLS quarterly incomes aggregated to the annual level, to obtain the underlying household debt amounts. The national average of these series matches very closely to those in the Fed Financial Accounts. Finally, I seasonally adjust the county-level series with U.S. Census's X-13 ARIMA-SEATS filter.

The majority of this household debt is in the form of mortgages. Also, county-specific interest rates on credit cards and auto loans are hard to get at a county level. For these two reasons, I approximate the cost of debt on all household debt with mortgage rates, which can be more easily constructed from public sources. Namely, I use are Fannie Mae's Single Family Loan Performance Data and Freddie Mac's Single Family Loan-Level Data Set. The population of both data sets includes a subset of the 30-year, fully amortizing, full documentation, single-family, conventional fixed-rate mortgages acquired by the government-sponsored enterprises (GSEs) between 1999 Q1 and 2020 Q2. The data includes information about the time of the loan origination as well as the quarterly reported loan performance. Some institutional details about these data and how I use them to construct quarterly county-level mortgage rates are allocated to Appendix B.[11]

[11]There are a few caveats that the data cannot take into account. First, the data indicate only where the loan is originated not where the borrower lives through the whole lifetime of the mortgage. So if people move to a new county before the loan is fully repaid then the remaining cost of the loan that a family needs to pay is misallocated to the old county instead of the new one. Since I do not observe the current home county of each mortgagor, only the loan origination county, I cannot address this possibility. However, given that usually people often fully pay out their old mortgage when selling the old house and take a new mortgage loan for the new house, this should eliminate the issue. Second, GSE data may not represent the cost of debt for most uncreditworthy borrowers i.e. those households that are not eligible for getting a government guarantee on their loan due to regulatory frictions related to imperfect documentation. In undocumented results, I adjust the rates upward based on the fraction of subprime borrowers in each county, who usually have been more likely to take mortgages through the private markets because they are more likely to be ineligible for the government guarantee, and find that the

D. Housing market and the rate of return

To construct housing wealth estimates I follow the procedure of Mian and Sufi (2014). I use the term "housing wealth" in this paper to refer to the total market value of housing, which also contains the levered component financed by mortgages and the home equity component. As we saw in the model, it is important to analyze the total market value while controlling the mortgage debt separately since the shocks to these two assets may produce differing consumption responses. I get median house prices per county for 2019 from the National Association of Realtors that is based on data from the American Community Survey and Federal Housing Finance Agency (FHFA) data[12]. I then use FHFA county house price index (HPI) to extrapolate the median price to history. If FHFA index is not available I use Zillow Home Value Index (ZHVI) instead. These price series are again interpolated to get quarterly series and then multiplied by the corresponding number of housing units in a county in a given quarter to obtain estimates of the market capitalization.[13] Additional details are allocated to Appendix B.

Return data for the county's housing market correspondingly comes from FHFA and Zillow. The implicit assumption in the empirical setting is that if people own residential real estate investments in addition to the houses they occupy these housing units locate within the same county. This assumption is backed by the evidence that most households make their real estate investments near themselves. I also ignore the rent yield component of the return which is unavailable for most counties.[14]

E. Outcome variables

Throughout the paper, I use two dependent variables: log change in quarterly payroll (wages & salaries) per working-age population and employment per working-age population in a county. For the

results for mortgage effects are quantitatively similar or even slightly larger than in the baseline case. Finally, it is useful to emphasize that this data is only based on interest rates on fixed-rate mortgages (FRM). Thus the effect of adjustable rate mortgages (ARM) is not measured. Furthermore, I cannot separate the debt data into ARM and FRM parts, so it is assumed that all household debt is held in fixed-rate mortgages. Fortunately, a large majority of the mortgages in the US indeed are FRMs, which mitigates the measurement error arising from this lack of ARM information. What is important to note, however, is that the county-level mortgage rate that I construct is a value-weighted average of the interest rate that individuals in that county currently pay on their remaining mortgages. How the rate is constructed thus captures how much the borrowers in a county on average actually pay interest on their outstanding mortgages—which is what we are mostly interested in—but not what would be the current interest rate (or refinancing rate) for a newly issued mortgage. This implies that changes in this average fixed mortgage rate occur because old mortgages (with potentially higher/or lower rates) are paid back, refinancing occurs for some loans, or new mortgages with new rates are issued.

[12] https://web.archive.org/web/20201122165343/https://www.nar.realtor/research-and-statistics/housing-statistics/county-median-home-prices-and-monthly-mortgage-payment

[13] I obtain the county-specific housing unit estimates from the US Census intercensal estimates for 2000-2009 and 2010-2019.

[14] My primary return data is calculated from Zillow ZHVI since it reports returns on a monthly basis, which can easily be converted to quarterly growth rates. If ZHVI has missing observations for some counties but FHFA HPI series do exist, I use FHFA data instead and calculate the quarterly series by interpolating the FHFA annual returns in this period following CRNS. The interpolation may introduce a small measurement error in the quarterly returns but the instrumental variable technique should account for this measurement error.

numerator of level series, I obtain payroll and employment data from Bureau of Labor Statistics' Quarterly Census of Wages and Employment (QCEW). The source data for the QCEW are quarterly reports filed with state employment security agencies by all employers covered by unemployment insurance (UI) laws. The QCEW covers roughly 95% of total employment and payroll, making the dataset a near universe of administrative employment records. I use the NAICS version of the data and, following CRNS, seasonally adjust the county-level payroll and employment data.

For the denominator of the level series, I obtain annual population estimates from the US Census Bureau, which are then interpolated to quarterly frequency. The payroll and employment figures are divided by the working age (15-69 years old) population, which provides a clean comparison of average labor market changes for counties that may be very different sized or have very different population demographics. This also partly protects the inferences from subsequent migration (or fertility and mortality) patterns that correlate with wealth shocks. Later, I will also test the effects of these wealth shocks on migration more formally.[15] Some details about the transcript errors in the QCEW data are allocated to Appendix B. We use some additional analysis using migration IRS-SOI Migration Data and the details relating to constructing the variables are presented in Section VI.

F. Summary statistics

After collecting and merging the data of different wealth series[16] I divide the total wealth per asset class in a county with quarterly total wages (i.e. payroll) to get the asset's wealth-to-labor income ratio for each county.[17]

Table I presents the summary statistics of the most relevant data series. We see that within each asset class there is considerable heterogeneity in each asset's wealth-to-labor income ratios and majority of this variation originates from the cross-section. I present some figures in the Internet Appendix that show using maps how the asset wealth varies between different counties. I utilize this heterogeneity in asset ownership between areas for causal identification.

[15]CRNS use total payroll and employment in absolute terms as their dependent variable and weight the observations in the regression with 2010 population estimates. This approach does not take into account changes in employment demographics, migration patterns nor differences in age distribution between counties. In addition, I find that the weighted regression estimates produce somewhat unstable estimates across horizons. Thus I settle on using per capita employment and payroll estimates as my dependent variables.

[16]Appendix B shows that when we aggregate county-level wealth series to the national level, they aggregate seem to match reasonably well the market values reported in Fed's Financial Accounts. This provides external validity that our capitalization approach is reasonably accurate.

[17]Note that CRNS use *annual* labor income in the denominator, whereas I use *quarterly* to stay consistent with my model and ease the quantitative interpretation. The latter approach just scales the estimates down by a factor of four but does not affect the results.

[Table I here]

The data that I use in this study run from 2001Q1 to 2019Q2, though with local projection the responses in wages and employment 9 quarters forward use data from as late as 2021Q3. The starting quarter for the sample, 2001Q1, is determined by two factors. First, the length of the county wealth series varies between asset classes. The longest, like the stock market wealth series, goes back in time until 1989 but with housing wealth and mortgage rates and household debt, for most counties, there exist a series only from around the year 2000 onwards. Second, the transition in industry classifications from SIC to NAICS caused a big discontinuity in employment and payroll series at the NAICS 2-digit industry level between 2000Q4 and 2001Q1. To ensure this artificial jump in tradable and non-tradable industry wage and employment series that obviously is not related to changes in actual economic conditions does not drive the results I use only data after the year 2000. Given that there were already very few observations before 2001, the loss in sample size is quite small. Note also that county-level HPI indices are not unavailable for all counties preventing the construction of housing wealth, which leaves me 2,715 unique counties.

V. Empirical methodology

One of the main identification challenges in estimating the causal effect of the return on capital is the so-called leading indicator issue. Return on capital may move today if people hear positive news regarding their future income. This means that the return on capital today may be endogenous with respect to future wages. For example, assume households in some regions hear the news that makes them expect wages to grow in the future. Hearing this positive news they may bid up local house prices today. This mechanically creates a sort of lead-lag relationship between house prices and wages though in fact the former had no role in causing the latter.

This means that running a simple OLS regression model may not incorporate all the necessary variables that are crucial in determining future wage growth and if it would we would not know when this is the case. However, using a suitable instrument for capital growth that is uncorrelated with the unobservable shocks will create the needed exogenous variation that we need to isolate the effects of capital on the local labor market outcomes. The *national* return on a specific form of capital can act as an instrument since it is likely to be strongly correlated with local capital shocks (thus satisfying the relevance condition) but simultaneously being independent of local labor market outcomes (thus satisfying the exclusion restriction). The latter condition is satisfied because local county-level shocks or news independently cannot have more than a tiny impact on aggregate shocks, which are determined at the national level. This idea is behind the Bartik (1991), also known as the Shift-Share, instrumental variables (IV) identification technique that has received much attention in recent literature. My empirical methodology uses this idea to estimate the causal effects. However, before I delve into details I first present the model specification in OLS form to point out what are the aspects that already work and what aspects we need to correct for. Then I show how to use Bartik IV in two-stage least squares (2SLS) regression design that improves the OLS model and how the second-stage regression of 2SLS is a particularly credible way to capture the causal effects of wealth shocks.

Motivated by this discussion and the expression for payroll growth derived in the model in Section III the OLS model takes the following form:

$$\Delta_{c,t-1,t+h}y = \sum_{f} \beta_f^h s_{f,c,t-1} r_{f,c,t-1,t} + {C_{c,t-1}^{y}}'\Gamma + \mu_c + \mu_{st} + \epsilon_{c,t-1,t+h} \tag{12}$$

$\Delta_{c,t-1,t+h}y$ is the difference between log outcome variable $y_{c,t}$ between periods $t-1$ and $t+h$ where I consider values of h from 0 to 8. For example with payroll, $\Delta_{c,t-1,t+h}y = \log\left(WL_{c,t+h}^{pc}\right) - \log\left(WL_{c,t-1}^{pc}\right)$ where $WL_{c,t-1}^{pc}$ is the per capita quarterly payroll in county c at quarter $t-1$. $s_{f,c,t-1}$ is the wealth-to-labor income ratio of asset f in a county c at the end of quarter $t-1$, this corresponds to term $\frac{V_t^f}{W_t L_t}$, while $r_{f,c,t-1,t}$ is the net return on that asset in quarter t and it corresponds to term dR_t^f in the Equation (11). The main variables of interest are β_f^h for all f where superscript h denotes the length of the response period we are investigating. These long horizon estimates correspond to local projections by Jordà (2005). The covariate matrix $C_{c,t-1}^{y}$ includes additional covariates. Since the model implies that it is in fact the interaction of $s_{f,t-1}$ and $r_{f,c,t-1,t}$, which is the main variable of interest I put the individual components of this interaction also into $C_{c,t-1}^{y}$ in case they are not absorbed by the fixed effects. μ_c and μ_{st} are county and state-time fixed effects respectively. $\epsilon_{c,t-1,t+h}$ is the error term. Now even this OLS model can account for many factors that may undermine a simpler research design. $C_{c,t-1}^{y}$ can include as many observable confounders as deemed necessary, μ_c controls for the permanent differences between counties that may relate to things like demographic factors, human capital or social norms, and μ_{st} absorbs all the variation that does not happen strictly at the local level and thus control for effects of aggregate or state level shocks like changes in interest rates or state-level legislation changes. The advantage of panel data with a wide cross-section is that $state \times time + county$ fixed effects can diminish the set of possible explanations. On the other hand, the flip side is that any national general equilibrium level effects are also absorbed by μ_{st}. Guren et al. (2020b) discuss this in detail.

Now as emphasized before, this OLS setup is prone to factors that may cause bias in our estimates of β_f^hs. Specifically, this happens when all $s_{f,t-1}$ and $r_{f,c,t-1,t}$ are endogenous and thus the interactions $s_{f,t-1}r_{f,c,t-1,t}$ are correlated with the error term. I next show how the Bartik IV setup provides a plausibly exogenous variation that corrects this.

A. 2SLS with Bartik instrument

We would like to estimate parameters β_f^h for our main variables of interest $x_{f,c,t-1,t}$ defined as

$$x_{f,c,t-1,t} := s_{f,c,t-1}r_{f,c,t-1,t} \tag{13}$$

Now a local wealth return of asset f is generally assumed to depend on exogenous *national* return $r_{f,t-1,t}$ and a local idiosyncratic shock $\tilde{e}_{f,c,t-1,t}$ that may react endogenously to local economic environment. If we, for simplicity, ignore the intercept and covariate terms and assume homogeneity in b_f across counties we can write that $r_{f,c,t-1,t} = b_f r_{f,t-1,t} + \tilde{e}_{f,c,t-1,t}$. Given that $b_f > 0$ since the asset rates/returns across counties are highly correlated with the national rates/return, we can form a Bartik instrument $z_{f,c,t-1,t}$ for $x_{f,c,t-1,t}$ as

$$z_{f,c,t-1,t} = s_{f,c,t-1}r_{f,t-1,t} \tag{14}$$

to estimate the β_f^h. $z_{f,c,t-1,t}$ satisfies the relevance condition since $Cov(s_{f,c,t-1}r_{f,c,t-1,t}, s_{f,c,t-1}r_{f,t-1,t}) = b_f Var(s_{f,c,t-1}r_{f,t-1,t}) > 0$, which holds if $Cov(s_{f,c,t-1}r_{f,t-1,t}, s_{f,c,t-1}\tilde{e}_{f,c,t-1,t}) = 0$ —a natural assumption given the orthogonality of $r_{f,t-1,t}$ and $\tilde{e}_{f,c,t-1,t}$.[18] On the other hand the exclusion restriction and the iden-

[18]In a setup where b_f is homogeneous across counties, the asymptotic estimate from our first stage regression would match this parameter. If b_fs are allowed to vary across counties, $z_{f,c,t-1,t}$ would still work as a valid instrument as long as $Cov(s_{f,c,t-1}r_{f,c,t-1,t}, s_{f,c,t-1}b_{f,c}r_{f,t-1,t}) > 0$ and $Cov(s_{f,c,t-1}b_{f,c}r_{f,t-1,t}, s_{f,c,t-1}\tilde{e}_{f,c,t-1,t}) = 0$. In addition, knowing $b_{f,c}$ we could construct even stronger instrument using $s_{f,c,t-1}b_{f,c}r_{f,t-1,t}$ instead of $s_{f,c,t-1}r_{f,t-1,t}$. Specifically following Guren et al. (2020a) we could obtain estimates of county-level $b_{f,c}$ by regressing $r_{f,c,t-1,t}$ on $r_{f,t-1,t}$ while controlling for possible confounders, for each county c and asset f and then collecting the regression coefficient of $r_{f,t-1,t}$, denoted as $\hat{b}_{f,c}$, and using it as an estimate of $b_{f,c}$. When I do this exercise the results are practically unchanged from the base case where $b_{f,c}$ is treated as homogeneous. This is because the strength of the instrument is not a problem in either case: $r_{f,c,t-1,t}$ and $r_{f,t-1,t}$ are highly correlated with or without allowing for different county-elasticities, $b_{f,c}$ or controlling for additional covariates. Furthermore, since this procedure is possible only when we observe $r_{f,c,t-1,t}$ it would not be possible for the stock market and bond market return. However, these assets in general receive special treatment since for the stocks we have constructed each county's stock market beta following CRNS —which indeed is allowed to vary across counties and time— and bond market wealth return is assumed to be the same across counties in the absence of other information. These differences are

tification assumptions of shift-share research design have received attention in recent literature. In general, the identifying assumptions can be divided into those that assume exogeneity of shifters (Borusyak, Hull, and Jaravel, 2022) and those assuming exogeneity of shares (Goldsmith-Pinkham, Sorkin, and Swift, 2020). With my setup the exogeneity of shifters is the more natural assumption since asset-to-labor-income ratios, $s_{f,t-1}$, may correlate systematically with county demographics.

Though we have multiple endogenous variables and multiple instruments, theoretically the crucial part is to use at least the corresponding instrument for each asset f. This alone ensures that each endogenous variable has at least one very strong instrument. After the first stage regressions, I estimate all the parameters β_f^h simultaneously for all f in a single second stage regression. So to spell this out, this means that our first stage for asset f takes the following form

$$x_{f,c,t-1,t} = \sum_f b_f z_{f,c,t-1,t} + C_{c,t-1}^{y}{}'\Gamma + \mu_c + \mu_{st} + \epsilon_{c,t-1,t} \tag{15}$$

where the first term contains the included Bartik instruments of which asset f is naturally the most important, matrix $C_{c,t-1}^{y}$ includes the controls of the second stage that we treat as predetermined. As we will see when analyzing the results, since $z_{f,c,t-1,t}$ is highly correlated with $x_{f,c,t-1,t}$ the first stage Wald-statistic is very large and the relevance condition of the instrument is not a problem.

Now to obtain consistent estimates β_f^h in the second stage, we require that for each asset f the following condition holds

$$E[r_{f,t-1,t}\xi_{f,t-1,t+h}] = 0 \tag{16}$$

where $\xi_{f,t-1,t+h} = \sum_c s_{f,c,t-1}\epsilon_{c,t-1,t+h}$ is the cross sectional average across counties at any time t and horizon h. This condition coincides with the exogeneity condition in Borusyak, Hull, and Jaravel (2022) with a single observed national shock and multiple (asymptotically infinite) areas and time periods. The estimates are consistent since the long time series dimension T and the wide cross-sectional county dimension N produce the needed asymptotics. Intuitively, this condition will not hold if the outcome variable (employment or payroll per capita) grows faster (corresp. slower) in areas with high (corresp. low) wealth-to-labor income ratios ($s_{f,c,t-1}$) for asset f for reasons that are not controlled in the specification ($\epsilon_{c,t-1,t+h}$) following a high (corresp. low) return on asset f ($r_{f,t-1,t}$). Importantly, it is not required that

further discussed below.

the wealth-to-labor-income ratios of asset f, $s_{f,c,t-1}$, are distributed randomly across counties. Indeed, it is plausible that the wealth levels are likely to correlate systematically with county demographics like the average education level and the share of retirees in the population.

Though the same exogeneity condition needs to hold for each f separately independently of other assets, and the identification comes from the same idea, we can in general divide the assets into two categories. Those assets where $r_{f,c,t-1,t}$ can be directly replaced by $r_{f,t-1,t}$ or by its transformation —as done by CRNS with the stock returns—and those assets where $r_{f,c,t-1,t}$ are explicitly instrumented with $r_{f,t-1,t}$. Stocks and bonds are in the former category. For bonds I assume that all counties hold the same bond portfolio and thus for bond return, it holds $r_{B,c,t-1,t} = r_{B,t-1,t}$, where the national return is assumed to be exogenous. The stock market returns are given as $r_{E,c,t-1,t} = b_{c,t}r_{E,t-1,t} + (1 - b_{c,t})r_{t-1,t}$ but since CRNS show betas are closely concentrated around one, we can approximate the equity shift-share $r_{E,c,t-1,t}s_{E,c,t-1} \approx r_{E,t-1,t}b_{c,t}s_{E,c,t-1}$, which means that if ones use the transformed shares $b_{c,t}s_{E,c,t-1}$ instead of $s_{E,c,t-1}$ then the equity market return $r_{E,t-1,t}$ also acts as the single (national) shifter. For these assets, the first stage is thus explicitly skipped. On the other hand, the category of assets where detailed data on local rates and returns is available, $s_{f,c,t-1}r_{f,c,t-1,t}$ are directly instrumented with $s_{f,c,t-1}r_{f,t-1,t}$. Deposits, household debt, and housing wealth are such assets. The national rates $r_{f,t-1,t}$ are obtained simply by averaging across county rates using the county's share of asset wealth (or debt) f out of total national wealth (or debt) f as weights. I show in the Appendix C how to formally derive the exogeneity condition and how the exogeneity condition is unchanged for the asset classes that are explicitly and implicitly instrumented with national rates of return.[19]

B. *Baseline specification*

My baseline specification considers the effect of wealth shock eight quarters forward (h=7) –which is the same as in CRNS– with outcome variable, y either log employment per capita or log quarterly payroll per capita. I include the following controls: county fixed effects, a state $\times$ quarter fixed effects, individual components $s_{f,c,t-1}$ and $r_{f,c,t-1,t}$ of the interaction terms, four lags of the instrumental variables, $\{s_{f,c,t-j-1}r_{f,t-j-1,t-j}\}_{j=1}^{4}$, Bartik shift-share measure of *predicted* employment growth based only

[19]Note that when constructing the national shifters some studies aggregate the local shifters while leaving out the particular local shifter that is being instrumented. Since my cross-section is wide and very granular, even the largest counties have a tiny weight in the aggregation, so this methodological choice has negligible impact.

on industry composition and four lagged outcomes $\{y_{c,t-j}\}_{j=1}^{4}$.[20] [21] I report standard errors two-way clustered by time and county. Clustering by county accounts for any residual serial correlation in asset returns within the same county. This might have less of an impact on stock returns with low serial correlation but a higher corrective effect for more persistent returns like interest rates and housing prices. Clustering by time allows for counties with high or low wealth in asset f to experience other common shocks. This follows with the recommendation of Adão, Kolesár, and Morales (2019) in the special case of a single national shifter.

C. Threats to identification and motivation for covariates

The identifying assumption requires that following a positive aggregate return on that asset f, counties with high wealth-to-labor-income ratios for that asset f do not experience unusually rapid payroll or employment growth —relative to their average and other counties in the same state and conditional on additional covariates— for reasons other than the wealth effect on local consumption expenditure. As in CRNS there exist two main threats to the identification. The first threat relates to the leading indicator channel discussed earlier. Wealth changes may react in a forward-looking manner to changes in expectations about future wages and employment. This possibility confounds the interpretation of the wealth effects in time-series studies. My cross-sectional research design requires only the weaker condition that counties with high and low wealth-to-labor-income ratios of asset f do not load differently on other aggregate factors that might co-move with the return on that asset – conditional on controlling all the other assets f and their wealth-to-income ratios.

To ensure that my instrumented shocks indeed satisfy the conditions that ensure a valid identification with local projection with instrumental variables (LP-IV) methodology, I follow the advice in Stock and Watson (2018) and include four lags of shocks, $z_{f,c,t-1,t}$, to ensure that the instrument is uncorrelated with past treatment shocks and four lags of the outcome variable $y_{c,t}$ to control for any pre-trends in payroll and employment. Bartik predicted employment growth captures the impact of industry-level growth that

[20] I denote the Bartik shift-shares predicted employment growth between $t-1$ and $t+h$ as $e^{B}_{c,t-1,t+h}$, which is defined as

$$e^{B}_{c,t-1,t+h} = \sum_{i \in NAICS3} \left(\frac{E_{c,i,t-1}}{E_{c,t-1}}\right)\left(\frac{E_{i,t+h} - E_{i,t-1}}{E_{i,t-1}}\right) \tag{17}$$

where $E_{c,i,t}$ denotes the (seasonally unadjusted) level of employment in NAICS 3-digit industry i in county c in period t. $E_{c,t}$ is the total employment in county c and $E_{i,t}$ is the seasonally-adjusted total national employment in industry i. When using sub-sectors of specific industries, like the non-tradables, the same idea is applied to total employment in that industry group.

[21] I include lags of the dependent variable levels and county fixed effect because of the large time dimension (roughly 70 quarters) of the data (Alvarez and Arellano, 2003). Large T also implies the Nickell bias becomes small (Nickell, 1981).

may affect regions with a specific kind of industry structure. This Bartik variable, for example, controls the possibility that stock market wealth concentrates on counties with more industries with high stock market betas, or that fixed income wealth concentrates on counties with more industries whose corporate financing activities are less sensitive to interest rates. This variable, proposed by CRNS, is also useful since its forward-looking nature makes it a good proxy of the *expected* growth in employment, although using a national shifter in our instrumental variable strategy is already robust to such feedback loop considerations. Finally, the county fixed effects capture the unobservable permanent differences between counties, for example, those arising from differences in historical or cultural factors that may cause non-wealth-driven differences between local labor market outcomes. State $\times$ time fixed effects control for time-varying aggregate effects and changes in state-level factors like regulatory environment that may affect labor market outcomes.

The second threat to identification relates to how to disentangle the consumption wealth effects from firm investment and hiring effects. This means that firms may adjust their wage setting and employment demand based on changes in the cost of equity and debt financing. In general, the aggregate time series analysis cannot separate the two effects from each other. However, with a wide cross-section, we can control for time-fixed effects that absorb any changes in the cost of equity and debt that are common across counties. The interaction with state fixed effects allows an even more accurate way to control this channel –especially if firms located in different states have time-varying differences in their cost of capital that relate to their home state. However, if these differences operate at an even more granular level and if counties with high stock market wealth have firms with the cost of capital more sensitive to the value of the stock market or counties with high fixed income wealth have firms that are more sensitive to interest rates then my estimates may capture not only the effects of consumption wealth channel but the aggregate effects of both consumption wealth channel and this corporate financing channel. The related collateral lending channel (Adelino, Schoar, and Severino, 2015) could likewise pose problems for identifying the consumption-wealth effects. The idea is that if most households and firms are credit-constrained then increases in asset prices will increase collateral values, which allow firms and starting entrepreneurs to obtain more financing that can subsequently be used for hiring more employees.

However, there are reasons to believe that it is household wealth effects that generate the responses in payroll and employment that I find. For one, if the investment-hiring channel—regardless of whether driven by collateral values or changes in the cost of capital—is at play it would equally affect firms operating in tradable, non-tradable, and construction sectors. However, I find essentially no effect of wealth shocks in tradable industries while non-tradable and, construction sectors are more likely to generate statistically and economically significant responses from wealth shocks, which is consistent with the wealth effect story. This alleviates the concerns that my estimates capture effects that are not related to the wealth-consumption channel.

The final issue that should be noted is that this discussion so far has only concentrated on the literature on wealth effects have an impact on labor demand. However, household wealth shocks may also affect employment through the labor supply channel. For example, prior work suggests that exogenous increases in wealth, via lottery winnings (Imbens, Rubin, and Sacerdote, 2001; Cesarini et al., 2017) or inheritance windfalls (Joulfaian and Wilhelm, 1994), reduce labor supply. Also, negative home equity may cause a reduction in labor supply (Bernstein, 2021). This means that the expected positive relationship between wealth shocks and employment outcomes that work through the labor demand channel might be mitigated or even reversed by the decrease in labor supply. This means that even in the tradable sector wealth effect can affect employment if the wealth shocks specifically hit the employees or potential employees of tradable industries.

Although it is difficult to separate the effects that arise due to labor supply and labor demand reasons, the results show no evidence of the labor supply moving and at minimum, we can say that the labor demand mechanism clearly dominates. This is because if the wealth shocks operate mainly through the labor demand channel, employment should move in the same direction of the wealth shock caused by changes in the labor demand curve. However, if the wealth effects operate only via the labor supply channel we should see a reduction in employment following a positive wealth shock since people would substitute consumption for leisure. Given that I find that the responses from wealth shocks consistently generate the same signed change in employment, I conclude that if any labor supply channel is at work, it seems to be of second-order importance to that caused by the labor demand channel. Furthermore in most of our results, payroll responds more than employment, reflecting either rising hours per employee or rising compensation per hour following a positive wealth shock. In the former case, it is the exact *opposite* of what should happen if workers were deciding to work less after becoming wealthier. In the latter case, then it could theoretically be caused either by an increase in labor demand or a decrease in labor supply but again since employment reaction is also positive, the former channel clearly dominates. Guren et al. (2020b) also argue that wealth effects have no impact on labor supply is a reasonable assumption –especially in the short run.

VI. Results

In this section, I investigate the wealth effects simultaneously for each asset f using the econometric framework detailed in the previous section. For completeness, I will start by presenting the results for the aggregate payroll per capita growth and employment per capita growth. However, based on economic theory these estimates may be noisy since wealth effects are assumed to operate specifically through the non-tradable and the construction sector but *not* through the tradable sector. Indeed, I find evidence of this below in Section VI.A.

[Table II here]

Table II shows these aggregate results. Before analyzing the estimates I emphasize a few statistics relating to the fit of our IV estimation. First, results from Wu-Hausman test suggest that endogeneity is indeed a problem with OLS and we can reject the null that there is no endogeneity in the OLS estimation. Second, the large first-stage Wald statistics suggest that none of our instruments is weak.[22] With these concerns out of the way, we can dive into analyzing the estimates from the second stage of the 2SLS estimation.

The interpretation of the estimates is as follows. β_f^h reveals what is the log change in outcome in a given quarter $t+h$ following a return or a rate shock that in dollar terms equals one quarter's labor income. The alternative formulation would be to state that β_f^h reveals what is the log point increase in outcome variable in quarter $t+h$ following wealth shock in asset f that absence of any other wealth shock doubles the county's income for the period t. The implicit assumption in these statements is that treatment effects are homogenous across counties and across time but are allowed to vary between asset classes. In Section VI.D below, I will discuss how relaxing this assumption affects the interpretation of these estimates.

Columns (1) and (4) report the results from the baseline specification for payroll growth and employment respectively, while the other columns show what happens if we exclude some controls. Although these alternative specifications in columns (2-3) and (5-6) can be considered to have issues that endanger a valid causal inference, they can still indicate to what degree the main results arise due to the correlation between the covariates and the treatment. The first row reports the housing wealth effects. We see that all columns except the first show results that are strongly statistically significant but the size of economic

[22]The test statistics from both tests in all specifications are statistically significant at 10% level at least.

significance is somewhat sensitive to controls. Economic interpretation is that a county that experiences an increase in housing wealth in quarter t that is the size of the total quarterly labor income will experience approximately 1.7 and 1.5 percentage points higher payroll and employment growth per capita by the quarter $t+7$ than a county with no increase in housing wealth. The payroll number is not statistically significant but the impulse response functions presented below suggest this lack of power only concerns horizon $h = 7$. The second row presents the wealth effect of changes in mortgage rates. This household debt channel shows relatively large negative effects (although not always statistically significant). But thinking about these effects is now a bit different than with housing wealth. Given that the shifters are now the average level of interest rate that the mortgagors in a county c have to pay between time $t-1$ to t, the shock does not represent the effect of increasing debt but how much the combination of high-interest rates with large debt burden weighs down the employment and payroll growth relative to an identical county that does not suffer from interest payments. The coefficient β_f^h now represents how much the outcome is lower in a quarter $t+h$ following an interest rate payment in quarter t that amounts to the total labor income from that period. Of course, thinking about interest payments that would be the size of the total county labor income is not reasonable, since if there was no additional income besides wages then all the income would go to interest payments. Instead, we can scale these figures to match some realistic interest payment sizes. For example, by dividing the estimates by ten, we can interpret the estimates as meaning that having a quarterly mortgage payment that equal 10% of the total quarterly labor income at quarter t causes the total payroll and employment per capita to be approximately 2.5 and 2.1 basis points lower in the quarter $t+7$ respectively relative to a county with no quarterly interest rate payments. These magnitudes indicate that household debt overhang can indeed have sizeable effects on an area's labor market outcomes. The third row tabulates the wealth effect from changes in deposit rates. Now we should interpret these estimates as telling how much payroll and employment per capita are larger in the quarter $t+7$ for a county that obtains an additional interest rate payment on its deposit amounting to a total quarterly labor income at quarter t relative to a county that obtains no additional income from deposit rates. The table shows no well-identified effects for payroll and employment per capita. Lastly rows 4 and 5 report estimates from shocks to bond and stock market wealth. The interpretation for coefficients is the same as it is with housing wealth effects. Also with these assets, we find no effects that is statistically significant at 5% level.

A convenient way to visualize the results when investigating the temporal variation of the effects across many horizons is to plot the impulse response from these local projections. Figure 3 does just that for each wealth shock. The graphs illustrate how much of the payroll and employment responses occur at period $h = 0$ when the shock occurs and how these responses evolve in subsequent quarters. The point estimates are depicted with a blue line, while the grey area around it represents the 95% confidence intervals using the two-way clustered standard errors. We see that for the asset classes where evidence of wealth effects was found the effects seem to be persistent: the impact of neither housing or mortgage shock has not drifted back to zero even after 2 years.

[Figure 3 here]

A. Heterogeneity in responses between industries

Now after establishing that wealth shocks seem to matter for aggregate county-level labor market outcomes, I will dig deeper to understand from which industries these effects originate. A natural extension is to investigate the differential response of these wealth shocks between tradable, non-tradable, and construction sectors. The idea that wealth shocks should have a larger effect on non-tradable industries originates from economic theory suggesting that higher income mainly increases consumption of non-tradable goods that are consumed locally and this idea is tested by several previous studies. Tradable industries on other hand produce goods that are mainly shipped away and thus should not be affected by the purchasing power of local consumers. Finally, the construction sector is also likely to be positively affected by increases in local household wealth since it can generate demand for the construction in the local economy for example in the form of more expensive residential houses.

I follow Mian and Sufi (2014) and CRNS and include NAICS 44-45 (retail trade) and 72 (food and accommodation) to non-tradable industries while for tradable industries I denote those that belong to NAICS 11 (agriculture, forestry, fishing and hunting), NAICS 21 (mining, quarrying, and oil and gas extraction), and NAICS 31-33 (manufacturing). Finally, I allocate NAICS 23 into the construction sector.

[Table III here]

Table III shows the results from regressions for non-tradable industries. For the stock market wealth we now find statistically significant effects for payroll. The fact that stock market wealth effects originate from non-tradable industries verifies the findings of CRNS. Conversely, for the housing wealth and mortgages, we see not effects in non-tradables, which suggest that with these assets the biggest driver of the aggregate county outcomes does not come from non-tradable industries.

Figure 4 plots the impulse response to payroll and employment from return or rate shock 9 quarters forward now for non-tradable industries only. The confidence intervals are large for most shocks so it is hard to claim anything certain about the impulse responses except about the impact of stock market wealth on per capita payroll.

[Figure 4 here]

Table IV tabulates the results from regressions for tradable industries. We find that the only significant variable with baseline specification is the bond market return, which is in fact negative at the horizon $h = 7$ for employment per capita growth. However as Figure 5 shows this finding concentrates for that particular horizon and in the next section we find its statistical significance to be somewhat sensitive to controls so in general, we can conclude that there is little evidence that the wealth shocks affect local labor market outcomes of tradable industries.

[Table IV here]

[Figure 5 here]

Table V tabulates the results from regressions for the construction sector.

[Table V here]

I find large and statistically significant results from housing wealth shocks, which thus reveal that the effect of housing wealth shock on aggregate outcomes must originate from the construction sector. We can interpret these effects as follows: Increases in the level of local house prices are likely to make construction projects more profitable. Specifically, a higher level of wealth for the consumers allows them to afford more expensive houses. This means higher profits and more profitable investment opportunities for the construction sector, which translate to more hiring. Note, that though this is not the traditional wealth effect channel as with more conventional product/service markets, the increase in hiring does not arise due to lower cost of capital to construction firms but due to higher prices in the product (now same as housing) markets generated by the wealth shock experienced by the customer base. Overall these results imply that the wealth effects from housing arise mostly from the construction sector. We also find that household debt and stock market wealth shocks have large negative and small positive effects respectively on the construction sector's employment. This implies that higher stock market wealth and lower interest rate expenses likely also translate to increased consumption of housing services, which subsequently supports construction sector employment and wages.[23]

[Figure 6 here]

Overall, the results lend support to the idea that wealth effects should affect the non-tradable industries and construction sector but not the tradable industries. In summary, the housing wealth effect seems to be channeled to the labor market primarily through increased wages and employment in the construction sector. Household debt seems to operate primarily through the construction sector consistent with the idea of negative wealth effect arising from mortgage rate increases. The deposit rate changes seem to have very little impact on the local labor market once controlling for other factors. Consistent with the results of CRNS, stock wealth estimates seem to originate mostly from the non-tradable sector but also in part from the construction sector. This second fact indicates that increases in stock market wealth are likely to generate some extra demand for local residential housing, thus supporting the construction sector.

B. Discussion

As outlined earlier, the motivation for this paper is that comparability between studies is often not possible and there is a risk of omitted variable bias, when focusing on one asset class individually. Now, I will i) discuss how to interpret my results relative to findings in prior work when it is possible and ii) assess how severe the omitted variable bias would have been if we had not conducted the comprehensive evaluation of these wealth effects.

In general these results seem to be qualitatively mostly in line with previous literature, that has found evidence of sizable housing wealth effects (Mian and Sufi, 2014; Guren et al., 2020a), and debt effects

[23] The difference in interpretation of these effects relative to nontradables and tradables is that since the construction sector produces a capital good (housing) that provides a service flow over many years a desire by local residents to increase their consumption of housing services following a positive wealth shock will result in a front-loaded response of employment in the construction sector. Regardless, the large response of residential construction provides evidence of a local demand channel at play.

(Keys, Piskorski, Seru, and Vig, 2012; Keys, Piskorski, Seru, and Yao, 2014; Bhutta and Keys, 2016) and modest or non-existent wealth effects from stock market wealth (Case, Quigley, and Shiller, 2011; Chodorow-Reich, Nenov, and Simsek, 2021).[24] What about quantitative comparison between estimates from different studies? Comparing the stock market wealth estimates to those in CRNS is quite straightforward since my methodology is somewhat similar to theirs. In the next section I offer results that align the small differences in our methodologies and offers directly comparable results. Even without doing this, by comparing horizon-by-horizon results, we will find estimates that are largely similar to those of CRNS. However, comparing between the estimates using the methodology of CRNS to some other methodology —for example those used in Guren et al. (2020a) (GMNS)— is not necessarily possible. To see why consider following equation that arises from models with just one form of capital and is underlying many prior work including GMNS.

$$\Delta y_{c,t-1,t+h} = \eta_f r_{f,c,t-1,t+h} + u_{f,c,t-1,t+h} \tag{18}$$

η_f is the elasticity of outcome with respect to wealth shock ($r_{f,c,t-1,t+h}$) in asset f and $u_{f,c,t-1,t+h}$ is the composite term that includes both controls and error term. Usually annual data is used and $h = 0$. We can immediately see that this is essentially a special case of our methodology if we replace η_f with $\beta_f^{h=0} \times s_{f,c,t-1}$. In other words, studies using the Equation 18 impose a restriction that $s_{f,c,t-1}$ is assumed to be a constant. Now if a study ignores this regional wealth heterogeneity it means that our elasticities cannot be directly compared to theirs. In principle one could try to proxy for η_f using $\beta_f \times \sum_c \sum_t s_{f,c,t-1}$ but if the true model indeed requires conditioning for differences in $s_{f,c,t-1}$ then the regression based on Equation 18 is likely not going to average the observations correctly to produce estimates, $\hat{\eta}_f$ that corresponds to $\beta_f \times \sum_c \sum_t s_{f,c,t-1}$. This is not an isolated problem since for example there are almost as many methodologies as there are papers. Now, these considerations do not mean that using some other specification like the one based on Equation 18 is necessarily wrong but it does prevent the direct comparison between the results since the data-generating process is assumed to be different. Indeed, this actual lack of comparability between most studies is what this paper tries to address.

Next, I will test how much these results would have differed if I had estimated these wealth effects individually i.e. if I had not controlled for wealth shocks in other asset classes. Table VI reiterates the baseline results and then shows what the β_f^h coefficients would have been had we included the shift share

[24]The fact that I find no evidence of wealth effects from fixed income wealth, while seemingly no one has investigated this before could also be evidence of publication bias.

components from just only one asset class at time.

[Table VI here]

There are several takeaways from Table VI. We see that the results can be substantially biased in either direction for many asset classes. For example, when we do not control for other asset classes the mortgage rate shocks are statistically significant in almost column, while actually being so only in the construction sector. Also, the housing wealth effects are severely biased upwards, while stock market wealth effects are biased downward when not controlling for multiple assets. These findings show the importance of controlling for other wealth shocks even when using a credible identification strategy like instrumental variables. This result also indicates that it is possible that prior research may have over-or underestimated the wealth effects for some assets if they have not controlled for other assets in their empirical strategy.

C. Robustness

In this section, I employ several robustness checks. Table VII shows the coefficients from our baseline regression when adjusted in some way. First, an important question to ask is whether should we use population weights. A short answer is no since the unweighted results presented above used payroll and employment *per capita* to ensure that we estimate the impact of wealth shock to average American. If we also weight the observations with the county population we put more weight on individuals and their implicit MPCs that live in larger counties. As noted by Chodorow-Reich (2020) in the presence of heterogeneous treatment effects, weighting by population can also decrease efficiency and, with instrumental variables, increase bias (problematic for us), especially with skewed weights in small samples. However, for the sake of completeness, it is worth presenting the results also from regressions using county population in 2010 as weights, since it may give some guidance whether the effects we obtained originate more from smaller or larger counties.

If we now look at population-weighted results tabulated in the first panel of Table VII we see quite similar estimates as with the unweighted case. This is reassuring since it proves that we see that the effects operate similarly in small and large counties, which supports the interpretation that the structural parameter we try to identify is stable in the cross-section of counties. In the second panel, I control for the interaction of shares $s_{f,c,t-1}$ with aggregate changes in human capital and non-corporate business wealth.[25]. The idea is that since I am controlling for the wealth and shocks to fixed income (bonds and deposits), stock market wealth, housing wealth and the household debt, the parts of household wealth that still remain unaccounted for are human capital and non-corporate business wealth. As shown by CRNS controlling for the interactions of wealth-to-labor income ratios of asset f and the shocks to aggregate human and non-corporate business wealth addresses the concerns that my wealth shocks may capture effects of these alternative forms of wealth.[26] Again the main findings change very little. Motivated by the news-driven business cycles such as in Beaudry and Portier (2006), in the third panel I interact each wealth-to-labor income ratio with Fernald (2014) measure of TFP growth between $t-1$ and $t+7$, but with little effect on estimates. The one exception in all these panels is the over-sensitivity of results from a bond market shock. Note that the standard errors (and occasionally point estimates) of bond shock sometimes appear as highly significant and other times as not at all. As pointed out in the previous section, we should thus be highly skeptical when conducting statistical inference about a bond market shock because of such over-sensitivity to covariates. The fourth and fifth panel considers the possibility that we use non-per capita outcomes without weighting observations and with weighting them with the county 2010 population. As emphasized earlier using non-per capita outcomes exposes the empirical model to the possibility that the absolute level of labor supply changes following a wealth shock due to population change –say migration– while the relative status of the labor market stays unchanged. However, it is worth presenting these results also since especially the specification underlying the results in the fifth panel closely resembles those of the baseline specification in CRNS. Comparing these estimates to theirs is the most direct comparison of the result to prior work that we can have. Specifically we just need to multiply my estimates by four to get corresponding figures to CRNS.[27] Doing so we find that our positive stock market wealth effects are close to those in CRNS –though somewhat smaller– and the fact that the effects appear in non-tradable but not in tradable industries is also in line with their results. For both the fourth and fifth panel, the main results for housing and the stock market change only slightly from my baseline estimates. However, the estimates from mortgage rate shocks become statistically significant in non-tradable and construction sector on employment (fifth panel) and payroll (fourth panel) specifications.

[25]I measure the aggregate changes in human and non-corporate business wealth as CRNS by $\log\left(\sum_{j=0}^{11} R^{-j} A_{t+j}\right) - \log(A_{t-1})$ for A_t= aggregate labor compensation (NIPA code A4002C) or aggregate non-corporate business income (nonfarm sole proprietor income and partnership income, NIPA code A041RC) and a quarterly discount factor $R = 1.03^{1/4}$. The rationale for constructing these two controls in this way is spelled out in CRNS in their footnote 14.

[26]Though capitalizing the business wealth is possible though challenging (Smith, Zidar, and Zwick, 2020), I have chosen to control this wealth channel as CRNS do it since the local shifters of business wealth would still be difficult to identify even if local shares were available.

[27]CRNS uses annual labor income in the denominator of shares whereas I use quarterly labor income.

This means by directly extending the CRNS baseline model to the household debt seems to produce large negative effects. It is possible however that this is due to subsequent migration patterns because we are not using per capita values in our outcomes. Indeed I find evidence of this in the next section. In the sixth panel, I use my baseline specification but remove all the fixed effects. This generally amplifies all the existing results. One could take this as proof that these results are even larger when general equilibrium dynamics are allowed to operate but obviously the endogeneity issues are more prevalent.

Overall, Table VII supports the interpretation that the findings that the housing wealth effect primarily operates through the construction sector and stock market wealth effect both through the construction and the non-tradable sector are robust to different specifications and controls. The effects of household debt, appear when using non-per capita outcomes indicating that the mortgage rate shocks correlate systematically with subsequent population change patterns. Next, I will investigate this further.

[Table VII here]

D. *Heterogeneity in treatment effects*

Now even though we have allowed heterogeneity between asset classes, the current specification assumes that treatment effects, captured by estimates of β_f^h are common across counties and across time. Obviously, this is potentially a strong assumption. To make progress on testing if there exists heterogeneity beyond the current specification, I parameterize the potentially heterogeneous coefficient for asset f as follows

$$\beta_{f,c,t}^h = \beta_{f0}^h + \beta_{f1}^h{}' d_{c,t}$$

That is I assume that this heterogeneity in coefficients is linear in some observable vector (or scalar) of characteristics $d_{c,t}$. For example, when $d_{c,t}$ is a scalar, under the presence of heterogeneous treatment effects, the coefficient times the main regressor can be written as

$$\beta_{f,c,t}^h s_{f,c,t-1} r_{f,c,t-1,t} = \beta_{f0}^h s_{f,c,t-1} r_{f,c,t-1,t} + \beta_{f1}^h d_{c,t} s_{f,c,t-1} r_{f,c,t-1,t}$$

We see that in addition to the baseline shift-share regressor, $s_{f,c,t-1} r_{f,c,t-1,t}$ there is another triple-interaction shift-share term $d_{c,t} s_{f,c,t-1} r_{f,t-1,t}$ that we need to control for. Since $d_{c,t} s_{f,c,t-1}$ can be considered just a new share $\tilde{s}_{f,c,t-1}$ the similar orthogonality condition applies for estimating β_{f1}^h consistently as for β^h_{f0}. Following CRNS and to ease the interpretation of the coefficients I consider only indicator variables. That is $d_{c,t} = I(o_{c,t} \in m)$ takes the value of one if observation $o_{c,t}$ belongs to a set m. I consider two dimensions of heterogeneity the sample period split, and net wealth level per capita.[28]

[Figure 7 here]

Figure 7 plots how the heterogeneity affects the total payroll estimates. The results do not support the interpretation that there is much heterogeneity in wealth effets. Perhaps hte only exception are the housing and deposit wealth effects which are larger in the post-2009 subsample. For the deposit rate shocks, the closeness of zero lower bound may have played some role in large deposit rate effects in the latter post-2009 period. Finally, I will discuss the implications of heterogeneity in treatment effects not captured by the parameterization approach.

A big and growing literature in econometrics (see e.g. Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Goodman-Bacon (2021)) show that two-way fixed effects (TWFE) regressions that can be presented as staggered difference-difference research designs—or its multiple extensions—may have potential shortcomings under heterogeneous treatment effects. Specifically, the regression coefficients may not be convex combinations of the individual treatment effects. This means that while the regression coefficient is a weighted average of the individual treatment effects, some of these weights may even be negative implying that it is possible that while all the individual treatment effects are positive the TWFE regression outputs a negative value for this weighted average treatment effect. Thus if there is a risk that heterogeneous treatment effects are plausible, the TWFE regression may give very misleading results.

Recent work by Dube et al. (2022) (henceforth DGJT) provides one especially applicable approach to tackling this challenge. Their approach, which they call the local projection difference-in-differences (LP-DiD) estimator has particular appeal over some of the alternatives since it allows for covariates, including the lagged outcomes, that allows for conditioning on the necessary information that ensure that the common trends assumption holds. Furthermore, the method can be applied using a simple least squares regression framework that we have used so far. DGJT proceed to show that LP-DiD can be implemented using regression via sample restriction or via additional control and interaction terms that

[28] I split states by tercile of their time-averaged real net wealth per capita (deflated by the price index for personal consumption expenditure). Net wealth is defined as the sum of total assets minus household debt. Following CRNS, I split by state wealth level since it maintains the identification of each coefficient as coming only from within-state variation.

produce numerically identical results. We proceed with the latter approach and introduce their general estimation equation below.

$$\Delta_{c,t-1,t+h}y = \beta^h \Delta D_{c,t} + \theta^h UC_{c,t} + \sum_{m=1}^{M} \gamma_{m,p}^h (1 + \rho_{m,p}^h UC_{c,t}) Controls\&FixedEffects_{c,t} + \epsilon_{c,t-1,t+h} \tag{19}$$

where $\Delta D_{c,t}$ is a treatment indicator and $UC_{c,t}$ is an "unclean control" indicator that takes a value of one if unit c at time t fails to satisfy clean control condition —that we soon specify— and 0 if it satisfies it. As DGJT discusses the clean control condition may vary based on application. For example, in our setup —if not controlled for—there may be autocorrelation between treatments, which presents us a choice of how to proceed: If we are interested in isolating the direct effect of a single treatment without taking into account indirect effects through the probability of future treatments, then we must condition on such future treatments. If interest is in characterizing the overall effect of receiving the treatment at time t inclusive of possible effects through inducing (or discouraging) future treatments, no such conditioning is necessary. As DGJT points out the latter would be a more accurate description of what is likely to happen in practice, while the former is a more accurate description of the treatment effect as generally conceived in the policy evaluation literature.

Since it may be relatively difficult to construct a sample that satisfies the requirements needed to estimate the first condition we proceed by studying the effects that incorporate these indirect effects.[29] For this case, the clean control condition states that an observation is a clean control (or treatment) if an observation satisfies either of the two conditions

$$\begin{cases} treatment \quad \Delta D_{c,t} = 1, \Delta D_{c,t-1} = ... = \Delta D_{c,t-H} = 0 \\ clean\,control \quad \Delta D_{c,t+h} = 0 \; for \; h = -H, ..., 0 \end{cases}$$

What is a reasonable lag length H is likely to vary based on application but in this paper H is set to

[29] For the direct effects only case, an observation satisfies the clean control (or is a clean treatment) if it satisfies either of the two conditions

$$\begin{cases} treatment \quad \Delta D_{c,t} = 1, \Delta D_{c,t+k} = ... = \Delta D_{c,t+1} = \Delta D_{c,t-1} = ... = \Delta D_{c,t-H} = 0 \\ clean\,control \quad \Delta D_{c,t+h} = 0 \; for \; h = -H, ..., k \end{cases}$$

but in practice, if it indeed is the case that shocks in treatment are serially correlated, then it is very difficult to find treatments that satisfy such conditions. This implies that the latter is a condition which is less strict is one that we can implement.

8—a choice that compromises between finding enough observations that satisfy the clean control condition and the risk of having prior treatments still affecting the outcomes. As one can see from the equations above, DGJT baseline analysis concerns setup with a single binary treatment. However, they propose guidelines on how to adapt the setup to continuous and/or several treatments.

With continuous treatment, we can adapt the clean control condition to define clean controls as 'stayers' (or alternatively 'quasi-stayers') as in Chaisemartin, D'Haultfoeuille, et al. (2022). What this means in practice in the setup that we take here is that we require that the instrument $z_{f,c,t-1,t}$ is "sufficiently" large and if not we set this observation to zero. The implicit assumption here is that the observations that were set to zero are so small that they do not have a material impact on household behavior. Specifically, we define this condition as follows: we assign an identifier to each $z_{f,c,t-1,t}$ observation into based on the deciles that observation belongs to within the distribution of treatments in that asset class f. We then assign a value of zero to all housing, bond and stock treatments if they do not belong to the smallest or the largest decile (within their asset class distribution) and keep the value of the treatment as it is otherwise. This exercise ensures that we are sampling to the treatments both very large negative shocks and very large positive shocks, because within these asset returns (shifters) there are both large negative and positive observations. Also, the fact that we sample only the top 10 and bottom 10 percent of observations within each asset ensures that i) only the most impactful observations are considered ii) there is enough observations in the control group because the admission to be considered as a clean control condition can be relatively strict. For the deposit and mortgage rates, there are only positive observations available. Because the observations within the smallest decile are close to zero and thus by construction are close to being defined as untreated, we consider only the largest observations as impactful. To be consistent in the number of observations we consider as "sufficiently" large across asset classes, for these interest-rate-shocked assets we consider the observations in the top 2 deciles of each distribution as treatments and assign a value of zero to the observations in the smallest 8 deciles. Note also that we are specifically interested in the shift-share interaction term, not just the return or the interest rate, i.e. the shifters, since if the wealth-to-labor-income ratio is close to zero, no matter how large the return shock is, the shock should not have a very big impact on household behavior since the asset constitutes such a small fraction of household wealth. We call these new adjusted Bartik instruments as $z^{adj}_{f,c,t-1,t}$.

On the other hand, with several treatments, we likewise have to modify the baseline setup slightly. Now we can consider units that enter one treatment but not the others as the treated units, and units that do not receive any treatment as the control following Chaisemartin and D'Haultfœuille (2022). We show also the results where the control condition is as in the baseline case but units can receive multiple treatments (from different asset classes) at the same time. Lastly, to keep things more transparent we use these $z^{adj}_{f,c,t-1,t}$-terms directly as the main regressors thus implicitly skipping the first stage.

[Table VIII here]

The first panel in Table VIII shows that the results show that for the baseline setup where all the assets are incorporated into the analysis, we find that large housing effects in the construction sector and small stock market effects in the non-tradable sector persist while other effects mostly disappear. Furthermore, from the second panel, we see that as with the baseline results in Table VI results for some assets may be quite different when not conditioning on other asset classes. This suggests that some endogeneity due to omitted variable bias may be present in this later panel and also within this new regression framework. Overall the results show that although there are some changes in the coefficient values, our baseline results seem to be robust to the possibility of heterogeneous treatment effects not captured by our current specification and the problems associated with TWFE regressions.

E. Migration responses

Measuring outcomes as per capita terms partly protects the empirical strategy from the possibility that the employment response may be not due to consumption wealth effects but due to a change in the supply of the employable population. For example, if the negative wealth shock leads to net migration of out the county then employment and payroll will fall just due to the fact that the county's working-age population drops but not because the wealth shocks produce a negative impact on the relative level of employment and payroll. This seems to be one obvious shortcoming in prior literature that use absolute levels of labor market outcomes. However, even though using per capita outcomes can correct this issue relating to the change in *quantity* of employable population, there is still a possibility that the wealth shocks are followed by changes in the migration patterns that change the *quality* of the average worker and thus the potential employment outcomes not associated with wealth effects. For example, consider the situation where an increase in house prices leads to a gentrification effect where low-income and possibly unemployed people are forced to migrate out of the county due to increased housing costs and these people are replaced by incoming movers who may be more likely to be employed. So if the change in house prices alleviates some labor market frictions by changing the composition of the workforce that leads to a change in the employment rate and payroll, then the labor market response might primarily operate via this channel and not via the consumption-wealth effect channel.

To investigate if this explanation is likely to distort the results I apply the methodology to new outcome variables that relate to the quantity and quality of migrants entering and exiting a county in a given quarter. Specifically for each county in each year, first I calculate the ratio of total in-moving migrants and out-moving migrants in any given year, which shows whether more people are moving to the county versus moving out of the county in any given period. Big changes in this ratio may generate similarly large changes in the total number of employees and thus reflect the change in *quantity* of the workforce that our empirical strategy already accounts for. Second, I calculate the ratio between the average annual general income (AGI) of an in-moving person and the average annual general income of an out-moving person in the previous year to capture a proxy of the difference in workforce *quality* of people coming or going. I collect these county immigration and emigration data for each year from the IRS-SOI's Migration Data [30].[31]

After constructing these migration variables, I apply the same baseline empirical methodology as for the labor market outcomes. Figure 8 summarizes the results for the main coefficients again in form of impulse response functions. The top row presents the impulse responses for the log change in average AGI of in-mover relative to out-mover whereas the bottom row presents the impulse responses for the log change in the number of in-movers relative to out-movers. We are mostly concerned by the graphs in the top row since they reflect the responses in the change of the quality of the workforce that the outcomes per capita in the baseline specifications do not necessarily control for, but the results for the bottom row may still be interesting in their own right. Broadly, the interpretation for these top row graphs is that if there is a response in either of these variables in an asset class, which follows a similar signed pattern to that corresponding to the local labor market responses then there is a risk that some of that employment and payroll response is due to the change in the composition of work-force. The absence of a large positive response in the quality of in-movers relative to out-movers following a housing shock indicates the gentrification explanation outlined above does not seem to play a major role. In fact,

[30] https://www.irs.gov/statistics/soi-tax-stats-migration-data

[31] Since, these data are reported annually I assume that the annual incoming and outgoing migrants and the aggregate annual general income of these movers are allocated to each quarter within that year in the same proportions. Then I calculate the cumulative log growth rate for both of the outcome variables as before where h goes from 0 to 8. This procedure removes not just the seasonal components in migration patterns—which is what we want—but also any non-seasonally related high-frequency variation that could have been useful when identifying the precise timing of the response in migration following a wealth shock. However, I suspect that the measurement error that this procedure introduces to the high-frequency growth rates does not distort the longer horizons effects much since these quarter-to-quarter level measurement errors are averaged out when h grows.

following a house price increase, there seems to be more people leaving the county and the average AGI of an incoming in-mover is lower than that of a out-mover. This latter fact is in fact the opposite of what the gentrification hypothesis would predict. However, these results are for most horizons statistically insignificant. Similar results apply for the other asset classes where we find generally no changes in net migration or changes in quality of in-mover and out-mover that are such that they could be confounding the results. In the top row, the only exception is the reduction in average AGI of in-mover relative to out-mover following a deposit rate shock. Since, the average AGI ratio impact is negative, like with housing effects, at most we may have slightly underestimated the deposit rates effects, which were estimated to be essentially zero in the baseline specifications. In the bottom row graphs, the one exception is the large net outflow of people following an increase in mortgage payments. It seems an increase in mortgage expenses, is followed by an outflow of people from that county. This result is likely to explain the finding from the previous section that when using non-per capita values of labor market outcomes the mortgage shocks appears and is highly negative and significant but not as much when using per capita outcomes.

Overall, we can interpret the results from this section suggesting that most wealth effects per asset classes are robust to presence of alternating migration patterns and the big negative household debt effects that arise when using non-per capita outcomes seem to come from the fact that many workers are leaving the county following a large dollar term increase in mortgage interest rates.

[Figure 8 here]

F. Structural interpretation

Now it is worth emphasizing that these micro-estimates that we have obtained cannot necessarily be interpreted as macro-estimates. Although often overlooked, this "Missing intercept" problem is prevalent in macroeconomics and there is no easy solution. Indeed, the best solution is likely to vary with the application (for related work see e.g. Wolf (2021), Guren et al. (2020b) and Chodorow-Reich (2020)). Next, I discuss how can we structurally interpret the results and provide some suggestive evidence from the aggregate level that can act as a proof of concept of our local labor market approach.

Although the local-level estimates are directly applicable only for regional-level policymakers, the credible identification of a granular setup helps us to reveal important information about things such as the MPCs of the underlying population or the relative importance of assets in transmitting wealth shocks.

To see the link to MPCs note that in the model, agent j's discount rate, ρ_j, matches her MPC. Thus the model implies that we can translate the payroll elasticity estimates to corresponding MPC estimates for agent j holding asset f with the following formula $\beta_f = MPC_{j(f)} \times \alpha$ where α is the labor share (see Appendix A).[32] Skipping details, CRNS draw similar conclusions, but as they point out—along with Guren et al. (2020b)— in a regional setup the right-hand side should be further multiplied by an extra term M, a local Keynesian multiplie. This M arises in a general equilibrium setup. Guren et al. (2020b) shows that this M in fact equals the local fiscal multiplier. These considerations imply we can evaluate the obtained regional elasticities β_f using an equation

$$\beta_f = M \times MPC_{j(f)} \times \alpha$$

CRNS calibrate $\alpha = 2/3$ and $M = 1.5$, which would suggest that we could interpret β_f directly as MPC out of wealth held in asset f per quarter. For example, this would imply that the marginal agent invested in housing (the "borrowers" in our model) has higher MPC than the marginal agent invested in stocks and bonds (the "savers" in our model).

Similarly, we can extrapolate these regional elasticities to the aggregate level by multiplying the estimates by the ratio of the *aggregate* Keynesian multiplier and the local Keynesian multiplier. This procedure may approximately give the true macro effect as pointed out by Wolf (2021). Similarly, CRNS argue that this aggregate multiplier is at least as large as the local Keynesian multiplier implying that the results we obtained establish a lower bound for the aggregate effects. Note that based on this approach what is noteworthy for us is that regardless what is the actual magnitude of the aggregate or the local multiplier, these multipliers multiply all estimates in the same proportions regardless of which asset produces it. Thus the ranking between the wealth effects produced by different assets persists even if we move to the aggregate level. Said differently, since we can compare the relative importance of assets at the local level, we can do the same inferences also at the aggregate level. This is useful for policymakers since that means they can use these results also when making aggregate-level policy decisions.

To take this argument to the data, let us analyze if our micro-elasticities can be used to to calculate empirically plausible macro-elasticity estimates, I present the impulse response functions in Figure 9 based monthly national-level time-series data from 1987M2-2019M7 from a simple local-projection regression analogous to the one analyzed above. In the aggregate-level we also have consumption data available,

[32] The zero-supply assets like mortgages are an exception since the payroll elasticities are the difference in the elasticities of both the lender and the borrower.

which allows us to cross-check our local labor market approach. While the OLS estimates in Figure 9 are merely suggestive, since they are potentially prone to some of the endogeneity issues that we tried to tackle with our micro-approach, they do serve two purposes.

First, the results allow us to benchmark for the micro-level elasticities: The behavior and magnitude of estimates are broadly similar to those in the micro-level regressions. Housing effects are large and positive, deposit and mortgage rates are large but noisy and stock market and bond market wealth effects are small and positive or insignificant. The macro effects are also often larger than micro results, which suggests that conditional on macro results not suffering from identification issues, the ratio of the aggregate Keynesian multiplier to local Keynesian multiplier is indeed above one. Overall, in light of this supportive evidence the magnitude of micro-estimates seem plausible.

Second, in the absence of granular consumption data, these results can show that our employment and payroll results can be reasonably interpreted as consumption-wealth effects. This is because i) the personal consumption expenditure and employment react to wealth shocks very similarly supporting the story that consumption-wealth effects are driving the results and ii) the magnitude of the wealth shock on PCE is approximately twice as large on employment. This is consistent with the theoretical model that suggests that labor market effects should be (α-times) smaller than consumption effects.

[Figure 9 here]

VII. Conclusion

This paper investigates how household wealth shocks impact the local labor market outcomes using a county-level panel data from the US. I incorporate all important forms of household wealth and debt simultaneously into the analysis. I find that the shocks to housing wealth and household debt produce the largest responses in payroll and employment growth, while the stock market wealth effects are small but positive. I find no effects from shocks to bond market wealth while deposit wealth shocks are not credibly identified. The housing wealth and mortgage rates effects operate mainly through the construction sector while stock market wealth is more so through the non-tradable sector. Comparing these estimated elasticities to model incorporating just single asset we see that multi-asset setup may deviate from single asset case sometimes significantly. This finding emphasizes the importance of controlling for all types of household wealth in the empirical strategy because of omitted variables bias. These results are important for understanding the household balance sheet channel of the monetary policy transmission mechanism and which assets play the most important role in it.

Appendix A Model

Here I show the detailed steps for deriving the Equation 11. As in the main text, we start from Equation 44 and plug-in individual wealth processes budget constraints using the definitions of N_t^j for each agent j. Then we get

$$\begin{aligned}\frac{dC_t}{C_t} &= (\rho_s-\rho_i)\frac{V_t^{Dep}}{C_t}dR_t^{Dep} + (\rho_i-\rho_b)\frac{V_t^D}{C_t}dR_t^D \\ &+ \rho_b\frac{V_t^H}{C_t}dR_t^H + \rho_s\frac{V_t^E}{C_t}dR_t^E + \rho_s\frac{V_t^B}{C_t}dR_t^B \\ &(-\rho_b^2\frac{V_t^H}{C_t} + (\rho_b^2-\rho_i^2)\frac{V_t^D}{C_t} + (\rho_i^2-\rho_s^2)\frac{V_t^{Dep}}{C_t} - \rho_s^2\frac{V_t^E}{C_t} - \rho_s^2\frac{V_t^B}{C_t})dt\end{aligned} \tag{20}$$

Then using the fact that the $C_t = \frac{1}{\alpha}W_tL_t$ and replacing C_ts and dC_ts we get

$$\begin{aligned}\frac{d(W_tL_t)}{W_tL_t} &= (\rho_s-\rho_i)\alpha\frac{V_t^{Dep}}{W_tL_t}dR_t^{Dep} + (\rho_i-\rho_b)\alpha\frac{V_t^D}{W_tL_t}dR_t^D \\ &+ \rho_b\alpha\frac{V_t^H}{W_tL_t}dR_t^H + \rho_s\alpha\frac{V_t^E}{W_tL_t}dR_t^E + \rho_s\alpha\frac{V_t^B}{W_tL_t}dR_t^B \\ &(-\rho_b^2\alpha\frac{V_t^H}{W_tL_t} + \alpha(\rho_b^2-\rho_i^2)\frac{V_t^D}{W_tL_t} + \alpha(\rho_i^2-\rho_s^2)\frac{V_t^{Dep}}{W_tL_t} - \alpha\rho_s^2\frac{V_t^E}{W_tL_t} - \alpha\rho_s^2\frac{V_t^B}{W_tL_t})dt\end{aligned} \tag{21}$$

After renaming the meta-parameters —for example $\beta_H = \rho_b\alpha$ and $\beta_{Dep} = (\rho_s-\rho_i)\alpha$— we can write this in simpler form as

$$\frac{d(W_tL_t)}{W_tL_t} = \sum_f \beta_f \frac{V_t^f}{W_tL_t}dR_t^f + \sum_f \gamma_f \frac{V_t^f}{W_tL_t}dt$$

which is the Equation 11 in the main text. Expressions for each β_f and γ_f are implicitly defined by the corresponding terms from the last two equations.

Appendix B Data

A Capitalizing income using IRS-SOI data

When constructing the stock market wealth and fixed income wealth I use county-level dividend and interest income data obtained from IRS website.[33] CRNS note that prior to 2010 the county files aggregate returns filed by the end of September of the filing year, corresponding about 95% to 98% of all returns filed that year. Notably, the county files before 2010 exclude some taxpayers who file form 4868, which allows a six-month extension of the filing deadline to October 15 of the filing year. To obtain a consistent panel, I first convert the SOI zip code level files to a county basis using the HUD USPS crosswalk.[34] I then implement the following algorithm: (i) for 2010 onward, use the county files; (ii) for 2006-2009, use the zip code files aggregated to the county level and adjusted by the ratio of 2010 dividends in the county file to 2010 dividends in the zipcode aggregated file; (iii) for 1989-2005, use the county file adjusted by the ratio of 2006 dividends as just calculated to 2006 dividends in the county files. I implement the same adjustment for interest income.

A.1 Capitalization approach for the stock market wealth

Capitalization approach of dividend income to get the stock market wealth estimates directly builds on CRNS. I replicate their procedure and extend it to later years. The details on this approach are well documented in the Online Appendix Section A of CRNS. The replication package is available in the website of American Economic Review. However, I outline here the main steps and the details that are needed when extending the data to later years.

I use the two-step methodology of CRNS when constructing the stock wealth estimates. First, county-specific dividend yields are constructed based on age demographics from U.S. Census Bureau and information about portfolio characteristics of different individuals based on statistics calculated from the large discount broker-dealer data used for example in Barber and Odean (2000). Then, these county-specific dividend yields are used to capitalize the dividend income from IRS-SOI data. Finally, additional adjustments are made to include stock market wealth held in non-taxable accounts (e.g. defined contribution retirement accounts) since The SOI data exclude dividends held in those. Similarly, the impact of non-public companies is discussed because dividend income reported on form 1040 includes dividends paid by

[33] *https://web.archive.org/web/20210319074846/https://www.irs.gov/statistics/soi-tax-stats-county-data.*

[34] I use HUD-USPS crosswalk file of 2010:Q1 that is the earliest crosswalk file available in *https://web.archive.org/web/20201208154609/https://www.huduser.gov/portal/datasets/usps$_c$rosswalk.html*

private C-corporations. However, they conclude that non-public C-corporation wealth does not appear to meaningfully affect our results.

A final word about the details of constructing the dividend yields and betas. For years 1989-2015 I get the county-specific beta estimates and dividend yields directly from the replication package of CRNS. For years 2016-2019 dividend yields I use the county-level values of 2015 adjusted for variation in aggregate dividend yield changes in 2016-2018. These temporal variation adjustments of dividend yields follow CRNS. Specifically, the average dividend yield in each year $\bar{DY_t}$ is adjusted to be equal to the dividend yield of value-weighted CRSP market portfolio.[35] That is each county dividend yield $DY^{old}_{c,t}$ is multiplied by a "temporal adjustment factor" to get adjusted series $DY^{new}_{c,t}$ as follows

$$DY^{new}_{c,t} = DY^{old}_{c,t} \times \frac{DY^{CRSP}_t}{\bar{DY_t}} \tag{22}$$

For betas, from 2016 onwards I use the 2015 values. County-level betas are very persistent from year to year so the approximation error from this is likely to be negligible.

A.2 Capitalization approach for deposit and bond market wealth

When capitalizing the interest income to get total fixed-income wealth I use a similar approach as was used when capitalizing dividend income. Like dividend income, IRS-SOI data reports the total interest income obtained by the households within a given county and year. As with dividend income, first I capitalize the reported interest income to obtain reported fixed-income wealth. Like with stock market wealth this figure needs be adjusted to include the bond wealth that is not based on the reported interest income in Form 1040. This adjustment that produces county-level estimates for total fixed-income wealth is detailed below. Finally, I allocate these wealth estimates to deposits and bond holdings components. These annual series are interpolated across quarters to get quarterly series. I denote the deposit share in year $t-1$ in county c with $DS_{c,t-1}$[36]. However, given that we do not observe this allocation at the county level I assume that in each county the fixed-income wealth is divided into bond holdings and deposit holdings in the same proportions DS_{t-1} using the aggregate share division in Fed Financial Accounts.

As mentioned above, the capitalization of interest income takes the following three steps, which are outlined here.

[35] I calculate the CRSP dividend yield as $DY^{CRSP}_t = Ret_t - Retx_t$ where Ret_t and $Retx_t$ denote the annual total return and annual return excluding dividends in year t.

[36] Not to confuse the reader I emphasize that I use t subscripts to denote a year t only for detailing the capitalization approach that before interpolation operates with annual frequency but the general analysis in this paper uses quarterly frequency with subscript t denoting a specific quarter.

Step 1: First, I calculate the county's annual interest rates on its bond and deposit holdings. Quarterly county deposit rates are annualized to get annual rates $r^{Dep}_{c,t-1,t}$. How to obtain these quarterly county deposit rates is presented in the next subsection. For bond holdings, similar microdata does not exist so I simply assume that all counties are invested in the same bond market portfolio with the same annual yield $y^{B}_{t-1,t}$. Note that this does not mean that the size of these holdings are same since the bond market wealth between counties varies based on interest income. Also, this bond portfolio yield is constructed following true market shares of different bond segments to obtain accurate capitalization factors. This is also detailed below. Using these rates we can calculate the average fixed income yield in a county as $\hat{y}_{c,t} = (1 - DS_{c,t-1})y^{B}_{t-1,t} + DS_{c,t-1}r^{Dep}_{c,t-1,t}$. Then we use it to capitalize the reported interest income in SOI by

$$\widehat{FixdI}_{c,t-1} = \frac{Int.income^{SOI}_{c,t-1,t}}{\hat{y}_{c,t}} \tag{23}$$

Step 2: However, although the individuals are generally required to report also the tax-exempt interest income in Form 1040 like that from directly held municipal bonds the SOI data still does not include all interest income. For example, interest income generated by the pension accounts is not reported– unless any distributions are taken from them.[37] Thus, $\widehat{FixdI}_{c,t-1}$ estimate does not include the fixed income wealth held in the retirement accounts. To correct for this I use SCF waves to measure how much an individual with a particular age and wealth has fixed income wealth in total relative to fixed income wealth that is required to be reported in Form 1040.[38] I denote this ratio for a median individual in age group age in an SCF wave t as

$$Multiplier_{age,t} = median(\frac{TotalFixdedIncomeWealth^{SCF}_{i(age),t}}{ReportableFixdedIncomeWealth^{SCF}_{i(age),t}}) \tag{24}$$

where $i(age)$ refers to the fact that individual i belongs to age group age in year t. After interpolating $Multiplier_{age,t}$ from the tri-annual SCF waves to the yearly level, I aggregate them up at the county level using population shares of specific age groups in a county c in year t.

$$Multiplier_{c,t} = \sum_{age} \frac{pop_{age,c,t}}{pop_{c,t}} Multiplier_{age,t} = \tag{25}$$

Finally this county and year-specific multiplier is used to adjust the implied fixed income based on

[37]https://taxsaversonline.com/do-i-report-401k-on-taxes/

[38]I use the SCF Bulletin data and calculate the fixed income wealth in retirement accounts by subtracting the equity part from total wealth held in quasi-liquid retirement accounts.

reported interest income $\hat{FixdI}_{c,t}$ upward to get the total fixed income wealth estimate.

$$FixdI_{c,t} = Multiplier_{c,t}\widehat{FixdI}_{c,t} \tag{26}$$

Step 3: Using the assumption about $DS_{c,t-1}$ we can back out the wealth estimates for deposits $Dep_{c,t-1}$ and bonds $B_{c,t-1}$ from these equations

$$Dep_{c,t-1} = DS_{c,t-1}FixdI_{c,t-1} \tag{27}$$

$$B_{c,t-1} = (1 - DS_{c,t-1})FixdI_{c,t-1} \tag{28}$$

Finally, these annual wealth series are interpolated to get quarterly series. Lastly, I specify how to construct deposit rates and bond yields for capitalization.

Step 1.1: For Step 1, in addition to the county deposit rate (whose construction is specified below), we need to calculate the yield of aggregate household bond portfolio $y^B_{t-1,t}$ from $t-1$ to t. I assume this bond yield is the weighted average yield of the three largest bond classes households hold: government bonds, corporate bonds, and municipal bonds. I use household ownership shares of different bond assets from Financial Accounts to obtain the weights. Using the annual series I calculate the portfolio weight for each year. Then I obtain annual yield series data from FRED for each asset class. For corporate bonds, I take an average of Moody's Seasoned Aaa (FRED ticker DAAA) and Baa (DBAA) Corporate Bond Yields, for government bonds I use 5 year Treasury rate (DGS5) and for municipal bonds I use State and Local Bonds yields (WSLB20).[39] I assume the bond portfolio has an average maturity of 5 years. Since the corporate bond and municipal bond yield series are constructed from bonds with maturities of 20 years or above, I subtract the term spread between 20-year and 5-year treasuries to transform these series to 5-year maturity yields. After obtaining yields and portfolio shares for each year, I use these data to calculate the yields for total bond portfolio $y^B_{t-1,t}$ as the weighted average of individual components

$$y^B_{t-1,t} = \frac{Munis_{t-1}}{B_{t-1}}y^{Munis}_{t-1,t} + \frac{Corps_{t-1}}{B_{t-1}}y^{Corps}_{t-1,t} + \frac{Gov_{t-1}}{B_{t-1}}y^{Gov}_{t-1,t} \tag{29}$$

[39] The series for municipal bonds is discontinued after 2016. To get municipal bond yield estimates for the remaining sample period post-2016, I fit a linear model with two corporate bond series and 20-year maturity government bond series for the period 1988 to 2016 and then use the obtained coefficients and post-2016 yields from right-hand side of the regression equation to calculate the implied yields for municipals bonds. R^2 from this model is over 95% and bond yields are closely correlated so any measurement error from this procedure is likely to be negligible.

where $Munis_{t-1}$, Gov_{t-1}, $Corps_{t-1}$, and B_{t-1} refer to households' municipal, government, corporate, and total bond wealth respectively. These shares sum to one.[40]

Step 1.2: For deposit rates, I use quarterly data starting from 1994 based on Federal Financial Institutions Examination Council's (FFIEC) Consolidated Report of Condition and Income (or "Call Reports") that all regulated financial institutions need to file periodically. [41] Since the Call Reports capture essentially all the banks in the US, we can use this data to calculate the rates that each bank pay on their deposits. I follow the banking literature (see e.g. Dick (2008) and Xiao (2020)) and calculate the bank-specific deposit rate by dividing interest expenses of domestic deposits by the total amount of domestic deposits.[42] Then these bank-specific deposit rates are allocated to counties based on the county's exposure to each bank based on FDIC Summary of Deposit (SOD) data, which contains information about the size of deposits of each branch in a particular county and which bank operates that branch. Specifically, I merge the SOD data to Call Report data using RSSDID identifiers which are unique numbers assigned by the Federal Reserve Board (FRB) to the top regulatory bank holding companies. Deposit rates per county are then calculated as

$$r^{Dep}_{c,t-1,t} = \sum_b \frac{Dep_{b,c,t-1}}{Dep_{c,t-1}} r^{Dep}_{b,t-1,t} \tag{30}$$

where $r^{Dep}_{b,t-1,t}$ is the bank-specific deposit rate. This definition assumes that the same bank b cannot price discriminate between counties it operates in by setting different deposit rates for bank branches located in different counties—for evidence of this, see e.g. Begenau and Stafford (2022). $Dep_{b,c,t-1}$ is the total amount of deposits in a given county c for a particular bank b at a quarter $t-1$. It is calculated as the sum of deposits of bank b in a branch h in a county c: $Dep_{b,c,t-1} = \sum_h DEPSUMBR_{b,h,c,t-1}$ where $DEPSUMBR$ is the branch deposits variable in SOD data. Branch h within a bank b is identified using branch id (BRNUM) and bank id (RSSDID) in the SOD data. Total deposits per county are then gotten simply by summing all the bank deposits within a county: $Dep_{c,t-1} = \sum_b DEPSUMBR_{b,c,t-1}$.[43]

[40]Notice that although interest from state and local government bonds is tax exempt, it has been reported on individual tax returns since 1987, which is why the municipal bond wealth can and should be incorporated too. See e.g. https://www.irs.gov/taxtopics/tc403.

[41]I obtain the data in an accessible format from the website of Phillip Schnabl. https://web.archive.org/web/20200225091512/http://pages.stern.nyu.edu/ pschnabl/data/data$_c$*allreport.htm*

[42]Due to imperfect bank reporting the data has some errors like negative deposits or unrealistically large expenses relative to deposits. I get rid of these few outliers by winsorizing the bank rate data at 1% and 99% levels.

[43]Total deposits $Dep_{c,t-1}$ in SOD data cannot be used for estimating the household deposit wealth because the data only covers regulated entities and a big share of household deposit wealth is held in money market accounts in shadow banks. Thus using the SOD data would then under-represent the total amount of deposits within a county. However, the deposit information from SOD can be useful in calculating these branch weights for deposit rates if these bank deposit rates are fairly representative of total deposit rates that is if the money market accounts of unregulated shadow banks pay the same

B Constructing household debt rates

In this section, I detail how I construct the average county mortgage rates using the Freddie Mac and Fannie Mae data.

Very brief background details for the US mortgage market are as follows. Most mortgages in the United States are sold to the secondary market after origination rather than staying on lenders' balance sheets. For example between 2004 and 2006, about 80 percent of all mortgages were securitized (Keys, Piskorski, Seru, and Vig, 2012). Most of these securitized loans are sold to government-sponsored enterprises (GSEs) Fannie Mae, Freddie Mac, and Ginnie Mae. Prior to 2004, roughly 80 percent of the securitized mortgage market was securitized by the GSEs and after 2007 essentially all securitization of mortgages has been conducted by the GSEs (Hurst et al., 2016). Thus looking at mortgages in the balance sheet of GSEs is likely to give a fairly comprehensive picture of the cost of household debt. To construct the cost of mortgage loans I use data on loans securitized by either Fannie Mae or Freddie Mac. Due to issues related to data coverage and comparability, I do not analyze loans securitized by Ginnie Mae. Specifically, the data sources I use are Fannie Mae's Single Family Loan Performance Data and Freddie Mac's Single-Family Loan-Level Data Set.[44][45] The population of both data sets includes a subset of the 30-year, fully amortizing, full documentation, single-family, conventional fixed-rate mortgages acquired by the GSEs between 1999 Q1 and 2020 Q2. The data includes information about the time of the loan origination as well as the quarterly reported loan performance.

I merge the loan performance file with the origination file using the loan identifier. In the merged data set, I keep five variables: loan identifier, time identifier, zip-code, current interest rate, and Unpaid Principle Balance (UPB), which denotes the size of the outstanding debt of debtor j. Due to disclosure reasons, the GSEs report the loans only at 3 digit zip-code level. I allocate these 3 digit zip-codes to counties using Zip-County crosswalks from the United States Postal Service (USPS)[46] and construct the county-level mortgage rates as follows

$$r^D_{c,t-1,t} = \sum_j \frac{UPB_{j,c,t-1}}{UPB_{c,t-1}} r^D_{j,c,t-1,t} \tag{31}$$

where $r^D_{c,j,t-1,t}$ is the current mortgage rate for borrower j in county c.

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deposit rates on average.

[44] https://capitalmarkets.fanniemae.com/credit-risk-transfer/single-family-credit-risk-transfer/fannie-mae-single-family-loan-performance-data

[45] http://www.freddiemac.com/research/datasets/sf$_{l}oanlevel_{d}ataset.page$

[46] https://www.huduser.gov/portal/datasets/usps$_{c}rosswalk.html$

In practice I do this aggregation in two steps where in the first step aggregate the individuals j within zipcodes z as

$$r^D_{z,t-1,t} = \sum_j \frac{UPB_{z,j,t-1}}{UPB_{z,t-1}} r^D_{z,j,t-1,t} \tag{32}$$

where $r^D_{z,j,t-1,t} = \sum_j \frac{UPB_{j,z,t-1}}{UPB_{z,t-1}} r^D_{j,z,t-1,t}$ and in the second step similarly aggregate zipcodes within a county.

Also since the granular level—last 2 digits of zipcodes—is missing, the zipcode-to-county mapping of USPS is not fully accurate since there are occurrences when a single zipcode is allocated to more than one county. In these instances, I allocate the loan to each of these counties. This will induce a measurement error in the rates but with reasonable conditions, this error is asymptotically small. To see why, note that the constructed county-level rates are a sum of correctly allocated zipcodes (z, c) and incorrectly allocated zipcodes $(z, -c)$. Formally,

$$r^D_{c,t-1,t} = \sum_z \frac{UPB_{z,c,t-1}}{U\bar{P}B_{c,t-1}} r^D_{z,c,t-1,t} + \sum_z \frac{UPB_{z,-c,t-1}}{U\bar{P}B_{c,t-1}} r^D_{z,-c,t-1,t} \tag{33}$$

where $U\bar{P}B_{c,t-1}$ is the sum of UPB of all loans allocated to that county. Three simple reasons will ensure that in practice the first term will dominate and the distortion from the second term will be asymptotically small or even if some small measurement error does exists, it will not bias estimates in a systematic way. First, the correctly allocated zipcodes are much larger so the weight on first term is much larger i.e. $\sum_z \frac{UPB_{z,c,t-1}}{U\bar{P}B_{c,t-1}} \to 1$ and $\sum_z \frac{UPB_{z,-c,t-1}}{U\bar{P}B_{c,t-1}} \to 0$. Second, it is the case that mortgage rates *within* the same county c, are more correlated with each other than a random sample of rates from multiple counties because of the common county-level component induced by the common environment. So in practice, the components in the misallocated rates $r^D_{z,-c,t-1,t}$ that differ from county c average rates are likely to cancel each other out on average, whereas accurately allocated rates $r^D_{z,c,t-1,t}$ do not. Finally, even if the above two reasons do not fully remove the noise term it is likely not to bias our estimates since there is no reason to expect the noise term to be systematically correlated with the county's labor market outcomes.

I convert the monthly loan performance data to quarterly by averaging across months in a quarter. I assume most mortgages have an annual compounding frequency and so I convert the annual mortgage rates to quarterly rates by dividing by four.

C Details on the housing wealth construction

I construct house price indices using the following algorithm. I start with FHFA HPI Index. First, I linearly interpolate the HPI index across quarters. I then set the base year-quarter to Q4 2019 and multiply each observation in the HPI index with the Q4 2019 median house price in each county based on the data from the National Association of Realtors. I then interpolate the number of housing units across quarters in each county (based on end-of-Q2 estimates). This data is based on U.S. Census Bureau's intercensal estimates. Finally, I multiply the number of housing units by the median price to obtain the total housing wealth in each county in each quarter.

D About the timing of different series and merging them

It is useful to clarify the timing issues since some of the annual series are measured at different points within a year and thus need to be aligned accordingly. First, housing units are measured yearly at the end of Q2 and the house prices are extrapolated backward from 2019 Q4 using calendar year return series, thus I first construct quarterly series for each component and only then merge them to obtain housing wealth estimates. In general, any annual data that are measured at different points of the year is merged only after the interpolation which allows for a more accurate way to match different data series. Second, the capitalization method for fixed income and stock market wealth uses income data with annual frequency. The income is capitalized to correspond the end of Q2 wealth, which is then interpolated across years.

E External validity for wealth measure construction

A good way to check whether the different wealth construction procedures give reasonable estimates is to aggregate up the county-level wealth estimates and see if the resulting aggregates match the corresponding numbers reported in Financial Accounts (FA).[47] Figure 10 reports the results from this aggregation exercise and the corresponding aggregate household level series in FA.

[Figure 10 here]

We include all five assets that we investigate in this paper while the "Net wealth" series just adds up the four asset series and subtracts the mortgage series either using the FA series or the aggregated series.

[47] Note that the aggregation procedure that is used by Fed in Financial Accounts to calculate the household wealth is likely not going to be perfect either but it is perhaps the best external benchmark there is.

We see that broadly the two series line up in each asset class. This gives us comfort that the different approaches to constructing wealth measures must work reasonably well. We can quantify this average fit by taking the mean of the ratio between the aggregated and the FA series for each asset to see how much our approach over- or underestimates the corresponding market value in Financial Accounts. These ratios are reported in Table IX.

[Table IX here]

Figure 10 and Table IX shows that the most obvious overestimation relative to FA benchmark happens when calculating the housing market value using Mian and Sufi, 2011 procedure. Possible reasons could be that with this approach we also include housing wealth that is not directly held by households but by companies or the government. [48] On the other hand, the aggregation exercise seems to underestimate the household owned fixed income wealth in the FA by approximately 12 percent. Possible reasons could be that capitalization factors are higher than estimated or that we underestimate the amount of bond wealth held in pension accounts that do not need to be reported to IRS when filing Form 1040.

While not perfect. Note that if such measurement error affected all the counties similarly we would expect that all else equal our housing wealth estimates would be somewhat underestimated while the fixed income wealth effects would be slightly overestimated. [49] I will report in the Internet Appendix how the main results would change if we did such adjustments to match the average level of financial accounts.

F Data errors in QCEW data

The NAICS version of the QCEW data contains several transcript errors prior to the year 2000. I follow CRNS and correct the transcription errors by hand, although, in fact, all of these transcript errors lie outside my sample period. Appendix F in Chodorow-Reich and Wieland (2019) reports these errors. However, even after correcting these errors, I find the data to contain some structural breaks, while the data provider notes that there may be numerous reasons for these shifts.[50] Given that usually, these

[48] However, this is unlikely to be the main reason since National Rental Home Council (NRHC) states that "large companies own approximately 300,000 single-family rental homes, less than 1.5% of the market, and only about 0.2% of the country's total housing stock". https://www.nahb.org/blog/2022/04/are-institutional-investors-taking-a-chunk-out-of-the-for-sale-housing-market

[49] This is because if we scale all the housing wealth estimates downward by a constant (1/1.28=0.78) this would result in a equally sized level increase in the coefficient of interest. The opposite effect would happen in case of underestimation of wealth.

[50] "Breaks in published data—sudden shifts in employment or wage levels at the macro-level can occur for a number of reasons. One major reason is a change in coding, due to either a physical relocation of an establishment, a change

shifts in levels are unrelated to actual fundamentals I winsorize the log-first differenced per capita series at 1% and 99% levels, which mitigates the risk that those few outlier observations in growth rates drive the results. The presented summary statistics indicate that this level of winsorizing is enough.

G Details for constructing the national level wealth and return series

Since, the Figure 9 uses monthly data instead of quarterly and a longer sample, we need to rely on some alternative sources. First, the aggregate wealth-to-labor income ratios are constructed based on Financial Accounts and interpolated to monthly frequency. For more details of these series above see Appendix B.E. For the denominator of the wealth-to-quarterly-labor-income ratios I use "Total wages and Salaries" (FRED Series BA06RC1A027NBEA). These are denoted in annual terms so I divide the observations with four to obtain ratios that match our micro-level ratios.

Second, since at the aggregate level we have monthly times series we need to construct monthly series for deposit rates and mortgage rates. To get these I use realized deposit interest expenses of domestic offices of commercial banks (FRED Series QBPQYTIEXDOFFDP) and divide them with the total amount of deposits outstanding (FRED Series DPSACBW027SBOG). This gives us a realized deposit rate for the aggregate commercial banks. The level matches and fluctuations of these series matches those where commercial bank deposit rates are obtained from different sources (e.g. Xiao (2020)). For the mortgage series we need to use the *realized* mortgage rate not those of the newly originated mortgages since we want to measure the mortgage expenses for the overall population not just new borrowers. To obtain these measures I use three FRED series "Mortgage Debt Service Payments as a Percent of Disposable Personal Income" (FRED Series MDSP), "Disposable Personal Income" (FRED Series DSPI) and "Mortgage Debt Outstanding by Type of Holder: Individuals and Other Holders" (FRED Series MDOTHIOH). For the monthly housing returns, I use the price returns from seasonally adjusted Case-Shiller House price index (FRED Series CSUSHPINSA). Stock market return and bond market return series, which are the same as used above, come from WRDS-CRSP and Thompson Reuters EIKON.

in primary economic activity, a change in industry definition, or the correction of a reporting error. Another reason is a change in the reporting status of an establishment. Some businesses with multiple establishments incorrectly identify themselves as a single unit. Eventually, if they are able to provide a breakout of economic and administrative detail for all of their subunits, it turns out that many of these units are in different counties and may require different industry codes." -https://www.bls.gov/opub/hom/cew/calculation.htm

Appendix C Empirical methodology

A Deriving the exogeneity condition

In this section of the Appendix I show how to formally obtain the exogeneity condition building on the exposition of CRNS. The second stage regression equation in matrix form is given as $\mathbf{Y} = \sum_f \beta_f^h \hat{\mathbf{X}} + \mathbf{C}^y \Gamma + \boldsymbol{\epsilon}$. Let $\mathbf{Y}$ denote the $NT \times 1$ vector of $\Delta_{c,t-1,t+h} y$ stacked over N counties and T time periods, the regressor fitted values from first stage (if such exists for asset f) are denoted as $\hat{\mathbf{X}}$ with dimensions $NT \times 1$ also, $\mathbf{C}^y$ the $NT \times K$ matrix of K covariates stacked over counties and time periods and $\boldsymbol{\epsilon}$ the $NT \times 1$ staked vector of $\epsilon_{c,t-1,t+h}$.

It follows that $plim \hat{\beta}_f^h = \beta_f^h$ if $lim_{N,T \to \infty} \hat{\mathbf{X}'_f} \boldsymbol{\epsilon} = 0$. Now with stock and bond returns, there is no first stage since we have assumed that all counties hold the corresponding market portfolio and thus the returns are (almost) the same for all counties so we can replace the $\hat{\mathbf{X}}_f$ with $\mathbf{X}_f = \mathbf{S}_f \mathbf{R}_f$ where the shared matrix $\mathbf{S}_f$ of asset f is a $NT \times T$ matrix containing the vector $(s_{1,t}^f, ..., s_{N,t}^f)$ in rows $N(t-1)+1$ to Nt of column t and zeros elsewhere and $\mathbf{R}_f$ the $T \times 1$ vector of market returns for asset f. Then the moment condition is $lim_{N,T \to \infty} (\mathbf{S}_f \mathbf{R}_f)' \boldsymbol{\epsilon} = lim_{N,T \to \infty} \mathbf{R}'_f \mathbf{S}'_f \boldsymbol{\epsilon} = lim_{N,T \to \infty} \sum_t r_{t-1,t}^f \sum_c s_{f,c,t-1} \epsilon_{c,t-1,t+h} = E[r_{t-1,t}^f \xi_t^f] = 0$.

With housing returns, deposit, and mortgage rates, there is heterogeneity in $r_{f,c,t-1,t}$ and thus we have a first-stage regression. The fitted values obtained from it for asset f are denoted as $\hat{\mathbf{X}}_f = \mathbf{P}'_Z \mathbf{X}_f$ where $\mathbf{P}_Z = \mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'$ is the projection matrix and $\mathbf{Z}$ is $NT \times I$ matrix of I instruments. As emphasized above, with multiple endogenous variables on the right-hand side, all instruments in the first stage are used to instrument for all endogenous variables. However, for illustrative purposes, it is easier to show the intuition behind the moment condition using a vector of a single instrument $\mathbf{Z}_f$ for each asset f that refers to the vector of corresponding endogenous variable $\mathbf{X}_f$. With instrumented variables the identifying assumption in the second stage is in fact assumption about the exogeneity of instruments, $lim_{C,T \to \infty} \mathbf{Z}'_f \boldsymbol{\epsilon} = 0$, but given again we have that $\mathbf{Z}_f = \mathbf{S}_f \mathbf{R}_f$ where $\mathbf{S}_f$ is a matrix of shares and $\mathbf{R}_f$ is a vector of national returns (shifters) on asset f this condition corresponds to the moment condition of non-instrumented variables – the exogeneity of national shifters $r_{f,t-1,t}$. The exogeneity of instruments in the second stage can be seen by applying the definition of fitted values to the second stage exogeneity condition. Formally

$$
\begin{aligned}
E[\hat{X}_f'\epsilon|Z_f, X_f] &= E[(P_Z X_f)'\epsilon|Z_f, X_f] = E[X_f' P_Z' \epsilon|Z_f, X_f] = E[X_f' Z_f (Z_f' Z_f)^{-1} Z_f' \epsilon|Z_f, X_f] \\
&= X_f' Z_f (Z_f' Z_f)^{-1} E[Z_f' \epsilon|Z_f, X_f]
\end{aligned}
$$

which implies $E[Z_f'\epsilon|Z_f, X_f] = 0$ is needed assuming the term outside the expectation in the last row is non-zero.

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Tables and figures

Table I Summary statistics

Statistic	N	Mean	St. Dev.	Min	Median	Max
Equity_wealth_to_wages	219,292	11.534	6.959	2.032	9.806	47.873
Bond_wealth_to_wages	223,891	2.442	1.585	0.369	2.012	9.232
Housing_wealth_to_wages	189,234	24.118	15.354	6.916	19.398	91.002
Household_debt_to_wages	229,124	7.304	3.648	1.443	6.709	14.608
Deposit_wealth_to_wages	223,891	4.625	2.835	0.800	3.834	16.961
Housing_return	195,361	0.677	1.493	−12.563	0.739	12.062
Stock_return	225,729	2.288	7.905	−24.187	2.955	18.456
Bond_return	242,117	0.989	2.170	−3.753	0.546	8.090
Deposit_rate	227,078	0.289	0.219	0.000	0.221	1.597
Debt_rate	233,303	1.289	0.244	0.456	1.305	1.924
Payroll_pc_growth	247,988	0.808	4.581	−13.871	0.818	15.103
Empl_pc_growth	247,988	0.008	2.557	−9.682	0.052	8.491
Nontradable_Payroll_pc_growth	247,186	0.763	4.551	−13.949	0.658	17.454
Nontradable_Empl_pc_growth	247,186	0.049	3.303	−11.581	−0.009	13.481
Tradable_Payroll_pc_growth	216,393	0.698	9.655	−33.450	0.170	34.900
Tradable_Empl_pc_growth	216,393	0.065	7.213	−26.270	0.024	26.443
Construction_Payroll_pc_growth	245,092	0.830	11.768	−40.700	0.737	41.902
Construction_Empl_pc_growth	245,092	0.016	8.266	−29.031	0.111	29.486
Inmover_Outmover_AGI_ratio_growth	183,553	−0.017	8.235	−35.606	0.000	35.800
Inmover_Outmover_ratio_growth	183,553	−0.038	7.349	−31.523	0.000	31.156

Notes: The quarterly return and rate series as well as growth rates are denoted with percentages (%).

Table II Wealth effects on total payroll and employment per capita growth: Bartik IV

Dependent Variables:	Payroll pc growth h=7			Employment pc growth h=7		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Hous Shift-Share	0.0140	0.0306**	0.0211***	0.0158**	0.0282***	0.0091***
	(0.0174)	(0.0138)	(0.0042)	(0.0077)	(0.0084)	(0.0028)
Debt Shift-Share	-0.2358	-0.3461	-0.2062	-0.1043	-0.1962	-0.0557
	(0.2870)	(0.3041)	(0.1897)	(0.1888)	(0.2023)	(0.1270)
Dep Shift-Share	-0.2910	-0.4593	-0.4982***	-0.2720	-0.4451	-0.2709**
	(0.4260)	(0.4471)	(0.1672)	(0.2470)	(0.2681)	(0.1175)
Eqty Shift-Share	0.0007	0.0007	0.0006	0.0005	0.0005	0.0003
	(0.0011)	(0.0008)	(0.0007)	(0.0005)	(0.0005)	(0.0005)
Bond Shift-Share	0.0009	0.0014	0.0013	0.0003	0.0008	0.0006
	(0.0019)	(0.0018)	(0.0011)	(0.0011)	(0.0011)	(0.0006)
Shares+Shifters indiv.	Yes	Yes	Yes	Yes	Yes	Yes
Lagged outcomes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged shocks	Yes	Yes	No	Yes	Yes	No
Bartik Forecast	Yes	No	No	Yes	No	No
Fixed-effects						
State-yq	Yes	Yes	Yes	Yes	Yes	Yes
County (2,716)	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
# State-yq	3,570	3,570	3,774	3,570	3,570	3,774
Observations	186,426	186,426	196,516	186,426	186,426	196,516
Adjusted R^2	0.50	0.47	0.46	0.51	0.48	0.48
Wald (1st stage), Hous Shift-Share	23.2	23.0	177.7	23.9	23.7	178.0
Wald (1st stage), Debt Shift-Share	14.7	14.7	69.0	16.1	16.1	69.3
Wald (1st stage), Dep Shift-Share	611.4	611.3	1,115.4	638.2	638.1	1,110.1
Wu-Hausman	2.6	6.5	43.8	3.4	11.8	18.2

Clustered (County & Time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table presents results from the second state of 2SLS regressions corresponding to equation (22) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t-1,t}$. The included controls vary accroding to specification. Shares+Shifters indiv. command includes the the wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t-1,t}$ - the individual components of interactions terms tabulated. Lagged outcomes comand includes the four lags of outcome variable levels y_c, log(payroll per capita) or log(employment per capita). Lagged shocks command includes the four lags of the instruments. Bartik forecasts command includes Bartik shift-share predicted employment or wage growth the corresponding to outcome variable and the specific horizon h.

Table III Wealth effects on nontradable payroll and employment growth per capita: Bartik IV

Dependent Variables:	Payroll growth h=7			Employment growth h=7		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Hous Shift-Share	0.0059	0.0066	0.0007	0.0036	0.0042	-0.0026
	(0.0117)	(0.0112)	(0.0041)	(0.0099)	(0.0091)	(0.0034)
Debt Shift-Share	-0.4375	-0.5102	-0.4549**	-0.4207	-0.4869*	-0.4463***
	(0.3199)	(0.3247)	(0.1841)	(0.2653)	(0.2712)	(0.1498)
Dep Shift-Share	0.1904	0.1554	-0.0074	-0.1415	-0.1714	0.1207
	(0.3726)	(0.3723)	(0.1764)	(0.2924)	(0.2927)	(0.1605)
Eqty Shift-Share	0.0016**	0.0015*	0.0015**	0.0011	0.0010	0.0011
	(0.0007)	(0.0008)	(0.0007)	(0.0007)	(0.0009)	(0.0008)
Bond Shift-Share	0.0004	0.0004	0.0006	0.0005	0.0006	0.0009
	(0.0016)	(0.0016)	(0.0010)	(0.0013)	(0.0013)	(0.0010)
Shares+Shifters indiv.	Yes	Yes	Yes	Yes	Yes	Yes
Lagged outcomes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged shocks	Yes	Yes	No	Yes	Yes	No
Bartik Forecast	Yes	No	No	Yes	No	No
Fixed-effects						
State-yq	Yes	Yes	Yes	Yes	Yes	Yes
County (2,715)	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
# State-yq	3,570	3,570	3,774	3,570	3,570	3,774
Observations	186,356	186,356	196,418	186,356	186,356	196,418
Adjusted R^2	0.40	0.39	0.39	0.41	0.40	0.42
Wald (1st stage), Hous Shift-Share	22.9	22.9	178.8	22.9	23.0	178.7
Wald (1st stage), Debt Shift-Share	18.5	18.6	69.2	18.7	18.7	68.8
Wald (1st stage), Dep Shift-Share	657.5	658.9	1,124.6	659.1	660.5	1,131.9
Wu-Hausman	7.0	6.8	12.7	8.2	7.3	6.3

Clustered (County & Time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table presents results from the second state of 2SLS regressions corresponding to equation (12) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t-1,t}$. The included controls vary accroding to specification. Shares+Shifters indiv. command includes the the wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t-1,t}$ - the individual components of interactions terms tabulated. Lagged outcomes comand includes the four lags of outcome variable levels y_c, log(payroll) or log(employment). Lagged shocks command includes the four lags of the instruments. Bartik forecasts command includes Bartik shift-share predicted employment or wage growth the corresponding to outcome variable and the specific horizon h.

Table IV Wealth effects on tradable payroll and employment growth per capita: Bartik IV

Dependent Variables:	Payroll growth h=7			Employment growth h=7		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Hous Shift-Share	-0.0317	-0.0091	-0.0084	-0.0088	0.0095	-0.0009
	(0.0272)	(0.0302)	(0.0123)	(0.0189)	(0.0217)	(0.0101)
Debt Shift-Share	-0.6601	-0.8644	-0.6741	-0.4817	-0.6521	-0.5441
	(0.7943)	(0.8143)	(0.5480)	(0.6513)	(0.6719)	(0.4483)
Dep Shift-Share	0.7731	-0.1161	-0.4430	0.3697	-0.3591	-0.5105
	(0.8886)	(0.9120)	(0.5121)	(0.8325)	(0.8417)	(0.4364)
Eqty Shift-Share	-0.0030**	-0.0030**	-0.0021	-0.0021*	-0.0022*	-0.0016*
	(0.0014)	(0.0014)	(0.0013)	(0.0012)	(0.0012)	(0.0008)
Bond Shift-Share	0.0010	0.0011	0.0004	-0.0008	-0.0007	-0.0002
	(0.0039)	(0.0041)	(0.0022)	(0.0030)	(0.0032)	(0.0014)
Shares+Shifters indiv.	Yes	Yes	Yes	Yes	Yes	Yes
Lagged outcomes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged shocks	Yes	Yes	No	Yes	Yes	No
Bartik Forecast	Yes	No	No	Yes	No	No
Fixed-effects						
State-yq	Yes	Yes	Yes	Yes	Yes	Yes
County (2,378)	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
# State-yq	3,515	3,515	3,715	3,515	3,515	3,715
Observations	162,847	162,847	166,926	162,847	162,847	166,926
Adjusted R^2	0.33	0.31	0.31	0.31	0.30	0.29
Wald (1st stage), Hous Shift-Share	23.7	23.7	175.4	23.6	23.6	175.2
Wald (1st stage), Debt Shift-Share	17.9	18.3	45.6	18.0	18.3	45.6
Wald (1st stage), Dep Shift-Share	888.2	889.2	1,402.9	889.7	890.8	1,403.2
Wu-Hausman	3.2	1.3	2.4	0.80	1.6	5.8

Clustered (County & Time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Notes: This table presents results from the second state of 2SLS regressions corresponding to equation (12) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t-1,t}$. The included controls vary accroding to specification. Shares+Shifters indiv. command includes the the wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t-1,t}$ - the individual components of interactions terms tabulated. Lagged outcomes comand includes the four lags of outcome variable levels y_c, log(payroll per capita) or log(employment per capita). Lagged shocks command includes the four lags of the instruments. Bartik forecasts command includes Bartik shift-share predicted employment or wage growth the corresponding to outcome variable and the specific horizon h.

Table V Wealth effects on construction payroll and employment per capita growth: Bartik IV

Dependent Variables:	Payroll pc growth h=7			Employment pc growth h=7		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Hous Shift-Share	0.1647***	0.1795***	0.1252***	0.1344***	0.1430***	0.0936***
	(0.0506)	(0.0544)	(0.0155)	(0.0373)	(0.0399)	(0.0117)
Debt Shift-Share	-3.750***	-3.845***	-0.9744	-1.947**	-2.003**	-0.2225
	(1.358)	(1.394)	(0.7894)	(0.9490)	(0.9694)	(0.5771)
Dep Shift-Share	1.647	1.520	-0.2058	0.8559	0.7824	-0.2231
	(1.161)	(1.192)	(0.6503)	(0.9090)	(0.9194)	(0.4865)
Eqty Shift-Share	0.0017	0.0023	-0.0004	0.0028	0.0031	0.0014
	(0.0033)	(0.0035)	(0.0031)	(0.0022)	(0.0023)	(0.0019)
Bond Shift-Share	0.0237***	0.0238***	0.0075	0.0135**	0.0136**	0.0040
	(0.0080)	(0.0085)	(0.0059)	(0.0057)	(0.0060)	(0.0043)
Shares+Shifters indiv.	Yes	Yes	Yes	Yes	Yes	Yes
Lagged outcomes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged shocks	Yes	Yes	No	Yes	Yes	No
Bartik Forecast	Yes	No	No	Yes	No	No
Fixed-effects						
State-yq	Yes	Yes	Yes	Yes	Yes	Yes
County (2,708)	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
# State-yq	3,570	3,570	3,774	3,570	3,570	3,774
Observations	185,878	185,878	195,169	185,878	185,878	195,169
Adjusted R^2	0.40	0.40	0.40	0.38	0.38	0.38
Wald (1st stage), Hous Shift-Share	23.1	22.8	179.2	23.1	22.8	179.2
Wald (1st stage), Debt Shift-Share	18.4	18.5	66.2	18.4	18.5	66.2
Wald (1st stage), Dep Shift-Share	649.1	652.0	1,191.3	649.1	652.0	1,191.3
Wu-Hausman	53.9	58.5	207.1	31.2	34.3	179.4

Clustered (County & Time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*
Notes: This table presents results from the second state of 2SLS regressions corresponding to equation (27) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t-1,t}$. The included controls vary accroding to specification. Shares+Shifters indiv. command includes the the wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t-1,t}$ - the individual components of interactions terms tabulated. Lagged outcomes comand includes the four lags of outcome variable levels y_c, log(payroll per capita) or log(employment per capita). Lagged shocks command includes the four lags of the instruments. Bartik forecasts command includes Bartik shift-share predicted employment or wage growth the corresponding to outcome variable and the specific horizon h.

Table VI Testing for omitted variable bias

Dependent Variables:	Payroll pc growth h=7			Employment pc growth h=7		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
			Baseline specification			
Hous Shift-Share	0.0059	-0.0317	0.1647***	0.0036	-0.0088	0.1344***
	(0.0117)	(0.0272)	(0.0506)	(0.0099)	(0.0189)	(0.0373)
Debt Shift-Share	-0.4375	-0.6601	-3.750***	-0.4207	-0.4817	-1.947**
	(0.3199)	(0.7943)	(1.358)	(0.2653)	(0.6513)	(0.9490)
Dep Shift-Share	0.1904	0.7731	1.647	-0.1415	0.3697	0.8559
	(0.3726)	(0.8886)	(1.161)	(0.2924)	(0.8325)	(0.9090)
Eqty Shift-Share	0.0016**	-0.0030**	0.0017	0.0011	-0.0021*	0.0028
	(0.0007)	(0.0014)	(0.0033)	(0.0007)	(0.0012)	(0.0022)
Bond Shift-Share	0.0004	0.0010	0.0237***	0.0005	-0.0008	0.0135**
	(0.0016)	(0.0039)	(0.0080)	(0.0013)	(0.0030)	(0.0057)
			Baseline specification with one asset class at a time			
Hous Shift Share	0.0057	-0.0036	0.2082***	-0.0013	-9e-04	0.1618***
	(0.0144)	(0.0302)	(0.047)	(0.0135)	(0.0189)	(0.0398)
Debt Shift Share	-0.2669*	-0.4803	-1.3168*	-0.221	-0.63*	-0.9563
	(0.1483)	(0.4527)	(0.7657)	(0.1366)	(0.3737)	(0.6281)
Dep Shift Share	-0.4289*	0.2182	-0.9355	-0.1051	-0.4702	-1.111*
	(0.2433)	(0.7192)	(0.9222)	(0.1463)	(0.6769)	(0.6567)
Eqty Shift Share	7e-04	-2e-04	0.0016	0	1e-04	0.002
	(5e-04)	(0.0015)	(0.0026)	(5e-04)	(0.0011)	(0.0021)
Bond Shift Share	-6e-04	-0.0029	-0.0132	8e-04	-0.0026	-0.0107
	(0.0012)	(0.0045)	(0.0118)	(0.0012)	(0.003)	(0.0087)

Notes: This table presents coefficients of the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ from the second state of 2SLS regressions corresponding to equation (12) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ is instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t-1,t}$. The included controls are those as in the baseline specification. Baseline controls that are included to every regression are: Shares+Shifters individually, that is wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t-1,t}$, four lags of outcome variable levels y_c four lags of the instrumented shocks (also for equities and bonds), Bartik shift-share predicted employment growth the corresponding to outcome variable and the specific horizon h. The first panel summarizes baseline results. The second panel shows what the coefficients β_f^h would have been if we used just one asset class at a time thus exposing the results to the risk of omitted variable bias.

Table VII Robustness tests

Dependent Variables:	Payroll growth pc h=7			Employment growth pc h=7		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
	Observations weighted by 2010 population					
Hous Shift-Share	0.0142	0.0027	0.0997***	0.0160	0.0128	0.0972***
	(0.0214)	(0.0676)	(0.0263)	(0.0130)	(0.0612)	(0.0215)
Debt Shift-Share	-0.3797	0.9585	-1.999*	-1.080***	1.263	-1.168
	(0.4423)	(1.553)	(1.059)	(0.3987)	(1.321)	(0.7886)
Dep Shift-Share	0.0795	-0.5874	0.1204	0.2647	-1.114	0.0131
	(0.5500)	(1.652)	(0.9477)	(0.3819)	(1.197)	(0.7455)
Eqty Shift-Share	0.0023**	0.0005	0.0029	0.0015	-0.0015	0.0029
	(0.0009)	(0.0024)	(0.0035)	(0.0009)	(0.0024)	(0.0023)
Bond Shift-Share	-0.0022	-0.0170	0.0123*	0.0006	-0.0108	0.0070
	(0.0043)	(0.0112)	(0.0073)	(0.0040)	(0.0097)	(0.0066)
	Control for interactions with Human cap. and Non-corp-bus. wealth					
Hous Shift-Share	0.0071	-0.0342	0.1626***	0.0049	-0.0127	0.1207***
	(0.0112)	(0.0276)	(0.0449)	(0.0095)	(0.0191)	(0.0340)
Debt Shift-Share	-0.4549	-0.3487	-3.450**	-0.4523	-0.2484	-2.211**
	(0.3345)	(0.7962)	(1.386)	(0.2828)	(0.6569)	(1.041)
Dep Shift-Share	0.3131	0.5892	1.649	-0.0667	0.3603	1.218
	(0.3750)	(0.8708)	(1.185)	(0.2954)	(0.8159)	(0.9467)
Eqty Shift-Share	0.0014**	-0.0030**	0.0012	0.0010	-0.0022**	0.0021
	(0.0006)	(0.0013)	(0.0030)	(0.0007)	(0.0011)	(0.0020)
Bond Shift-Share	0.0007	0.0001	0.0265***	0.0008	-0.0015	0.0181***
	(0.0018)	(0.0043)	(0.0081)	(0.0015)	(0.0033)	(0.0060)
	Control for interactions with TFP growth					
Hous Shift-Share	0.0055	-0.0225	0.2425***	-0.0032	-0.0036	0.1757***
	(0.0145)	(0.0335)	(0.0880)	(0.0092)	(0.0254)	(0.0643)
Debt Shift-Share	-0.9166	-2.094	-8.559*	-0.4264	-1.622	-5.833*
	(0.7602)	(1.905)	(4.590)	(0.5083)	(1.548)	(3.256)
Dep Shift-Share	0.3471	1.424	3.094	-0.1911	0.8050	2.252
	(0.4448)	(1.110)	(2.235)	(0.3320)	(0.9953)	(1.612)
Eqty Shift-Share	0.0016*	-0.0029	0.0065	0.0004	-0.0022	0.0055
	(0.0009)	(0.0022)	(0.0059)	(0.0006)	(0.0018)	(0.0040)
Bond Shift-Share	0.0013	0.0059	0.0387*	-0.0003	0.0038	0.0269*
	(0.0033)	(0.0082)	(0.0211)	(0.0022)	(0.0067)	(0.0152)

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Dependent Variables:	Payroll growth h=7			Employment growth h=7		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
			Non-per capita outcomes			
Hous Shift-Share	0.0205	-0.0190	0.1765***	0.0174	0.0026	0.1350***
	(0.0133)	(0.0276)	(0.0548)	(0.0118)	(0.0198)	(0.0417)
Debt Shift-Share	-1.332***	-1.531*	-4.775***	-1.335***	-1.358**	-3.389***
	(0.3596)	(0.8090)	(1.391)	(0.2930)	(0.6661)	(1.049)
Dep Shift-Share	0.8800**	1.599*	2.616**	0.5319	1.193	2.069**
	(0.3978)	(0.8451)	(1.231)	(0.3512)	(0.7789)	(0.9653)
Eqty Shift-Share	0.0027***	-0.0020	0.0031	0.0021***	-0.0011	0.0037
	(0.0009)	(0.0012)	(0.0035)	(0.0007)	(0.0009)	(0.0025)
Bond Shift-Share	0.0030	0.0032	0.0259***	0.0032**	0.0014	0.0184***
	(0.0018)	(0.0039)	(0.0086)	(0.0015)	(0.0031)	(0.0065)
	Non-per capita outcomes with observations weighted by 2010 population					
Hous Shift-Share	0.0187	0.0070	0.1006***	0.0191	0.0163	0.0993***
	(0.0183)	(0.0634)	(0.0283)	(0.0121)	(0.0578)	(0.0220)
Debt Shift-Share	-1.557***	-0.1927	-3.244***	-2.225***	0.1154	-2.354***
	(0.5501)	(1.558)	(1.112)	(0.5232)	(1.322)	(0.8415)
Dep Shift-Share	0.8132	0.1091	0.9828	0.9291*	-0.4231	0.7997
	(0.6142)	(1.584)	(1.034)	(0.5318)	(1.092)	(0.7918)
Eqty Shift-Share	0.0035***	0.0019	0.0047	0.0028***	-9.47×10^{-5}	0.0045*
	(0.0012)	(0.0031)	(0.0034)	(0.0009)	(0.0024)	(0.0023)
Bond Shift-Share	0.0030	-0.0131	0.0156*	0.0054	-0.0067	0.0100
	(0.0047)	(0.0122)	(0.0079)	(0.0044)	(0.0090)	(0.0072)
	Obs. weighted by share employment in corresp. industry in 2010					
Hous Shift-Share	0.0125	-0.0368	0.1894***	0.0098	0.0082	0.1480***
	(0.0122)	(0.0467)	(0.0607)	(0.0112)	(0.0342)	(0.0446)
Debt Shift-Share	-0.5705	-1.209	-4.132**	-0.5272*	-0.8331	-2.554*
	(0.3655)	(1.049)	(1.686)	(0.2916)	(0.8619)	(1.286)
Dep Shift-Share	0.3579	0.6674	2.822*	0.0291	0.0716	2.019*
	(0.3962)	(1.296)	(1.495)	(0.2997)	(1.116)	(1.121)
Eqty Shift-Share	0.0018**	-0.0021	0.0041	0.0013*	-0.0006	0.0042
	(0.0007)	(0.0019)	(0.0039)	(0.0007)	(0.0016)	(0.0027)
Bond Shift-Share	0.0003	0.0052	0.0273***	0.0006	0.0027	0.0183**
	(0.0018)	(0.0057)	(0.0094)	(0.0015)	(0.0044)	(0.0071)

Notes: This table presents coefficients of the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ from the second state of 2SLS regressions corresponding to equation (27) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ is instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t}$. The included controls are those as in the baseline specification. But either add the control noted in the subtitle or use per capita values. Baseline controls that are included to every regression are: Shares+Shifters individually, that is wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t}$, four lags of outcome variable levels y_c four lags of the instrumented shocks (also for equities and bonds), Bartik shift-share predicted employment growth the corresponding to outcome variable and the specific horizon h.

Table VIII LP-DiD + Bartik IV: Including the indirect effects

Dependent Variables:	Payroll growth h=3			Employment growth h=3		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
	Unclean control defined for all asset classes jointly					
Hous Shift-Share	-0.0004	-0.0033	0.0384***	-0.0026	0.0017	0.0286***
	(0.0020)	(0.0055)	(0.0055)	(0.0016)	(0.0045)	(0.0044)
Debt Shift-Share	-0.0327**	-0.0353	-0.1161***	-0.0173	-0.0285	-0.0924***
	(0.0132)	(0.0392)	(0.0395)	(0.0112)	(0.0334)	(0.0296)
Dep Shift-Share	-0.0711	-0.6435**	-0.1706	0.0301	-0.5875**	0.1143
	(0.1073)	(0.3086)	(0.2423)	(0.1091)	(0.2781)	(0.1866)
Eqty Shift-Share	0.0015***	-0.0015	-7.09×10^{-5}	0.0012*	-0.0005	0.0009
	(0.0006)	(0.0009)	(0.0018)	(0.0007)	(0.0007)	(0.0013)
Bond Shift-Share	-0.0008	-0.0017	0.0022	-0.0008	-0.0015	0.0005
	(0.0008)	(0.0018)	(0.0023)	(0.0007)	(0.0014)	(0.0020)
	Unclean control defined for one asset class at a time					
Hous Shift-Share	-0.0005	0.0030	0.0209***	-0.0003	0.0055	0.0148**
	(-0.1650)	(0.6068)	(3.696)	(-0.1366)	(1.483)	(2.407)
Debt Shift-Share	-0.0186	-0.0544	-0.0660*	-0.0108	-0.0482	-0.0552*
	(-1.560)	(-1.582)	(-1.928)	(-1.145)	(-1.536)	(-1.819)
Dep Shift-Share	-0.1208	-0.3788	-0.2369	-0.1138	-0.1910	0.0795
	(-1.190)	(-1.281)	(-1.021)	(-1.462)	(-0.7584)	(0.4086)
Eqty Shift-Share	0.0011	-0.0005	0.0002	0.0003	2.97×10^{-5}	0.0013
	(1.499)	(-0.3967)	(0.1504)	(0.5331)	(0.0289)	(0.8710)
Bond Shift-Share	-0.0006	-0.0001	0.0039	-0.0009	-0.0009	0.0029
	(-0.4283)	(-0.0656)	(1.635)	(-0.8441)	(-0.5573)	(1.077)

*Notes:*This table presents coefficients of the interaction term $s_{f,c,t-1}r_{f,t-1,t}$ from the LP-DiD OLS regression where the specification follows equation (12) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ is replacec with $s_{f,c,t-1}r_{f,t-1,t}$ term. Term $s_{f,c,t-1}r_{f,t-1,t}$ is adjusted so that observations of "quasi-stayers" are set to zero and only largest shocks are considered relevant treatments –details of this adjustment are specified in the main text. Baseline controls that are included to every regression are: Shares+Shifters individually four lags of outcome variable levels y_c, Bartik shift-share predicted employment growth the corresponding to outcome variable and the specific horizon h, stateXtime + county fixed effects and an unclean control indicator and its is interaction with all covariates and fixed effects in the spirit of equation (19). The upper panel defines the unclean control as presented in the main text while requiring that there does not occur other asset class shocks at the same point in time. The lower panel relaxes the unclean control condition and allows simultaneously occurring asset class shocks.

Asset	**Housing**	**Stocks**	**Mortgages**	**Bonds**	**Deposits**	**Net Wealth**
Ratio	1.28	1.04	1.07	0.88	0.88	1.11

Table IXMean ratio of aggregated market value relative to market value in Financial Accounts This table shows measures how much on aggregate we over- or under-estimate the market value of a particular asset class when constructing the wealth measures. If ratio is larger (smaller) than one the aggregated market value of the asset (Agg) from county observations on average over(under)-estimate the figure reported in Fed Financial Accounts (FAc).

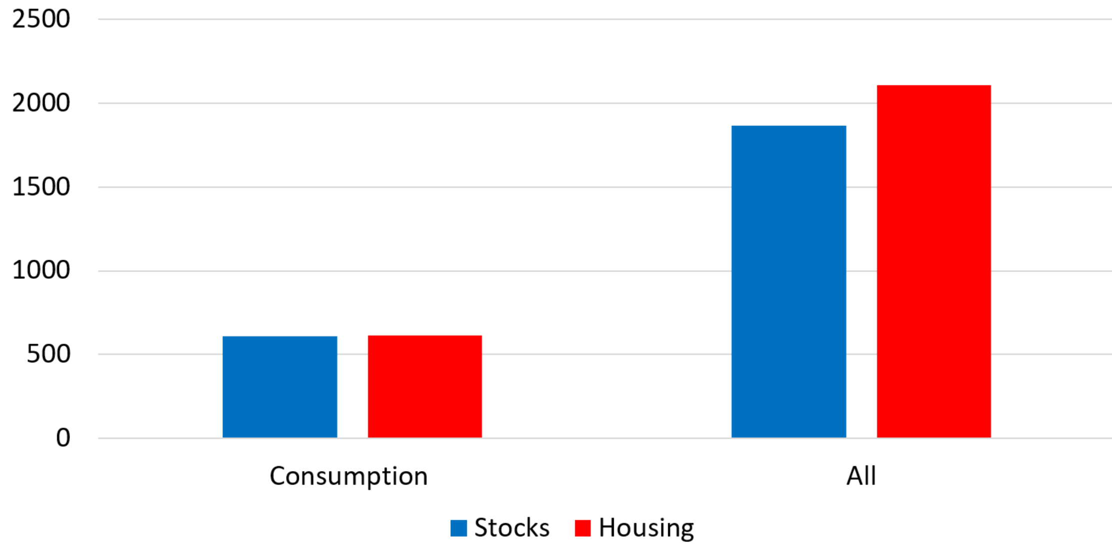

Figure 1. Stock market/housing co-occurrences with economic phrases (# in same paragraph) in texts of the minutes and transcripts of FOMC meetings

This figure shows counts of economic phrases that relate to consumption or all economic topics that occur within the same paragraph of the FOMC minutes, in which a phrase associated with the stock (housing) market is mentioned. The sample period is 1994–2016 and digits are based on Table 8 and A2 in Cieslak and Vissing-Jorgensen (2021). Specifically, "Consumption" category shows the counts from phrases that relate to following words: consumption, consumer sentiment, personal consumption expenditure, pce, retail sales, consumer spending, motor vehicle, consumer expenditures, consumer confidence and household spending. "All" category shows the counts from phrases that relate to the aforementioned and the following words: disposable income, economic activity, economic outlook, inflation, economic growth, final demand, exports, productivity, business investment, business activity, business spending (and similar business related terms), potential output, real gdp, gdp growth, energy prices, economic expansion, industrial production, inventories, residential construction, wealth or net worth, labor market, and employment or unemployment. For more information about which phrases are associated with stock market or housing market see Table 5 and A13 in Cieslak and Vissing-Jorgensen (2021) respectively.

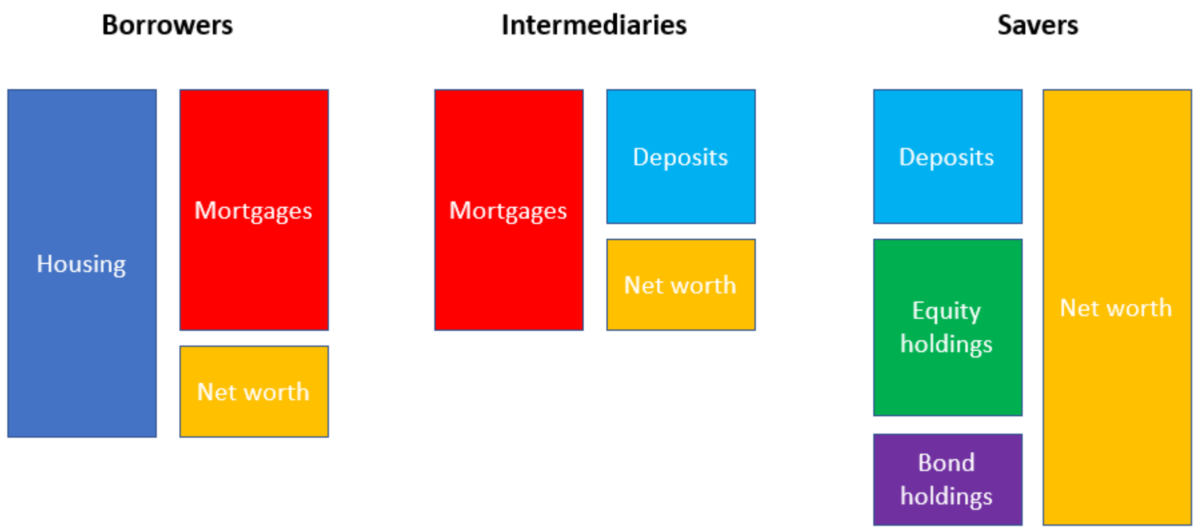

Figure 2. The balance sheets of the capital market participants

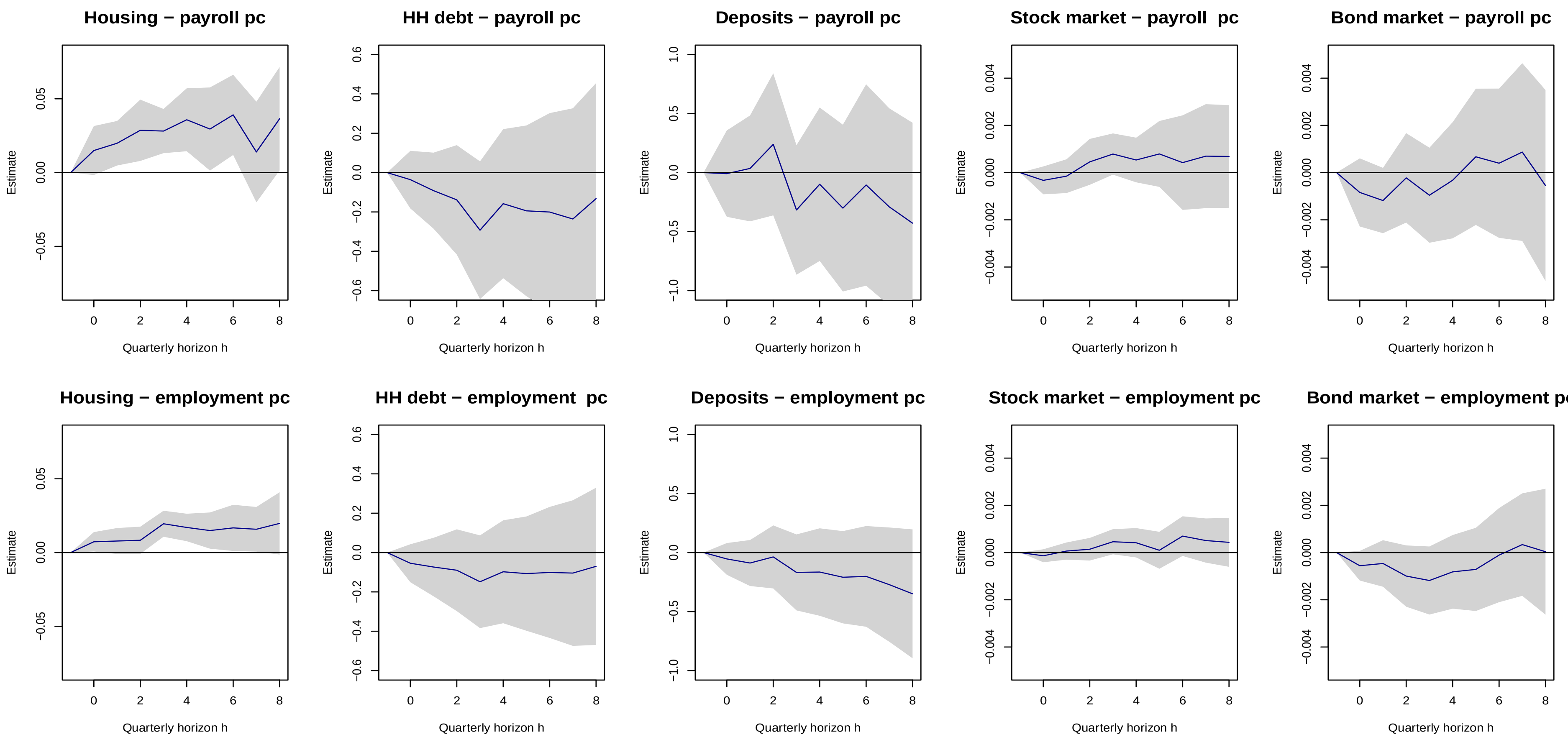

Figure 3. Impulse response to total payroll and employment.

This figure presents the impulse responses of total payroll and employment per capita from return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth, and bond market wealth 9 quarters forward. The horizontal axis represents the horizon h while the vertical axis corresponds to the value of β_h coefficient of the corresponding asset class f. The specification controls for shift-share components individually, four lags of log level of outcome and four lags of the instrument and a Bartik predicted employment growth. The blue line plots the point estimates from the 2SLS regressions. The grey area plots the 95% confidence intervals. Note the different scales in the vertical axis between asset classes.

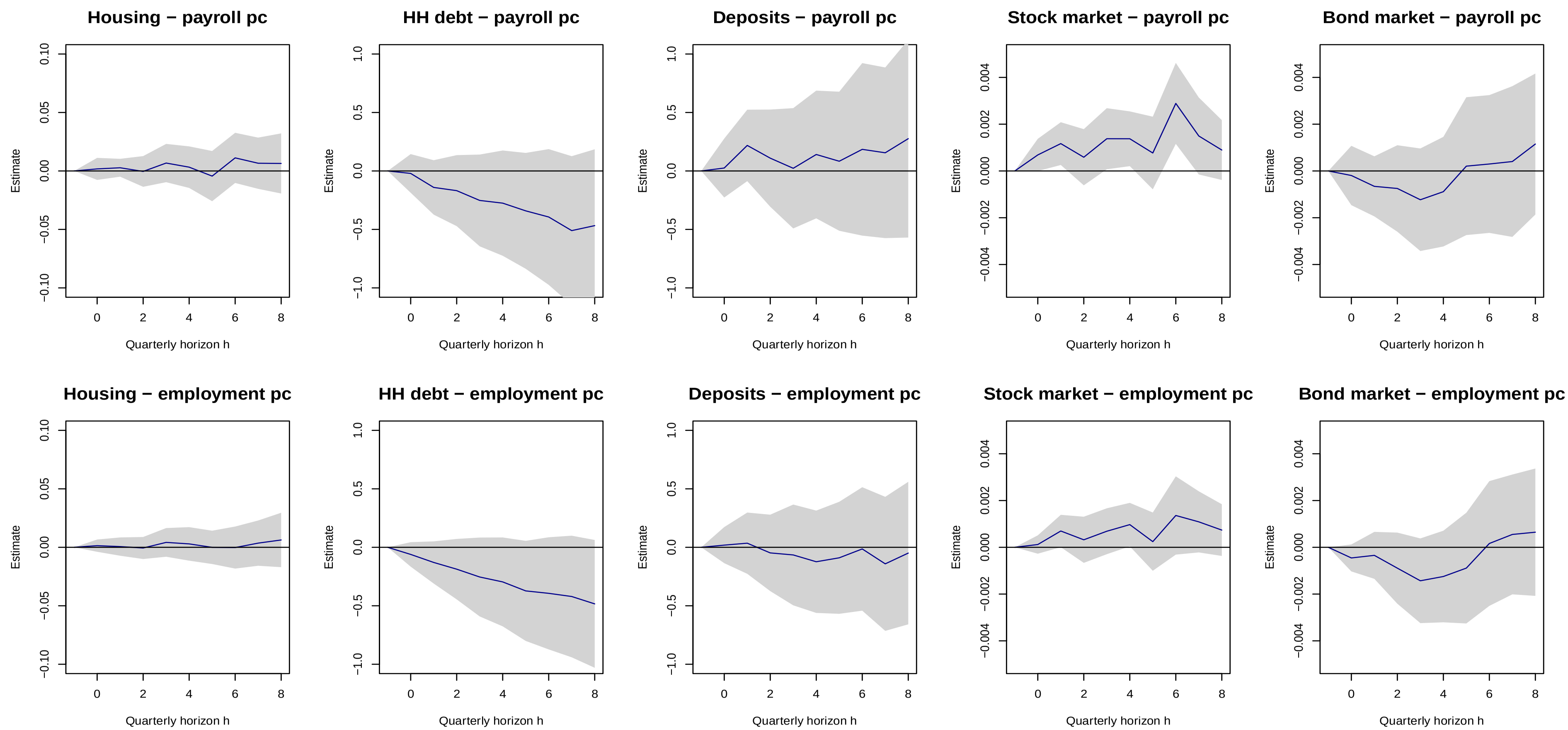

Figure 4. Impulse response of nontradable payroll and employment.
This figure presents the impulse responses of nontradable payroll and employment per capita from return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth, and bond market wealth 9 quarters forward. The horizontal axis represents the horizon h while the vertical axis corresponds to the value of β_h coefficient of the corresponding asset class f. The specification is based on unweighted regressions and controls for shift-shares components, Bartik employment forecasts corresponding to each horizon h, four lags of log level of outcome, four lags of the instrument. The blue line plots the point estimates from the 2SLS regressions. The grey area plots the 95% confidence intervals. Note the different scales in the vertical axis between asset classes.

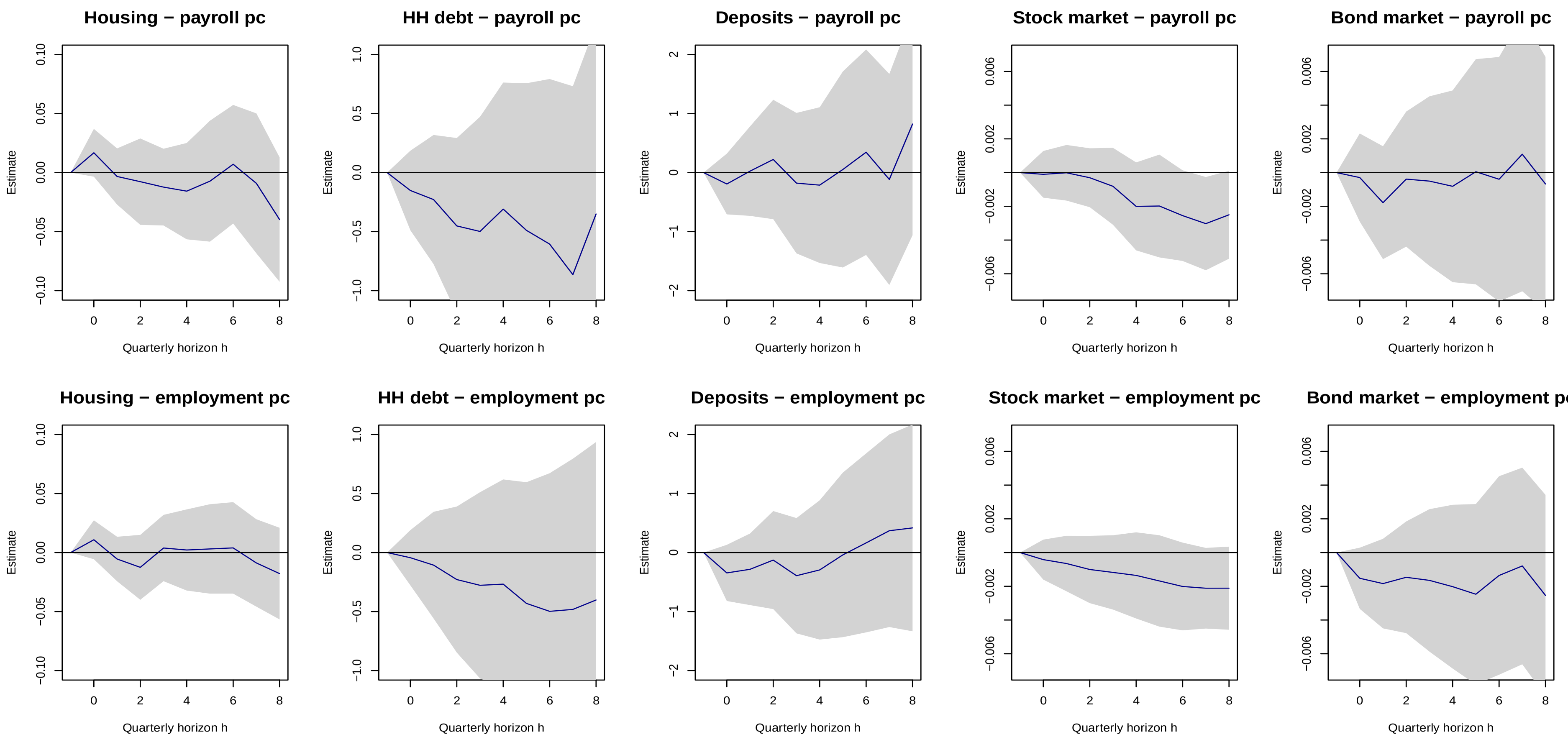

Figure 5. Impulse response of tradable payroll and employment.
This figure presents the impulse responses of tradable payroll and employment per capita from return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth, and bond market wealth 9 quarters forward. The horizontal axis represents the horizon h while the vertical axis corresponds to the value of β_h coefficient of the corresponding asset class f. The specification is based on unweighted regressions and controls for shift-shares components, Bartik employment forecasts corresponding to each horizon h, four lags of log level of outcome, and four lags of the instrument. The blue line plots the point estimates from the 2SLS regressions. The grey area plots the 95% confidence intervals. Note the different scales in the vertical axis between asset classes.

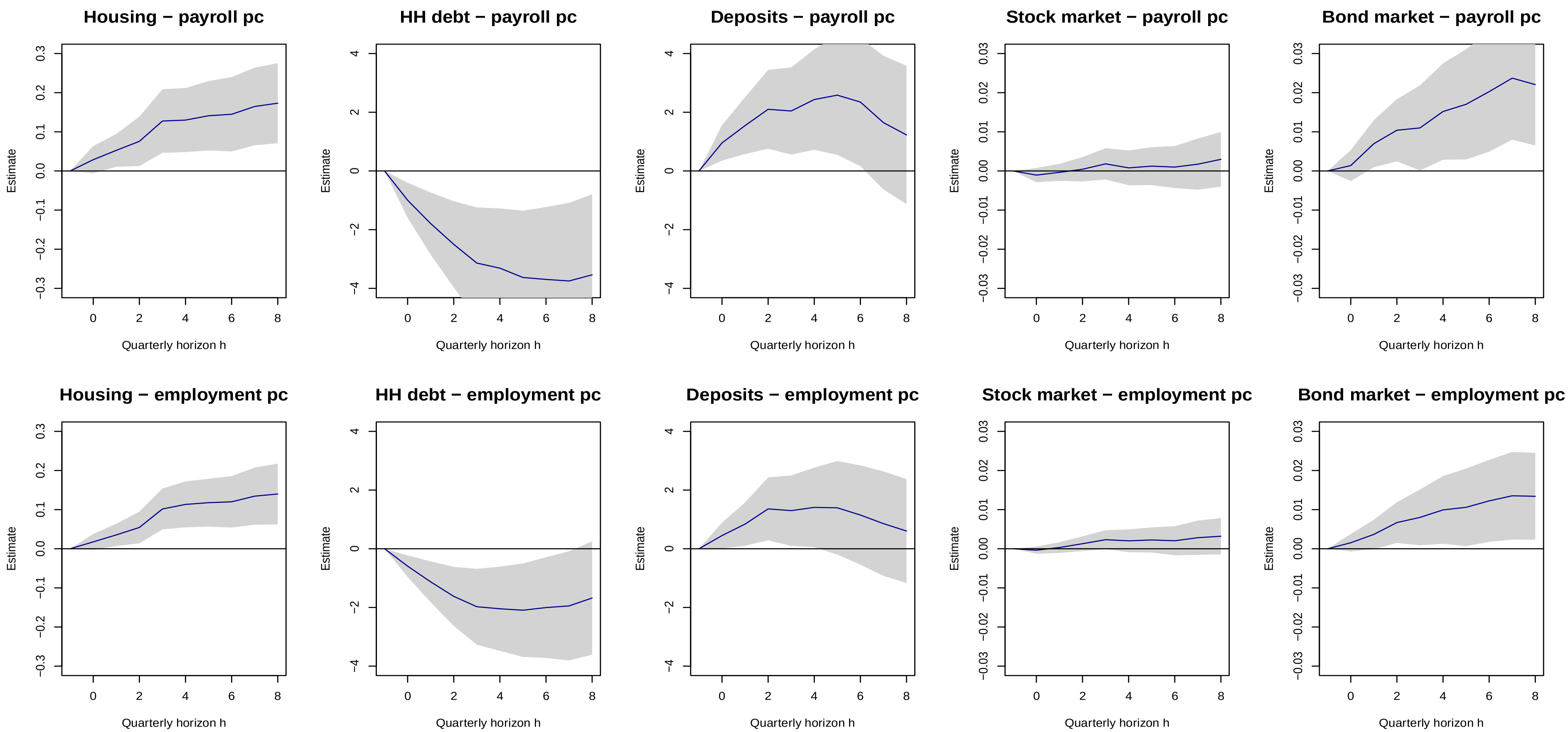

Figure 6. Impulse response of construction payroll and employment.
This figure presents the impulse responses of the construction sector's payroll and employment per capita from return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth, and bond market wealth 9 quarters forward. The horizontal axis represents the horizon h while the vertical axis corresponds to the β_h coefficient value of the corresponding asset class f. The specification is based on unweighted regressions and controls for shift-shares components, Bartik employment forecasts corresponding to each horizon h, four lags of log level of outcome, and four lags of the instrument. The blue line plots the point estimates from the 2SLS regressions. The grey area plots the 95% confidence intervals. Note the different scales in the vertical axis between asset classes.

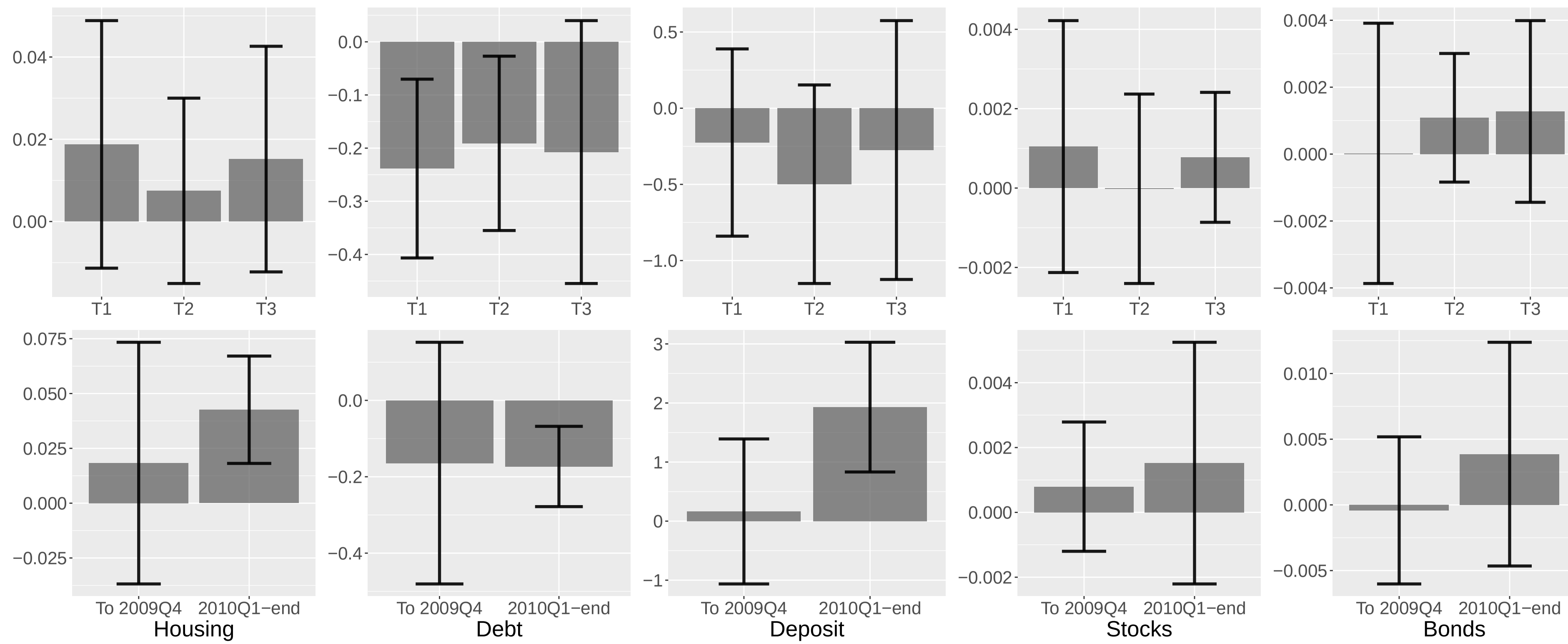

Figure 7. Heterogeneity

This figure reports the coefficients β_f^h from estimating second stage regression of Equation 12 for the total payroll per capita at horizon h = 7, where group m indexes before or after 2009:Q4 (bottom row) or tercile of the state's per capita net wealth distribution (top row). The whiskers show the 95% confidence intervals

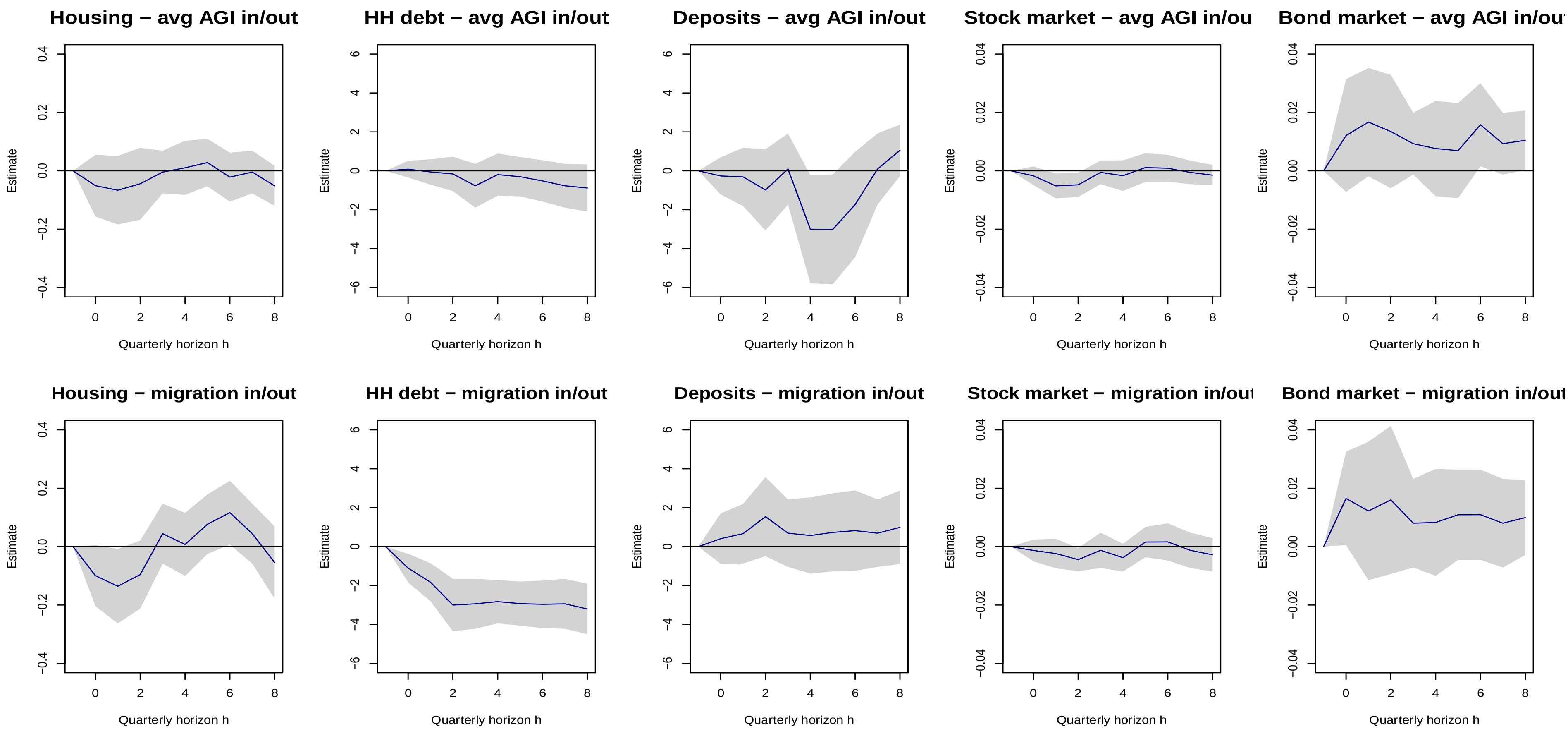

Figure 8. Impulse response of different measures of migration.
This figure presents the impulse responses of the log change in ratio of average annual general income of an in-mover and out-mover (top row) and log change in number of in-movers relative to out-movers (bottom row) from return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth and bond market wealth 9 quarters forward. The horizontal axis represents the horizon h while the vertical axis corresponds to the β_h coefficient value of the corresponding asset class f. The specification is based on unweighted regressions and controls for shift-shares components, Bartik employment forecasts corresponding to each horizon h, four lags of log level of outcome, and four lags of instrument. The blue line plots the point estimates from the 2SLS regressions. The grey area plots the 95% confidence intervals for the single asset case. Note the different scales in the vertical axis between asset classes.

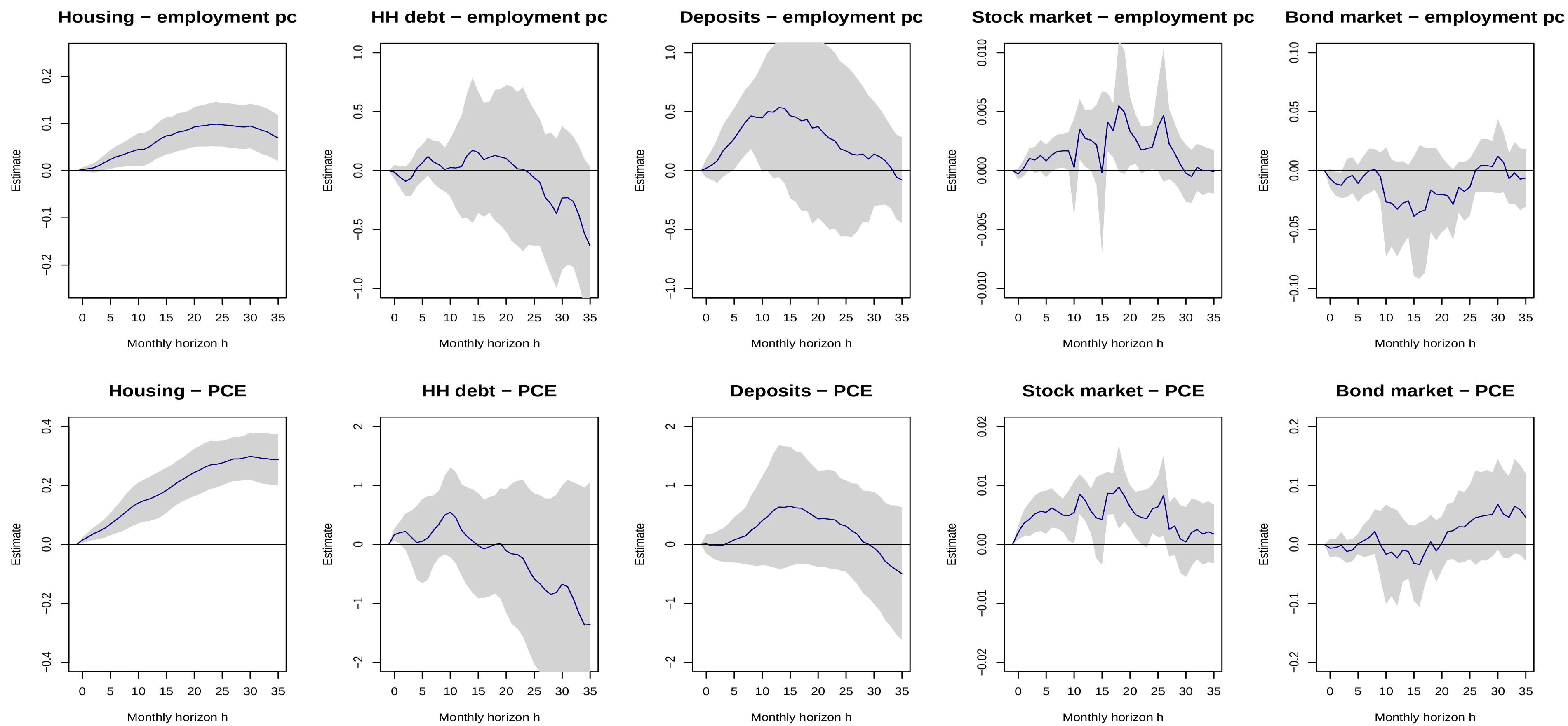

Figure 9. Impulse response of employment per working-age population and personal consumption expenditures (PCE). This figure presents the impulse responses of the log change in employment per working-age population (FRED series LNS12300060) and personal consumption expenditures (PCE) from a return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth, and bond market wealth 36 months forward. The horizontal axis represents the horizon h while the vertical axis corresponds to the value of β_h coefficient of the corresponding asset class f. The asset price shocks are monthly return shocks while the interest rate shocks are defined as the first-differenced interest rates per annum. Each return shock is interacted with the corresponding wealth-to-quarterly labor income ratio. The specification controls all the individual wealth-to-labor income ratios and 12 lags of log level of outcome to ensure no-pre trends. The blue line plots the point estimates from the OLS regressions. The grey area plots the 95% confidence intervals based on Newey-West corrected standard errors with a lag length of 24. Note the different scales in the vertical axis between asset classes.

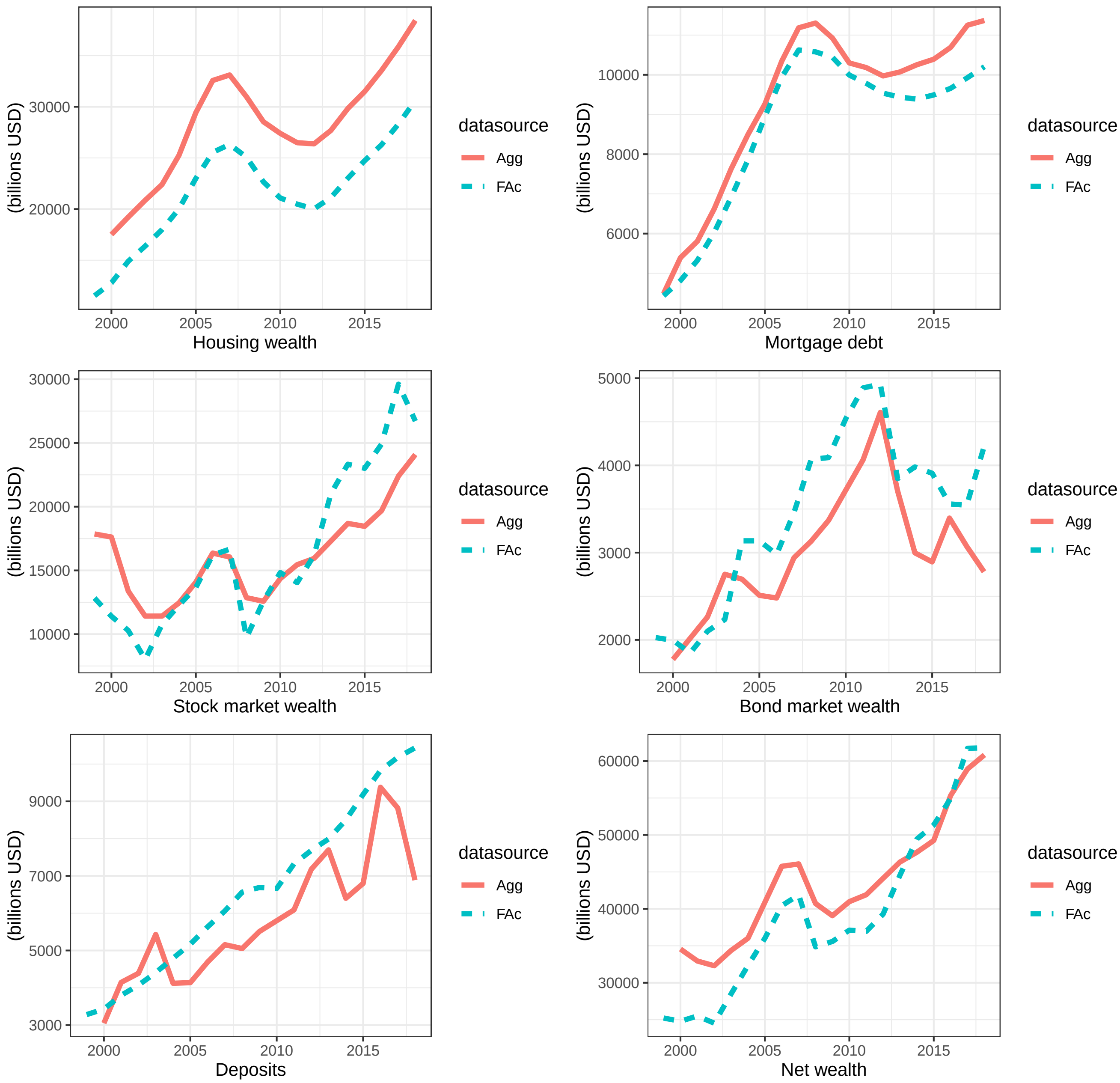

Figure 10. National household wealth and debt series comparison when the granular county level asset series are aggregated up and matched to aggregate wealth series from Fed Financial Accounts (FA). Details on the construction of county wealth series are in the Appendix B. The FA series are constructed as follows: Housing wealth series is Households and nonprofit organizations; real estate at market value (LM155035005 table B.101 line 3). Deposit wealth is the sum of these FA series: Households and nonprofit organizations; checkable deposits and currency; asset (FL153020005 table B.101 line 11) and Households and nonprofit organizations; total time and savings deposits; asset (FL153030005 table B.101 line 12).Bond market wealth is the sum of these FA series: Households and nonprofit organizations; Treasury securities; asset (LM153061105 table B.101 line 15), Households and nonprofit organizations; agency- and GSE-backed securities; asset (LM153061705 table B.101 line 16), Households and nonprofit organizations; municipal securities; asset (LM153062005 table B.101 line 17) and Households and nonprofit organizations; corporate and foreign bonds; asset (LM153063005 table B.101 line 18). Stock market wealth is the sum of these FA series: Households and nonprofit organizations; corporate equities; asset (LM153064105, table B.101e line 15) and Households and nonprofit organizations; indirectly held corporate equities; asset (LM153064175, table B.101e line 16). Mortgage debt series is Households and nonprofit organizations; one-to-four-family residential mortgages; liability (FL153165105, table B.101h line 22).

Internet Appendix to ”Household wealth and local labor markets: Which asset classes matter?”

Abstract

This Internet Appendix provides additional results to accompany the paper “Household wealth and local labor markets: Which asset classes matter?” by Paul Rintamäki.

IA1. Additional summary information

This section plots graphs of the asset wealth-to-quarterly labor income for the U.S. counties.[51] What these figures nicely illustrate is that there is a lot heterogeneity between levels of asset wealth to labor income but also what asset wealth is important in a particular region. For example the financial wealth is important in very different regions than where housing wealth is important. Debt-to-labor income ratios are presented in Figure IA1, whereas stock market, bond market- and deposit wealth-to-labor income ratios are presented in Figures IA2, IA3 and IA4 respectively. Housing wealth-to-labor income ratios are illustrated in Figure IA5. Housing wealth has slightly more missing observations (illustrated with grey color) and thus this figure also shows geographically, which counties are excluded from the analysis. Finally, Figure IA6 sums of the wealth series and subtracts the debt series to give a more holistic picture of the net wealth status between counties.

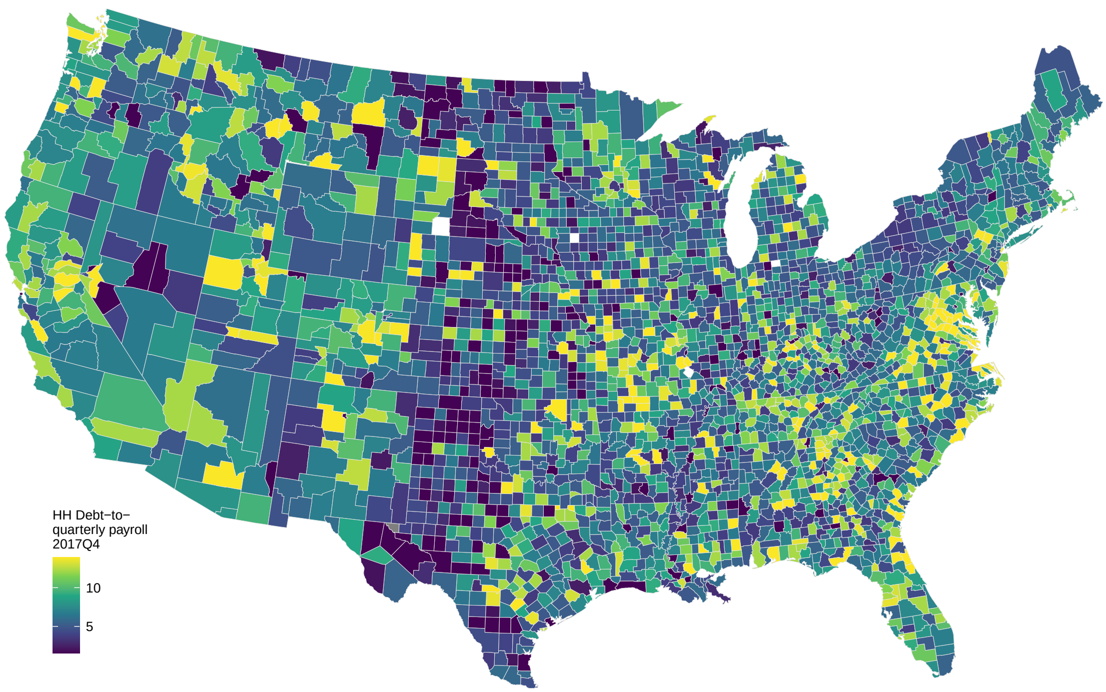

Figure IA1. Debt-to-quarterly labor income at the end of 2017

[51] These wealth-to-labor income estimates as used in the paper are winsorized at 1% and 99% level to remove the outliers that may have been caused either because of mismeasurement of wages or wealth.

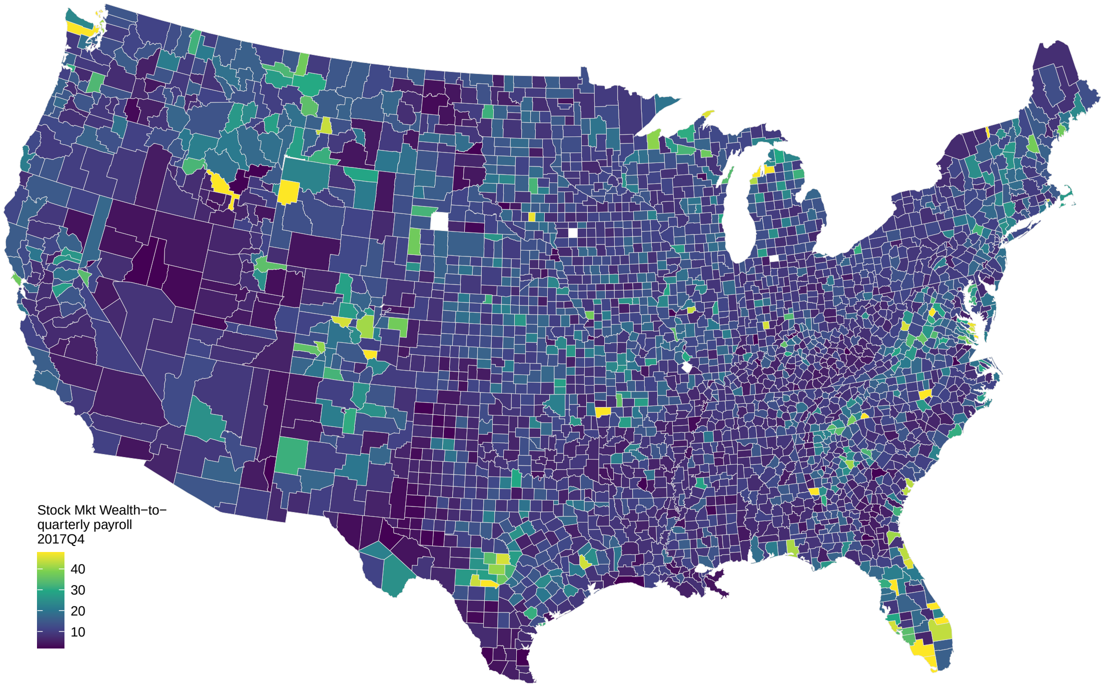

Figure IA2. Stock market wealth-to-quarterly labor income at the end of 2017

IA2. Details on the impulse response interpretation

Definition of impulse response at horizon h to reduced form disturbance z_t at time t is

$$\begin{aligned}\beta^h &= E[y_{t+h}|z_t = 1, \mathcal{F}_t] - E[y_{t+h}|z_t = 0, \mathcal{F}_t] \\ &= E[y_{t+h} - y_t|z_t = 1, \mathcal{F}_t] - E[y_{t+h} - y_t|z_t = 0, \mathcal{F}_t]\end{aligned}$$

where $\mathcal{F}_t$ is a set that includes y_t and all the information that we include the model that might be relevant for agent's forecasting decision. We see that we can interpret the β^h as the difference in levels of y_{t+1} when $z_t = 1$ occurs and when $z_t = 0$ occurs. Alternatively we interpret β^h as the difference in cumulative growth rates $y_{t+1} - y_t$ when $z_t = 1$ occurs and when $z_t = 0$.

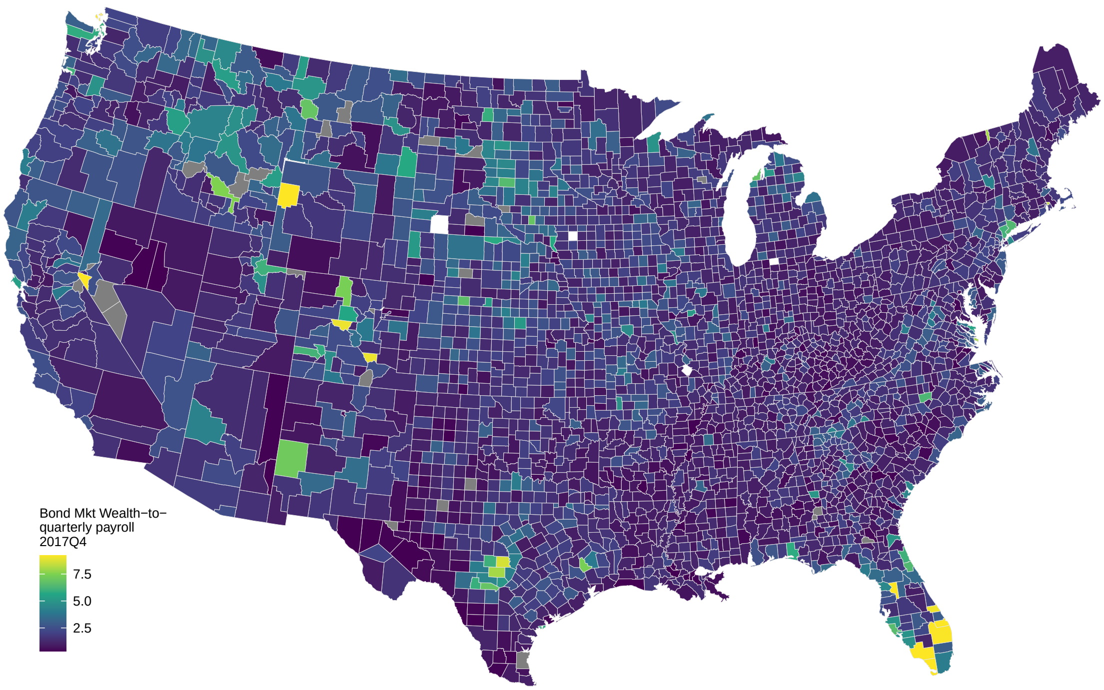

Figure IA3. Bond market wealth-to-quarterly labor income at the end of 2017

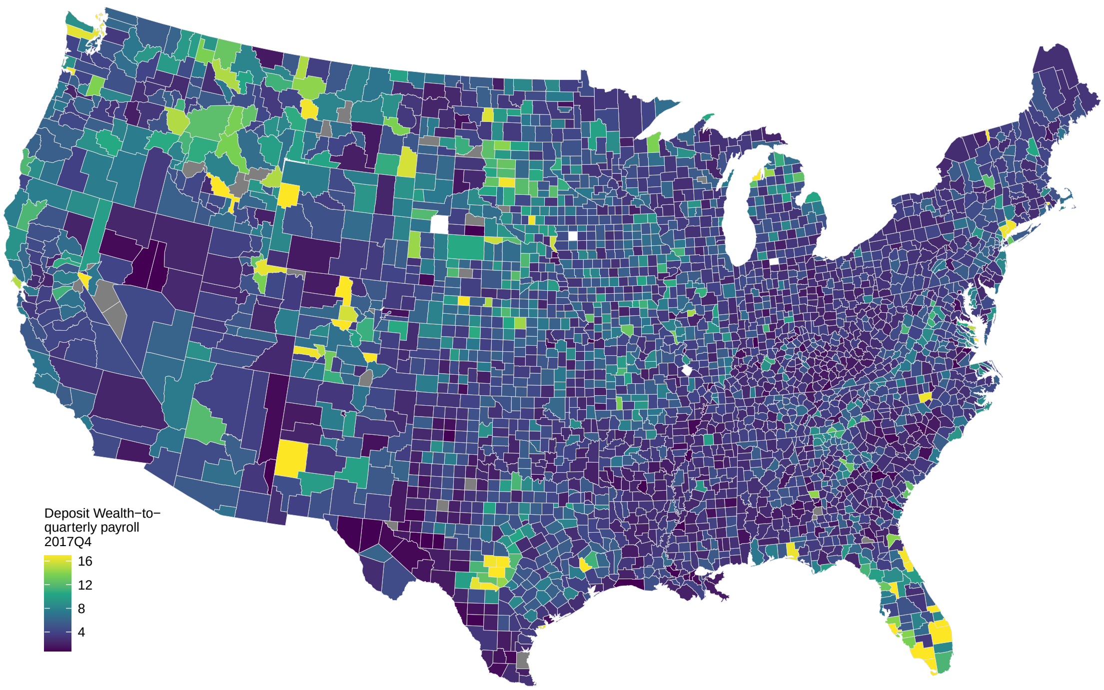

Figure IA4. Deposit wealth-to-quarterly labor income at the end of 2017

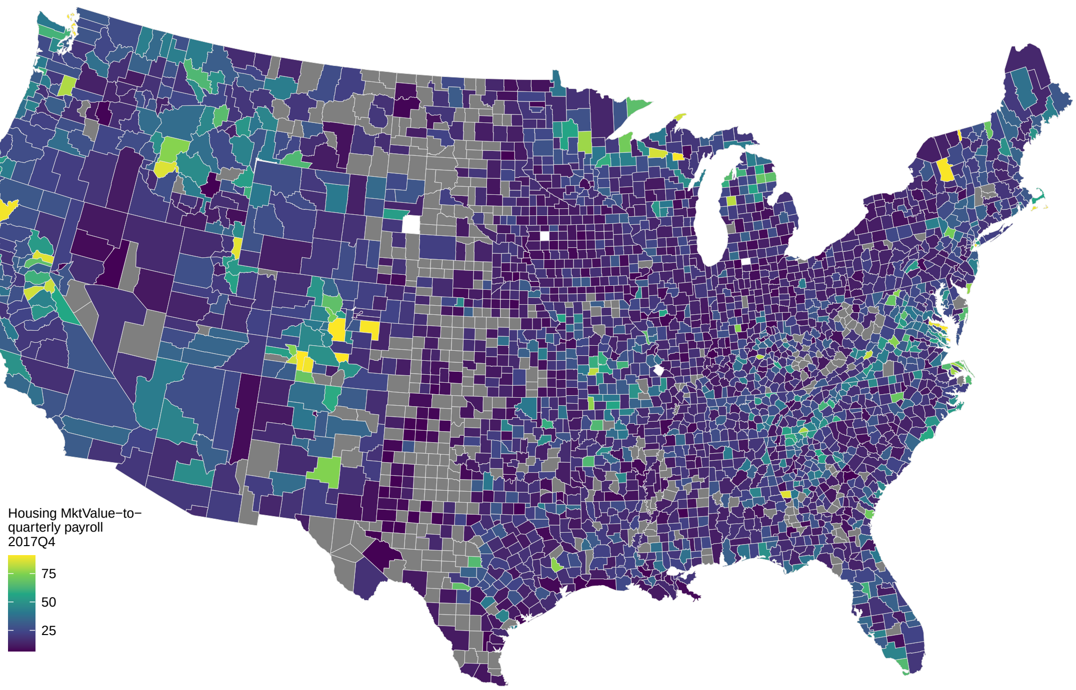

Figure IA5. Housing wealth-to-quarterly labor income at the end of 2017

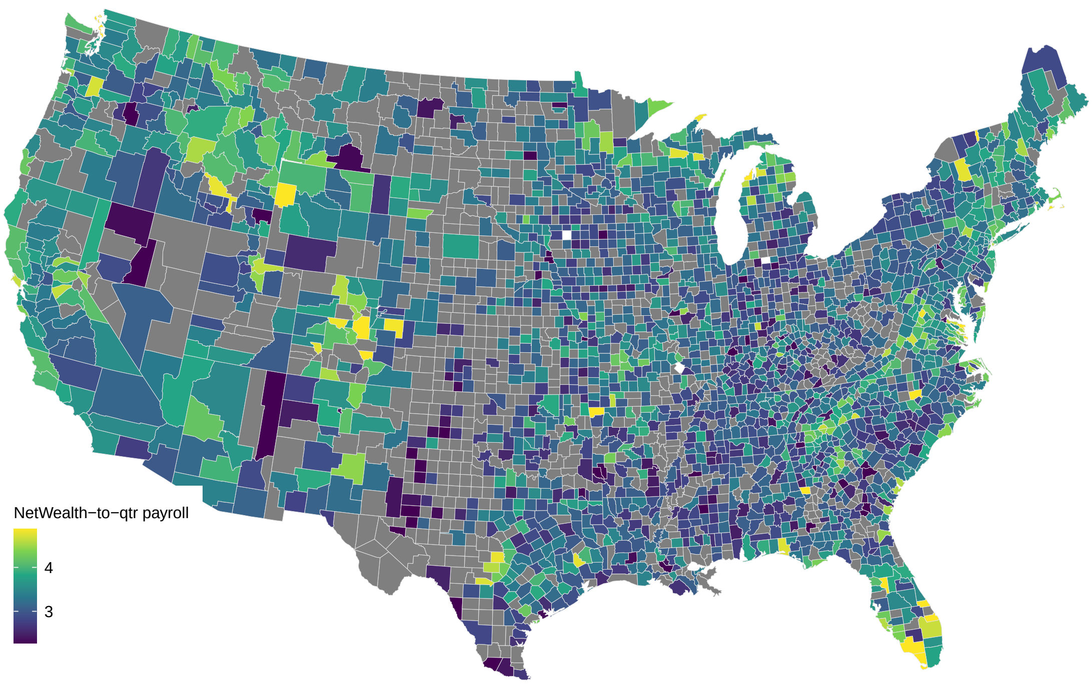

Figure IA6. Net wealth-to-quarterly labor income at the end of 2017

IA3. Responses of personal income and labor share

So far we have focused specifically on how wealth shocks affect local labor market outcomes. This was partly because maximum employment is one of the Fed's mandates but also because the consumption-wealth effects suggest a clear link with labor market outcomes and wealth shocks. However, there is no reason to restrict our analysis to just labor income. In fact, seeing what kind of response people's personal income as a whole has following a wealth shock may help us understand other mechanisms at play. Let us proceed as previously but now change the dependent variables to the county's personal income per capita and the county's labor share. I use personal income (per capita) at the county level from the U.S. Bureau of Economic Analysis obtained through Fed's (now discontinued) GeoFred website and I calculate the labor share of the county's total income as the ratio between the total wage income from IRS and the total personal income from BEA. [52] Table IA1 reports these results for horizon $h = 7$.

How should we interpret the results relative to earlier ones? The personal income per capita interpretation is similar to payroll per capita results. Furthermore, the growth in labor income approximately corresponds to the growth rate difference between labor income $\Delta y^L_{t-1,t+h}$ and capital income $\Delta y^K_{t-1,t+h}$) with a proportionality constant of an average capital share $(1-\alpha)$.That is

$$\Delta LS_{t-1,t+h} = (1-\alpha)(\Delta y^L_{t-1,t+h} - \Delta y^K_{t-1,t+h})))$$

This means that if a particular wealth shock had no impact on labor income but it did have an impact on labor share then the only way for this to happen is that capital income, $\Delta y^K_{t-1,t+h}$, changed.

[52] Note, since most of the capital income to a county does not necessarily originate from the county's local sources, one should think this labor share more as describing the received income composition of a county and not how the gains from production are allocated locally. Note, also that the equal-weighted average county labor share is lower than the aggregate labor share since counties with a bigger share of aggregate personal income have higher labor shares.

Table IA1 Wealth effects on personal income per capita growth and labor share growth: Bartik IV

Dependent Variables:	Pers.inc. pc growth h=7			Labor share growth h=7		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Hous Shift-Share	-0.0002	0.0051	0.0029	0.0164	0.0162	0.0051
	(0.0110)	(0.0113)	(0.0025)	(0.0171)	(0.0171)	(0.0039)
Debt Shift-Share	-0.1360**	-0.1787**	-0.1307**	-0.0951	-0.0930	-0.0302
	(0.0678)	(0.0701)	(0.0590)	(0.0932)	(0.0942)	(0.0761)
Dep Shift-Share	-0.2347	-0.4102	-0.2748***	-0.6041	-0.5968	-0.0264
	(0.2618)	(0.2667)	(0.0990)	(0.3637)	(0.3652)	(0.1938)
Eqty Shift-Share	0.0014*	0.0014	0.0009	-0.0013	-0.0013	-0.0018
	(0.0008)	(0.0008)	(0.0008)	(0.0010)	(0.0010)	(0.0012)
Bond Shift-Share	0.0006	0.0008	0.0005	0.0010	0.0010	0.0001
	(0.0010)	(0.0009)	(0.0008)	(0.0017)	(0.0017)	(0.0015)
Shares+Shifters indiv.	Yes	Yes	Yes	Yes	Yes	Yes
Lagged outcomes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged shocks	Yes	Yes	No	Yes	Yes	No
Bartik Forecast	Yes	No	No	Yes	No	No
Fixed-effects						
State-yq	Yes	Yes	Yes	Yes	Yes	Yes
County (2,674)	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
# State-yq	3,111	3,111	3,315	3,111	3,111	3,315
Observations	159,646	159,646	169,560	159,510	159,510	169,390
Adjusted R^2	0.61	0.60	0.59	0.54	0.54	0.52
Wald (1st stage), Hous Shift-Share	20.0	20.0	129.5	20.4	20.4	129.8
Wald (1st stage), Debt Shift-Share	8,783.2	8,803.8	11,189.0	8,998.6	9,058.7	11,337.1
Wald (1st stage), Dep Shift-Share	777.6	771.0	1,625.2	773.5	764.7	1,609.7
Wu-Hausman	40.5	45.0	42.3	24.3	24.4	61.3

Clustered (County & Time) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents results from the second state of 2SLS regressions corresponding to equation (22) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t-1,t}$. The included controls vary accroding to specification. Shares+Shifters indiv. command includes the the wealth-to-labor income ratios $s_{f,c,t-1}$ and local returns $r_{f,c,t-1,t}$ - the individual components of interactions terms tabulated. Lagged outcomes comand includes the four lags of outcome variable levels y_c, log(personal income per capita) or log(labor share). Lagged shocks command includes the four lags of the instruments. Bartik forecasts command includes Bartik shift-share predicted employment or wage growth the corresponding to outcome variable and the specific horizon h.

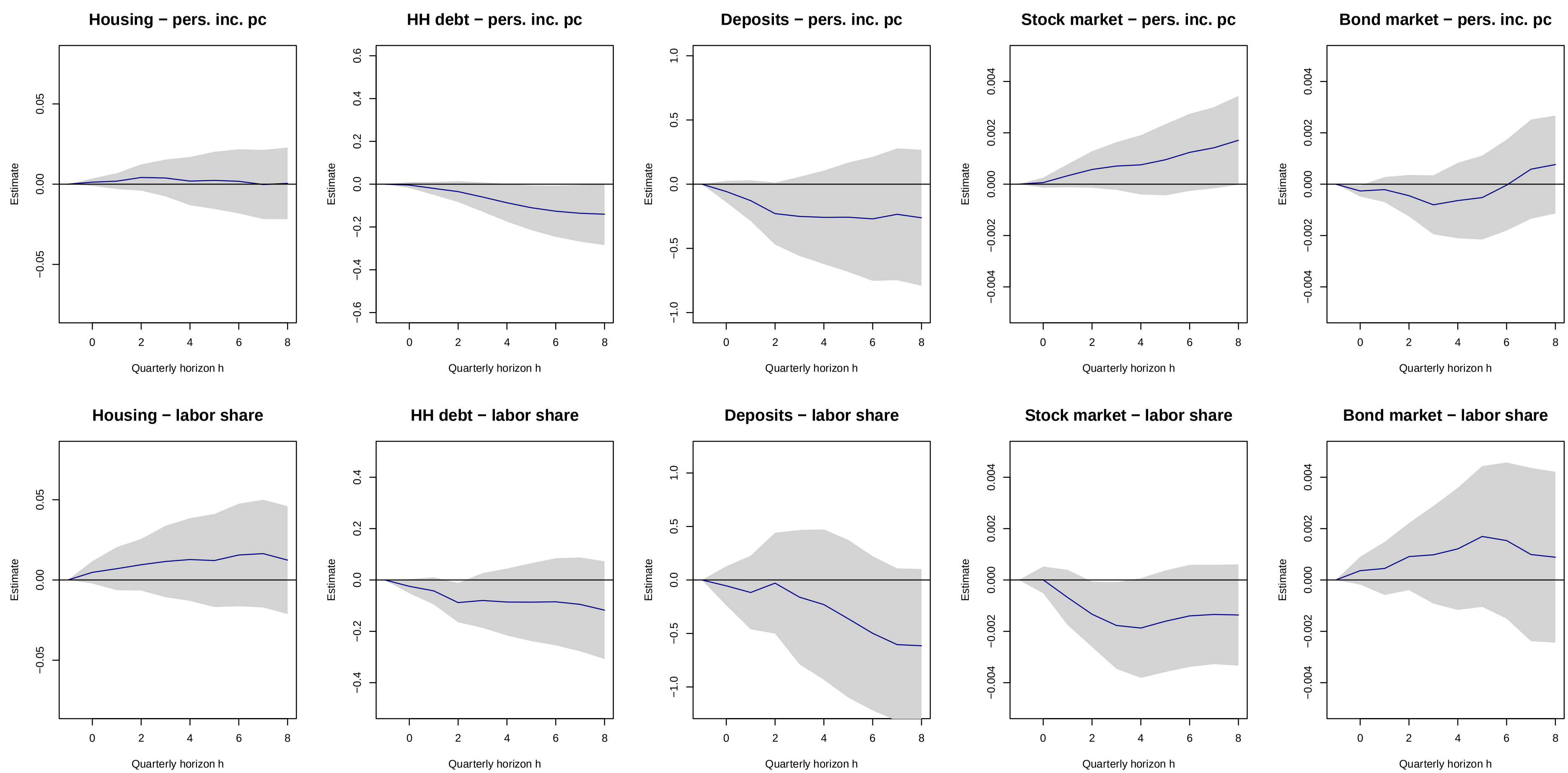

Figure IA7. Impulse response of personal income per capita and county's labor share. This figure presents the impulse responses of the log change in personal income per capita and log change in county's labor share from return or rate shock to housing wealth, household debt, deposit wealth, stock market wealth and bond market wealth 9 quarters forward. The horizontal axis represent the horizon h while the vertical axis corresponds to the value of β_h coefficient of the corresponding asset class f. The specification is based on unweighted regressions and controls for shift-shares components, Bartik employment forecasts corresponding to each horizon h, four lags of log level of outcome, four lags of instrument. The blue line plots the point estimates from the 2SLS regressions. The grey area plots the 95% confidence intervals for the single asset case. Note the different scales in vertical axis between asset classes.

Figure IA7 shows the impulse response functions. When we look at the impulse responses we see that the stock market wealth shocks generate persistent increases in personal income per capita, while the persistent decline in labor share. This is consistent with the previous results that showed no response in county's labor income per capita following a stock market shock if capital income changes even more.

Can we make sense of these findings? First, as we saw from Figure 8, the wealth shocks for stock market wealth are not followed by notably different migration patterns. So from the face of it, population base does not seem to change following these shocks. In addition, we found no clear evidence of labor supply changing following the wealth shocks which means that the increase in capital income is because individuals quit working.Thus it is likely that these shocks may just increase the personal saving rates of individuals that generate higher capital income by construction. I leave a further analysis of the background reasons behind these fascinating contrasts in the responses of labor income, personal income, and labor share to future research.

IA4. Additional robustness checks

Tables IA2 show additional robustness checks when we weight the observations using different weighting schemes of when fixing the wealth-to-labor income ratios to 2000Q4 values.

IA5. Model: Additional details

A. Solving the rates for deposits and mortgages

In practice the deposit rates and mortgage rates are highly influenced by the national market, and this fact is also in the core of the empirical analysis. In this sense, we could have modelled all the return process as exogenous, not just the risky ones, the implications would have been the same. However, the purpose of this section is to show how the return for the inside assets (deposits and mortgages) arise endogenously in the model. This being said, the following analysis is thus mainly theoretical and has less connection to the empirical analysis but it might be useful in understanding how changes in housing wealth could transmit to mortgage rates and thus intrinsically link these markets.

Table IA2 Additional robustness tests

Dependent Variables:	Payroll growth h=3			Employment growth h=3		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
	Observations weighted by debt-to-assets ratio					
Hous Shift-Share	0.0045	-0.0222	0.2563***	0.0040	-0.0025	0.1504***
	(0.0128)	(0.0311)	(0.0738)	(0.0109)	(0.0190)	(0.0339)
Debt Shift-Share	0.0243	-1.291***	-1.594***	-0.2058*	-0.1429	-0.3489
	(0.1344)	(0.3317)	(0.4377)	(0.1051)	(0.3913)	(0.3617)
Dep Shift-Share	0.0561	1.168	-1.838	-0.0980	1.133	-0.0766
	(0.3798)	(0.8905)	(1.339)	(0.2524)	(0.8325)	(0.8036)
Eqty Shift-Share	0.0021**	-0.0009	0.0031	0.0016**	-0.0022*	0.0034
	(0.0008)	(0.0019)	(0.0032)	(0.0008)	(0.0012)	(0.0024)
Bond Shift-Share	-0.0010	-0.0039	0.0196**	-7.97×10^{-5}	-0.0035	0.0104**
	(0.0013)	(0.0047)	(0.0085)	(0.0010)	(0.0029)	(0.0043)
	Observations weighted by 2010 homeownership share					
Hous Shift-Share	0.0062	-0.0320	0.1718*	0.0034	-0.0108	0.1252
	(0.0119)	(0.0462)	(0.1012)	(0.0101)	(0.0197)	(0.1296)
Debt Shift-Share	-0.4669	-0.3663	-3.803**	-0.4197	-0.3767	-2.237
	(0.3284)	(0.8381)	(1.610)	(0.2718)	(0.6694)	(1.525)
Dep Shift-Share	0.2318	0.6323	1.676	-0.1366	0.4854	1.254
	(0.3768)	(1.047)	(1.933)	(0.3020)	(0.8168)	(2.361)
Eqty Shift-Share	0.0016**	-0.0030*	0.0017	0.0011	-0.0023**	0.0020
	(0.0007)	(0.0015)	(0.0043)	(0.0007)	(0.0011)	(0.0044)
Bond Shift-Share	0.0005	0.0003	0.0235**	0.0006	-0.0010	0.0180
	(0.0016)	(0.0051)	(0.0101)	(0.0013)	(0.0033)	(0.0132)
	Observations weighted by gross wealth per capita 2010					
Hous Shift-Share	0.0026	-0.0391	0.1469***	0.0011	-0.0031	0.0882***
	(0.0150)	(0.0318)	(0.0478)	(0.0118)	(0.0194)	(0.0259)
Debt Shift-Share	-0.4970	0.1026	1.838	-0.4999	0.2359	-1.541*
	(0.3763)	(0.9164)	(1.479)	(0.3285)	(0.6892)	(0.9137)
Dep Shift-Share	-0.0326	0.6102	-3.170**	-0.2802	0.9412	0.4413
	(0.4289)	(1.128)	(1.239)	(0.3541)	(0.9206)	(0.7691)
Eqty Shift-Share	0.0017**	-0.0031	0.0020	0.0009	-0.0023*	0.0024
	(0.0007)	(0.0024)	(0.0022)	(0.0007)	(0.0012)	(0.0016)
Bond Shift-Share	0.0006	-0.0034	-0.0010	0.0006	-0.0051	0.0083*
	(0.0017)	(0.0053)	(0.0085)	(0.0014)	(0.0037)	(0.0048)

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Dependent Variables:	Payroll pc growth h=3			Employment pc growth h=3		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
	Obs. weighted by share employment in corresp. industry in 2010					
Hous Shift-Share	0.0125	-0.0368	0.1894***	0.0098	0.0082	0.1480***
	(0.0122)	(0.0467)	(0.0607)	(0.0112)	(0.0342)	(0.0446)
Debt Shift-Share	-0.5705	-1.209	-4.132**	-0.5272*	-0.8331	-2.554*
	(0.3655)	(1.049)	(1.686)	(0.2916)	(0.8619)	(1.286)
Dep Shift-Share	0.3579	0.6674	2.822*	0.0291	0.0716	2.019*
	(0.3962)	(1.296)	(1.495)	(0.2997)	(1.116)	(1.121)
Eqty Shift-Share	0.0018**	-0.0021	0.0041	0.0013*	-0.0006	0.0042
	(0.0007)	(0.0019)	(0.0039)	(0.0007)	(0.0016)	(0.0027)
Bond Shift-Share	0.0003	0.0052	0.0273***	0.0006	0.0027	0.0183**
	(0.0018)	(0.0057)	(0.0094)	(0.0015)	(0.0044)	(0.0071)
	Obs. weighted by share of population 15-34 years in 2010					
Hous Shift-Share	0.0049	-0.0391	0.1469***	0.0011	-0.0031	0.0882***
	(0.0118)	(0.0318)	(0.0478)	(0.0118)	(0.0194)	(0.0259)
Debt Shift-Share	-0.4093	0.1026	1.838	-0.4999	0.2359	-1.541*
	(0.3030)	(0.9164)	(1.479)	(0.3285)	(0.6892)	(0.9137)
Dep Shift-Share	0.1651	0.6102	-3.170**	-0.2802	0.9412	0.4413
	(0.3785)	(1.128)	(1.239)	(0.3541)	(0.9206)	(0.7691)
Eqty Shift-Share	0.0018**	-0.0031	0.0020	0.0009	-0.0023*	0.0024
	(0.0007)	(0.0024)	(0.0022)	(0.0007)	(0.0012)	(0.0016)
Bond Shift-Share	0.0003	-0.0034	-0.0010	0.0006	-0.0051	0.0083*
	(0.0016)	(0.0053)	(0.0085)	(0.0014)	(0.0037)	(0.0048)
	Obs. weighted by share of population 65+ years in 2010					
Hous Shift-Share	0.0071	-0.0391	0.1469***	0.0011	-0.0031	0.0882***
	(0.0115)	(0.0318)	(0.0478)	(0.0118)	(0.0194)	(0.0259)
Debt Shift-Share	-0.3494	0.1026	1.838	-0.4999	0.2359	-1.541*
	(0.3304)	(0.9164)	(1.479)	(0.3285)	(0.6892)	(0.9137)
Dep Shift-Share	0.0882	0.6102	-3.170**	-0.2802	0.9412	0.4413
	(0.3650)	(1.128)	(1.239)	(0.3541)	(0.9206)	(0.7691)
Eqty Shift-Share	0.0014**	-0.0031	0.0020	0.0009	-0.0023*	0.0024
	(0.0007)	(0.0024)	(0.0022)	(0.0007)	(0.0012)	(0.0016)
Bond Shift-Share	-0.0003	-0.0034	-0.0010	0.0006	-0.0051	0.0083*
	(0.0016)	(0.0053)	(0.0085)	(0.0014)	(0.0037)	(0.0048)

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Dependent Variables:	Payroll pc growth h=3			Employment pc growth h=3		
	Non-tr.	Tr.	Cons.	Non-tr.	Tr.	Cons.
	Wealth-to-labor income ratios fixed to 2000Q4 values					
Hous Shift-Share	0.1193*	-0.0682	0.3132**	0.0215	-0.0568	0.2361
	(0.0665)	(0.0720)	(0.1511)	(0.0257)	(0.0422)	(0.1451)
Debt Shift-Share	-1.784	-2.472	-4.662	-0.1745	-1.185	-5.593
	(1.318)	(1.958)	(4.689)	(0.5460)	(0.8024)	(5.135)
Dep Shift-Share	3.054	4.574	13.79	-0.4719	2.072	13.55
	(2.596)	(4.608)	(8.623)	(1.224)	(2.311)	(9.247)
Eqty Shift-Share	0.0002	-0.0057**	0.0022	0.0011**	-0.0037**	0.0011
	(0.0023)	(0.0026)	(0.0055)	(0.0004)	(0.0014)	(0.0061)
Bond Shift-Share	0.0377	-0.0828	-0.0744	0.0296***	-0.0286	-0.0842
	(0.0622)	(0.0760)	(0.1826)	(0.0091)	(0.0318)	(0.1823)
	OLS Baseline specification					
Hous Shift-Share	-0.0011	-0.0208***	0.0192***	-0.0031*	-0.0148***	0.0155***
	(0.0020)	(0.0058)	(0.0051)	(0.0016)	(0.0044)	(0.0039)
Debt Shift-Share	-0.0950	-0.0723	-0.0946	-0.2259**	-0.1296	-0.0203
	(0.1123)	(0.3053)	(0.3734)	(0.0905)	(0.2436)	(0.2981)
Dep Shift-Share	0.1993	0.5732	-0.0132	-0.0402	0.4521	0.1955
	(0.2516)	(0.5043)	(0.5346)	(0.1631)	(0.4395)	(0.3942)
Eqty Shift-Share	0.0014**	-0.0032**	-0.0008	0.0008	-0.0023**	0.0008
	(0.0007)	(0.0013)	(0.0038)	(0.0006)	(0.0011)	(0.0026)
Bond Shift-Share	-0.0002	-0.0003	0.0010	0.0011	-0.0015	0.0006
	(0.0014)	(0.0019)	(0.0069)	(0.0012)	(0.0012)	(0.0071)

Notes: This table presents coefficients of the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ from the second state of 2SLS regressions corresponding to equation (27) where the interaction term $s_{f,c,t-1}r_{f,c,t-1,t}$ is instrumented with corresponding Bartik instrument $s_{f,c,t-1}r_{f,t}$. The included controls are those as in the baseline specification, except the shifters and shares individually are dropped from the specification when fixing the wealth-to-labor income ratios to avoid the collinearity. Other controls are four lags of outcome variable levels y_c, four lags of the instrumented shocks (also for equities and bonds), Bartik shift-share predicted employment growth the corresponding to outcome variable and the specific horizon h.

Now I show how the rates $\frac{dV_t^{Dep}}{V_t^{Dep}}$ and $\frac{dV_t^D}{V_t^D}$ are determined by the exogeneous risky capital processes. If all the agents had access to *same* riskless asset it should give the same return for all agents. However, given that intermediaries are operating in two segmented markets where the stochastic discount factor of counterparties define *different* risk free rates, the intermediaries want to borrow at the low rate and lend at the high rate. The wedge κ_t between these rates can be thought to represent the intermediary's market power or rents from intermediation induced by the market segmentation. With no loss of generality I denote the instantaneous risk free rate on deposits as r_t and the difference between deposit and mortgage rate as κ_t. That is

$$\frac{dV_t^{Dep}}{V_t^{Dep}} = r_t dt \tag{34}$$

$$\frac{dV_t^D}{V_t^D} = (r_t + \kappa_t)dt \tag{35}$$

Due to this discrepancy in borrowing and lending rates, the intermediary could make infinite profits by borrowing at a low rate and lending at a high rate. However, I assume that intermediaries have capital requirements so that their short position can never be larger than some upper bound χ. This is analogous to intermediaries having to maintain some minimal capital ratio. When intermediaries are borrowing from savers and lending to borrowers this implies $\frac{V_t^{Dep}}{N_t^i} \leq \chi$. Since the intermediary's return is always riskless they will borrow the maximum amount that the capital requirement allows and thus this upper bound will bind. Formally if we let $\kappa_t \geq 0$ which correspond to the case where the savers are financing the borrowers and $(1 - \theta_t^i)$ is the amount of funds invested in the asset which intermediaries are long (the mortgages) then we can show through following steps

$$\begin{aligned}
\frac{dN_t^i}{N_t^i} &= (1 - \theta_t^i)dR_t^l + \theta_t^i dR_t^b \\
&= (1 - \theta_t^i)(r_t + \kappa_t)dt + \theta_t^i r_t dt \\
&= (r_t + (1 - \theta_t^i)\kappa_t)dt \\
&= (r_t + (1 + \chi)\kappa_t)dt
\end{aligned}$$

that intermediary net wealth growth is an increasing function of risk-free borrowing rate r_t, spread κ_t, and the looseness of capital requirement χ.

Now we solve the risk-free rates induced by the savers and borrowers optimizing conditions. First note that the price of risk $\varsigma_{b,t}$ to borrowers are given by the asset Sharpe ratio (see e.g. Brunnermeier and Sannikov, 2016). This implies

$$\mu_{H,t} - (r_t + \kappa_t) = \varsigma_{b,t}\sigma_{H,t} \tag{36}$$

Now with log preferences $\varsigma_{b,t} = \sigma_{N_b,t} = \frac{H_t}{N_{b,t}}\sigma_{H,t}$. Thus

$$(r_t + \kappa_t) = \mu_{H,t} - \frac{H_t}{N_{b,t}}\sigma^2_{H,t} \tag{37}$$

Rearranging we see

$$\frac{H_t}{N_{b,t}} = \frac{\mu_{H,t} - r_t - \kappa_t}{\sigma^2_{H,t}} \tag{38}$$

that implies that borrower leverage increase when the expected return for housing is high and mortgage rates are low while the housing return variance is small. Similarly, for savers where I denote the savers' risky asset portfolio return and volatility (a weighted average of equity and bond returns) with $\mu_{EB,t}$ and $\sigma_{EB,t}$ it holds that

$$r_t = \mu_{EB,t} - \frac{E_t + B_t}{N_{s,t}}\sigma^2_{EB,t} \tag{39}$$

Subtracting the equation (39) from equation (37) we get the spread

$$\kappa_t = \mu_{H,t} - \mu_{EB,t} - \left(\frac{H_t}{N_{b,t}}\sigma^2_{H,t} - \frac{E_t + B_t}{N_{s,t}}\sigma^2_{EB,t}\right) \tag{40}$$

Notably, it also holds that though intermediaries can engage in some (limited) arbitrage their wealth does not grow to crowd out the wealth of other agents since intermediaries' wealth is always dependent on the returns for other agents. Furthermore, the other agents have the benefit that they can invest in risky assets, which pay out higher absolute returns. If we specified some functional form for asset returns we could solve the model by solving the differential equations of wealth shares involving the joint wealth and asset positions. This would probably be simplest in the special case with no intermediary at all i.e. the borrowing and lending rates would equal ($\kappa_t = 0$) and $V_t^{Dep} = V_t^D$. However, due to the partial equilibrium nature of the model, I do not solve these differential equations since for the purpose of this research question and for our empirical analysis the solutions are likely to add very little insight. Finally, it is worth noting that by choosing the processes for asset returns appropriately we could obtain a stationary solution where no household group dominates the economy in the long run despite differences in patience.

B. Including the inflation dynamics

In this section, I will illustrate that with an exogenous inflation rate, the empirical specification stays unchanged relative to the real model. To start, consider now there exists an exogenous inflation process $\frac{dP_t}{P_t}$ and a price level P_t that affects the dynamics wealth of each asset class. Thus now it is the nominal return on wealth f that is exogenous and defined as

$$\frac{d(P_t V_t^f)}{P_t V_t^f} := dR_t^{f,nom} \tag{41}$$

We will see that everything goes through as before. A growth of nominal consumption is

$$\frac{d(P_t C_t)}{P_t C_t} = \sum_j \frac{P_t C_{j,t}}{P_t C_t} \frac{d(P_t C_t^j)}{P_t C_t^j} \tag{42}$$

Then we can use the fact that $P_t C_t^j = \rho_j P_t N_t^j$ and $d(P_t C_t^j) = \rho_j d(P_t N_t^j)$ to get

$$\frac{d(P_t C_t)}{P_t C_t} = \sum_j \frac{\rho_j P_t N_{j,t}^j}{P_t C_t} \frac{d(P_t N_t^j)}{P_t N_t^j} \tag{43}$$

Then let us specify $\frac{d(P_t N_t^j)}{P_t N_t^j}$ for each agent as previously. For example borrowers

$$\frac{d(P_t N_t^j)}{P_t N_t^j} = \frac{P_t V_t^H}{P_t N_t^j} \frac{d(P_t V_t^H)}{P_t V_t^H} - \frac{P_t V_t^D}{P_t N_t^j} \frac{d(P_t V_t^D)}{P_t V_t^D} - \rho_b dt = \frac{P_t V_t^H}{P_t N_t^j} dR_t^{H,nom} - \frac{P_t V_t^D}{P_t N_t^j} dR_t^{D,nom} - \rho_b dt \tag{44}$$

Let us then plug these agent specific wealth processes into equation above. This gives

$$
\begin{aligned}
\frac{dP_tC_t}{P_tC_t} &= (\rho_s - \rho_i)\frac{P_tV_t^{Dep}}{P_tC_t}dR_t^{Dep,nom} + (\rho_i - \rho_b)\frac{P_tV_t^D}{P_tC_t}dR_t^{D,nom} \\
&+ \rho_b\frac{P_tV_t^H}{P_tC_t}dR_{t,nom}^H + \rho_s\frac{P_tV_t^E}{P_tC_t}dR_t^{E,nom} + \rho_s\frac{P_tV_t^B}{P_tC_t}dR_t^{B,nom} \\
&(-\rho_b^2\frac{P_tV_t^H}{P_tC_t} + (\rho_b^2 - \rho_i^2)\frac{V_t^D}{C_t} + (\rho_i^2 - \rho_s^2)\frac{P_tV_t^{Dep}}{P_tC_t} - \rho_s^2\frac{P_tV_t^E}{P_tC_t} - \rho_s^2\frac{P_tV_t^B}{P_tC_t})dt
\end{aligned}
\tag{45}
$$

Finally let us use the fact that the $P_tC_t = \frac{1}{\alpha}P_tW_tL_t$ and replacing P_tC_ts and $d(P_tC_t)$s we get

$$
\frac{d(P_tW_tL_t)}{P_tW_tL_t} = \sum_f \beta_f \frac{P_tV_t^f}{P_tW_tL_t}dR_t^{f,nom} + \sum_f \gamma_f \frac{P_tV_t^f}{P_tW_tL_t}dt \tag{46}
$$

where the meta-parameters β_f and γ_f are defined as before. This is the actual model we test in the empirical section.