Local Scars of the US Housing Crisis[*]

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Abstract

We show that the 2006-09 US housing crisis had scarring local effects. For a given county, a 10% reduction in housing wealth from 2006 through 2009 led to a 3.3% decline in employment by 2018, and a commensurate decline in value added. This persistent effect occurred despite the shock having no significant impact on labor productivity and only a short-lived impact on household demand, house prices, and household leverage. We find that the local labor market adjustment to the housing shock was particularly costly: local wages did not respond, and long-run convergence in the local labor market slack instead took place entirely through population losses in affected regions. These results on population adjustment leading to mean-reversion in local slack extend the seminal observations by Blanchard and Katz (1992) to the effects of a temporary and identified local demand shock. Additionally, we show that the housing bust, compared with the housing boom, had asymmetric effects on employment and wages, indicating a role for downward wage rigidity.

JEL classification: G01; R23; E24

Keywords: US housing collapse; Scarring effects; Persistent regional effects; Local labor markets; Asymmetric effects; Downward wage rigidity; Sectoral effects

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1 Introduction

Can a temporary macroeconomic shock cast a long shadow even if it does not directly destroy capital or affect labor productivity? The housing crisis of 2006-09 suggests that this may be the case as, by many measures, the US economy appears to have taken very long to recover from it (Coibion, Gorodnichenko, and Ulate, 2017).[1] As pointed out by Fernald, Hall, Stock, and Watson (2017), however, it can be hard to disentangle the effects of a one-time shock from underlying trends. Identifying persistent responses to the crisis, and shedding light on the mechanisms that may underly them, can help inform targeted policies to mitigate the long-term impact of large shocks. For instance, as the world economy shuts down in response to a pandemic, policymakers need to worry about its aftermath. To the extent that much of the economic effect of the pandemic is through a severe but temporary reduction in demand for certain goods and services, some of its long-term impacts might resemble the ones observed after the 2006-09 housing crisis.

We provide causal evidence for very persistent local impact of the housing cycle in the US. We find that its local effect was highly asymmetric, with little local output or employment effect in the boom phase but very persistent employment, GDP, and population losses during the bust. Its impact on the downturn appears to operate through the demand side since we find no significant change in labor productivity and only temporary effects on measures of labor market slack, such as the employment-to-population ratio and the unemployment rate. Moreover, the shock had a short-lived impact both on household demand (as measured by nontradable-sector employment) as well as house prices and household leverage, lending credence to its temporary nature.

Regarding the labor market adjustment to these scarring effects on employment, we find no role for wage adjustment. In particular, although wages rose marginally with the housing boom, they did not react at all to the housing bust, implying a potential role for downward wage rigidity. Together, those findings imply that regional labor market adjustment took place entirely through population movements, for which we provide direct additional evidence. While the observation of permanent population movements leading to adjustment in slack is consistent with classic findings by Blanchard and Katz (1992) for general local shocks, the lack of local wage reactions and asymmetries in labor market adjustment between boom and bust phases are novel findings, that are specific to the identified housing shock.

We start our analysis by documenting some general patterns: US states and counties with a more substantial housing decline during 2006-09 had a lower level of employment and output in 2018, relative to pre-2002 trend. Critically, the divergence is entirely a post-crisis phenomenon, with different locations behaving similarly in the boom years. The housing bust, therefore, plays a unique role in driving regional differences in employment and output. These permanent changes occur even though regional gaps in house prices, household leverage, and measures of local labor market slack converge back to mean after some years.

Next, we undertake a formal econometric exercise to give a causal interpretation of these pat-

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[1]Such a slow recovery from a mostly transient demand shock is also consistent with cross-country evidence from Reinhardt and Rogoff (2009) and Jordà, Singh, and Taylor (2020).

terns. The econometric model allows us to formally control for local trends and various local characteristics that may correlate with the local impact of macroeconomic shocks other than housing. We use the Saiz (2010) housing-supply elasticity as an IV to further eliminate the role of local shocks that may simultaneously affect local output and housing wealth.[2] While the Saiz (2010) instrument is by now an "industry standard," we take extra care in precisely showing conditions for it to be valid in our application.[3]

We estimate impulse responses to the identified 2006-09 US county-level housing shock, adapting Jordà's (2005) local projection to a cross-sectional context. We verify that the shock is associated with a boom-bust cycle in house prices and household leverage that is largely finalized by 2014. We also find that we can interpret the identified shock as a demand shock, with no significant short- or long-run effect on labor productivity. Nevertheless, the initial 2006-09 shock has contractionary effects on employment and output as far out as 2018. In particular, we find that at the county level, a 10% negative housing shock in 2006-09 leads to a 3.3% drop in employment and a commensurate drop in output in 2018 compared with 2006. Moreover, there are no significant effects on employment during the 2002-2006 period, indicating that the employment losses relative to 2006 are also losses relative to the counterfactual case in which there was no housing cycle. This shows clearly the asymmetric nature of the housing shock.

We next find that regional slack measures, such as employment-to-population ratios and unemployment rates return to their pre-crisis (2002) averages around 2009-10. Moreover, this convergence in slack occurs during a period in which the effects on employment continue to increase progressively. It follows that the convergence in regional slack happens because of slow population adjustment as workers move out of hard-hit areas. We indeed show direct evidence for such smooth population adjustment over time.

These findings on long-lasting effects on employment and output combined with more transient effects on regional slack raise the critical question of what happens to wages. Again we find evidence for asymmetric effects. While the housing shock appears to lift wages marginally in the boom sphase, there is no evidence of wage contraction in the bust. We also show that identifying the housing shock is essential for this result, as OLS estimates would imply wage effects. The difference emerges because our IV procedure isolates the impact of the housing shock from that of productivity shocks, which are well-known to drive a positive comovement between wages and employment/output. Those results, in turn, imply that evidence on wage rigidity and, more generally, Phillips Curve coefficients based on regional data depend on the nature of the shock and should be interpreted with care even if they exploit a massive shock such as the 2006-09 housing crisis.

Next, we investigate heterogeneous effects across sectors. Sectoral data provide evidence both of asymmetry between boom and bust phases, as well as of the transient impact on demand. While

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[2]Apart from Mian, Rao, and Sufi (2013) and Mian and Sufi (2014), the instrument has been used recently to gauge the effects of the housing cycle by Stroebel and Vavra (2014) and Davis and Haltiwanger (2019).

[3]Specifically, we show that it is valid if, after allowing for controls, it is uncorrelated with the sensitivity of individual locations to aggregate shocks (including the housing shock). Our analysis addresses existing criticism of the instrument head on and shows that our results are robust to a wide range of stringent control schemes.

the housing cycle only lifts construction employment during the boom, it has a more widespread effect during the bust. We revisit Mian and Sufi's (2014) results regarding the employment effects on non-tradables, and find that those are indeed significant in the short-run but not so in later years, suggesting the demand shock was temporary. In contrast, we find evidence for persistent effects on high-skilled services employment.

Lastly, we sort counties by their ex-ante growth rates from 1990-2000 and run the regression on separate quantiles. We find that the long-lasting employment effects increase with ex-ante local growth trends, and are not present in the low-growth counties. The additional sensitivity of fast-growing regions to the demand shock is consistent with findings by Glaeser and Gyourko (2005).

Our results have implications for optimal currency areas as they highlight that local adjustment to asymmetric demand shocks in the US took place through labor mobility over several years rather than through wage movements. Therefore, even for the US economy, local adjustment to temporary asymmetric shocks can involve very long-lasting and costly changes. Additionally, the higher effects we find on ex-ante fast-growing regions suggest that a very large shock like the current pandemic may have persistent effects on the dynamism of the overall economy if labor reallocates from more-affected to less-affected regions.

Our paper connects to the literature on the local dynamic responses to shocks, building on seminal work by Blanchard and Katz (1992) and Davis, Loungani, and Mahidhara (1997). A recent application of their methodology to the Great Recession is in Yagan (2019). We add to that work by explicitly isolating the effects of the housing shock from other sources of local variation. This distinction turns out to be essential to uncover the lack of local wage and productivity adjustment in response to the crisis. More broadly, the local scars of the housing crisis that we establish echo findings that changes in trade tariffs have very persistent effects in local labor markets (Dix-Carneiro and Kovak, 2017), and that differences in local economic conditions are very persistent (Amior and Manning, 2018).

Recent empirical work in macroeconomics has frequently exploited regional variation to understand the labor market impact of the housing cycle. Crucially, Mian and Sufi (2014) in a seminal paper show the short-run effects of the housing crash on labor markets due to lower household demand. Papers following their study have focused, for the most part, on similar short-run dynamics. For instance, Gertler and Gilchrist (2018) examine the effect of housing shocks on local employment over two and a half years; Gilchrist, Siemer, and Zakrajsek (2018) examine asymmetries in the two-year impact of house price fluctuations in boom and bust phases, and Guren, McKay, Nakamura, and Steinsson (2018) show how the one-year reaction of retail employment to house prices has changed over time. A similar focus on short-run variation also underlies estimates based on structural or quantitative models, such as Jones, Midrigan, and Philippon (2018) and Beraja, Hurst, and Ospina (2019). In comparison, we directly estimate the dynamics of multiple local economic variables over the almost 20 years encompassing the housing boom-bust cycle and its aftermath.

The need for such a holistic view of the housing cycle, that is, a joint examination of both the housing boom and bust phases, is proposed by Charles, Hurst, and Notowidigdo (2018). In particular, they find a symmetric movement of employment-to-population ratios between boom and bust, with labor market slack measured in that way converging back to its pre-housing boom levels by 2011. We add to their work by examining a wider range of variables over a longer time period, finding that effects on employment, output, population, and wages are, in fact, asymmetric over the housing cycle. Those in turn lead to local scarring effects on employment and output, lasting for more than ten years after the pre-crisis peak. We then uncover a mechanism for the convergence in employment-to-population ratios: it occurs through population losses in the most affected regions during the housing crisis.

Finally, from a methodological standpoint, our results highlight a key difference between local and aggregate elasticities and economies. The adjustment through population movement is not available at the county level. Therefore, if wages are rigid, then demand shocks can have persistent effects on aggregate slack even if that is not apparent in regional data. The results, therefore, echo Dupor, Karabarbounis, Kudlyak, and Mehkari's (2018) point, that explicitly allowing for regional spillovers can alter how estimates of local effects of shocks extrapolate to aggregate elasticities. The findings in our paper should help inform general equilibrium models, as one component we show to be important empirically is population losses in more affected regions that lead to convergence in economic slack with no response of wages or productivity.

2   Data and Motivating Evidence

We next describe in detail the data we use in the paper as well as present some stylized facts that serve as motivating evidence for our econometric analysis.

2.1   Data

The primary dataset we use is the Quarterly Census of Employment and Wages (QCEW) from the Bureau of Labor and Statistics (BLS). It draws on employment and wages of establishments reporting to unemployment insurance programs, and covers more than 95% of jobs in the US. It is the dataset of choice for the Bureau of Economic Analysis (BEA) for the production of national accounting estimates and for the BLS as a frame for the Current Employment Statistics.[4] The dataset includes total employment and wage bill by industry and county. In an extended analysis, we also use the American Community Survey (ACS) data to complement the wage-regression results by constructing an adjusted wage index.

For other important variables, we use additional data sources. We draw on the Local Area Unemployment Statistics (LAUS) dataset from BLS for the county-level unemployment rate and employment-to-population ratio. To examine the local responses of output to the housing shock,

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[4]Compared to the County Business Patterns, it is more encompassing, since it includes government employees and a few other industries.

we use Local Area Gross Domestic Product (LAGDP) data for 2002 through 2018 from BEA on county-level GDP that has been made available recently. Moreover, in order to investigate migration patterns, we use population data from the County Resident Population Estimates from the US Census Bureau after 2000 and the US Intercensal County Population data before that. For some robustness checks and some splits by worker demographics and firm characteristics, we use the Quarterly Workforce Indicators (QWI) from the US Census Bureau.

On the household finance side, we obtain debt-to-income (DTI) ratios for different counties using data on household debt from the Equifax/Federal Reserve Bank of New York Consumer Credit Panel (CCP) made available as part of the extended Financial Accounts of the United States on the Federal Reserve Board of Governors website.[5] For comparability with prior work, we use the change in housing net worth (defined below) made available in Mian and Sufi's (2014) replication files. For a robustness check, we use 2000 census data to construct a ratio of housing net wealth to income. Finally, we use county-level CoreLogic's HPI data as a measure of house prices.

For more details on data sources and construction, see Appendix B.

2.2 Descriptive Facts

We now show suggestive evidence for large and persistent local effects of the housing crisis. In particular, we are interested in understanding how changes to housing net worth around the housing crisis affected local outcomes, such as employment, output, house prices, employment-to-population ratios, and unemployment rates. Most importantly, we evaluate the extent to which those effects can be characterized as transitory or permanent.

We follow Mian and Sufi (2014) in defining the change in housing net worth in a given region n from 2006 through 2009 (Δ_{06-09}Net Worth$_n$) by
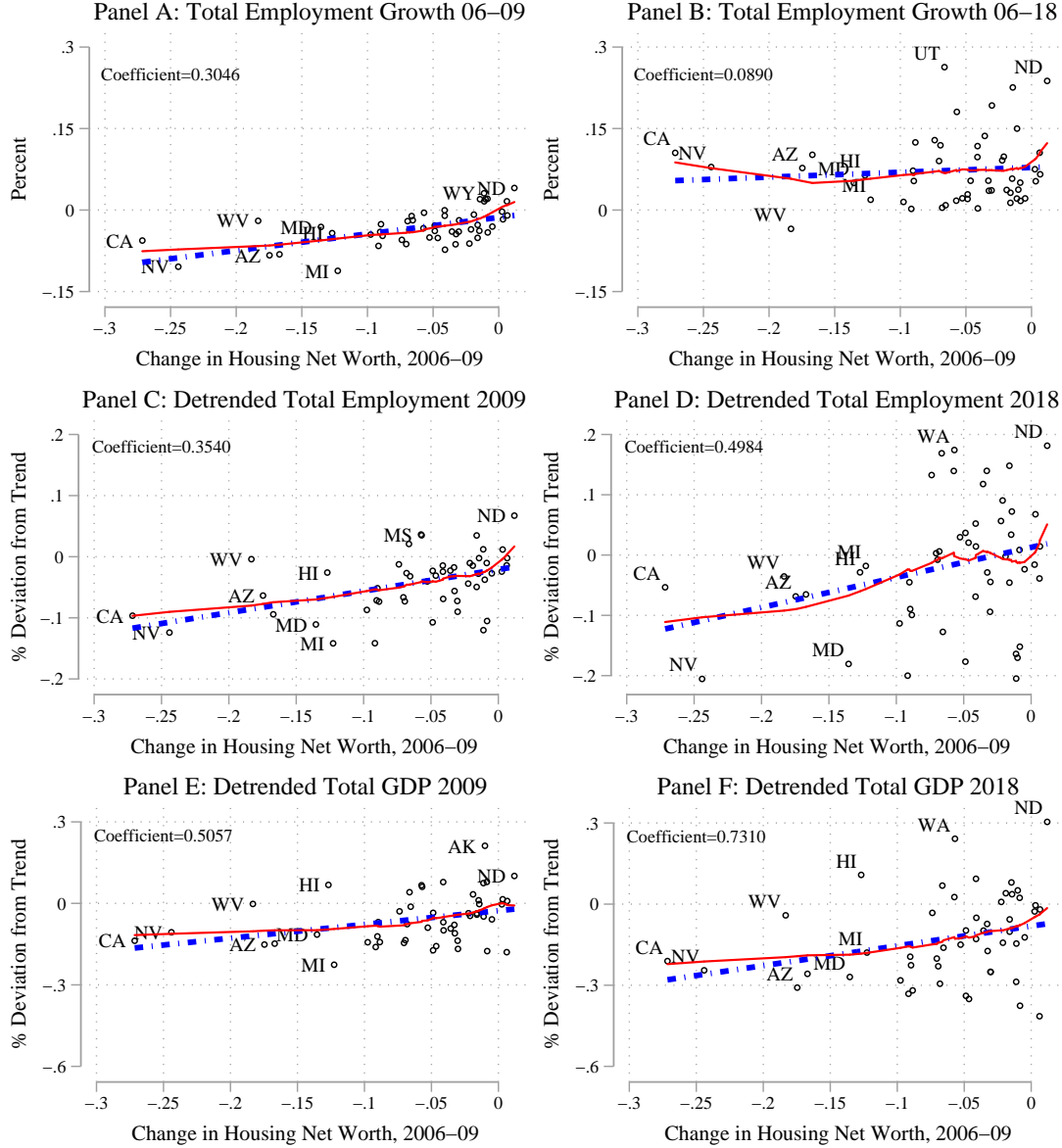
$$\Delta_{06-09}\text{Net Worth}_n = \Delta_{06-09}\text{House Price Index}_n \times \frac{\text{Housing Wealth}_{n,2006}}{\text{Housing Wealth}_{n,2006} + \text{Financial Wealth}_{n,2006} - \text{Debt}_{n,2006}}.$$

That is, the change in household net worth due to housing is given by the change in the house price index multiplied by a leverage term calculated using initial asset positions.

We start with an exercise that both depicts basic patterns clearly and shows the importance of adequately accounting for heterogeneous trends when examining the long-run local responses to the housing cycle. Panels A and B in Figure 1 show the change in employment in different states starting in 2006 and ending in 2009 and 2018, respectively. They are plotted against the change in housing net worth from 2006 through 2009.[6] Panels A and B in Figure 1 clearly show that high net worth shock regions were also the ones that suffered more pronounced losses in employment over the shorter period. Still, the picture appears more muddled over the longer horizon. Those results,

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[5]At the time of writing, the data was available at the source link: https://www.federalreserve.gov/releases/z1/dataviz/household_debt/county/map/#state:all;year:2018

[6]State-level changes in housing net worth are calculated by taking a weighted average of county-level changes from the Mian and Sufi (2014) replication file.

Panel A: Total Employment Growth 06–09

Panel B: Total Employment Growth 06–18

Panel C: Detrended Total Employment 2009

Panel D: Detrended Total Employment 2018

Panel E: Detrended Total GDP 2009

Panel F: Detrended Total GDP 2018

Notes: The upper panels plot the state-level total employment growth from 2006-09 (Panel A) and that from 2006-18 (Panel B) versus the changes in housing net worth from 2006-09. The middle panels plot the 2009 and 2018 detrended state-level total employment (Panel C and D) versus the changes in housing net worth from 2006-09. The lower panels plot the 2009 and 2018 detrended state-level total GDP (Panel E and F) versus the changes in housing net worth from 2006-09. Employment and GDP trends are calculated by taking average growth rates from 1998-2002 for each state and using those to project 2002 employment and GDP linearly into the future. Detrended employment and GDP values are deviations from those trends. Blue dashed lines are linear predictions, and red solid lines plot nonparametric LOESS regression.

Figure 1: Changes in Employment, Output and Housing Net Worth (State Level)

however, neglect the existence of different trends: for instance, since California was a fast-growing state before the housing boom, one would naturally expect its employment levels to catch up with other states.

Allowing for heterogeneous trends changes the picture substantially, as clearly indicated by
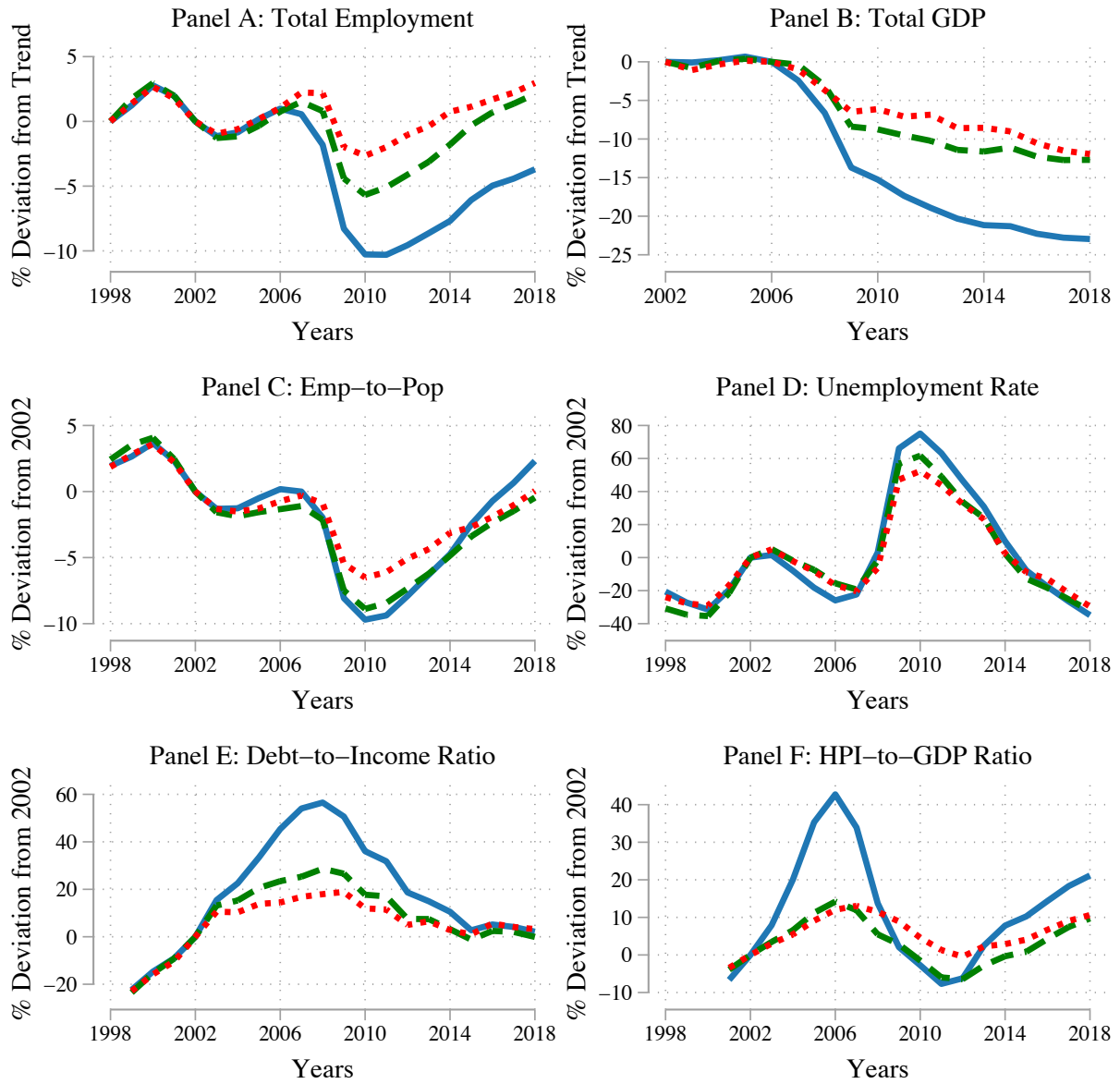
Panels C and D in Figure 1, which show the difference between employment in each state, and what one would have projected it to be in 2002 based on 1998-2002 growth rates. By choosing 2002 as our base year, we also discount any differential gain from the credit boom years between 2003 and 2006. When trends are removed in this way, the relationship between the change in housing net worth and changes in employment over the longer horizon becomes very stable and linear. Panels E and F in Figure 1 shows a similar long-run effect on GDP, once we remove trends in the same way.

A more compact way of showing the same stylized facts is to sort counties by quantiles in terms of the size of the change in housing net worth from 2006 through 2009. This procedure also compares more directly to our econometric results as we use county-level data for that purpose. Figure 2 shows deviations from trend for employment and output and deviations from 2002 for other variables, once counties are grouped in that way. The lines refer to progressively lower quantiles in terms of the impact. The behavior of employment within those different bins is depicted in Panel A. There is not a noticeable difference between the top and bottom quantiles of the net worth shock before the recession starts. Counties with sharper net worth declines, however, lose more jobs in the recession years, and those relative losses persist in the long-run. The same persistent movement is reflected in GDP, as shown in Panel B.

Panels C and D of Figure 2 present some information on the underlying adjustment mechanisms. They show that there was a small but noticeable difference in measures of slack such as employment-to-population ratio and the unemployment rate. That difference dissipates after a few years. In combination, Panels A-D indicate that, over the more extended period, counties that experienced a more considerable housing shock saw their local labor markets adjust back to a national benchmark in large part through a reduction in population.

Finally, Panels E and F show the evolution of variables related to household wealth. Panel E shows the difference in the evolution of the debt-income ratio. Counties that experienced the most substantial housing net worth losses were also ones where household debt increased relatively more in the boom years. After 2008, those regions then experienced the greatest degree of deleveraging. Similarly, in Panel F, we can observe a transitory difference in house price index between counties, with counties where housing net worth fell by the most also experiencing a larger house-price boom pre-crisis. The differences in house prices dissipate a few years after the bust. Therefore, house prices and debt-to-income ratios had largely converged to pre-boom levels before 2018, even as employment and GDP differentials remained persistently high.

Taken together, the panels of Figure 2 imply that a transitory shock to house prices might generate persistent local reductions in employment and population with the long-term convergence in slack measures taking place in large part through population movements. We describe next how we disentangle the effect of the housing shock from other sources of local change and give this pattern a causal interpretation.

Panel A: Total Employment
Panel B: Total GDP
Panel C: Emp–to–Pop
Panel D: Unemployment Rate
Panel E: Debt–to–Income Ratio
Panel F: HPI–to–GDP Ratio

By severity of housing net worth drop 06–09 from Mian and Sufi (2014)

Top 33%   Middle 33%   Bottom 33%

Notes: The upper panels plot the percent deviation of employment (Panel A) and GDP (Panel B) from their trends by grouping counties in terms of the severity of housing-net-worth drop. Employment trend is calculated by taking average growth rates from 1998-2002 for each county and using those to project 2002 employment linearly into the future. The GDP trend is calculated by using average growth rates from 2002-06 for each county. The middle panels plot the percent deviation of employment-to-population ratio (Panel C) and unemployment rate (Panel D) from their 2002 levels. The lower panels plot the percent deviation of DTI ratio (Panel E) and HPI-to-GDP ratio (Panel F) from their 2002 levels.

Figure 2: Changes in Variables by Housing Net Worth Quantiles (County-Level)

3   Disentangling the Effects of the Housing Shock

The figures in Section 2.2 suggest that regions where the 2006-09 housing shock was more severe also exhibited relatively lower employment and output as late as 2018. This may not be a causal relationship, however. For example, a persistent increase in demand for products from a specific region would lead to local increases in both employment and house prices. We disentangle the causal relationship from the housing shock through a combination of controls and instrumental variables, as we discuss now in detail.

3.1   The Basic Econometric Model

In order to estimate the impact of the housing shock on local outcomes, we assume that an outcome X in location n at time t is determined by:

$$\ln X_{n,t} = \alpha_n + g_n t + \gamma_t \eta_n + e_{n,t}, \tag{1}$$

where η_n is the housing shock, g_n is a region-specific trend-growth term, and the residual $e_{n,t}$ summarizes all other shocks affecting local conditions. The parameter γ_t captures the time-varying effect of the housing shock on period t outcome variables.

The residual $e_{n,t}$ can be decomposed into aggregate and idiosyncratic components, so that

$$e_{n,t} = \sum_{r=1}^{R} \lambda_n^r z_t^r + u_{n,t}, \tag{2}$$

where z_t^r is one out of R aggregate driving forces (such as nationwide increases in demand for certain products), λ_n^r is the local sensitivity to that aggregate shock (such as the share of the industry in the location), and $u_{n,t}$ is a shock idiosyncratic to the location. The division of $e_{n,t}$ between aggregate and idiosyncratic components is such that $u_{n,t}$ cannot be predicted from fixed regional characteristics. That is, for any W_n that is fixed in time,

$$\lim_{N \to \infty} \frac{1}{N} \sum_{n=1}^{N} W_n u_{n,t} = 0. \tag{3}$$

This decomposition corresponds, up to some additional technical assumptions, to an approximate factor model, as in Chamberlain and Rothschild (1983).[7]

In order to estimate the model, we take differences from the pre-recession peak year 2006 to eliminate the location fixed effects α_n:

$$\ln X_{n,t} - \ln X_{n,2006} = g_n t + (\gamma_t - \gamma_{2006}) \eta_n + e_{n,t} - e_{n,2006}, \tag{4}$$

which gives our main econometric specification. We then proxy for g_n with pre-housing-boom growth rates.[8] As in Section 2.2 above, we measure the housing shock η_n with the housing net

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[7]It holds without loss of generality so long as the number of aggregate shocks driving local-level employment is not too large, and we allow for enough aggregate factors. If there is some W_n for which equation (3) does not hold, then we can define $z_t^{R+1} \equiv \frac{cov[u_{n,t}, W_n]}{var(W_n)}$ and $\lambda_n^{R+1} \equiv W_n$, and substitute $u_{n,t}$ for $\hat{u}_n, \equiv u_{n,t} - \frac{cov(u_{n,t}, W_n)}{var(W_n)} W_n$, in which case $\frac{1}{N} \sum_{n=1}^{N} \hat{u}_n W_n = 0$.

[8]1998-2002 for most data and, due to data limitations, 2002-2006 for GDP.

worth loss between 2006 and 2009. Therefore, the more negative the estimated value for η_n, the bigger is the housing shock. Then, if $X_{n,t}$ is house prices, for example, we would expect $\gamma_t - \gamma_{2006} < 0$ in the boom years and $\gamma_t - \gamma_{2006} > 0$ in years after the bust.

3.2 Handling Identification Concerns

When estimating $\gamma_t - \gamma_{2006}$ in equation (4), the main identification concern is that a non-housing shock may simultaneously drive the housing net worth loss and appear in the residual term $e_{n,t} - e_{n,2006}$. For example, a shock that increases local productivity, or demand for local products, might generate both an increase in housing net worth and in local output or employment. Moreover, in equation (2), $e_{n,t} - e_{n,2006}$ may vary across locations because of heterogeneous sensitivity both to aggregate shocks, captured in $\lambda_n^r(z_t^r - z_{2006}^r)$, and to purely local shocks $u_{n,t} - u_{n,2006}$.

We describe next the precise way in which we handle these concerns, with a mix of controls and instrumental variables.

3.2.1 Controlling for Sensitivity to Aggregate Shocks

In our baseline specification, from which we report results in Section 4, we control for the effects of aggregate shocks, $\sum_r \lambda_n^r (z_t^r - z_{2006}^r)$, using shares of employment in 23 different 2-digit-level industries. The key assumption is that those 23 industry shares are enough to span the effect of aggregate shocks on the outcome of interest.[9] Industry shares are particularly well-suited to eliminate local differences in response to aggregate cost or demand shocks to particular industries. They also capture other systematic differences in local economies that could influence local response to aggregate shocks. For example, locations specializing in the production of durable manufacturing may be more susceptible to any national shock, since durables are more cyclically sensitive. In contrast, places that concentrate on financial services may be more responsive to monetary or financial shocks. Lastly, those are also the primary set of controls used by Mian and Sufi (2014), allowing for the direct comparability of our baseline results to theirs.

In Section 5.1, we do two exercises that allow us to control for a greater range of macroeconomic shocks. In the first one, we regress local employment on identified aggregate monetary and financial shocks using pre-2002 data. We then use the estimated coefficients as controls. In the second one, we note that $e_{n,t} - e_{n,2006}$ has a factor structure and extract the local factor loadings λ_n with a principal components model. Finally, for further robustness, we also examine specifications including as controls various socioeconomic characteristics of the population of the region (race, income, homeownership, education, unemployment rates, poverty, and urbanization) in extended specifications.

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[9]More formally, we assume that $\sum_r \lambda_n^r (z_t^r - z_{2006}^r) = \sum_j \mu_t^j s_n^j$, where s_n^j is the share of sector j and μ_t^j is a time-specific coefficient to be estimated.

3.2.2 An IV Framework

With appropriate controls for the local sensitivity to aggregate shocks, the regression residual captures local shocks, u_n. Those are likely to affect both housing wealth and local outcomes of interest, leading to simultaneity bias. We now describe the IV framework that we use to guard against this ex-post simultaneity bias that would arise in an OLS framework.

Given equation (3), the local shock u_n is purely "random" in that it is not predictable based on fixed local characteristics. Therefore, any such characteristic that correlates with housing net worth changes around the crisis is an appropriate instrument. The Saiz (2010) instrument used by Mian and Sufi (2014) clearly passes that test. It measures the local elasticity of housing supply given by geographical or regulatory constraints. Mian and Sufi (2014) propose it as an instrument for the housing shock because lower housing supply elasticity would allow house prices to increase more quickly in the run-up years from 2002-06, thus allowing households to raise more debt in comparison to their incomes. Following the findings of a nonlinear relationship between housing supply elasticity and local housing cycles (Gao, Sockin, and Xiong, 2016), we use a discretized version of the instrument with separate dummy variables for elasticity terciles.

3.2.3 Location-Specific Sensitivity to Housing Shock

A further concern may arise if we allow the local sensitivity to the housing shock ($\gamma_{n,t}$) to be location-specific. In that case, the local residual $u_{n,t}$ becomes

$$u_{n,t} = (\gamma_{n,t} - E[\gamma_{n,t}])\eta_n + \tilde{u}_n, \tag{5}$$

where $\tilde{u}_n$ now captures the true local idiosyncratic shocks and $E[\gamma_{n,t}]$ is the average marginal effect of the shock. OLS will estimate the average marginal effect only if $\gamma_{n,t}$ is uncorrelated with our measure for η_n. This need not be the case, however. For example, households may borrow more aggressively during the boom if they live in a region where employment is more stable. This would then lead to larger housing shocks to occur in regions that are less sensitive to those shocks. Moreover, the IV strategy will be susceptible to this type of bias if the instrumental variable is itself correlated with local sensitivity to the shock. We tackle this specific problem in Section 5.2 and find that, if anything, the bias is in the direction of downplaying the magnitude of the employment effects.

4 Results

We first present our baseline results, obtained from estimating equation (4) for different outcomes, using local industry shares as controls. We then consider heterogeneous effects across sectors and growth trajectories.

4.1 Baseline Results

We start with the baseline impulse responses of various outcomes to the housing shock (again, as measured by the reduction in housing wealth from 2006 through 2009), estimated using both OLS and IV regressions. We compute those by estimating equation (4) separately for each year, with controls for industrial composition and pretrends, as described above. The impulse response functions are then just the estimated coefficients on the housing net worth losses. This procedure corresponds to applying Jordà's (2005) local projection method using a shock that varies over the cross-section instead of over time. All figures in this section thus show the estimated values of $\gamma_t - \gamma_{2006}$ in equation (4), together with 95% confidence intervals.

4.1.1 Mediating Variables: House Prices, Leverage, and Labor Productivity
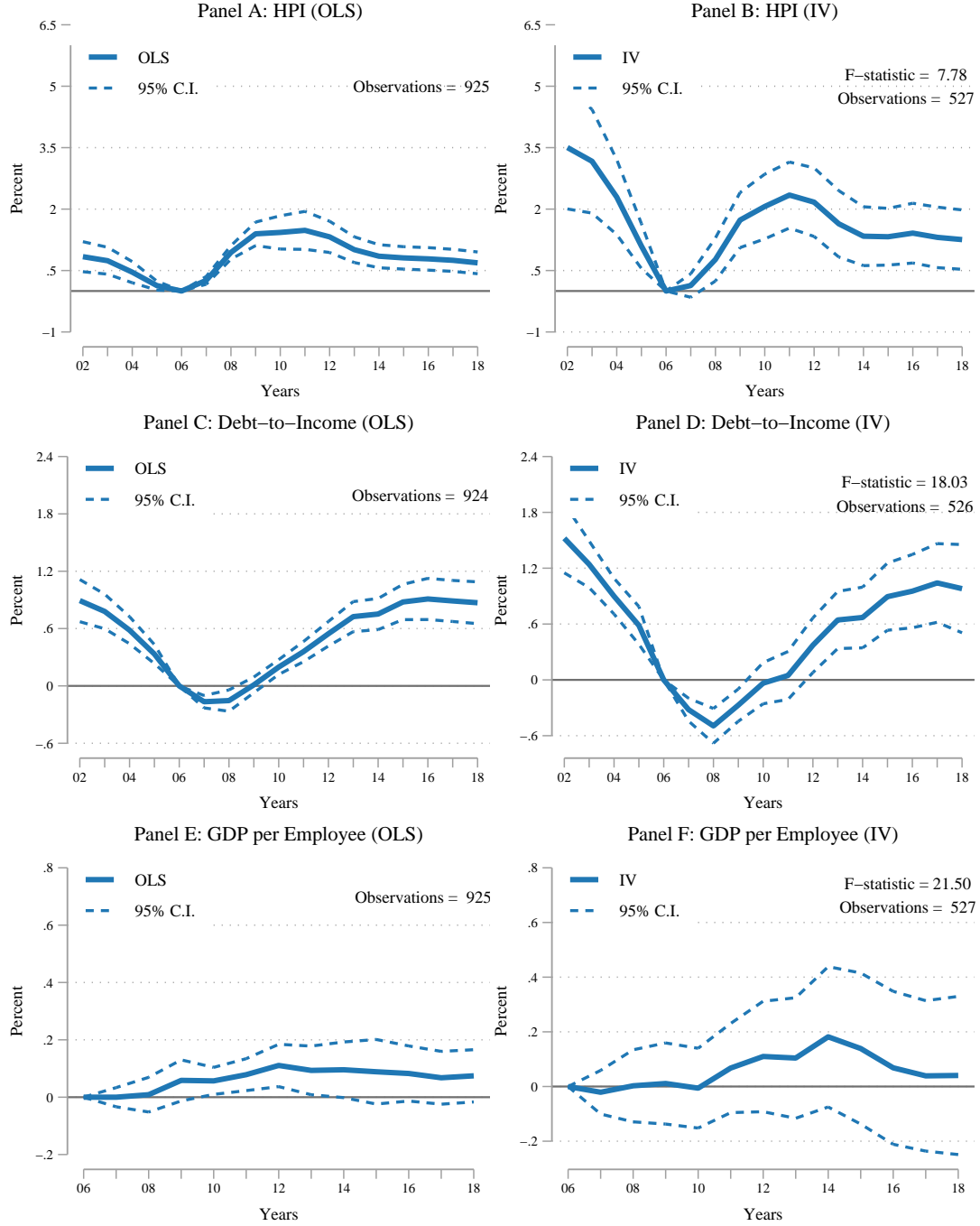
We start by assessing the results on variables that are likely to mediate the response of employment and output to the housing shock. First, almost by definition, the housing shock should have an impact on local house prices. Moreover, theories of protracted propagation such as Guerrieri and Lorenzoni (2017) emphasize that financial or wealth shocks can have protracted demand-side effects as households are forced to de-lever.[10] Finally, Anzoategui, Comin, Gertler, and Martinez (2019) show that one channel through which transitory shocks can have persistent effects is by permanently depressing productivity.

For all variables, we show OLS and IV results. As discussed before in Section 3.2, OLS results mix the effects of shocks to housing wealth on local outcomes with the simultaneous effect of productivity shocks (and, more generally, other shocks on all observables). By mixing in the impact of many shocks, OLS results are more closely comparable to the classic exercises done by Blanchard and Katz (1992), and to more recent analyses by Yagan (2019). Like our OLS specification, those papers do not discern explicitly between different sources of local fluctuation, whether supply or demand, temporary or permanent. In contrast, the IV specification attempts to isolate the effects of the housing shock. As we will see, results for both estimators are qualitatively similar in many, but not all, instances.

First, we check that the housing net worth losses indeed capture the boom-bust cycle in house prices. Panels A and B of Figure 3 confirm this to be the case. Counties which experienced the largest reduction in housing wealth from 2006 through 2009 were also subject to the strongest boom-bust cycle in house prices. IV responses are more pronounced, indicating that those are more effective at singling out the boom-bust cycle. Conversely, the OLS estimates are likely to be contaminated by the simultaneous response of household net worth and house prices to productivity shocks. Looking at dynamic implications, the losses in house prices bottom out around 2011. Then, by 2011, the differences in house prices across counties stabilize at close to 2004 levels.

Much of the post-crisis literature has emphasized the role of household deleveraging in delaying the recovery from the recession. Indeed, Panels C and D of Figure 3 show that during the boom

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[10]Berger, Guerrieri, Lorenzoni, and Vavra (2017) and Justiniano, Primiceri, and Tambalotti (2015) exploit the interaction between debt and housing values in quantitative models.

Panel A: HPI (OLS)

OLS
95% C.I.   Observations = 925

Panel B: HPI (IV)

IV
95% C.I.   F−statistic = 7.78
Observations = 527

Panel C: Debt−to−Income (OLS)

OLS
95% C.I.   Observations = 924

Panel D: Debt−to−Income (IV)

IV
95% C.I.   F−statistic = 18.03
Observations = 526

Panel E: GDP per Employee (OLS)

OLS
95% C.I.   Observations = 925

Panel F: GDP per Employee (IV)

IV
95% C.I.   F−statistic = 21.50
Observations = 527

Notes: The figure plots the impulse responses of HPI-to-GDP ratio (Panels A and B), debt-to-income ratio (Panels C and D), and output per worker (Panels E and F) to the 2006-09 housing shocks. The left columns are results from OLS estimations, and the right columns are results from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends for HPI-to-GDP ratio are captured by the growth rate in HPI from 1998-2002, while prior trends for debt-to-income ratio are growth rate in debt-to-income from 2002-06. Sample weights (by the number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate confidence intervals.

Figure 3: Changes in Housing Prices, Debt-to-Income, and Labor Productivity

years, household leverage rises relatively more in the more affected regions, peaking in 2008, two years after the peak in house prices. Deleveraging takes over after that, but leverage is mostly back to 2004 levels by 2015 and is stable after that.

Finally, Panels E and F of Figure 3 show the effects on labor productivity (GDP per worker). It is clear that while the OLS results show a relationship between housing net worth losses from 2006 through 2009 and labor productivity changes over time, that relationship is absent in the IV estimates. This result is important for two reasons as we go forward and interpret the rest of the results. First, they show that the long-term effects of the housing crisis that we document below do not arise from a reduction in productivity but instead, operate through other channels. Second, the difference between OLS and IV again indicates that OLS results are likely to be contaminated by other shocks, especially those that have persistent labor productivity impact.

4.1.2 Scarring Effects on Economic Activity

We have just verified that the effects of the housing shock on house prices and household leverage were short-lived and that there were no effects on labor productivity. In spite of that, we now show that they had very persistent effects on employment and GDP.

Panels A and B of Figure 4 confirms the basic descriptive findings of long-run effects from section 2.2: While up to 2006, the housing cycle did not appear to generate a discernible difference in employment levels between counties, after the bust, the most affected counties experienced significantly larger employment losses, which persisted in the long-run. That same difference, which persists in the long-run, occurs in county-level GDP, as shown in Panels C and D of Figure 4. Interestingly, the IV results imply larger effects over the long-run. This may happen if local productivity shocks are relatively short-lived, so that they have a larger effect on housing net worth losses over a three-year period than on employment or GDP over 12 years.[11]

In terms of magnitudes, from our IV results, we find that at the county level, a 10% negative housing shock in 2006-09 leads to a 3.3% drop in employment and 4.6% drop in output in 2018 compared to 2006. For a sense of economic importance, the estimates imply that going from the 90th to the 10th percentile of change in housing net worth distribution reduces employment by 5.7% and GDP by 8% in 2018 compared with 2006. For comparison, going from the 90th to the 10th percentile of the 2006-18 employment-growth distribution reduces employment growth rate by 28.8 percentage points and GDP growth rate by 33.3 percentage points.[12]

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[11]To see that, considered the simplified model:

$$X = \beta\eta + \gamma u,$$
$$N = \eta + \phi u,$$

where X is a local outcome, N is the change in housing net worth around the crisis, η is the housing shock, u is a local productivity shock, and β, ϕ, and γ are strictly positive. Assuming that u and η are orthogonal, if we estimate β by running an OLS regression of X on N, we have $\beta^{OLS} = \frac{cov(X,N)}{var(N)} = \beta + \left(\frac{\gamma}{\phi} - \beta\right)\frac{\phi^2 var(u)}{var(\eta)+\phi^2 var(u)}$. The bias is negative if $\frac{\gamma}{\phi} < \beta$ and is positive otherwise. For example, a negative bias will occur if productivity shocks have an impact on housing net worth changes from 2006 through 2009 ($\phi > 0$) and no effect on local output or employment in 2018 ($\gamma = 0$).

[12]In terms of short-run effects, we find that at the county level, a 10% negative housing shock in 2006-09 leads to
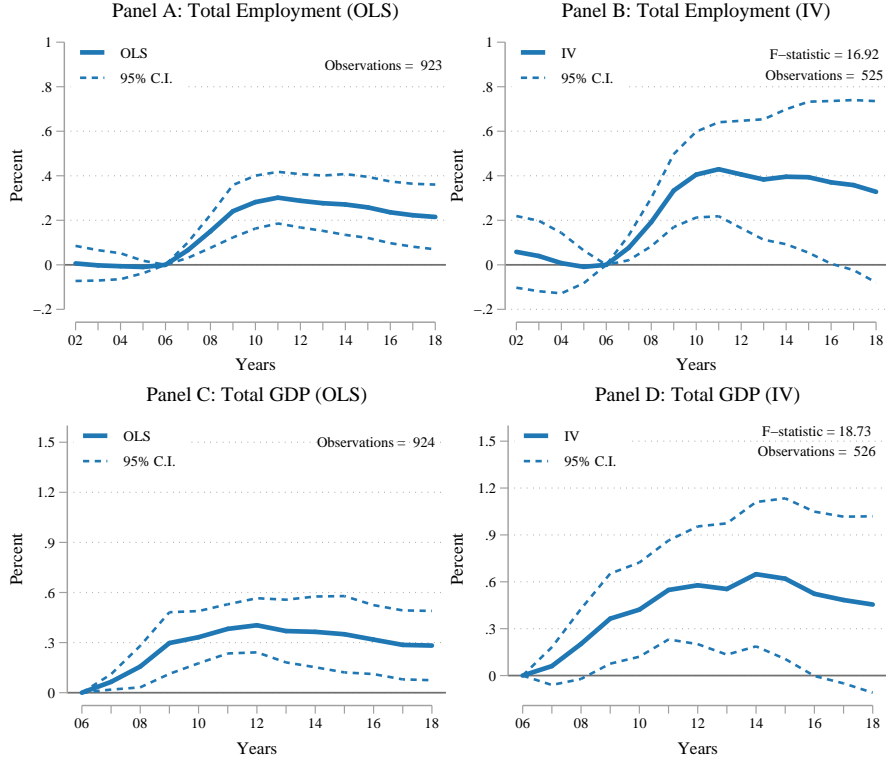
Panel A: Total Employment (OLS)   Panel B: Total Employment (IV)

Panel C: Total GDP (OLS)   Panel D: Total GDP (IV)

Notes: The figure plots the impulse responses of total employment (Panels A and B) and total GDP (Panels C and D) to the 2006-09 housing shocks. The left columns are results from OLS estimations, and the right columns are results from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends for employment are the growth rate in employment from 1998-2002, while prior trends for GDP are growth rate in GDP from 2002-06. Sample weights (by the number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure 4: Changes in Employment and GDP

Overall, the dynamic reaction of employment mirrors classic findings by Blanchard and Katz (1992). The IV results show that this is true also when we separately identify the shock to housing wealth. We further find the same persistent impact on GDP using newly available data constructed by the BEA.

4.1.3   Mean Reversion in Labor Market Slack

Having established long-run effects on employment and GDP of the housing shock, we now turn to the effects on local labor market slack. This is an important question that was also examined by Blanchard and Katz (1992). They find that while local shocks have permanent effects on employment levels, they have only a temporary impact on measures of local labor market slack,

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a 3.3% drop in employment and a 3.6% drop in output in 2009 compared with 2006. This short-run employment elasticity is very similar to the estimate in Mian and Sufi (2014). Focusing ten years out, until 2016, we find that at the county level, a 10% negative housing shock in 2006-09 leads to a 3.7% drop in employment and 5.2% drop in output in 2016 compared with 2006. These ten-year estimates imply that going from 90th to 10th percentile of change in housing net worth distribution reduces employment by 6.4% and GDP by 9% in 2016 compared with 2006.
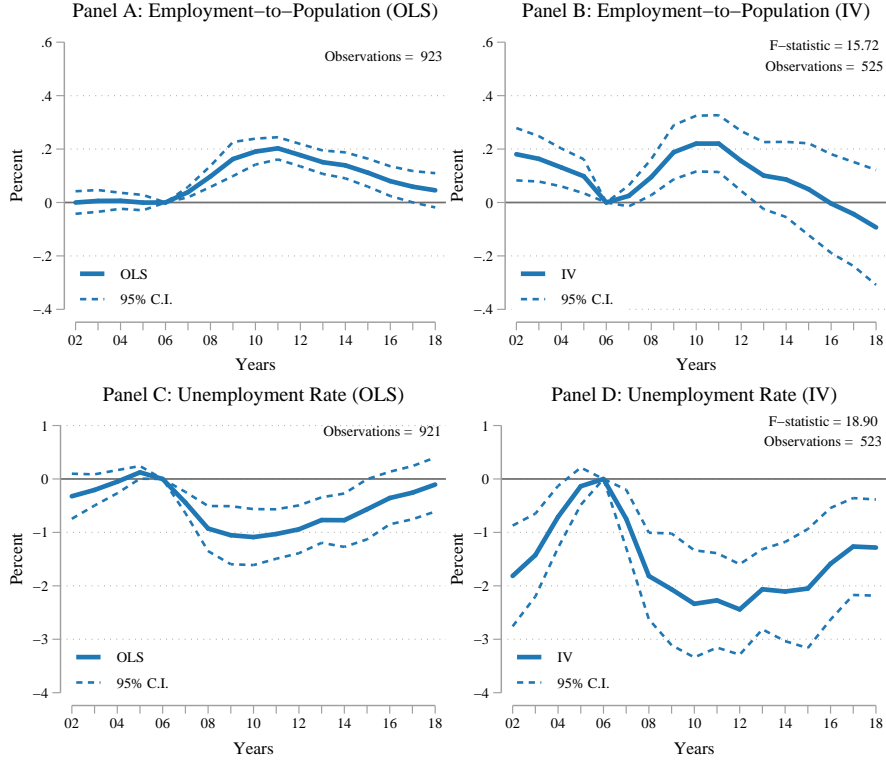
Panel A: Employment–to–Population (OLS)

Panel B: Employment–to–Population (IV)

Panel C: Unemployment Rate (OLS)

Panel D: Unemployment Rate (IV)

Notes: The figure plots the impulse responses of county-level employment-to-population ratio (Panels A and B) and unemployment rate (Panels C and D) to the 2006-09 housing shocks. The left columns are results from OLS estimations, and the right columns are results from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends are log changes in employment-to-population ratio or unemployment rate from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.
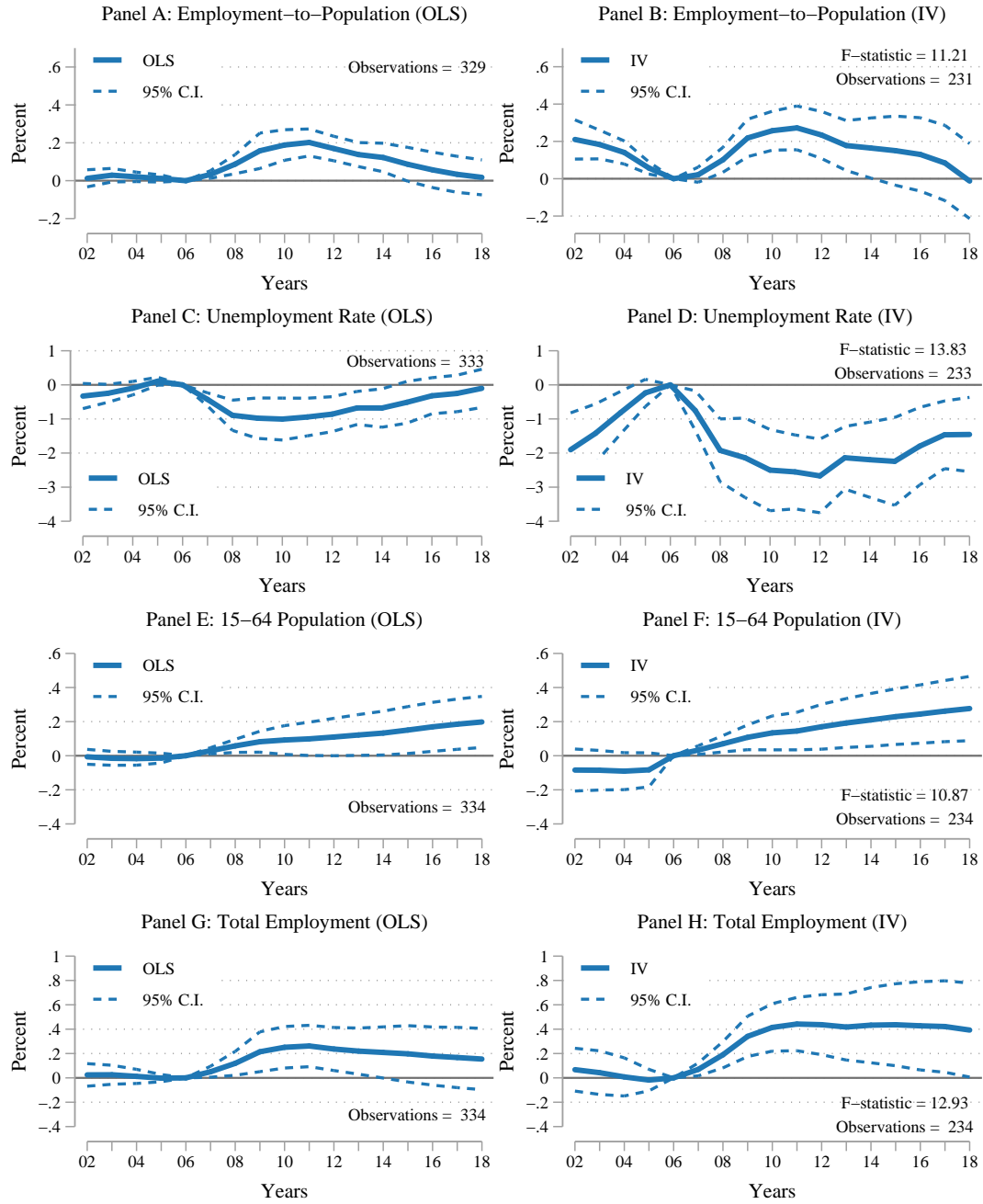
Figure 5: Changes in Employment-to-Population Ratio and Unemployment Rate

such as the employment-to-population ratio and the unemployment rate. They interpret those results with population changes across regions in response to the shock, which leads to mean reversion in local slack.

Such mean-reverting dynamics for local slack appear clearly in Figure 5, both for the employment-to-population ratio (Panels A and B) and the unemployment rate (Panels C and D). Moreover, IV results show that the housing shock was also relevant in tightening labor markets in the boom period, consistent with increasing housing prices helping sustain local economic activity.[13] There is no comparable boom-phase tightening, however, in the OLS results, again indicating that those largely capture the effect of other local shocks whose timing happened to coincide with the housing crisis.

If much of the adjustment takes place through population movements, regressions at the county level might not be appropriate. This is because the effect of the shock can spill over to neighboring

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[13]These results are in line with Charles, Hurst, and Notowidigdo (2018), who also focus on measures of labor market participation, and argue for a symmetric boom-bust in employment-to-population ratio at the regional level from 2002-2011.

Panel A: Employment–to–Population (OLS)

Panel B: Employment–to–Population (IV)

Panel C: Unemployment Rate (OLS)

Panel D: Unemployment Rate (IV)

Panel E: 15–64 Population (OLS)

Panel F: 15–64 Population (IV)

Panel G: Total Employment (OLS)

Panel H: Total Employment (IV)

Notes: The figure plots the impulse responses of CBSA-level employment-to-population ratio (Panels A and B), unemployment rate (Panels C and D), 15-64 population (Panels E and F), and employment (Panels G and H) to the 2006-09 housing shocks. The left columns are results from OLS estimations and the right columns are results from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends are log changes in employment-to-population ratio or unemployment rate from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure 6: Changes in CBSA Employment-to-Population, Unemployment Rate, Population, and Employment

counties. Many individuals may live in one county and work or shop in another so that loss of employment in a locality need not be borne by the local population in the same way as in completely self-contained regions. To deal with these issues, Figure 6 reproduces the results for labor-slack measures at the Core Based Statistical Areas (CBSAs) level, which are by definition collections of counties linked by commuting.[14] The general patterns remain unchanged from Figure 5.

If employment changes permanently while the employment-to-population ratio does not, then the adjustment must take place through population movements at the CBSA level. Panels E and F in Figure 6 verify that to be true. Population reacts smoothly, but persistently, to the shock in both OLS and IV specifications. The employment response in Panels G and H of Figure 6, shown for comparison, depict long-term effects at the CBSA level consistent with the scarring effects we showed at the county level in Panels A and B of Figure 4.

4.1.4 Asymmetric Effects on Wages

Our results on population changes playing a key role in regional slack adjustment raise a natural question on the behavior of wages. We, therefore, investigate the role that wages play in helping equilibrate local labor markets as house prices fluctuate. Responses of local aggregate wage per worker (from QCEW) are depicted in Figure 7 at the county level (Panels A and B) and also at the CBSA level (Panels C and D) to allow for the possibility that individuals may commute between neighboring counties. In Panels E and F, we allow for shifts in labor force composition following Katz and Murphy (1992).[15] Those require the use of ACS micro-data.
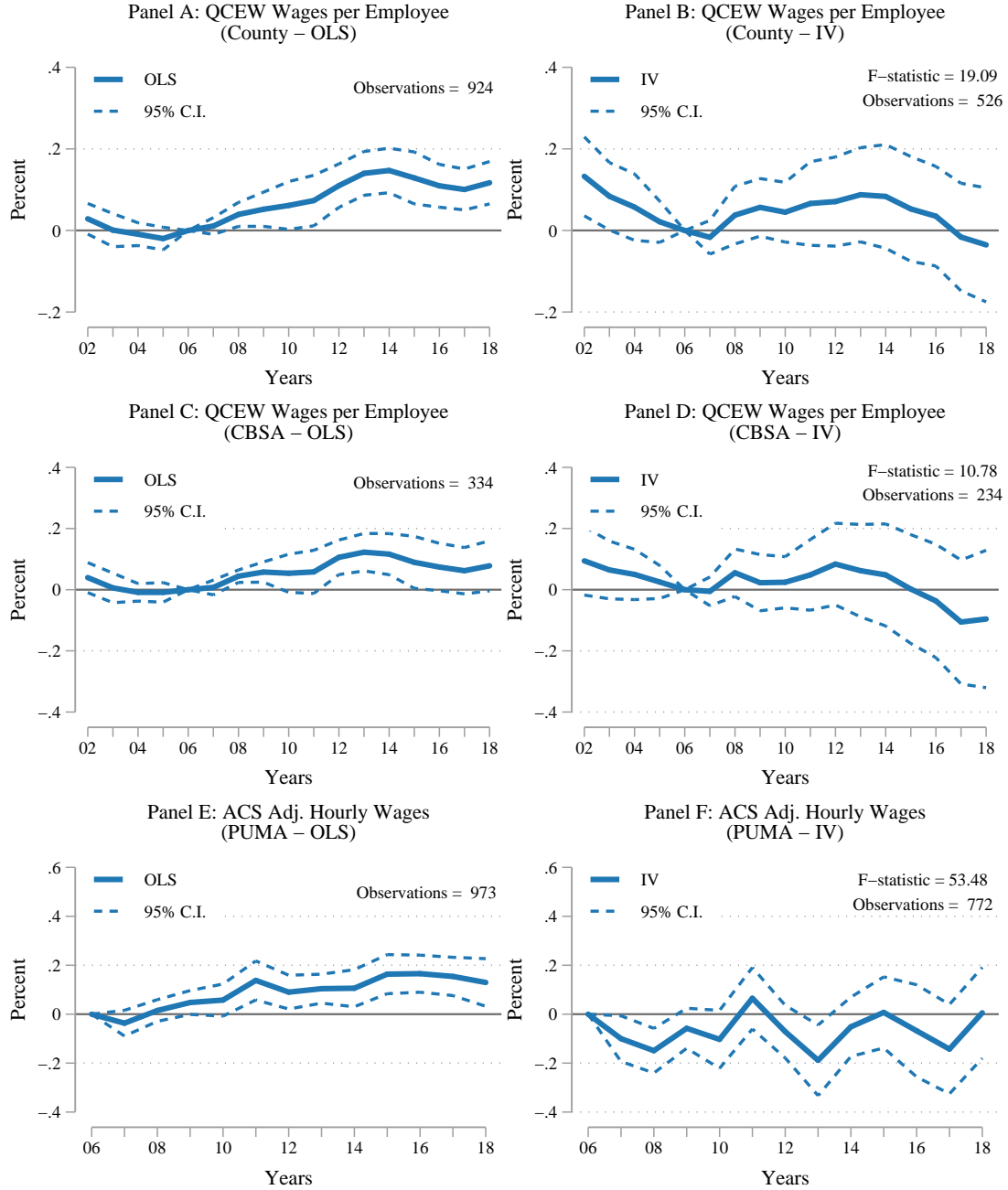
These results contain the most meaningful differences between OLS and IV estimates. With OLS, there is no difference in wages before the housing peak, but afterward, wages increase persistently in more-affected locations. In contrast, the IV results have the opposite pattern: wages at first increase faster in places that are more affected by the housing boom (as was the case with labor market slack), but then they do not adjust downward as the boom turns into a bust. These results thus suggest an asymmetric adjustment of wages consistent with the literature emphasizing downward wage rigidity. In particular, downward wage rigidity has recently been documented in microeconomic data by (Grigsby, Hurst, and Yildirmaz, 2019). Moreover, it can play a very important role in hindering the adjustment of regions within a currency union to asymmetric shocks in the presence of limited labor mobility, as shown in (Schmitt-Grohé and Uribe, 2016). The contrast between OLS and IV highlights that while wages may react to some shocks, they do not seem to react to the exogenous negative net worth shock suffered by many localities in the recession.[16]

Overall, the results in this section imply that the housing boom-bust cycle had a very persistent impact on local employment and output and that as wages did not adjust, local labor market adjustment took place first through an increase in local slack measures and, after several years,

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[14]CBSAs are groups of counties tied to a "core" center with 10,000 people or more through commuting patterns.

[15]We describe the adjustment method in more detail in Appendix B.

[16]Our OLS results are in line with those found by Beraja, Hurst, and Ospina (2019), who find a positive correlation between wages and employment outcomes at the state level during the recession, using ACS data.

Panel A: QCEW Wages per Employee
(County – OLS)

Panel B: QCEW Wages per Employee
(County – IV)

Panel C: QCEW Wages per Employee
(CBSA – OLS)

Panel D: QCEW Wages per Employee
(CBSA – IV)

Panel E: ACS Adj. Hourly Wages
(PUMA – OLS)

Panel F: ACS Adj. Hourly Wages
(PUMA – IV)

Notes: The figure plots the impulse responses of QCEW wages per employee at the county level (Panels A and B) and at the CBSA level (Panels C and D) to the 2006-09 housing shocks. Panels E and F plot the impulse response of hourly wages at PUMA level using adjusted ACS data. The adjustment procedure follows Beraja, Hurst, and Ospina (2019) and is described in Appendix B. The left columns are results from OLS estimations, and the right columns are results from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends for wages per employee are the growth rate of it from 1998-2002. Sample weights (by the number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure 7: Changes in QCEW Wages per Employee and ACS Adjusted Hourly Wages

through population movements. The results also highlight the need to use an IV strategy to distinguish the role of the housing shock from the common joint reactions of employment and house prices to productivity shocks.

We now examine how the housing crisis differed across industries and cities in different growth trajectories.

4.2 Heterogeneous Effects

Mian and Sufi (2014) use the difference in outcomes between employment in the tradable and non-tradable sectors to argue for a household demand effect of the housing crisis. In this section, we revisit and extend their comparisons, replicating their findings over the short-run, but finding a more complicated picture over the longer horizon.

We also examine the differential effect of the housing shock on cities that have different growth trends. This way of splitting the sample is motivated by Glaeser and Gyourko's (2005) observation that cities with stronger growth trends tend to react more strongly to shocks. If that is the case, one may expect the reallocation of labor-force to happen away from fast-growing locations and towards slow-growing ones, with potentially problematic implications for aggregate dynamism of the economy.

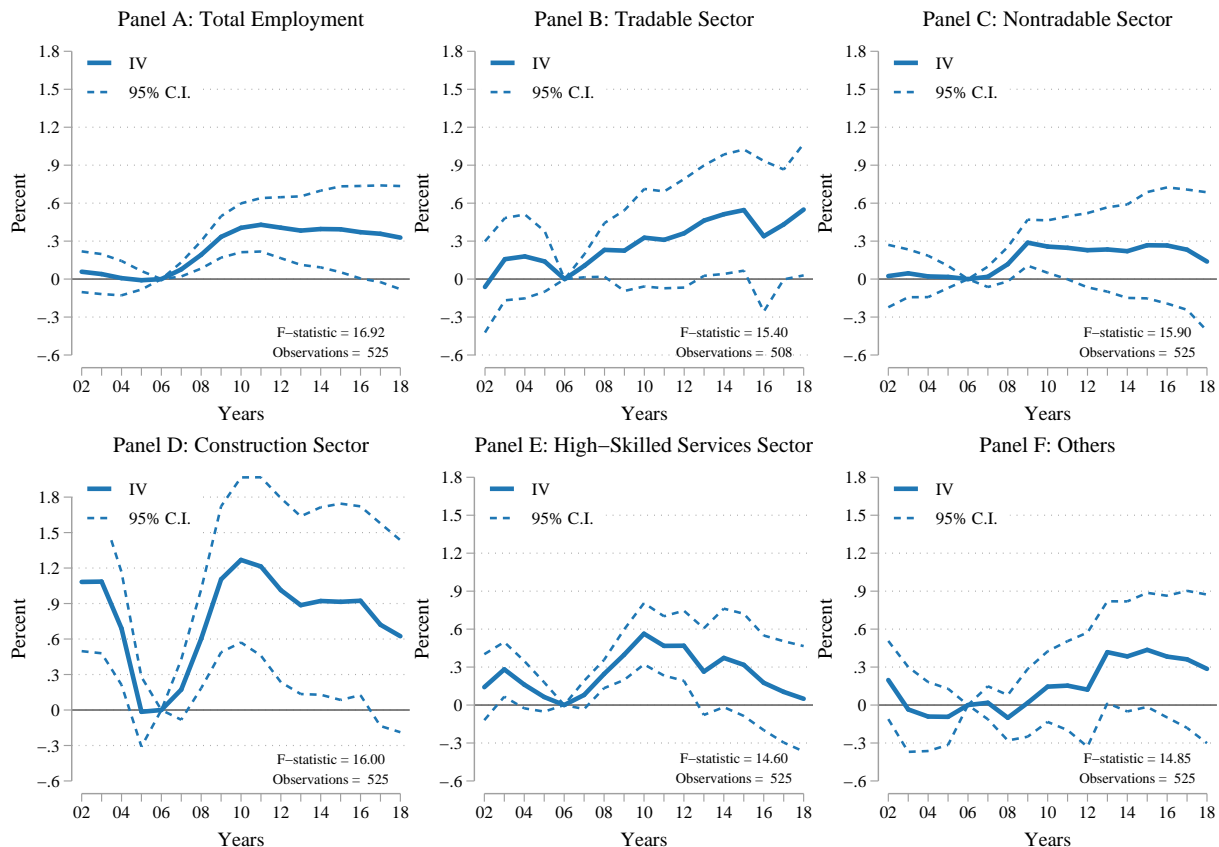### 4.2.1 Differential Effects across Sectors

We start by investigating the impact of the housing net worth shock on employment within sub-sectors. Those can be useful to evaluate if our results are broad-based or particular to specific sectors. For example, Mian and Sufi (2014) show that the short-term impact of the housing shock was particularly relevant among non-tradables, reinforcing the interpretation of the shock as having its main impact through household demand.

We split the sample into five sub-sectors: tradable (mainly manufacturing), non-tradable (retail and restaurants), construction, high-skilled services (professional and business services, educational services, and health services) and others (including, among others, wholesalers and transportation services). In these sectoral splits, we follow Mian and Sufi (2014) directly, except that we further split the "others" sector from their decomposition into two: a high-skilled and the rest. We describe the details of these splits in Appendix B.

These sectoral-level employment results are presented in Figure 8 (we repeat the exercise for wages in Appendix Figure A.5). As Panel D shows, construction presents an evident boom-bust pattern, but in other sectors, there is no pre-2006 dynamics.[17] This is a further sign of the asymmetric effect of the housing cycle, with its effects being concentrated in one sector in the up-swing, but more sectorally dispersed in the down-swing.

As in Mian and Sufi (2014), in Panel C, we find sizable effects on non-tradable employment over the first few years of the recession, but that loses statistical significance shortly thereafter.

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[17]Howard (Forthcoming) finds that a local construction boom has a positive effect on local labor markets, amplifying the effects of other local shocks.

| Panel A: Total Employment | Panel B: Tradable Sector | Panel C: Nontradable Sector |
| Panel D: Construction Sector | Panel E: High–Skilled Services Sector | Panel F: Others |

Notes: The figure plots the impulse responses of employment to the 2006-09 housing shocks by sectors. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends for sectoral employment are the growth rate of employment in each sector from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals. See Appendix B.1 for the details of sectoral splits.

Figure 8: Changes in Employment by Sector

Intriguingly, as Panel E makes clear, we find a larger and more sustained effect on high-skilled services. Lastly, like in Mian and Sufi (2014), Panel B shows that there is no statistically significant short-run effect on tradable sectors.[18]

To summarize our sectoral results, we find that the housing boom affected primarily construction employment but in the bust, its effects spilled over to other sectors, notably non-tradables and the high skilled sector in the short-run. The effects on the non-tradable sector were transient, suggesting again that the housing shock was a temporary shock, while intriguingly, the effects were more long-lasting on the high-skilled sectors.

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[18]The estimates do suggest, however, that there are marginally significant effects on the tradable sector over the longer period.
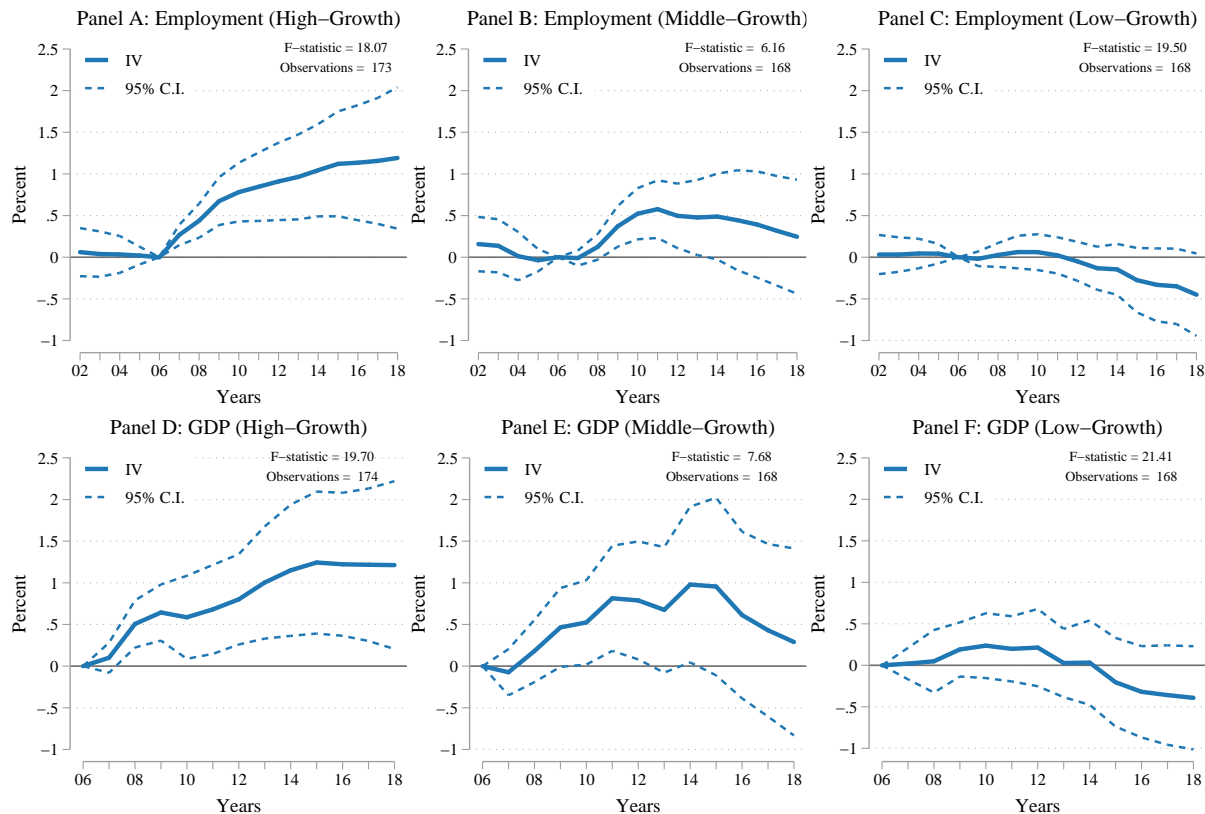
Notes: The figure plots the impulse responses of employment and GDP to the 2006-09 housing shocks by ex-ante growth rates. We separate counties based on their employment growth rates from 1990-2001 into three groups: high-growth counties are those with growth rates above the 66th percentile; middle-growth counties are those with growth rates from the 33rd through 66th percentiles; low-growth counties are those with growth rates below the 33rd percentile. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends are the growth rates of employment from 1998-2002. Sample weights (by the number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure 9: Changes in Employment and GDP by Local Growth Trend

4.2.2 Differential Effects Across Ex-Ante Growth Trends

Next, we investigate how the effect of the net worth shock depends on local growth trends. For example, Glaeser and Gyourko (2005) have emphasized that one might expect fast-growing cities to exhibit larger employment responses to demand shocks than slow-growing cities. In addition, investigating such heterogeneity is interesting since a larger effect of the housing shock on fast-growing cities would tend to compound its national effects.

We examine whether we can find similar differences in the effects of the housing wealth shock. We separate counties based on their employment growth rate from 1990-2001 in three groups: High-growth counties are those with growth rates above the 66 percentile; middle-growth counties have growth rates between the 33 and 66 percentiles; and the low-growth counties are those with a growth rate below the 33 percentile. Figures 9 shows the results for employment (Panels A-C) and

output (Panels D-F). We find that the results confirm with Glaeser and Gyourko (2005)'s findings: The employment effect on ex-ante high growing counties is about 50% larger than the effect on ex-ante middle growth counties while the effect on ex-ante slow-growing counties is not statistically significant from zero. For GDP, the effects are consistently significant overtime only for the ex-ante high growing counties.

5   Sensitivity Analysis

We now report results from several robustness exercises, where in particular, we allow for additional controls for the local sensitivity of aggregate shocks and address the criticism of the housing supply elasticity instrument that has emerged in the past years. We find that those largely either leave the findings unaltered or strengthen them. We then use the QWI data to evaluate whether the results depend on different types of workers and firms. Finally, we also present results using the alternate IV proposed by Charles, Hurst, and Notowidigdo (2018).
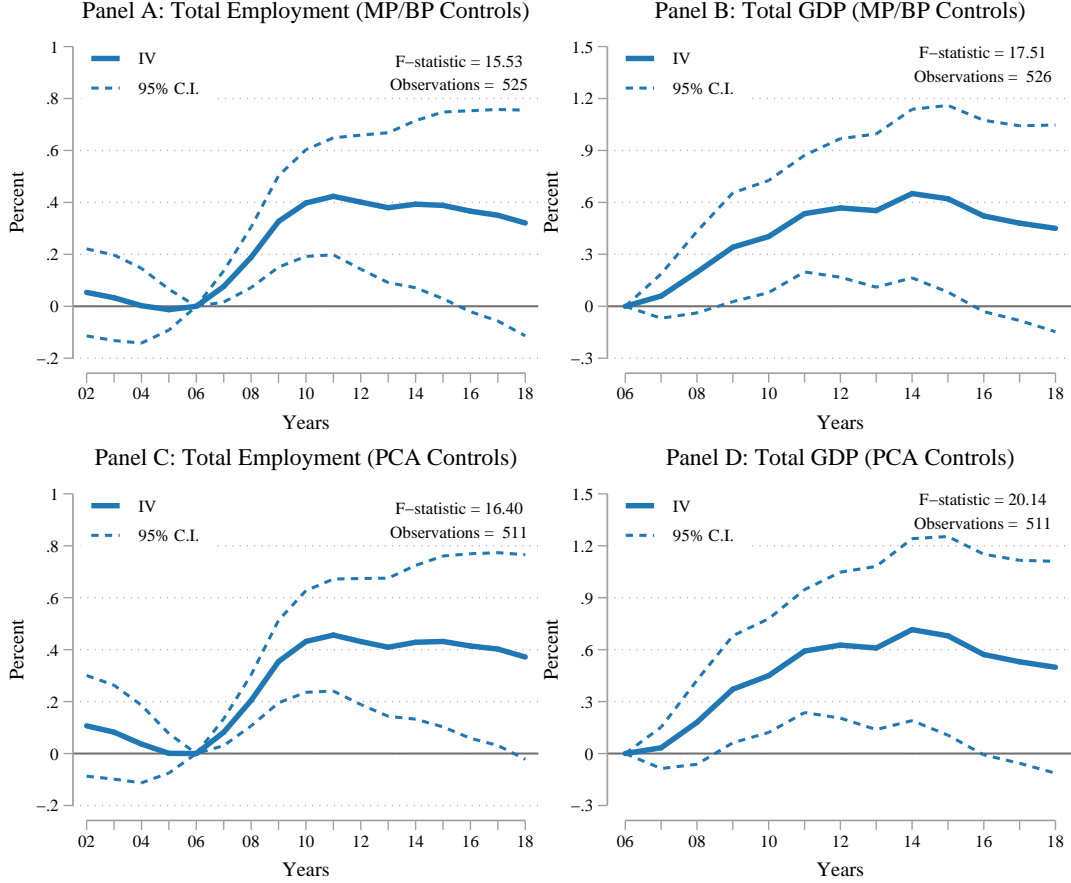
5.1   Further Controls for Macroeconomic Factors

The error term in our econometric model has a factor structure, as in equation (2). It follows that the effect of aggregate shocks require a set of controls that span the terms $\lambda_n^r \left(z_t^r - z_{2006}^r \right)$. Our baseline estimate controls for those shocks by assuming that local sectoral composition predicts well how different regions react to different shocks. We now refine our controls for aggregate effects in two ways. First, we regress the quarterly employment growth rate for each county on two exogenous shocks: (i) monetary policy shocks from Romer and Romer (2004) extended through 2002, and (ii) excess bond premium shocks from Gilchrist and Zakrajšek (2012). The coefficients on each shock indicate how much a county is sensitive to both shocks. We use the sensitivity of each county to both shocks as controls. The results for employment and output are in Panels A and B of Figure 10. It is clear that they are very similar to our baseline results above in Figure 4.

Second, we use principal components to estimate a factor model on

$$\ln L_{n,t+\tau} - \ln L_{n,t} = \sum_{r=1}^{R} \hat{\lambda}_{\tau,n}^r \left(\hat{z}_{t+\tau} - \hat{z}_t \right) + u_{n,t+\tau} - u_{n,t},$$

where we allow the factor loadings to depend on τ for increased flexibility. We estimate the principal component data using pre-2002 data. If the effect of macroeconomic shocks is stable over time, the estimated factor loadings $\hat{\lambda}_{\tau,n}^r$ should span it, given enough factors and sample size. The factor model provides a stringent test of the effects of the 2006-09 housing shock to the extent that one of the estimated factors could include the effect of smaller housing shocks that occurred in the past. The results for employment and output are in Panels C and D of Figure 10. Those are also clearly very similar to our baseline results above in Figure 4.

Panel A: Total Employment (MP/BP Controls)

Panel B: Total GDP (MP/BP Controls)

Panel C: Total Employment (PCA Controls)

Panel D: Total GDP (PCA Controls)

Notes: The figure plots the impulse responses of employment and GDP to the 2006-09 housing shocks with additional aggregate controls. Panels A and B show results from including three main factor loadings from the principal component analysis for 1990-2001 employment growth. Panels C and D show results from including each county's sensitivity to monetary shocks from Romer and Romer (2004) and to excess bond premium shocks from Gilchrist and Zakrajšek (2012). All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included in all specifications. Prior trends for employment are the growth rate in employment from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals

Figure 10: Changes in Employment and GDP with Additional Aggregate Controls

5.2 Controlling for Location-Specific Sensitivity to Housing Shock

While the Saiz (2010) instrument is plausibly orthogonal to idiosyncratic regional shocks that occurred around the time of the Great Recession, it might be correlated with location-specific characteristics related to their sensitivity to the housing wealth shock. For example, in locations with lower housing supply elasticity, house prices may be larger, leading to more initial leverage. The need for high leverage may, in turn, restrict the pool of individuals living in the city to those who have stable income and employment. This would lead to a correlation between the Saiz (2010) instrument and the effects of the housing shock that biases the estimate away from the average effect of the housing bust (see Section 3.2.1).

Figure 11 shows how allowing for these considerations changes the estimates for the employ-
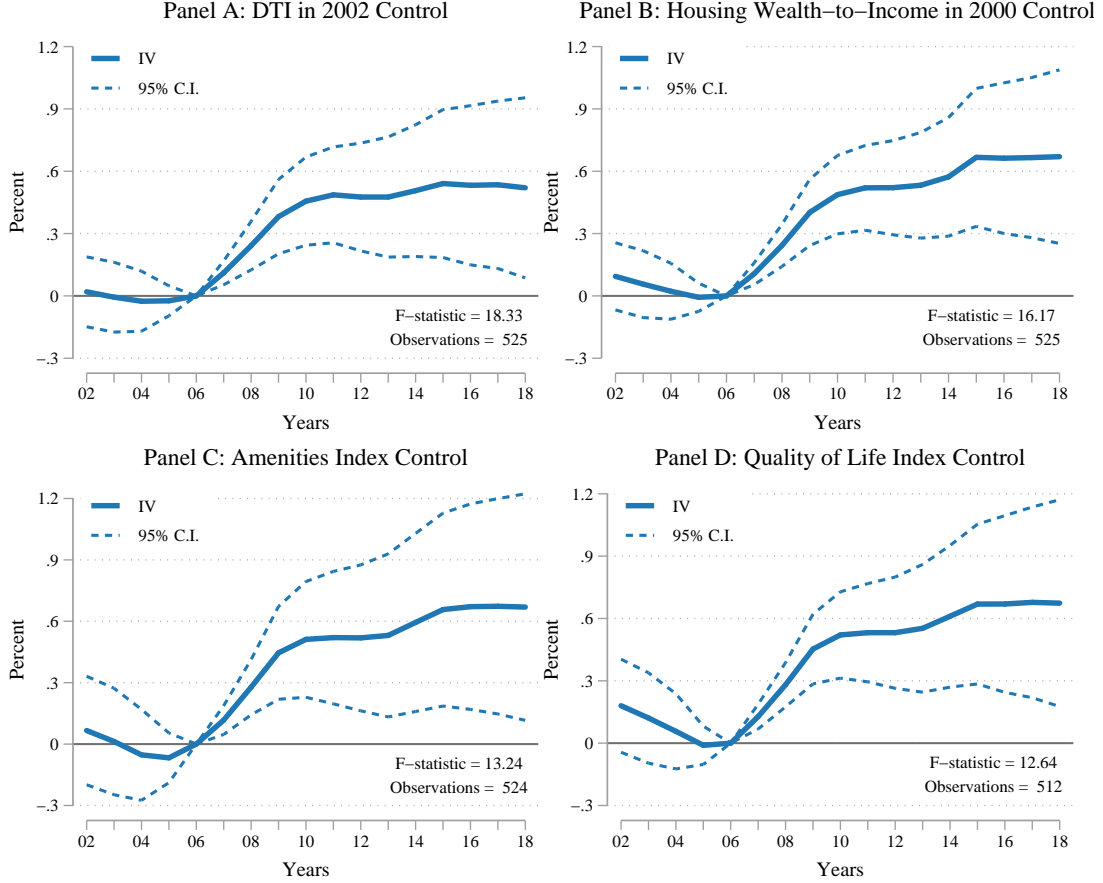
Notes: The figure plots the impulse responses of employment to the 2006-09 housing shocks with additional controls for credit access and quality of life. Panel A shows results from including the debt-to-income ratio in 2002 as a control, while Panel B shows results from including housing wealth-to-wage income ratio in 2000 as a control. Panel C shows results from including an index of local geographic amenities from the US Department of Agriculture as a control, while Panel D shows results from including the measure of the quality of life from Albouy (2008) as a control. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included in all specifications. Prior trends for employment are the growth rates in employment from 1998-2002. Sample weights (by the number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure 11: Changes in Employment with Additional Controls for Credit Access and Quality of Life

ment impact of the housing shock (we repeat the exercise for wages in Appendix Figure A.4). Panel A introduces the debt-to-income ratio in 2002 as a control, while Panel B uses the housing wealth-to-income ratio in 2000 as a control. The inclusion of ex-ante debt-to-income incorporates the possibility that the development of local financial markets may vary systematically with the geographical attributes of the city. We find that once we control for initial differences in debt-to-income, there is an increase in the estimated coefficient on the effect of the housing net worth on employment. This is consistent with the hypothesis above that individuals may be more willing to borrow when they are less exposed to local shocks. Controlling for the initial housing wealth-to-income ratio has a similar effect, indicating that much of the initial debt-to-income can be tied to

the larger relative importance of housing wealth.

More generally, the same presence of large bodies of water and uneven terrain that affects local housing supply elasticities are themselves attractive to homeowners, leading to higher home prices, a more highly skilled population, and greater economic development. To allow for that possibility, we now introduce controls for local quality of life.[19]

Panel C shows the results when we repeat our exercise using as controls an index of local geographic amenities constructed by the US Department of Agriculture. It combines six measures of climate, topography, and water area that reflect preferred environmental qualities (warm winter, winter sun, temperate summer, low summer humidity, topographic variation, and water area). There is again an increase in the estimated coefficient on the effect of the housing net worth shock on employment.

Finally, in Panel D we show results when we include the measure of quality of life constructed by Albouy (2008) as a control. In order to construct it, Albouy (2008) uses after-tax real wages in each location, using the result that in a spatial equilibrium, differences in real wages between cities for a worker with the same attributes should reflect a compensating differential in local amenities. Those real wages should capture any impact on the demand for living in those places from the geographical features captured by the Saiz (2010) instrument. We find that including such a control also increases the size of the estimated coefficient, implying that the effect of higher amenities on the local economy and population composition attenuates the effect of the housing wealth shock.

5.3   Other Robustness

We report in Appendix A.1 results from several robustness and sensitivity exercises. In particular, we investigate different sets of controls, including demographic characteristics. We also exclude observations from "sand states," (California, Nevada, Arizona, and Florida), which experienced the worst effects of the crisis. In Appendix Figures A.1 and A.2, we find that results are robust to those modifications for both OLS and IV regressions.

In Appendix Figure A.3, we also find that our result about the zero overall impact of the housing shocks on wages is robust to the inclusion of a full set of controls. As shown in Appendix Figure A.4, this result is also robust to the addition of local credit access or quality of life index as controls. Moreover, Appendix Figure A.7 examines whether changes in ACS wages at the regional level differ by education and age, and we find that they are the same as our baseline results of no response. Appendix Figure A.5 shows the responses of wages per employee to the housing shock by sectors. While the total wages per employee do not respond to the housing shock, there are substantial declines in wages per employee in the non tradable and construction sectors.

We then present several additional results using the QWI, which in particular allows us to split our analysis by worker and firm characteristics. First, in Appendix Figure A.8, we show regression results for employment and earnings per employee using QWI data and find that the

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[19]This is a point made by Davidoff (2013).

results are very similar to our baseline results. Appendix Figures A.9 and A.10 next show the impacts of employment and earnings per employee to the housing shock by workers' education, age, and gender groups. We find that employment losses are mostly broad-based, while earnings do not respond.

Next, we use QWI data to group firms by their employment size and by their age. Appendix Figure A.11 shows that employment in large (more than 250 employees) or old companies (older than five years) are more strongly affected by the housing shock. Appendix Figure A.12 then shows the impacts of earnings per employee to the housing shock by firms' size and age groups, with largely no consistent effects.

Finally, we also present results using the alternate IV proposed by Charles, Hurst, and Notowidigdo (2018).[20] Appendix Figures A.13 – A.16 show results that correspond to our main baseline results reported in Figures 4 – 7. The results are aligned with our main findings, even with this alternate IV.

6   Conclusion

We show that the housing net worth collapse of 2006-09 had scarring effects across US counties. To do so, building on Mian and Sufi (2014), we use an IV strategy to establish causality for the dynamic and long-run effect of the initial (2006-09) housing net worth shock on future regional outcomes. We first show that counties that had a larger loss in housing net worth in that period had more depressed employment and output as late as 2018. In addition, we find that the local housing boom-bust cycle had asymmetric effects with little local output or employment effect in the boom phase but very persistent employment, GDP, and population losses during the bust. When we probe various potential criticisms to the validity of the IV strategy, we find that, if anything, it tends to understate the average impact of those shocks.

The effect of the housing crisis was well-characterized as mostly operating through the demand side since we find no significant change in labor productivity and only temporary impact on measures of labor market slack, such as employment-to-population ratio and the unemployment rate. Moreover, we show that the negative housing shock had a short-lived impact both on household demand (as measured by non tradable-sector employment) as well as house prices and household leverage, lending credence to its temporary nature. On the labor market adjustment to these scarring effects on employment, we find no role for wage adjustment. In fact, we find indications that downward wage rigidity may have played a role since wages did increase marginally with the housing boom but did not react at all to the housing bust. Together, those findings imply that local labor market adjustment took place entirely through population movements, for which we provide additional direct evidence.

The results suggest that future work leveraging regional US data to understand macroeconomic responses to temporary shocks might consider modeling labor movements explicitly since those

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[20]They identify housing bubbles based on discontinuities in house prices during the boom phase.

constitute an adjustment mechanism that is at work at the local level but not available at the national level. It also calls attention to asymmetric local effects of aggregate shocks, as they tend to have very persistent effects. Their distributive and allocative implications might be of interest for further analysis.

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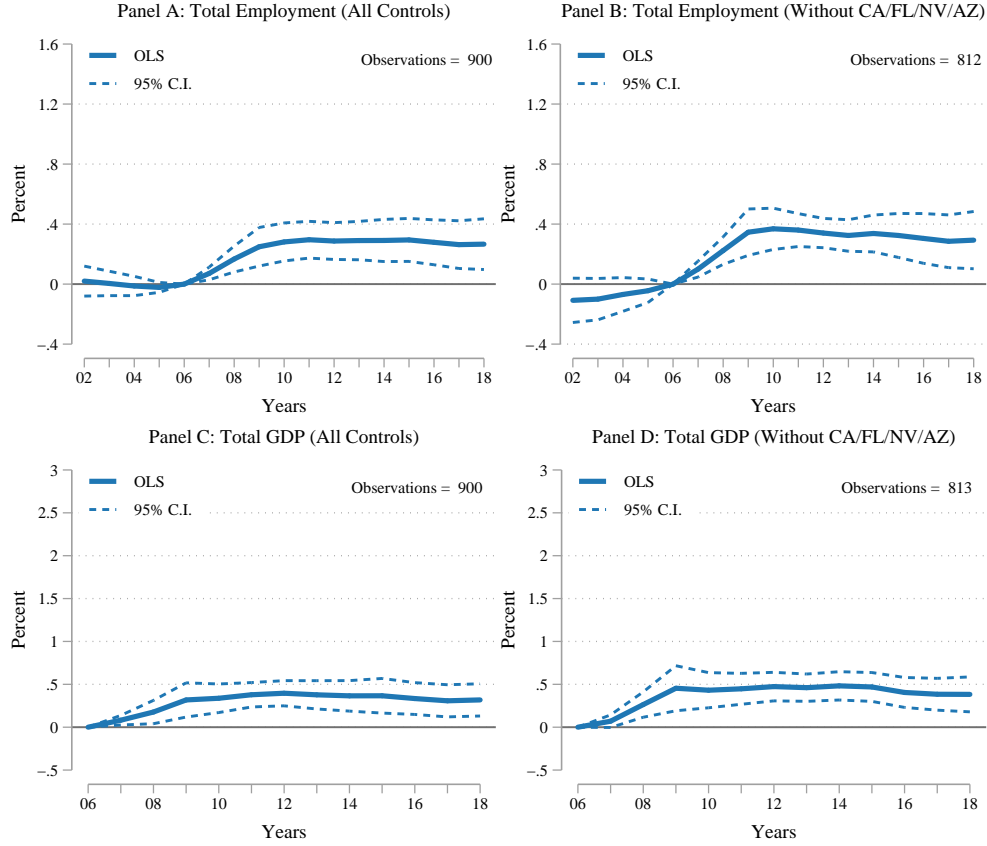# A Additional Robustness and Sensitivity Analysis

A.1 Robustness and Sensitivity Analysis

In this Appendix, we report results from several robustness and sensitivity exercises. In particular, we investigate different sets of controls, most especially demographic characteristics. We also exclude observations from "sand states," (California, Nevada, Arizona, and Florida) where, other work has pointed out, were concentrated the worst effects of the crisis. In Appendix Figures A.1 and A.2, we find that results are robust to those modifications for both OLS and IV regressions.

In Appendix Figure A.3, we also find that our result about the zero overall impact of the housing shocks on wages are robust to the inclusion of a full set of controls. As shown in Appendix Figure A.4, this result is also robust to the inclusion of local credit access or quality of life index as controls. Moreover, Appendix Figure A.7 depicts changes in ACS wages at the regional level split by education and age. We find that these results are consistent with our baseline results of no response. Appendix Figure A.5 shows the responses of wages per employee to the housing shock by sectors. While the total wages per employee do not respond to the housing shock, there is substantial declines in wages per employee in the non tradable and construction sectors.
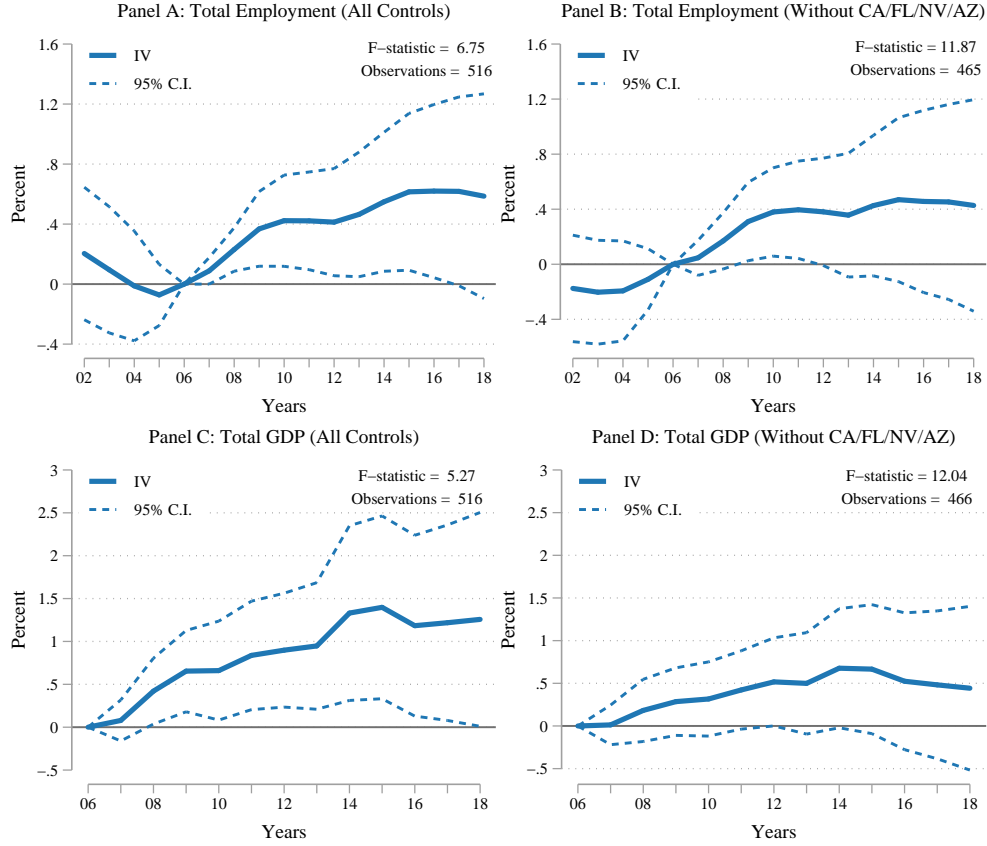
We then present several additional results using the QWI, which in particular allows us to split our analysis by worker and firm characteristics. First, in Appendix Figure A.8, we show regression results for employment and earnings per employee using QWI data and find that the results are very similar to our baseline results. Appendix Figures A.9 and A.10 next show the impacts of employment and earnings per employee to the housing shock by workers' education, age, and gender groups. We find that employment losses are mostly broad-based, while earnings do not respond. We next group firms by their employment size or by their age in the QWI. Appendix Figure A.11 shows that employment in large (more than 250 employees) or old companies (older than five years) are more strongly affected by the housing shock. Appendix Figure A.12 then shows the impacts of earnings per employee to the housing shock by firms' size and age groups with largely no consistent effects.

Finally, we also present results using the alternate IV proposed by Charles, Hurst, and Notowidigdo (2018). Appendix Figures A.13 – A.16 show results that correspond to our main baseline results reported in Figures 4 – 7. The results are aligned to our main findings even with this alternate IV.
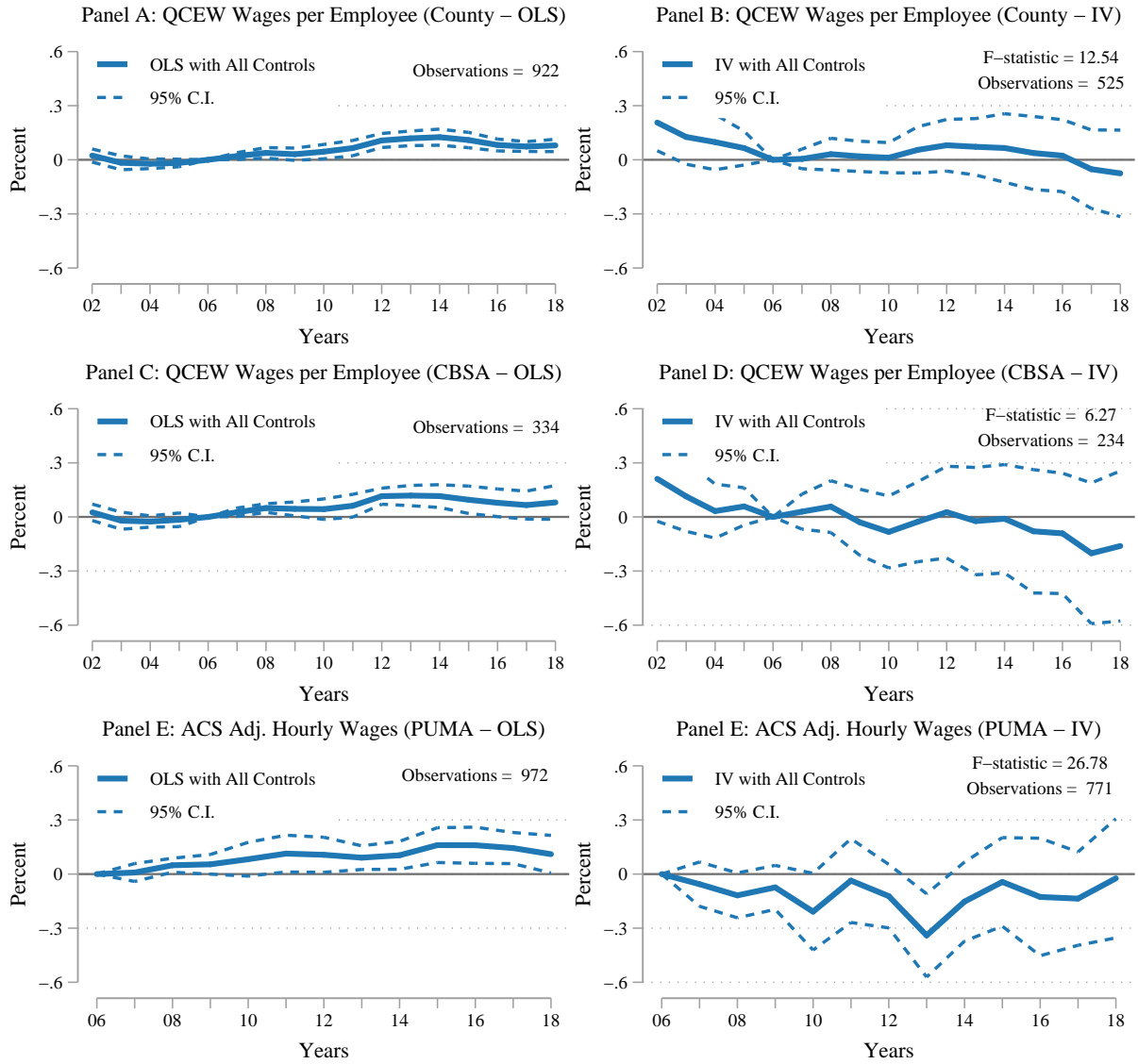
Panel A: Total Employment (All Controls)

Panel B: Total Employment (Without CA/FL/NV/AZ)

Panel C: Total GDP (All Controls)

Panel D: Total GDP (Without CA/FL/NV/AZ)

Notes: The figure plots the impulse responses of employment and GDP to the 2006-09 housing shocks with a full set of controls (Panels A and C) or without "sand states" (Panels B and D) using OLS regressions. In full set of controls, we include the 2002 shares of 23 industries, prior trends, various socio-economic characteristics of the population of the region (race, income, home ownership, education, unemployment rates, poverty, and urbanization), and employment shares for construction and government. Moreover, we include 2002 debt-to-income ratio, quality of life index, and amenity index as controls in the all-controls specification. Sand states include California, Nevada, Arizona, and Florida. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.1: Changes in Employment and GDP with Additional Controls (OLS)

Panel A: Total Employment (All Controls)

Panel B: Total Employment (Without CA/FL/NV/AZ)

Panel C: Total GDP (All Controls)

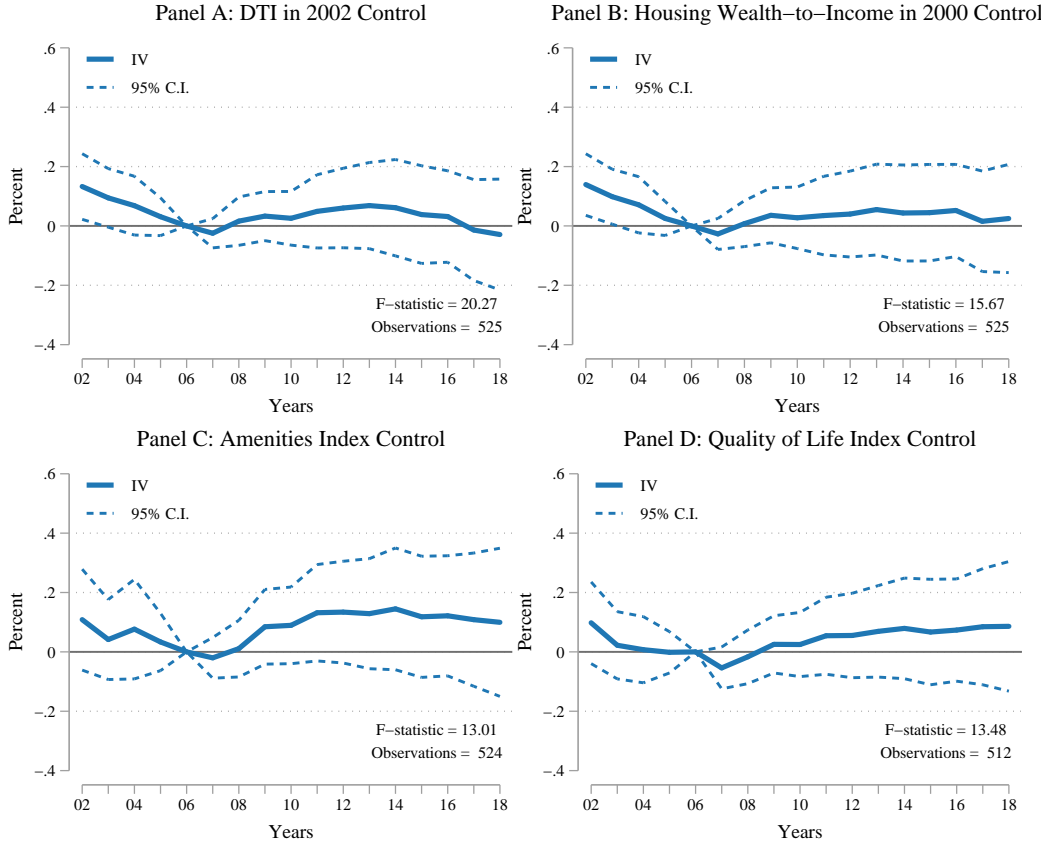**Panel D: Total GDP (Without CA/FL/NV/AZ)**

Notes: The figure plots the impulse responses of employment and GDP to the 2006-09 housing shocks with a full set of controls (Panels A and C) or without "sand states" (Panels B and D) using IV regressions. In full set of controls, we include the 2002 shares of 23 industries, prior trends, various socio-economic characteristics of the population of the region (race, income, home ownership, education, unemployment rates, poverty, and urbanization), and employment shares for construction and government. Moreover, we include 2002 debt-to-income ratio, quality of life index, and amenity index as controls in the all-controls specification. Sand states include California, Nevada, Arizona, and Florida. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.2: Changes in Employment and GDP with Additional Controls (IV)

Panel A: QCEW Wages per Employee (County – OLS)

Panel B: QCEW Wages per Employee (County – IV)

Panel C: QCEW Wages per Employee (CBSA – OLS)

Panel D: QCEW Wages per Employee (CBSA – IV)

Panel E: ACS Adj. Hourly Wages (PUMA – OLS)

Panel E: ACS Adj. Hourly Wages (PUMA – IV)

Notes: The figure plots the impulse responses of wages per employee at county level (Panels A and B) and at CBSA level (Panels C and D) using QCEW data to the 2006-09 housing shocks with a full set of controls. Panel E and F plot the impulse response of hourly wages at PUMA level using adjusted ACS data with a full set of controls. The adjustment procedure follows Beraja, Hurst, and Ospina (2019) and is described in Appendix B. The left columns are results from OLS estimations and the right columns are results from IV estimations. In full set of controls, we include the 2002 shares of 23 industries, prior trends, various socio-economic characteristics of the population of the region (race, income, home ownership, education, unemployment rates, poverty, and urbanization), and employment shares for construction and government. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.3: Changes in QCEW Wages per Employee and ACS Adjusted Hourly Wages with Additional Controls

Panel A: DTI in 2002 Control

Panel B: Housing Wealth–to–Income in 2000 Control

Panel C: Amenities Index Control

Panel D: Quality of Life Index Control

Notes: The figure plots the impulse responses of wages per employee to the 2006-09 housing shocks with additional controls for credit access and quality of life. Panel A is the baseline result, while Panel B shows results from including debt-to-income ratio in 2002 as a control. Panel C shows results from including an index of local geographic amenities from Albouy (2008) as a control, while Panel D shows results from including the measure of quality of life from Albouy (2008) as a control. The 2002 shares of 23 industries and prior trends are included in all specifications. Prior trends for wages per employee are the growth rates in wages per employee from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.4: Changes in Wages per Employee with Additional Controls for Credit Access and Quality of Life

Notes: The figure plots the impulse responses of wages per employee by sectors to the 2006-09 housing shocks. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends for sectoral wages per employee are the growth rate of wages per employee in each sector from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.5: Changes in Wages per Employee by Sector

Panel A: ACS Employment (Education <= 16)

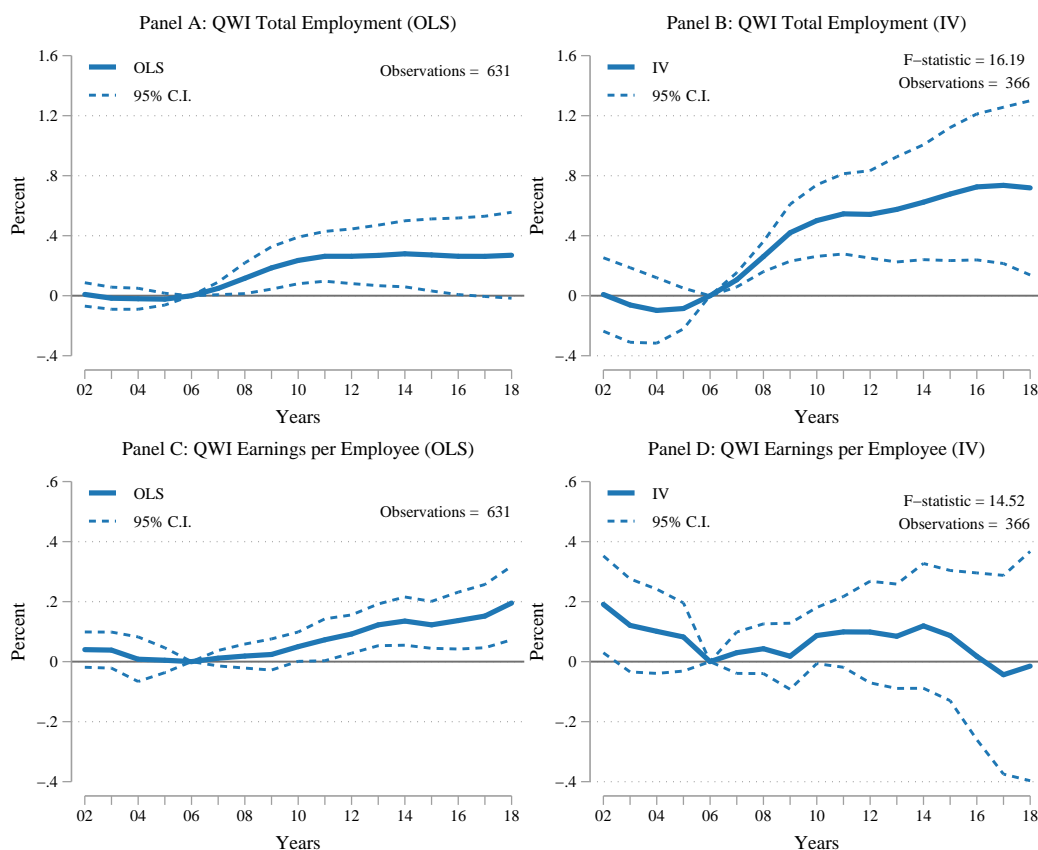Panel B: ACS Employment (Education > 16)

Panel C: ACS Employment (Age 25–40)

Panel D: ACS Employment (Age 41–55)

Notes: The figure plots the impulse responses of employment to the 2006-09 housing shocks by education and age groups using ACS data at PUMA level. Panel A shows results from the group with less than a college degree, while Panel B shows results from the group a bachelor's degree or more. Panel C shows results from the group with ages 25-40, while Panel D shows results from the group with ages from 41-55. All the results are from IV estimations. The 2002 shares of 23 industries are included. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

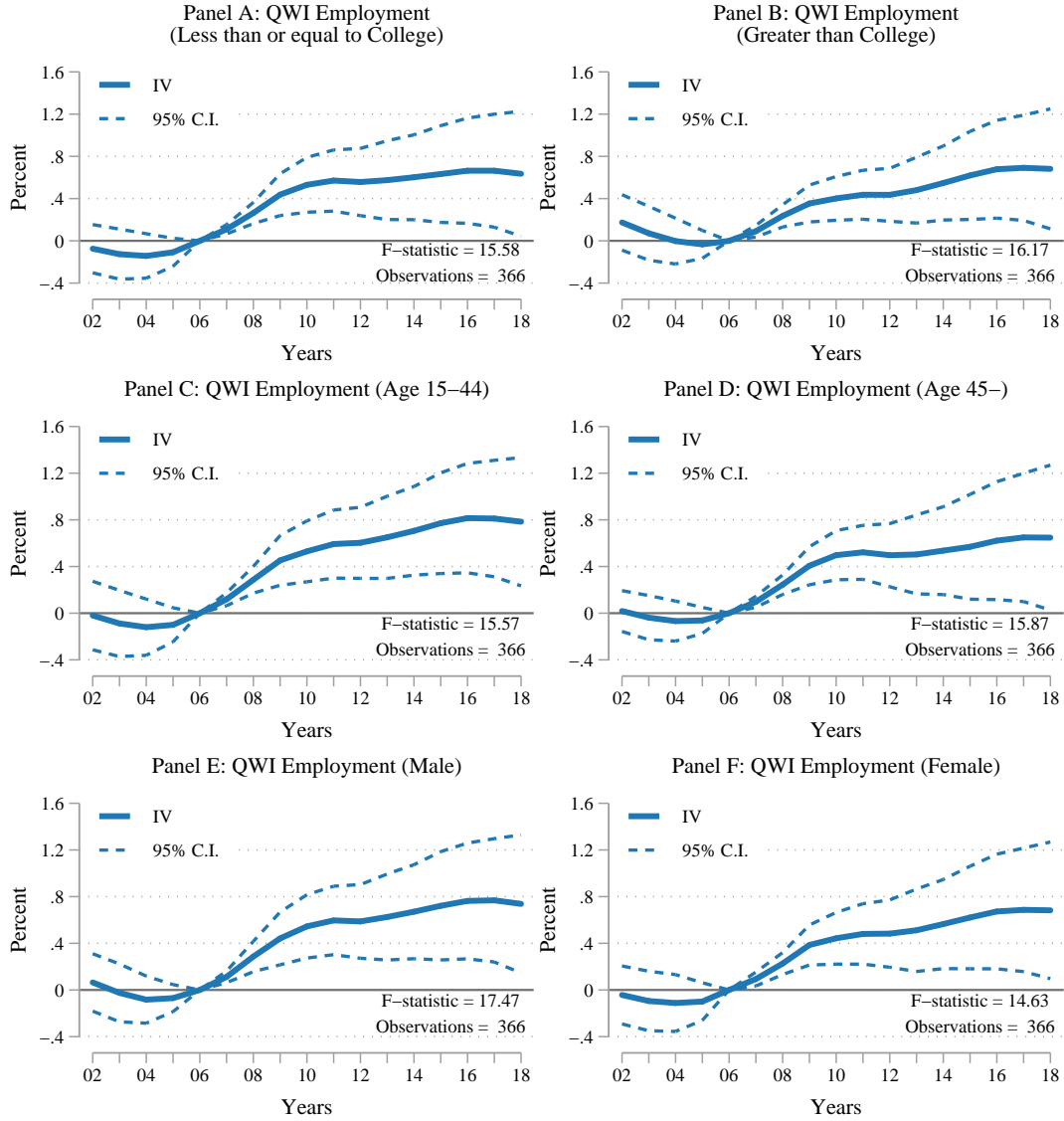Figure A.6: Changes in ACS Employment by Education and Age Groups

Panel A: ACS Average Hourly Wages (Education <= 16)

Panel B: ACS Average Hourly Wages (Education > 16)

Panel C: ACS Average Hourly Wages (Age 25–40)

Panel D: ACS Average Hourly Wages (Age 41–55)
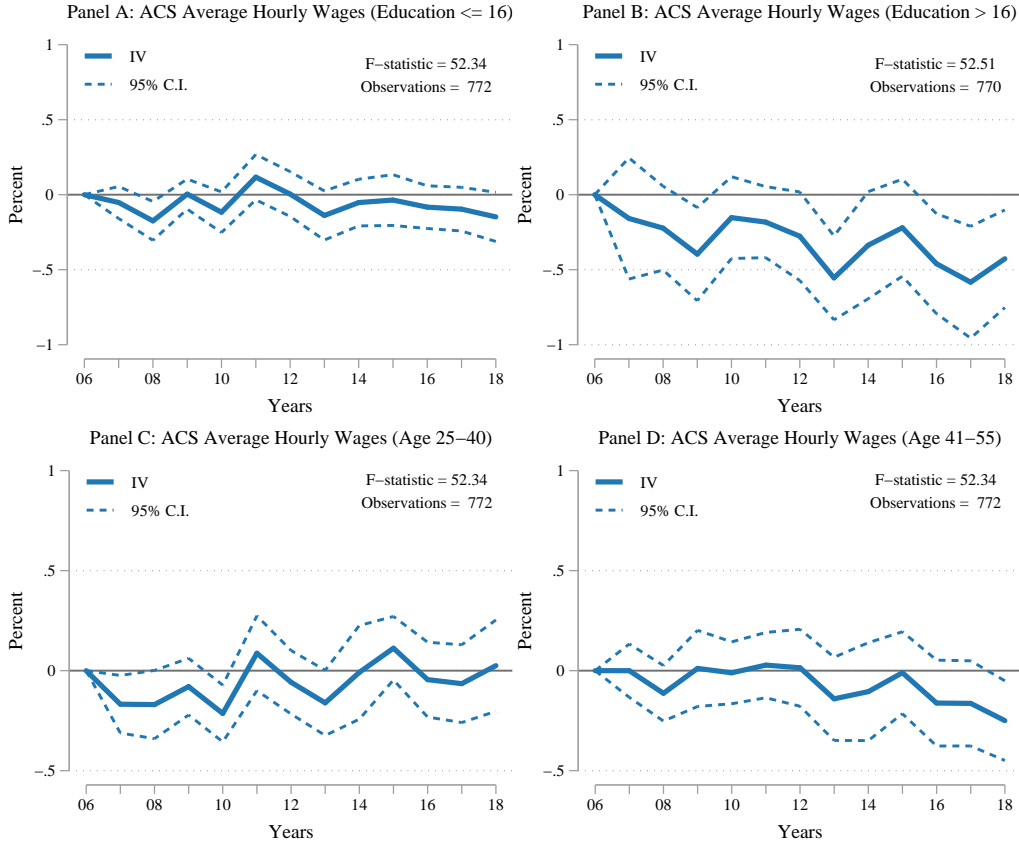
Notes: The figure plots the impulse responses of hourly wages to the 2006-09 housing shocks by education and age groups using ACS data at PUMA level. Panel A shows results from the group with less than a college degree, while Panel B shows results from the group a bachelor's degree or more. Panel C shows results from the group with ages 25-40, while Panel D shows results from the group with ages from 41-55. All the results are from IV estimations. The 2002 shares of 23 industries are included. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

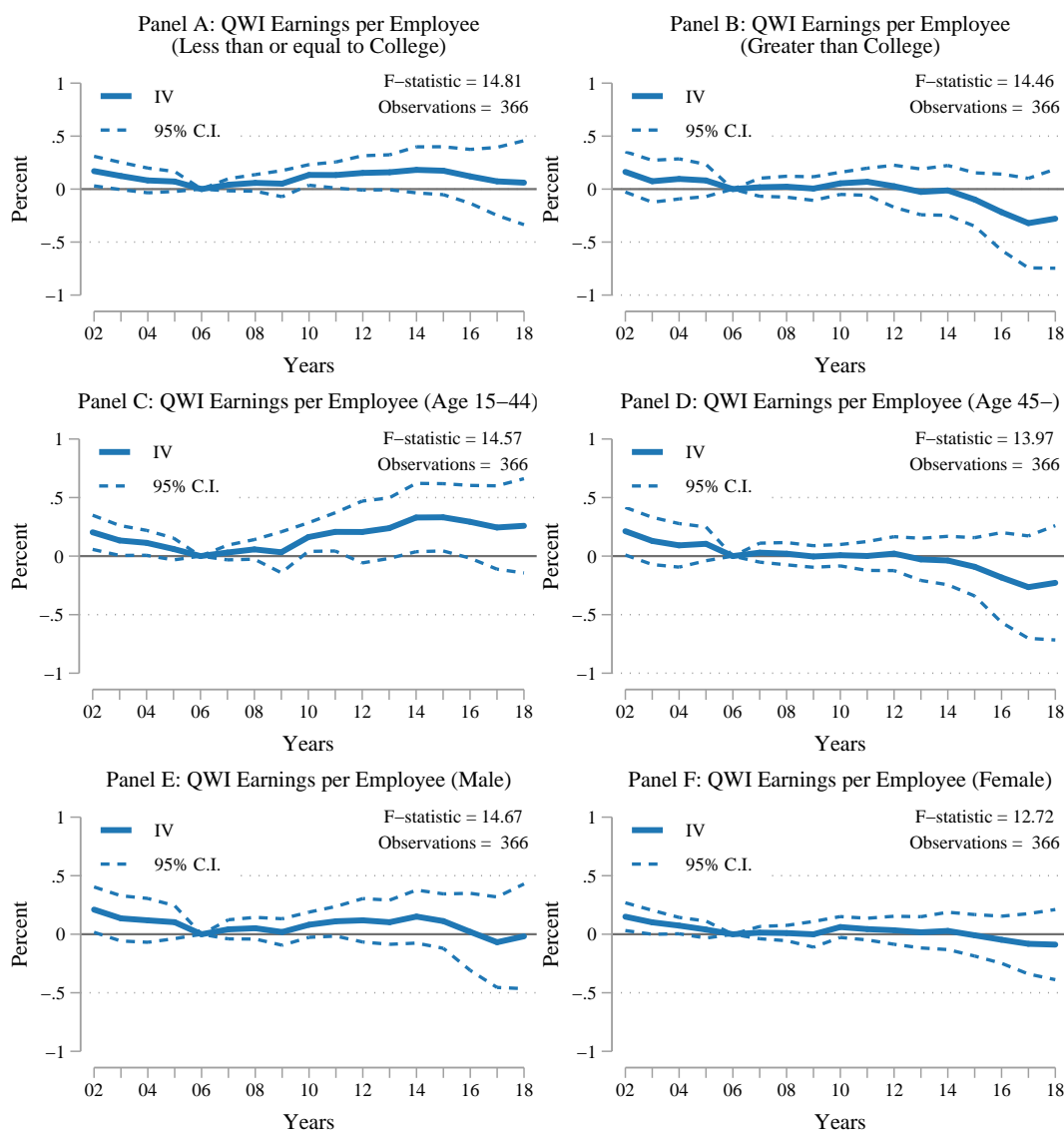Figure A.7: Changes in ACS Hourly Wages by Education and Age Groups

Panel A: QWI Total Employment (OLS)

Panel B: QWI Total Employment (IV)

Panel C: QWI Earnings per Employee (OLS)

Panel D: QWI Earnings per Employee (IV)

Notes: The figure plots the impulse responses of employment (Panels A and B) and earnings per employee (Panels C and D) to the 2006-09 housing shocks using QWI data. The left columns are results from OLS estimations and the right columns are results from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends for employment and earnings per employee are the growth rates in those variables from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.8: Changes in QWI Employment and Earnings per Employee by Year

Notes: The figure plots the impulse responses of employment to the 2006-09 housing shocks by workers' age and education groups using QWI data. Panel A shows results from the group with a college degree or less, while Panel B shows results from the group with more than a bachelor's degree. Panel C shows results from the group with ages 15-44 while Panel D shows results from the group of ages 45-plus. Panel E shows results from the group of males, and Panel F shows results from the group of females. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends are the growth rates in outcome variables from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.9: Changes in QWI Employment by Education and Age Groups

Panel A: QWI Earnings per Employee
(Less than or equal to College)

Panel B: QWI Earnings per Employee
(Greater than College)

Panel C: QWI Earnings per Employee (Age 15–44)

Panel D: QWI Earnings per Employee (Age 45–)

Panel E: QWI Earnings per Employee (Male)

Panel F: QWI Earnings per Employee (Female)

Notes: The figure plots the impulse responses of earnings per employee to the 2006-09 housing shocks by workers' age and education groups using QWI data. Panel A shows results from the group with a college degree or less, while Panel B shows results from the group with more than a bachelor's degree. Panel C shows results from the group with ages 15-44 while Panel D shows results from the group of ages 45-plus. Panel E shows results from the group of males, and Panel F shows results from the group of females. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Prior trends are the growth rates in outcome variables from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.10: Changes in QWI Earnings per Employee by Education and Age Groups
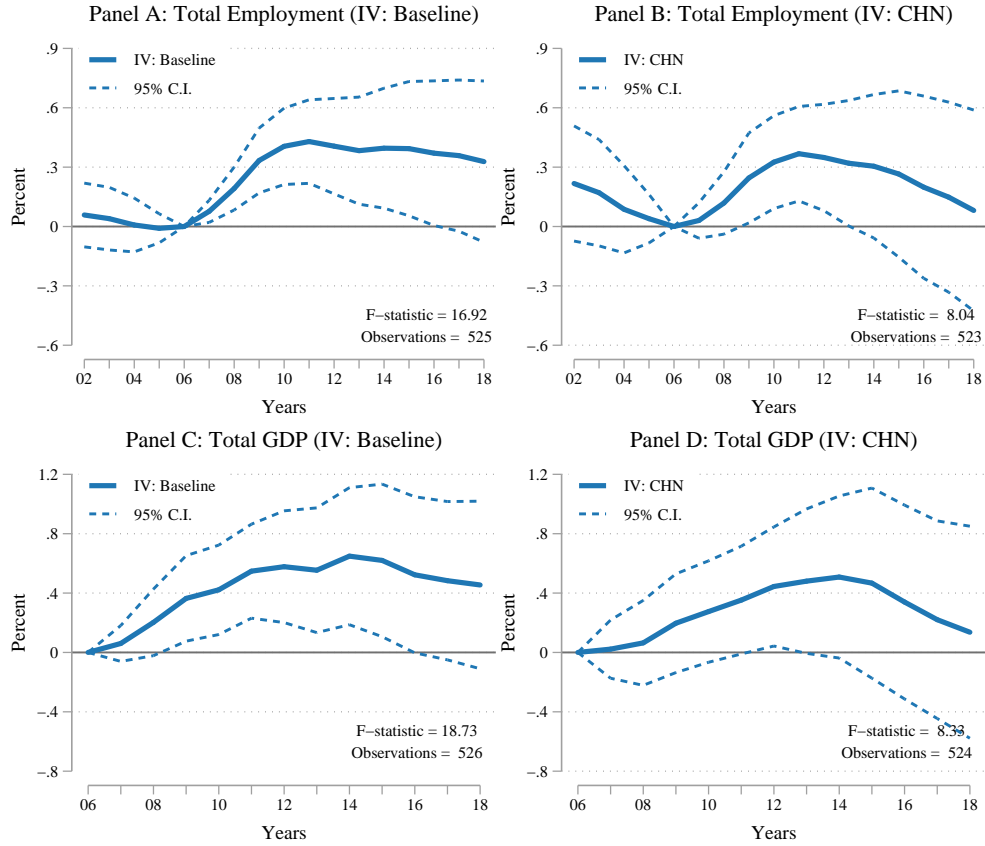
Panel A: QWI Employment (Firm Size < 250)

Panel B: QWI Employment (Firm Size >= 250)

Panel C: QWI Employment (Firm Age <= 5)

Panel D: QWI Employment (Firm Age > 5)

Notes: The figure plots the impulse responses of employment to the 2006-09 housing shocks by firms' age and size groups using QWI data. Panel A shows results from the group with employment less than 250, while Panel B shows results from the group with employment 250 or more. Panel C shows results from the group with firm ages 0-5, while Panel D shows results from the group with firm ages greater than 5. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

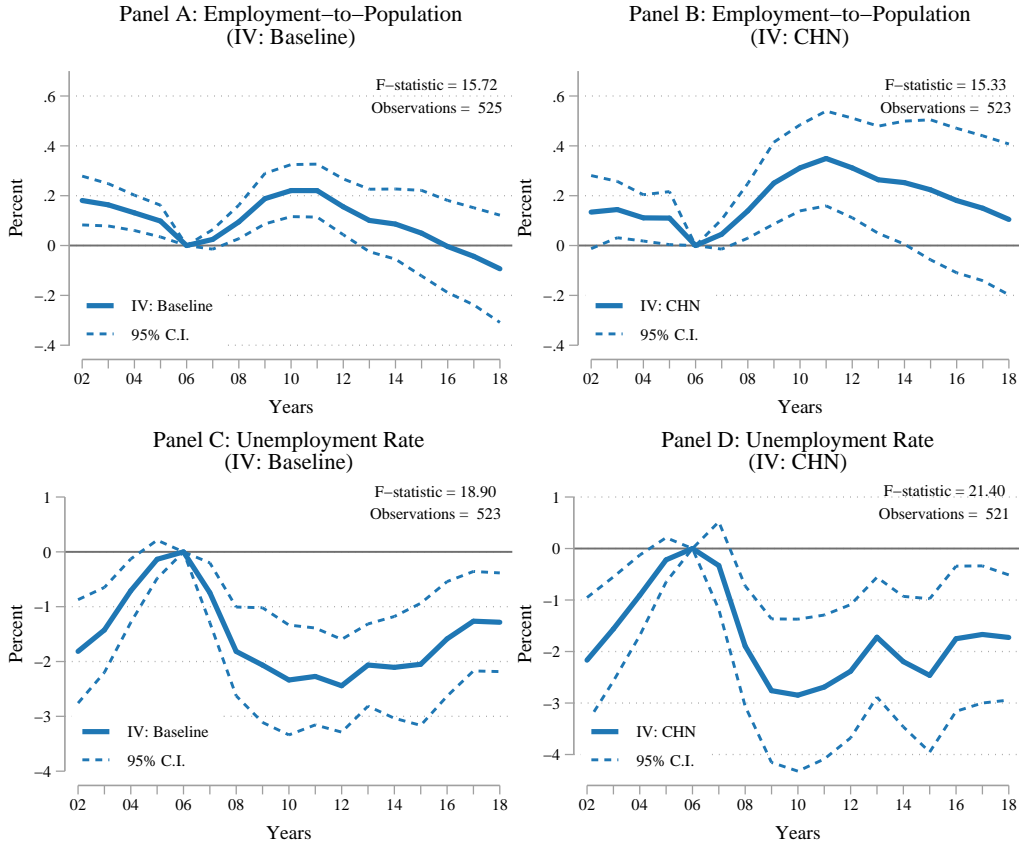Figure A.11: Changes in QWI Employment by Firm Size and Age Groups
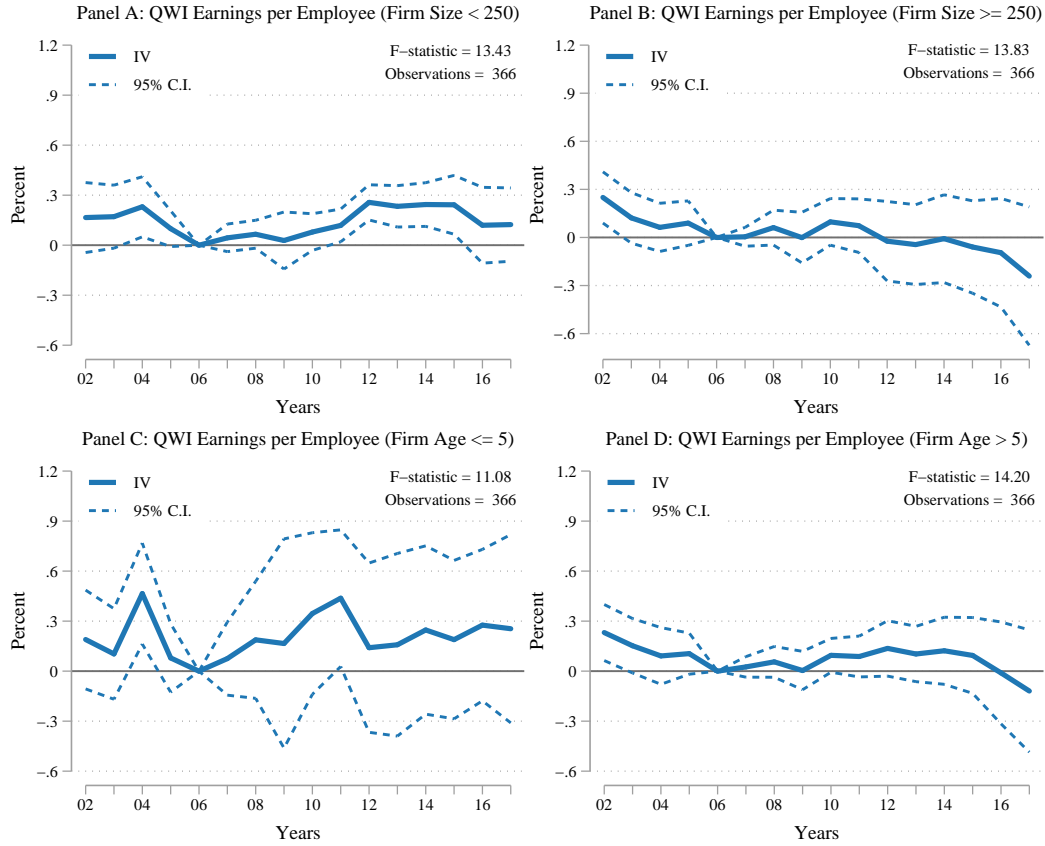
Notes: The figure plots the impulse responses of earnings per employee to the 2006-09 housing shocks by firms' age and size groups using QWI data. Panel A shows results from the group with employment less than 249 while Panel B shows results from the group with employment more than 250. Panel C shows results from the group with firm ages between 0 and 5 while Panel D shows results from the group with firm ages greater than 5. All the results are from IV estimations. The 2002 shares of 23 industries and prior trends are included. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.12: Changes in QWI Earnings per Employee by Education and Age Groups
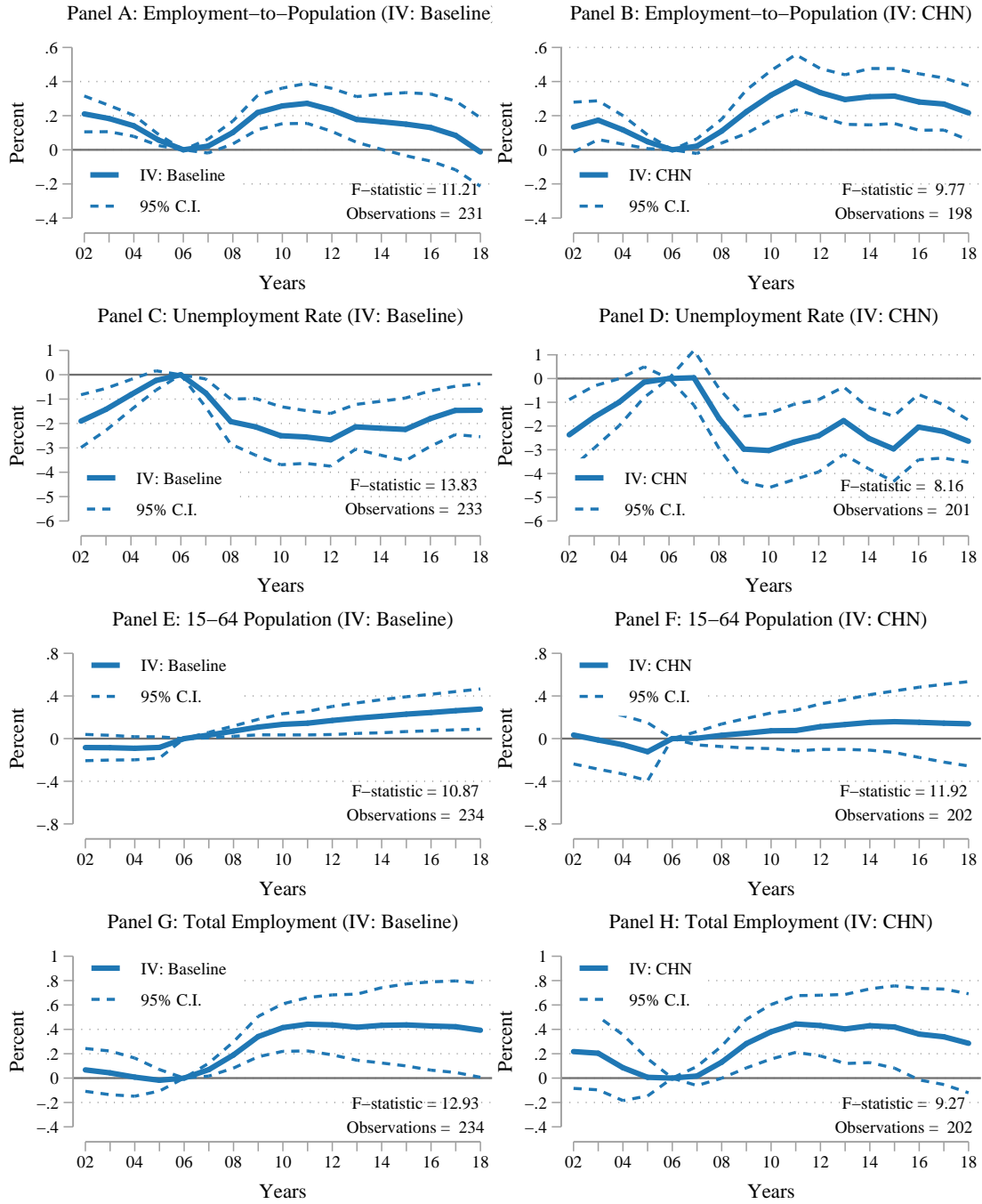
Panel A: Total Employment (IV: Baseline)

Panel B: Total Employment (IV: CHN)

Panel C: Total GDP (IV: Baseline)

Panel D: Total GDP (IV: CHN)

Notes: The figure plots the impulse responses of total employment (Panels A and B) and total GDP (Panels C and D) to the 2006-09 housing shocks. The left columns are results from the baseline IV estimations and the right columns are results from IV estimations using the instruments from Charles, Hurst, and Notowidigdo (2018). The 2002 shares of 23 industries and prior trends are included. Prior trends for employment are the growth rate in employment from 1998-2002 while prior trends for GDP are growth rate in GDP from 2002-06. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

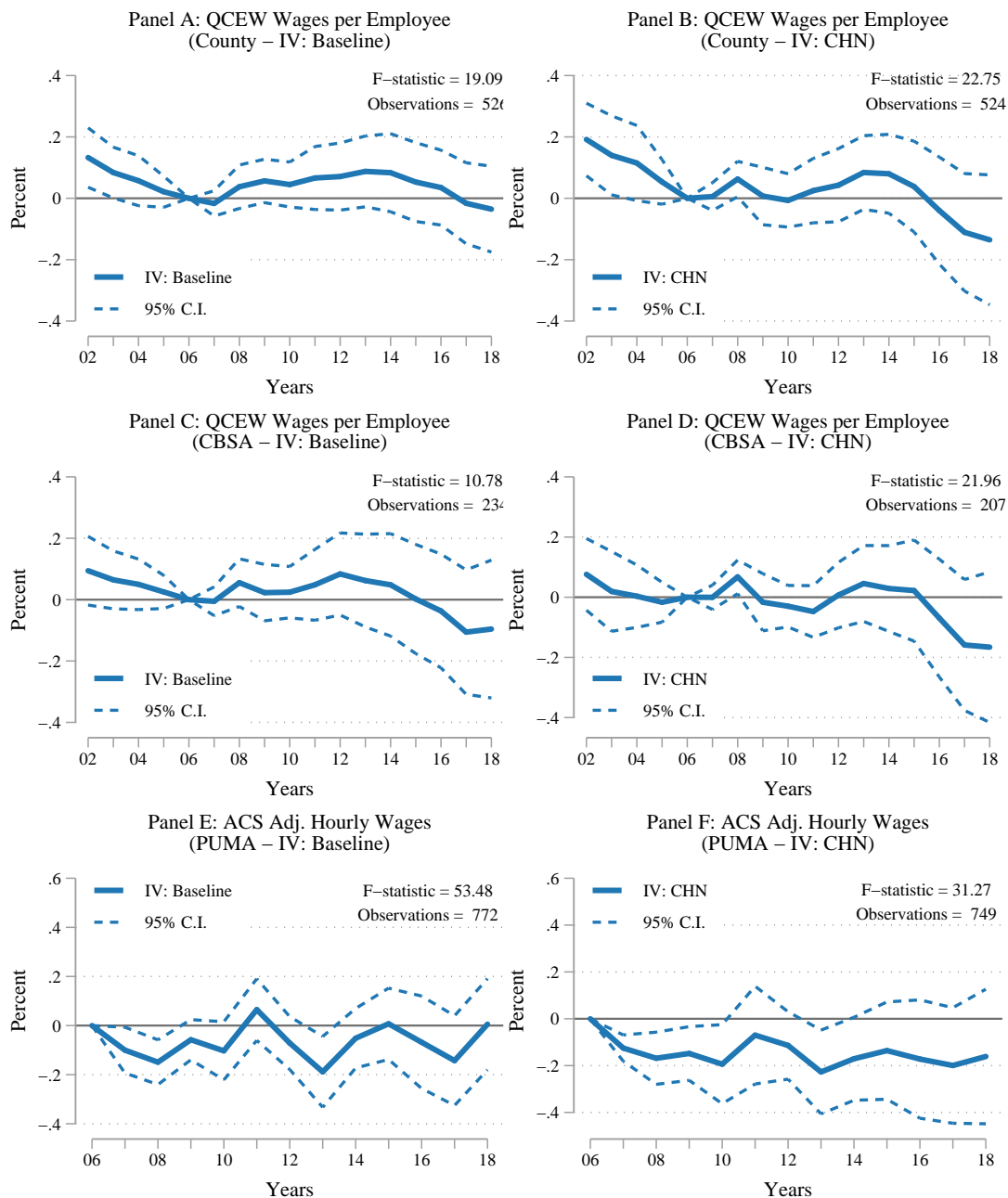Figure A.13: Changes in Employment and GDP by Year

Panel A: Employment–to–Population (IV: Baseline)

Panel B: Employment–to–Population (IV: CHN)

Panel C: Unemployment Rate (IV: Baseline)

Panel D: Unemployment Rate (IV: CHN)

Notes: The figure plots the impulse responses of county-level employment-to-population ratio (Panels A and B) and unemployment rate (Panels C and D) to the 2006-09 housing shocks. The left columns are results from the baseline IV estimations and the right columns are results from IV estimations using the instruments from Charles, Hurst, and Notowidigdo (2018). The 2002 shares of 23 industries and prior trends are included. Prior trends are log changes in employment-to-population ratio or unemployment rate from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.14: Changes in Employment-to-Population and Unemployment Rate by Year

Panel A: Employment–to–Population (IV: Baseline)

Panel B: Employment–to–Population (IV: CHN)

Panel C: Unemployment Rate (IV: Baseline)

Panel D: Unemployment Rate (IV: CHN)

Panel E: 15–64 Population (IV: Baseline)

Panel F: 15–64 Population (IV: CHN)

Panel G: Total Employment (IV: Baseline)

Panel H: Total Employment (IV: CHN)

Notes: The figure plots the impulse responses of CBSA-level employment-to-population ratio (Panels A and B), unemployment rate (Panels C and D), 15-64 population (Panels E and F), and employment (Panels G and H) to the 2006-09 housing shocks. The left columns are results from the baseline IV estimations and the right columns are results from IV estimations using the instruments from Charles, Hurst, and Notowidigdo (2018). The 2002 shares of 23 industries and prior trends are included. Prior trends are log changes in employment-to-population ratio or unemployment rate from 1998-2002. Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.15: Changes in CBSA Employment-to-Population and Unemployment Rate by Year

Panel A: QCEW Wages per Employee
(County – IV: Baseline)

Panel B: QCEW Wages per Employee
(County – IV: CHN)

Panel C: QCEW Wages per Employee
(CBSA – IV: Baseline)

Panel D: QCEW Wages per Employee
(CBSA – IV: CHN)

Panel E: ACS Adj. Hourly Wages
(PUMA – IV: Baseline)

Panel F: ACS Adj. Hourly Wages
(PUMA – IV: CHN)

Notes: The figure plots the impulse responses of wages per employee at county level (Panels A and B) and at CBSA level (Panels C and D) using QCEW data to the 2006-09 housing shocks. Panels E and F plot the impulse response of hourly wages at PUMA-level using adjusted ACS data. The adjustment procedure follows Beraja, Hurst, and Ospina (2019) and is described in Appendix B. The left columns are results from the baseline IV estimations and the right columns are results from IV estimations using the instruments from Charles, Hurst, and Notowidigdo (2018). The 2002 shares of 23 industries and prior trends are included. Prior trends for wages per employee are the growth rate of it from 1998-2002 . Sample weights (by number of households) are applied to all specifications. Robust standard errors (clustered by state) are used to calculate the confidence intervals.

Figure A.16: Changes in QCEW Wages per Employee and ACS Adjusted Hourly Wages by Year

B   Data

1. **Employment, Unemployment and Wages Measure**

   (a) **QCEW county-level employment**

       i. QCEW monthly employment data represent the number of covered workers who worked during, or received pay for, the pay period that included the 12th day of the month.

       ii. Sample period 1990-2018

           A. Main analysis: 2006-09(18) changes in employment

           B. Control for pre-trends: 1998-2002 changes in employment

       iii. 5 sectoral employment from NAICS 2-digit industry classification

           A. Tradable / Nontradable / Construction / High-skilled service sectors / Others

           B. NAICS 2-digit QCEW code are in Appendix B.1.

       iv. Industry controls (employment share controls)

           A. NAICS 2-digit QCEW sectoral employment shares of private employment (23 industries)

   (b) **QCEW CBSA- and state-level employment**

       i. Aggregate the county-level employment by CBSA or by state

   (c) **QCEW wages data**

       i. QCEW wages data represent the total compensation paid during the calendar quarter regardless of when the services were performed.

       ii. We use annual average wages in each county.

   (d) **BLS Local Area Unemployment Statistics**

       i. The Local Area Unemployment Statistics (LAUS) produces monthly and annual employment, unemployment, and labor force data for counties.

       ii. We use annual average unemployment rate and employment-to population ratio in each county.

   (e) **Quarterly Workforce Indicators**

       i. The Quarterly Workforce Indicators (QWI) provide local labor market statistics by industry, worker demographics, employer age and size.

       ii. We use annual average of beginning of quarter employment and annual average of monthly earnings of employees who worked at the beginning of the reference quarter in each county.

2. **GDP**

   (a) **BEA Local Gross Domestic Product**

    i. GDP by county is the value of goods and services produced by the county's economy less the value of goods and services used up in their production. It is the substate counterpart of the nation's GDP. GDP by county statistics are also the foundation for metropolitan and micropolitan GDP statistics.

    ii. Sample period 2001-18

        A. Main analysis: 2006-09(18) changes in GDP

        B. Control for pretrends: 2002-06 changes in GDP

    iii. Five sectoral GDP from NAICS 2-digit industry classification

        A. Tradable / Nontradable / Construction / High-skilled service sectors / Others

    iv. Industry controls (employment share controls)

        A. NAICS 2-digit QCEW sectoral employment shares of private employment (23 industries)

3. **Population**

  (a) US Census Bureau Annual County Resident Population Estimates (from 2000-2016)

  (b) For pre-2000, use Census US Intercensal County Population Data, 1970-2014 from NBER (http://www.nber.org/data/census-intercensal-county-population.html)

  (c) Use total population by each county

4. **Housing Net Worth**

  (a) We use the measure of housing net worth shocks constructed by Mian and Sufi (2014). Below is the brief description of how they construct the housing net worth shocks in Mian and Sufi (2014).

  (b) "One of our key right-hand-side variables is the change in household net worth between the end of 2006 and 2009. We define net worth for households living in county i at time t as $NW_{it} = S_{it} + B_{it} + H_{it} - D_{it}$, where the four terms on the right hand side represent market values of stocks, bonds, housing, and debt owed, respectively. We compute the market value of stock and bond holdings (including deposits) in a given county using IRS Statistics of Income (SOI) data. We estimate the value of housing stock owned by households in a county using the 2000 Decennial Census data as the product of the number of homeowners and the median home value. We then project the housing value into later years using the CoreLogic zip code level house price index and an estimate of the change in homeownership and population growth. Finally, we measure debt using data from Equifax Predictive Services that tells us the total borrowing by households in each county in a given year." (Mian and Sufi (2014) p. 2200.)

5. **DTI Control**

(a) Compute DTI at different geographical levels using data on household debt from the Equifax/Federal Reserve Bank of New York Consumer Credit Panel (CCP) made available as part of the extended Financial Accounts of the United States on the Federal Reserve Board of Governors website and the data on household income from the Bureau of Labor Statistics (BLS).

    i. At the time of writing, the Equifax/FRB NY CCP data was available at the source link: https://www.federalreserve.gov/releases/z1/dataviz/household_debt/county/map/#state:all;year:2018

(b) Calculate DTI as the ratio of aggregate household debt from Equifax (excluding student loans) to aggregate income (from BLS).

    i. Calculate aggregate household debt by summing individual household debt in the CCP within each geographical area and multiplying by the sampling ratio.

    ii. Use data from the BLS, which reports income earned by workers covered by unemployment insurance programs overseen by the Department of Labor. Income is reported quarterly and aggregated to annual amounts for each geographic region, including counties, CBSAs, and states.

6. **ACS Data**

(a) To construct adjusted wage data, we use data from the 2000 census and the 2001-14 American Community Surveys (ACS). Following Beraja, Hurst, and Ospina (2019), we calculate hourly wages for prime-age males by restricting our sample to only males ages 25-54, who live outside of group quarters, have no self-employment income, and who are not in the military. We calculate the hours worked by multiplying weeks worked last year and usual hours worked per week. We divide wage and salary income by the hours worked to calculate the hourly wages for each individual. We exclude any individual with a zero wage and truncate the measured wage distribution at the top and bottom one percent.

We adjust the hourly wages by creating a composition-adjusted wage measure following Katz and Murphy (1992). We divide our sample into six age bins (25-29, 30-34, 35-39, 40-44, 45-49, 50-54) and four education bins (completed years of schooling < 12, = 12, between 12 and 16, and 16 and more). We then adjust the wage index by averaging over those wages for 24 groups with fixed weights to calculate the wage for different educational and age groups within each geographic unit and estimate an adjusted wage index by averaging over those wages with fixed weights. We use the share of each demographic group in each geographic level during 2005 as the fixed weights.

(b) To construct an ACS employment measure, we restrict our sample to people (both male and female) who live outside of group quarters.

7. **Others**

(a) Quality of life data by Albouy (2008) (Table A.1. in http://davidalbouy.net/PDF/improvingqol.pdf)

(b) Amenities index (Natural amenities scale: https://www.ers.usda.gov/data-products/natural-amenities-scale/)

(c) All regressions are weighted by the 2000 total number of households from census.

B.1 Industry Categorization

- Tradable sector:
  - NAICS 11 Agriculture, forestry, fishing and hunting
  - NAICS 21 Mining, quarrying, and oil and gas extraction
  - NAICS 31-33 Manufacturing

- Nontradable sector:
  - NAICS 44-45 Retail trade
  - NAICS 72 Accommodation and food services

- Construction sector:
  - NAICS 23 Construction
  - NAICS 53 Real estate and rental and leasing

- High-skilled services sector:
  - NAICS 51 Information
  - NAICS 52 Finance and insurance
  - NAICS 54 Professional and technical services
  - NAICS 55 Management of companies and enterprises
  - NAICS 56 Administrative and waste services
  - NAICS 61 Educational services
  - NAICS 62 Health care and social assistance

- Others:
  - NAICS 22 Utilities
  - NAICS 42 Wholesale trade
  - NAICS 48-49 Transportation and warehousing
  - NAICS 71 Arts, entertainment, and recreation
  - NAICS 81 Other services, except public administration
  - NAICS 92 Public administration