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Urban Re-evaluation, Housing Wealth, and Consumption: Evidence from the COVID-19 Pandemic

Joakim Davidsson (42422)^{*}

Abstract

Do increases to house prices generate responses in consumption? I use the cross-sectional variation in US house prices generated by the COVID-19 pandemic and urban flight to estimate elasticities and marginal propensities to consume out of house prices (MPCH). I find that for a 1% increase to housing net worth, consumption increases by 0.839%. The average MPCH is estimated at 6.2 cents per dollar increase in house prices. Consumption in wealthier counties is found to respond less, in line with the notion of a concave consumption function; some evidence of heterogeneity in MPCH's due to leverage is also found. These findings therefore highlight the role of heterogeneity in explaining the relationship between asset prices and consumption dynamics. Additionally, I find evidence of a shift in consumer preferences following the pandemic. Cities that saw a larger decline in demand for urban housing also saw a shift away from in-person services and urban amenities, such as food and accommodation.

Keywords: Consumption, MPC, House Prices, Wealth Effects

JEL Codes: D12, E21, G51, R21

Supervisors: Prof. Andreea Enache[†], Prof. Paolo Sodini[‡]
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Discussants: Evan Shokor Qasim
Examiner: Magnus Johannesson

^{*}42422@student.hhs.se

[†]andreea.enache@hhs.se

[‡]paolo.sodini@hhs.se

I. Introduction

Due to the transition to remote work and migration away from urban city centers brought on by the COVID-19 pandemic, the US experienced an overall surge in housing demand, and, in particular, a rise in suburban house prices relative to those in the urban core⁴. Nationally, house prices increased by 32.27% percent between January 2020 and January 2023, for thirty of the largest US cities, the median house price increased by 42.99% percent in the suburbs, and 36.00% percent in the urban core (see [table 1](#)). Did the rise in housing wealth generate measurable consumption responses?

Whether consumers increase their spending in response to income or wealth shocks is central to understanding the propagation of shocks and policy in the macroeconomy. Traditional models tell us that consumption responds to shocks in wealth and house prices when assumptions regarding complete risk-sharing and borrowing do not hold ([Cochrane 1991](#); [Carroll and Kimball 1996](#)). Finding evidence of consumption responses out of wealth shocks may therefore serve as an indication that these friction-less representative agent models are not suited to explain consumption dynamics.

As a widely owned asset, housing wealth is useful for studying questions regarding the respective impacts of wealth and asset prices on the real economy. If boom-bust cycles in housing markets cause declines in consumer spending and unemployment, it may motivate policy intervention that aim to stabilize housing markets and household balance sheets. It also carries implications for the role of heterogeneity in macroeconomic modelling, as the assumptions that generate said relationship between wealth and consumption are the same used to emphasize the role of heterogeneity in the macro literature. These modelling choices in turn carries implications for policy (see e.g., [Kaplan et al. 2018](#); [Broer et al. 2023](#)).

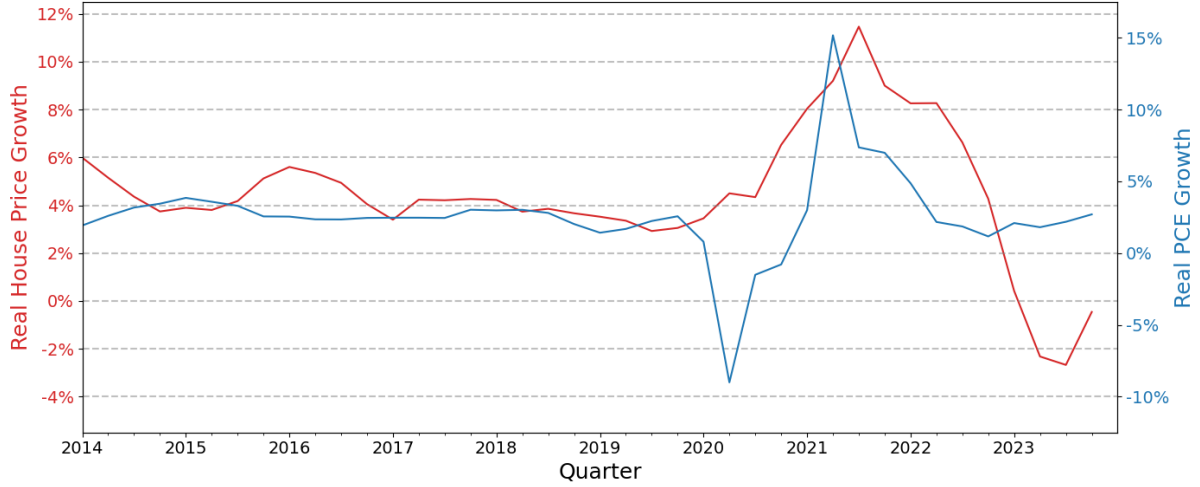
Using county-level data and re-evaluation of urban real estate during COVID-19 as a source of cross-sectional variation, I estimate that for a 1% increase to housing wealth consumption increases by 0.835%. The average Marginal Propensity to Consume out of Housing wealth (MPCH) out of a 1 dollar house price increase is estimated at 6.2 cents. My findings therefore reject the notion of perfect risk-sharing in consumption.

I also find evidence for heterogeneity in MPCH's. Theory suggests that consumption is a concave function in wealth, which can be generated either by a lack of consumption insurance or an inability to smooth consumption over time. By interacting the housing wealth shock with the county per capita wealth I find that MPCH's decrease as wealth increases, indicating the concave relationship as suggested by theory. I also split the sample into equally sized bins by regional median household income and find that the MPCH is hump-shaped in income, as predicted by theory ([Berger et al. 2018](#)).

Theory also suggests that the MPCH should increase in leverage [Berger et al. \(2018\)](#), as higher leverage may indicate less liquid to self-insure consumption. However, underwater households may be unable to extract equity from their home, implying one should observe a sharp decline in MPCH's at the highest levels of leverage [Berger et al. \(2018\)](#). I find some evidence supporting this prediction when splitting the sample into bins by their degree of leverage and estimating an MPCH for each bin.

⁴[Gupta et al. \(2022\)](#) find a relationship between the re-evaluation of urban real estate and urban flight and the demand for remote work. [Mondragon and Wieland \(2022\)](#) estimate a relationship between housing demand and the demand to work from home.

Figure 1: Real House Price Growth vs Real PCE Growth



Notes: This figure plots the quarterly Real House Price Growth compared to the same quarter in the previous year (red, left axis), and the Real Personal Consumption Expenditure (PCE) growth compared to the same quarter in the previous year (blue, right axis). Both are measured at the national level. The house price data is the Zillow Home Value Index (ZHVI) measured at the national level. The real PCE data is the Fred Series PCECC96.

Causally identifying the relationship between housing wealth and consumption is challenging due to the likely endogenous relationship and potential for reverse causation and measurement error. To address these concerns I use several instrumental variable strategies in both cross-sectional and panel-settings, and find that the results are robust to multiple research designs and specifications. As such, this paper not only contributes by providing estimates derived from a novel house price shock, but also as an opportunity to evaluate previously used identification strategies in a new setting. Moreover, finding a significant relationship across multiple research strategies in-itself provides useful inference as it increases the requirements put on any endogenous factor(s) that would simultaneously invalidate each result.

In the cross-sectional setting I employ two strategies. First, I instrument the house wealth shocks with the pre-pandemic share of remote workers in a county, following [Mondragon and Wieland \(2022\)](#) who study the impact of remote work and migration on housing demand during the COVID-19 pandemic. To my knowledge, I am the first to do so in the context of the housing wealth effect. My main results derive from estimations with this instrument.

Second, I follow [Mian et al. \(2013\)](#) who instrument house wealth shocks with local housing supply elasticity estimates by [Saiz \(2010\)](#). Making this paper a useful test for the external validity of this strategy.

In the panel setting I adopt the approaches of [Aladangady \(2017\)](#) and [Guren et al. \(2021\)](#) respectively. The former instruments house price growth with the interaction of the [Saiz \(2010\)](#) instrument and the real yield on a ten year treasury bill, whilst the latter develop an instrument based on the sensitivity of a county's house prices to regional house price shocks (discussed further below).

Given the unique nature of the pandemic and house price shock, it is not only of interest to study whether it has brought about changes to *how much* we consume, but also *what* we consume. While the house price shock mechanically generates cross-sectional variation in

housing wealth, it may also reflect a change in preferences and demand for urban amenities (e.g., restaurants and bars). Not only is this a potential latent and endogenous factor when estimating housing wealth effects, it also carries implications for the future growth of urban economies.

For the 47 largest US cities I find that consumption in categories typically associated with urban centers, such as accommodation and food services, recovered well below the overall expenditure category (7.92% c.f., 14.36%) (table 2). Is there a relationship between a decline in consumption in categories typically associated with urban centers and the pandemic? The magnitude of the re-evaluation of urban housing relative to suburban housing can be quantified at the city-level by estimating the elasticity of house prices with respect to their distance from the city center. This estimated object is referred to as the price gradient, and the change in the price gradient captures the change in the relative value of suburban housing. This measure is useful when studying the pandemics' impact on consumption preferences, as it can be interpreted as a shift in the demand for urban density and amenities. That is, it quantifies the extent to which the pandemic impacted the preference for suburban vs. urban living. Predictably, areas with stricter COVID-19 policy responses, a higher presence of industries easily adapted to remote work, and negative migration saw greater re-evaluations; where, re-evaluations are measured as the change to the estimated price-gradients between 2019 and 2023.

Using the same measure as an independent variable, I estimate statistically significant relationships with spending growth of urban categories (food & accommodation and in-person services), whilst no significant relationship with grocery and remote spending is found. In a similar exercise, I regress the relative spending growth of said consumer categories on the change in the price gradient and I find the same relationships.

These results indicate a shift away from the consumption of urban amenities in cities that saw a greater decline in relative demand for urban housing, suggesting that the preference shift away from urban housing is also reflected in the consumption of urban amenities.

II. Literature Review

II.A. Consumption and Housing Wealth

II.A.1. Theoretical Background. What proportion of a shock to wealth is translated into consumption? What is the propensity to consume out of wealth shocks? These questions are central to modern macroeconomics, as they address fundamental aspects of household's financial decision making, and carries implications for the dynamics of consumption; the propagation of macroeconomic shocks; the role of heterogeneity; and optimal policy.⁵ Hous-

⁵For further background and a review of the relevant literature see Kaplan and Violante (2022). The marginal propensity to consume (MPC) is defined as the consumption response from a wealth or income shock. I emphasize the fact that the MPC I estimate is out of housing wealth, which I denote as MPCH. Unless otherwise stated, the MPC refers to the marginal propensity to consume out of labour income. This is in line with other papers in the literature (see e.g., Berger et al. 2018; Guren et al. 2021). Some authors use the term 'MPC' to denote consumption responses to changes in income or wealth in dollar terms (e.g., the MPC for a \$500 windfall gain); I use it both in the context of elasticities (% change in consumption for a 1% change in house prices/wealth) and dollar terms. Which one is implied should be clear from the context

ing is a widely owned asset, making it suitable for studying the aforementioned questions.

A branch of the literature on housing and consumption has emphasized the role of housing as a distinct asset in life-cycle models, characterizing it by its illiquidity; tenure choice; equity withdrawals costs; and more (Berger et al. 2018). However, housing wealth shocks may also reflect variations in an agent’s total net worth, making them useful for studying questions regarding the general relationship between wealth and consumption. For example, Mian et al. (2013) use negative housing wealth shocks during the great recession to study its impact on consumer spending at the zip-code and county levels. By scaling regional house price shocks by the ratio of housing wealth to net worth, the authors account for household’s balance sheets in the transmission of house price shocks on consumption. In this sense, the authors use shocks to housing wealth to draw inference regarding the general relationship between wealth and consumption and the role of heterogeneity, rather than treating housing as a theoretically distinct asset.

Standard models of consumption predict that idiosyncratic shocks to a household’s wealth should not impact their level of consumption. When households are assumed to have access to complete markets, such that any shocks to their income or wealth can be perfectly insured against, Cochrane (1991) shows that idiosyncratic wealth shocks should not generate changes in consumption. That is, he shows that the relationship between consumption and wealth can be reduced to a log-linear equation as in (1), where $\Delta \ln W_{it}$ is a log wealth shock, $\Delta \ln C_{it}$ denotes consumption growth, and ϵ_{it} is a white-noise term. β_1 – interpreted as the elasticity of consumption with respect to wealth – should equal zero.

$$\Delta \ln C_{it} = \beta_0 + \beta_1 \Delta \ln W_{it} + \epsilon_{it} \quad (1)$$

Departures from this result arises when households face borrowing constraints or insurable income/wealth risks. For example, if information asymmetries give rise to credit rationing and borrowing constraints, a rise in house prices increases the value of the agent’s collateral, which in turn may relax the borrowing constraint and allow the agent to increase their consumption. Similarly, if markets are incomplete and households face uninsurable income/wealth risks, they have to self-insure through the accumulation of precautionary savings. A positive wealth shock therefore reflects an increase in their ability to self-insure, allowing the household to commit less of their income to savings and more towards consumption. Carroll (2001) argues that relaxing either assumption regarding perfect insurance or borrowing yield the same result: consumption is a positive concave function of wealth, and the MPC is positive and decreasing in wealth.⁶

From these theoretical insights, Mian et al. (2013) center their paper around two testable predictions, (i) Does consumption respond to exogenous shocks to (housing) net worth? Where positive findings would lead to the rejection of the hypothesis that consumption is fully insured, and (ii) does consumption respond less to wealth shocks as wealth increases? Which would suggest that consumption is concave in wealth.

Modelling housing as a distinct asset implies different channels for the transmission of housing wealth shocks to consumption, however the story is reminiscent to that of general

if not explicitly stated.

⁶For further discussions on the concavity of consumption see e.g., Holm (2016) and Carroll and Kimball (1996).

wealth. [Sinai and Souleles \(2005\)](#) show that, in a model with no risk to non-housing wealth and income, homeowners with lengthy tenures are perfectly hedged against house price shocks, as increases in house prices are offset by increases to the implied cost of housing services; leading to consumption not responding to housing wealth shocks.⁷

Similarly to the general wealth case outlined above, when households are unable to fully insure risks and borrow freely, we expect consumption to move with house price shocks. [Berger et al. \(2018\)](#) show that, in a model with borrowing constraints and incomplete markets, the household’s consumption response to a housing wealth shock is approximately equal to their marginal propensity to consume out of transitory income shocks (MPC) multiplied by their housing wealth. Put differently, the MPCH is positive and increases as households are more constrained (higher MPC) or as their housing wealth increases. The MPCH is therefore predicted to be largest for agents in the middle of the wealth distribution, as these agents are most likely to simultaneously be homeowners and liquidity constrained (i.e., have a large MPC). Factors such as household leverage, liquid wealth, size of the house price shock, and more, provide further sources of MPCH heterogeneity.⁸

Why is estimating MPC’s and their heterogeneity important? The MPC and its distribution plays a significant role in consumption dynamics and modern macroeconomics. It carries implications for the transmission and welfare implications of monetary policy, fiscal policy, risk-taking and more, and has motivated a rich literature dedicated to studying the role of the MPC, the assumptions that generate it (and its distribution) in the macroeconomy.⁹

II.A.2. Empirical Evidence Empirical research that links consumption and house prices goes back several decades (see e.g., [Attanasio and Weber 1994](#); [Muellbauer and Murphy 1990](#); [K. E. Case et al. 2005](#)) The likely endogenous relationship between house prices and consumption has long been recognized in the literature. For example, it was pointed out by [King \(1990\)](#) in an exchange with [Muellbauer and Murphy \(1990\)](#) regarding the causes of the UK consumption boom of the 1980s, that, whilst increases to housing wealth may cause higher consumption, both variables may be driven by common factors such as an increased expected lifetime income.¹⁰ These concerns have since motivated the use of causal identification strategies that aim to isolate the causal effect of housing wealth shocks on consumption.

Instrumental variable estimation is a common approach for identifying plausibly exogenous variation in housing wealth. [Mian et al. \(2013\)](#) use estimates of local housing supply elasticities based on topological data and local land-use regulations, initially developed by [Saiz \(2010\)](#). The estimated MPCHs range between 0.6% – 0.8% for a 1% increase to housing wealth, and a 5 - 7 cents increase in consumption per dollar increase to housing wealth. Their findings indicate that consumption is concave in wealth, as predicted by consumption theory

⁷[Buiter \(2008\)](#) and [Campbell and Cocco \(2007\)](#) make similar arguments.

⁸Whilst wealthy households hold high levels of housing wealth, their MPC out transitory income shocks is low, as they are less likely to be constrained. Leading to a lower MPCH.

⁹This is discussed at greater length in section [section VII.](#) For an accessible summary of the implications of heterogeneity on policy see [Moll \(2020\)](#)

¹⁰Intuitively, if a household’s expected lifetime income increases, they will likely increase both their consumption and their demand for housing. Underlying factors such as trade liberalization, technological progress, and demographic changes may lead to higher expected incomes, and consequentially higher consumption and housing demand.

(Carroll and Kimball 1996). Later studies have since criticized the plausible exogeneity of the Saiz instrument Davidoff (2016) due to its correlation with long-term productivity trends which in turn correlates with consumption.¹¹

Aladangady (2017) employs the Saiz instrument in a panel setting which allows for the inclusion of fixed effects, which are argued to absorb long-term productivity growth factors. Using micro-data on consumption from the CEX survey between 1986 and 2008, the MPCH is estimated at 4.7 cents per dollar increase in house prices. Their findings indicate no consumption responses by renters. On one hand this is expected as they do not benefit from the increase in wealth; on the other hand, they may be future home-buyers who respond by lowering consumption and increase their savings.

Aladangady (2017) also investigates the role of collateral constraints, and whether price shocks transmit to consumption purely via an endowment effect or additionally via a relaxation of collateral constraints. They find that households with a high debt-service to income ratio (DSR) primarily respond to house price increases. Similarly, highly levered households (loan-to-value ratio above 0.8) are also found to be more sensitive to house price rises. These findings are rationalized as indicating that consumption responses are primarily driven by a relaxation of borrowing constraints.

In addressing the Davidoff (2016) criticism against the Saiz instrument, Guren et al. (2021) construct a novel panel ‘sensitivity’ instrument. This also allows them to absorb long-term trends via the implementation of fixed effects. They estimate a house price elasticity of 0.084 between 1978 and 2017, which translates to a 2.67 cents increase in consumption per dollar house increase. They also find that the elasticity has declined over time, reaching a peak of 0.2 around the mid-90’s, declining to just below 0.1 in the 2000s. The authors find no support for boom-bust asymmetries in the elasticity nor that the elasticity is more pronounced around financial crises.

Graham and Makridis (2023) develop a shift-share style instrument, using the share of house age and room size respectively. Using Household Level data on non-durable consumption from the Nielsen Consumer Panel they estimate house price elasticities ranging 0.9 – 0.11 and MPCH’s in dollar terms ranging 0.78 – 0.93 cents per dollar increase. These estimates are much smaller than those estimated from total spending. However, they are close to previous MPCH measures for non-durable goods, for example Mian et al. (2013) estimate an MPCH of 1.6 cents the same goods category.

II.B. Consumption and the COVID-19 Pandemic

To my knowledge, no paper has studied the relationship between housing wealth and consumption during the COVID-19 pandemic, however several papers leveraged proprietary microdata to study the direct consequences of the COVID-19 pandemic on consumption (see e.g., Baker et al. 2020; Chetty et al. 2023; Cox et al. 2020).

Heterogeneous responses across income groups and expenditure-categories are documented. Chetty et al. (2023) find that the largest percentage decline in spending in March–April 2020 was observed amongst higher income brackets. Spending by the same group also recovered slower than lower and middle income brackets. By 2021 spending had recovered to and

¹¹See section [section V](#). for a lengthier discussion regarding the endogeneity concerns of the Saiz instrument.

begun to exceed January 2020 levels for both high and low income groups.

They also show that the fall in consumption was dominated by in-person services such as hotels and restaurants. [Baker et al. \(2020\)](#) corroborate this finding, showing a substitution from in-person services and travel to groceries and delivery services. This sets the pandemic apart from previous recessions such as the great recession of 2008, where the fall in consumption was driven by durables and non-durables.

III. Data

III.A. Consumption

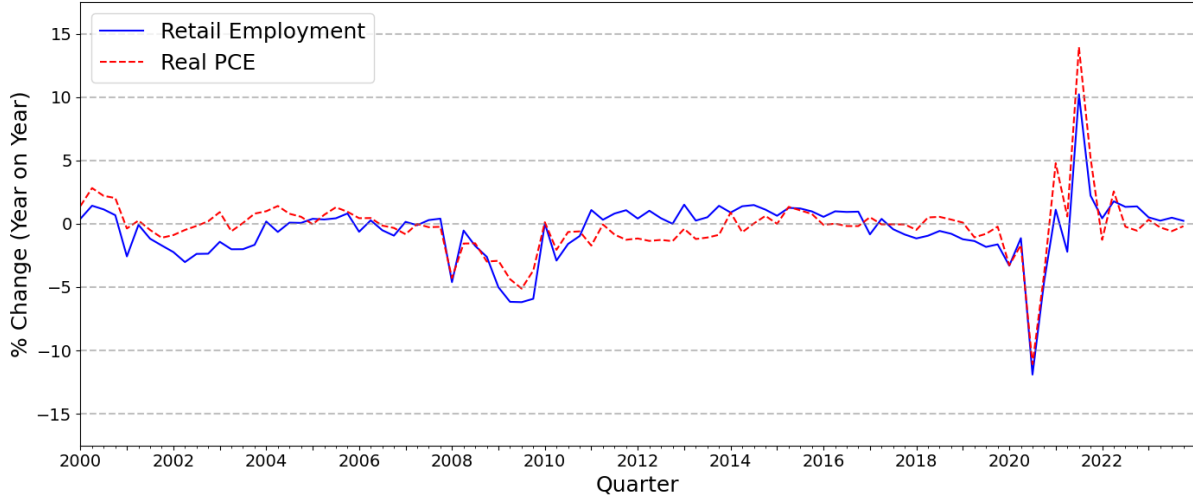
As consumption data is not made public at the county level, I follow [Guren et al. \(2021\)](#) and proxy consumption with regional retail employment. Prior studies often rely on proprietary and non-public individual-level data such as credit card spending [Mian et al. \(2013\)](#). This necessitates a substitute for consumption data when relying solely on public data. [Guren et al. \(2021\)](#) argue that regional measures of retail employment growth is an ample proxy for regional consumption.¹² The intuition for the use of retail employment is that it is an intermediate good in consumption, and should therefore reflect any underlying changes to PCE. It is common practice to use retail employment in lieu of accurate regional consumption expenditure data. For example, the regional Bureau of Economic Analysis (BEA) Personal Consumption Expenditure (PCE) measure at the state level is imputed using retail employment in-between census years (the US census is a decennial survey).

[Guren et al. \(2021\)](#) show that, at a regional level, when measurement error is controlled for via instrumental variable estimation, consumer spending and retail employment move almost one-for-one. In [figure 2](#) I plot the close relationship between the year-on-year growth of retail employment (blue) and Real PCE (blue) since 2000. The two series move closely together which provides assurance, even throughout the pandemic, which provides important assurance of the proxy's relevance. This is crucial as it addresses any concerns regarding a decoupling between consumption and retail employment during the pandemic. I therefore use the same proxy and similarly interpret estimated housing wealth elasticities in [section VI](#). as consumption responses. I obtain employment data at the county level from the Quarterly Census of Employment and Wages by the BLS. I use data at the 2-digit NAICS code level, 44-45 which covers the whole retail sector.

[Guren et al. \(2021\)](#) show that the emergence of online retail has not impacted the relationship between the retail employment and real PCE over the 21st century. One may however be concerned that the pandemic, due to lockdown policies and lingering shifts in preferences and consumer behaviour, has lead to a decoupling between the variables. It should be noted that the NAICS sector 44-45 encapsulates both online and physical retail, so even if the pandemic has brought on a shift towards either, its growth would still be captured. As discussed in [section II](#)., there is evidence that categories were hit differently during the pandemic. However, this was primarily a away shift away from in-person services

¹²For a wider discussion regarding the publicly and privately available alternative consumption measures, see [Guren et al. \(2021\)](#). One important point they raise is that several private providers of regional consumption measures use retail employment growth to impute values at the regional levels.

Figure 2: Real Personal Consumption Expenditure Growth vs Retail Employment Growth



Notes: This figure plots the four quarter percentage change of aggregate retail employment (blue) and aggregate real PCE (Blue). I control for a linear trend in the series respectively, as the subsequent analysis either controls for time fixed-effects or is purely cross-sectional. The Aggregate retail employment is FRED series CEU4200000001 and the aggregate real PCE is FRED series PCECC96.

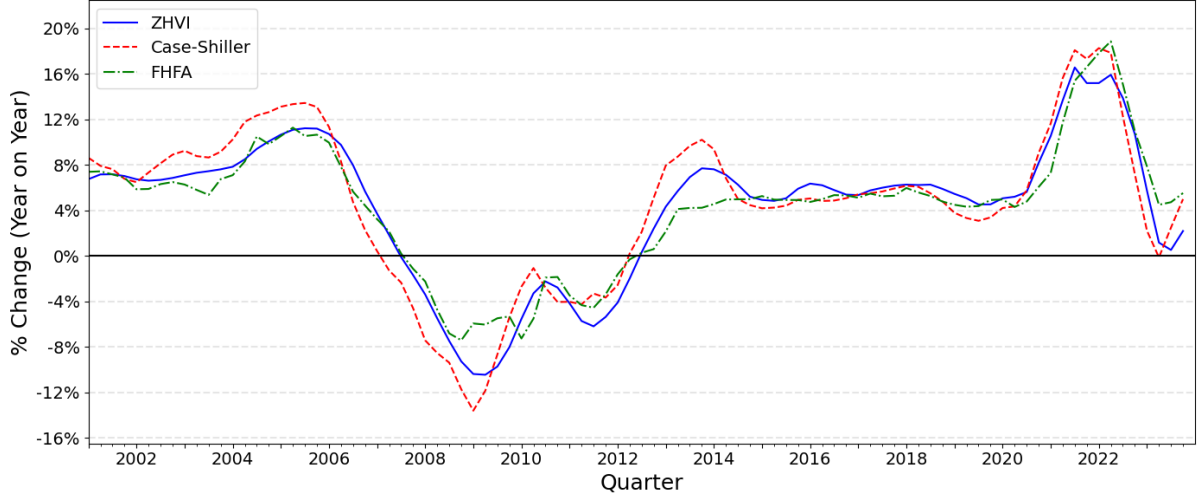
such as hotels, restaurants etc. This should imply an even stronger relationship between retail employment and consumer spending as durable and non-durable goods make-up a larger portion of spending.

To study changes in consumer preferences at the city-level I use credit card expenditure data from Affinity made available by Opportunity Insights [Chetty et al. \(2023\)](#). This is a useful resource for studying how the pandemic may have impacted city-consumption, as similar data covering different consumption categories is not made publicly available elsewhere. For example, BLS Consumer Expenditure Survey produces two-year averages of city-level expenditures for a few relevant categories such as 'food away from home'; in contrast, the Affinity data covers a wide range categories, where the in-person services category includes: accommodation and hotels, restaurant and bars, recreation and entertainment, and more.

Nonetheless, there are drawbacks to using the Affinity data. It derives from credit card spending data, which the providers acknowledge biases the data towards types of consumption typically associated with credit card spending. The data is made available as relative growth rates indexed relative to January 2020, which does not afford researchers much flexibility. The data is reliant on January 2020 levels being representative of the pre-pandemic period. With this in mind, I weigh the usefulness of the breadth of consumer categories available study against concerns of the representativeness of the data, and limit the analysis to a narrow set of questions regarding city-consumption where these issues are ideally not detrimental to the conclusions drawn.¹³

¹³For the same reasons outlined above, accompanied with poor coverage at the county-level, I opted to not use this data when estimating wealth effects. Given the strong relationship between PCE and retail employment and the almost twice as large coverage of counties, I deemed using a proxy to be the preferred option.

Figure 3: Comparison of House Price Index Series



Notes: This figure plots the 4-quarter growth of the Zillow Home Value Index (ZHVI, Blue), the S&P CoreLogic Case-Shiller U.S. National Home Price Index (FRED Series: CSUSHPINSA, Red), and the FHFA All-Transactions House Price Index for the United States (FRED Series: USSTHPI, Green).

III.B. House Prices

House price data is obtained from Zillow, an online real-estate marketplace, who compute the 'Zillow Home Value Index' (ZHVI), a regional home price index made publicly available at multiple geographies including the county, metropolitan, zip and national levels. The ZHVI reflects regional home values in the 35th to 65th percentile and includes all single-family residences and condominiums/co-ops.¹⁴ The index is a seasonally adjusted and smoothed measure of home values for the given geographic area. The key strength of this data is that its made available at the county and zip levels, which is otherwise not made publicly available.

Figure 3 plots the percentage change in home prices compared to the previous year of the ZHVI at the national level, alongside other widely used home prices - the Case-Shiller Home price Index and the FHFA all Transactions House Price Index. The indexes all move closely together, especially the FHFA and Zillow data, the latter of which was used by Guren et al. (2021) at the CBSA level. The co-movement provides assurance that the ZHVI data is accurate and representative of.

III.C. Net Worth and Housing Wealth

Following Mian et al. (2013), I define housing net worth shocks as regional housing wealth relative to total net worth, scaled by the growth in regional home prices. Formally, I denote it as $W_{i,t} = \Delta \ln p_{i,t} \times \frac{H_{i,t}}{H_{i,t} + A_{i,t} - D_{i,t}}$, where $p_{i,t}$ is the house price index in county i at time t , $H_{i,t}$ is the value of the housing stock, $A_{i,t}$ is the value of other financial assets, and $D_{i,t}$ is the value of household debt.

¹⁴The data is publically available at: <https://www.zillow.com/research/data/>. For more information on how the index is computed see: <https://www.zillow.com/research/methodology-neural-zhvi-32128/>

This variable is useful as it captures a household balance sheet perspective on the housing wealth shock. As discussed above, a county’s exposure to the house price shock during the COVID-19 pandemic is driven by geographic location; but the net worth shock will also be driven by the area’s household balance sheet composition. Areas where housing wealth is a larger share of total net worth, e.g., due to higher home-ownership rates, are mechanically more exposed to house price shocks. Meanwhile an area with a higher degree of leverage will, all else equal, be more exposed to house price shocks due to the amplifying effect of leverage.

I define $H_{i,t}$ as the product of the number of housing units and the ZHVI in county i at time t . The number of housing units is obtained from the American Community Survey (ACS) 5-year estimates between 2015-2019. Financial assets $A_{i,t}$ is defined as county i ’s share of the total household financial assets in the flow of funds data.¹⁵ County shares are constructed as the share of taxable non-wage income from the IRS Statement of Income (SOI) data. The value of household debt is similarly defined as the county’s share of the total household debt in the flow of funds data,¹⁶ where the county share is using the county-level household debt data based on the FRBNY Consumer Credit Panel Survey made available by the FED.¹⁷

Whilst being structurally interpretable, the net worth shock measure is susceptible to measurement error, as it relies on the accuracy of the multiple data sources. Instrumental variable strategies addresses measurement error whenever the error-component of the independent variable is orthogonal to the instrument used. To supplement the net worth shock analysis, I also estimate elasticities using house price growth without any scaling, albeit at the cost of losing the balance sheet perspective. Moreover, the net worth shock does not account for human capital, which is a key unobserved component of household wealth. This further motivates the use of instrumental variable strategies, as discussed in [section V](#).

Following [Mian et al. \(2013\)](#) I also define a financial assets shock, which is similarly defined to the housing net worth shock but uses the increase in the stock prices and the ratio of financial assets to net worth in place for the house price shock and housing net worth ratio respectively. I construct this variable for the sake of completeness, but refrain from including it as a control in my final results. First the two variables are highly co-linear with an R2 of roughly 20%. In [figure 4](#) I plot the binscatter relationship between the two variables, which demonstrates a strong negative relationship.

III.D. Control Variables

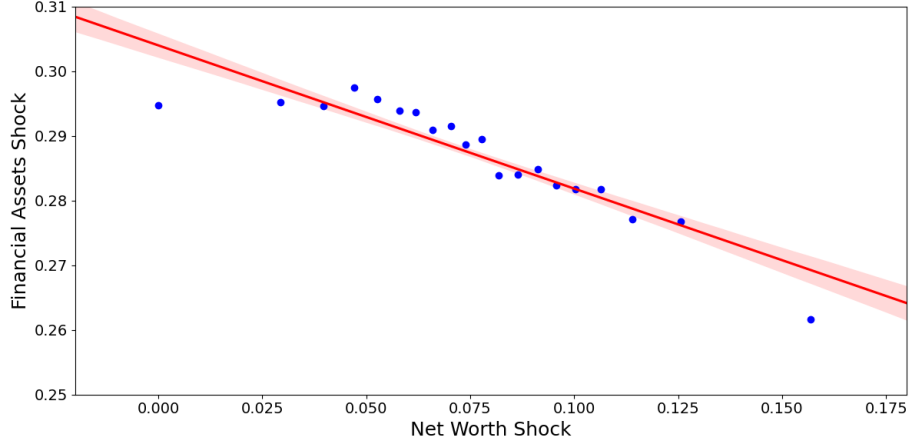
I use a wide range of control variables including county level demographics (median age, median household income, population density, and the share of population that is black) sourced from the ACS. Location tracking data is obtained from Google. Data on COVID-19 policy response is obtained from The Oxford COVID-19 Government Response Tracker (OxCGRT) ([Hale et al. 2021](#)). County-level employment data, used to compute industry shares, is obtained from the Quarterly Census of Employment and Wages (QCEW). Migra-

¹⁵FRED series used is Households; Total Assets: BOGZ1FL192000005Q.

¹⁶FRED series used is Households; Total Liabilities: BOGZ1FL194190005Q.

¹⁷See [here](#).

Figure 4: Housing Net Worth Shock vs. Financial Assets Shock



Notes: This figure plots the binscatter of Net Worth Shock and the Financial asset Shock. The Net Worth Shock is sorted by size and divided into 20 equally sized bins. For each bin the average financial asset shock is computed and plotted as scatters in blue. The regression line of the underlying data is plotted in red. The estimated regression coefficient is -0.905 with a T-statistic of -24.22. The regression has an R-squared of 0.2.

tion data is obtained from the US postal service (USPS), and is computed as the per capita net migration rate.

III.E. Summary Statistics

In [table A.1](#) I present summary statistics of the key variables used in this paper. The data is defined at the county-level and covers 2,344 out of 3,143 US counties. This implies that the data covers almost 97% of the US population. The average net worth shock at 0.077 is comparable in size to that of [Mian et al. \(2013\)](#) who during the great financial crisis find an average shock of -0.063.

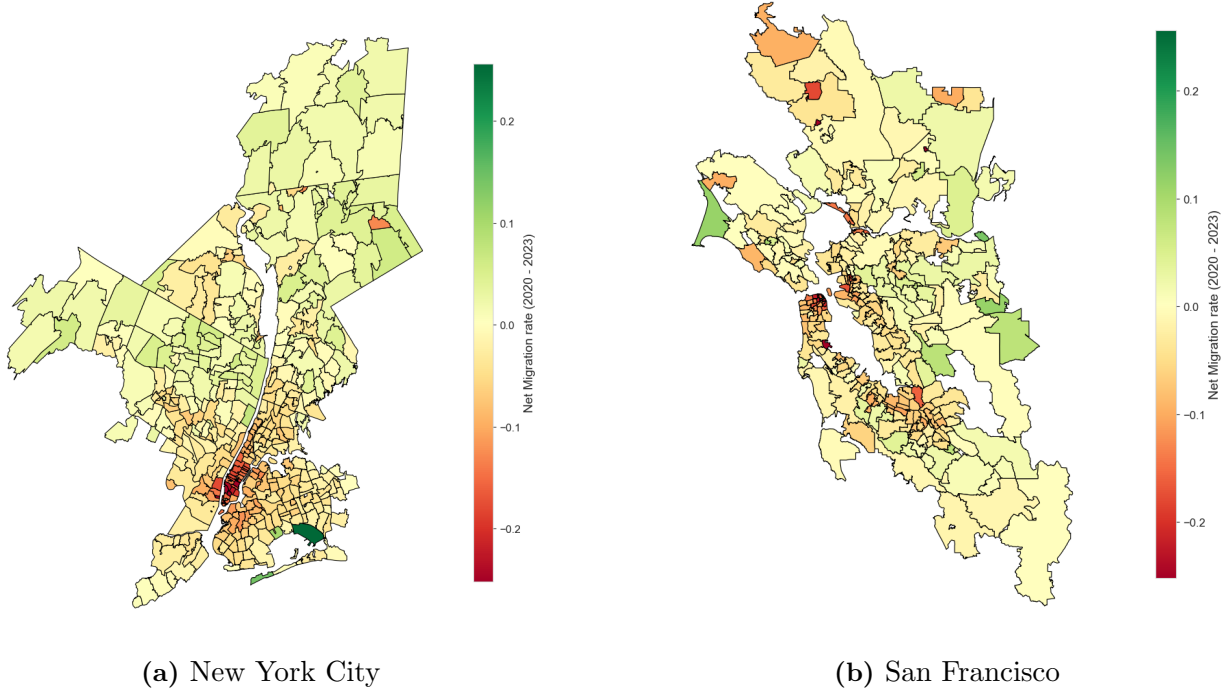
IV. House Price Shock

The COVID-19 pandemic and the subsequent shift to remote work and urban flight have had a profound impact on the US housing market. In this section I provide background and document key facts regarding the re-evaluation of urban real estate and the cross-sectional variation in house price growth it has generated. I argue that this re-evaluation of urban housing is rooted in a preference shock away from urban density. I also present initial evidence of the fact that this preference shift has negatively impacted the consumption of urban amenities.

IV.A. House prices and Re-evaluation of Urban Real Estate

Using New York City and San Francisco with their surrounding counties as illustrative examples, I plot the spatial distribution of net migration rates per 100 residents by each zip code between 2020 and 2023 in [figure 5](#). We observe large negative outflows in the city

Figure 5: Net Migration per Capita (2020 - 2023)



Notes: Cumulative net migration at the ZIP-code level across 2020-2023 relative to 2019 population estimates. Maps consist of respective city centers and their surrounding areas. Migration data is from the USPS.

centers and near zero to positive rates in the surrounding suburbs. This pattern exemplifies the urban flight brought on by the pandemic.

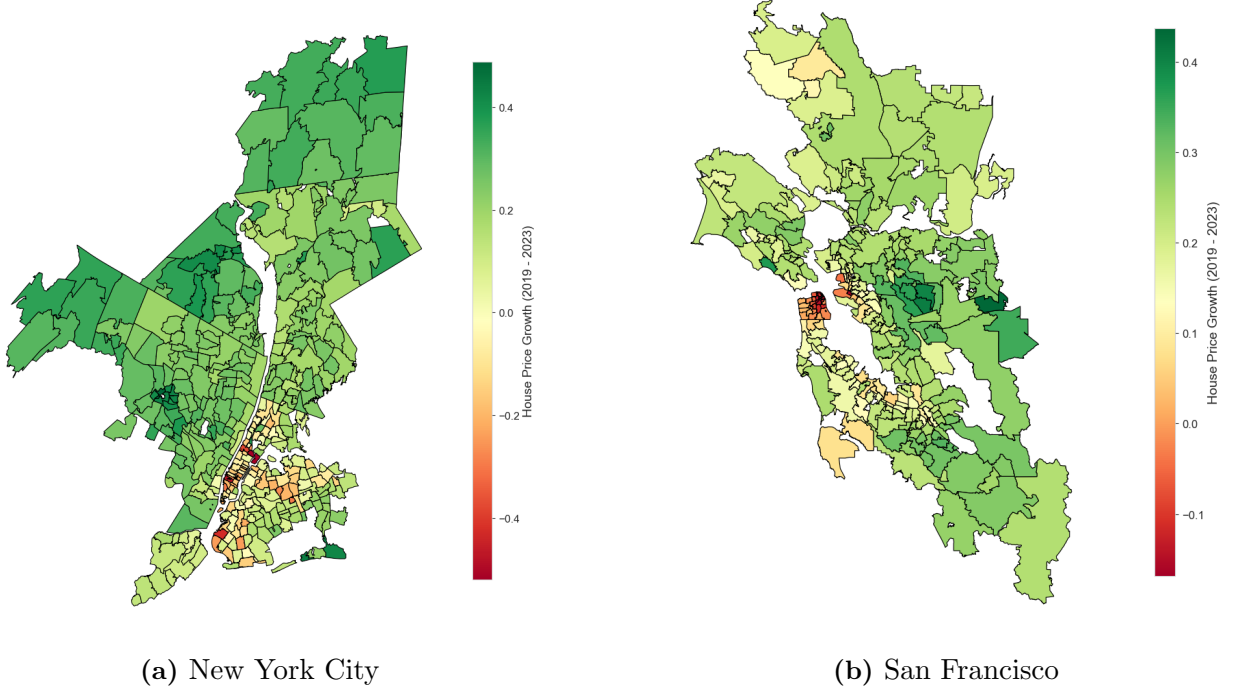
In [figure 6](#) I plot the equivalent distribution maps of house price growth. Exhibiting a similar pattern, growth is higher in the suburbs compared to the city centers, with some zip codes at the urban cores experiencing price declines.

Together these plots visually demonstrate the decline in demand for urban density brought on by the pandemic. People are migrating away from city centers and moving into suburban zip codes. The same zip codes are seeing house prices increases relative to the city centers.

This pattern extends to a wider set of cities, as I compute the net migration and house price growths of the 30 largest Metropolitan Statistical Areas in the US and compare the rates of the counties that represent the urban core and those in the suburbs/periphery; these are tabulated in [table 1](#). Consistent with the narrative of an urban re-evaluation and urban preference shock, I find net negative migration and lower house price growth in city centers compared to the surrounding areas.

Is it possible to quantify the re-evaluation shock? Yes, following [Gupta et al. \(2022\)](#) and [Albouy et al. \(2018\)](#), I estimate the elasticity of house prices with respect to the distance to the city center - this is also known as the price gradient. The natural prior is that house prices decline as one moves away from the city center, implying a negative price gradient. If this gradient flattens over time, it is indicative of a re-evaluation of urban real estate.

Figure 6: House Price Growth (2019 - 2023)



Notes: Percentage change in yearly average house prices between 2019 and 2023 at the ZIP-code level. Maps are constructed using zip code level data and include the respective city centers and their surrounding areas. House price data is the ZHVI from Zillow.

Formally, the price gradient can be obtained as δ_t in the following regression:

$$\ln p_{i,j,t} = \delta_t \mathbb{1}_t \{ \ln(1 + d(z_{i,j}, \bar{z}_j)) \} + \xi_t + \alpha_j + \gamma X_{i,j,t} + \epsilon_{i,j,t} \quad (2)$$

where $p_{i,j,t}$ is the house price in zip code i in city j at time t , $d(z_{i,j}, \bar{z}_j)$ is the distance between the centroid of zip i (z_i) and the city center $\bar{z}_j$, $\mathbb{1}_t$ is a time indicator, ξ_t is a time fixed effect, α_j is a county fixed-effect, $X_{i,j,t}$ is a vector of control variables, and $\epsilon_{i,j,t}$ is the error term.

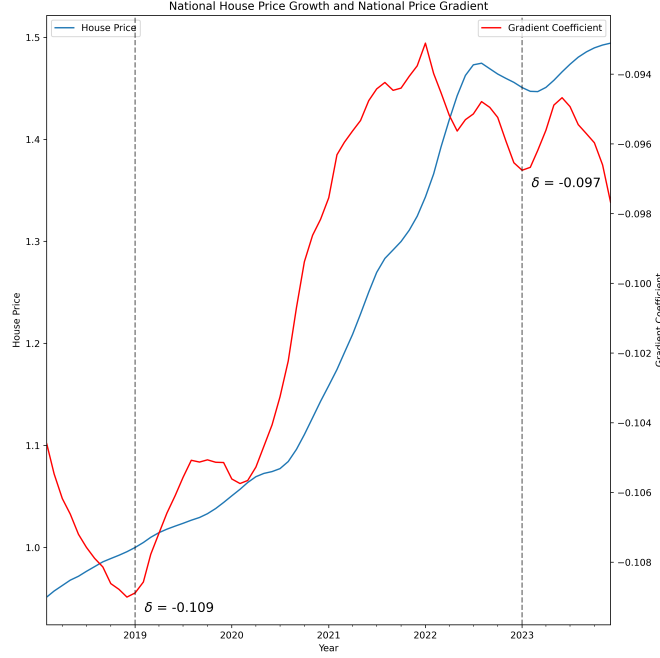
The interpretation on δ_t is that it captures the estimated percentage change in house prices as one moves one percent further from the city center, with the time dummy implementation allowing for the price gradient to vary over time. Following [Albouy et al. \(2018\)](#),

Table 1: Comparing Urban Core and Periphery

	Net Migration	House Price Growth	Consumption Growth
Periphery	1.67%	42.99%	3.03%
Urban Core	-4.02%	35.99%	-6.41%

Notes: Net migration (2019-2023), House price growth is computed as the county-level growth between 2019 and 2023, and consumption growth is the growth in the consumption proxy growth between (2019 – 2023). Sample consists of the 30 largest Metropolitan Statistical Areas (MSAs), urban core is defined as the county corresponding to the principal city of the MSA, periphery are defined as the remaining counties in the MSA.

Figure 7: House Prices vs. Price Gradient (National)



Notes: National house prices (ZHVI) (blue, left axis) normalized to one at January 2019 and the price gradient, estimated as δ_t from [equation \(2\)](#) (red, right axis). Both series area at the monthly level.

I use the location of the city hall to denote the center of the city.¹⁸

In [figure 7](#) I plot the estimated price gradient for the US between 2018 and 2023 alongside the national house prices (normalized to one at January 2019) for the same period. We observe a flattening of the price gradient, going from -0.109 in January 2019 to -0.097 in January 2023, implying a flattening of 0.012 over the period.

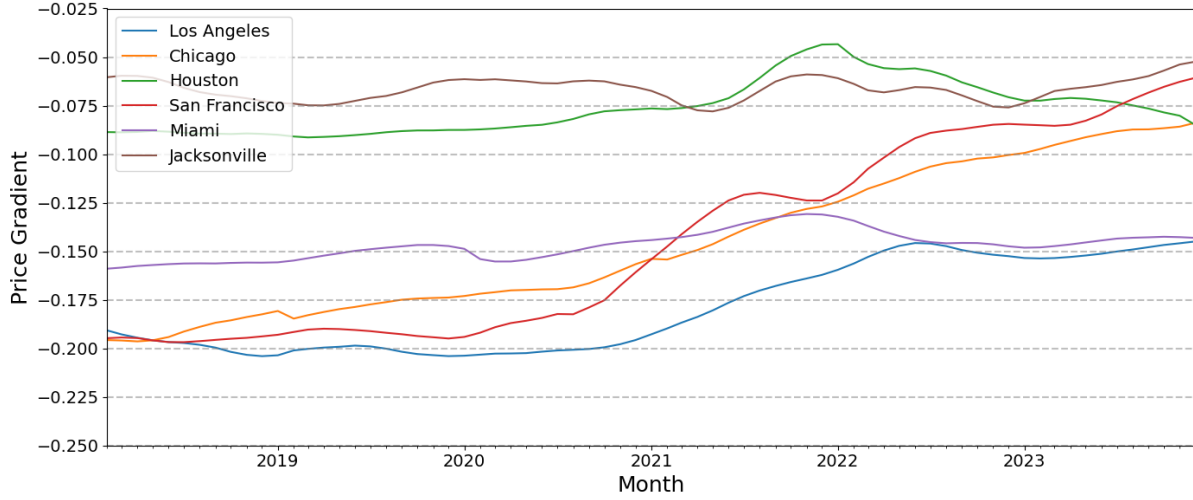
These findings are consistent with those of [Gupta et al. \(2022\)](#) who estimate the price gradient between 2018 and January 2021, and find a flattening similar magnitude (0.012). The shock is seemingly persistent, although some reversal is observed in 2022 and 2023. This suggests that there has been a shift to preferences for urban density driven by the pandemic.

The re-evaluation shock is not uniform across cities, with some cities experiencing a more pronounced flattening than others. In [figure 8](#) I plot the price gradient estimates at the city-level for three cities in Republican states (Houston, TX; Jacksonville and Miami, FL) and three cities in Democratic states (Los Angeles and San Francisco, CA; Chicago, IL). We find that in the former case the price gradient is largely unchanged over time, whilst in the latter we observe significant increases. This illustrates how the price gradient has moved with the strength of COVID-19 policy responses – under the assumption that Democratic states responded more strongly than Republican.

In [figure 9](#) I plot the binscatter relationships between the change in the price gradient between 2019 and 2023 for 49 US cities against both the COVID-19 Policy Stringency Index from OxCGRT ([Hale et al. 2021](#)) and the share of local jobs that can be done remotely

¹⁸The location and co-ordinate data for the city and town halls was obtained via ArcGIS and ESRI. The data draws upon the US Geological Survey. See [here](#) for further information (Accessed Feb 2024).

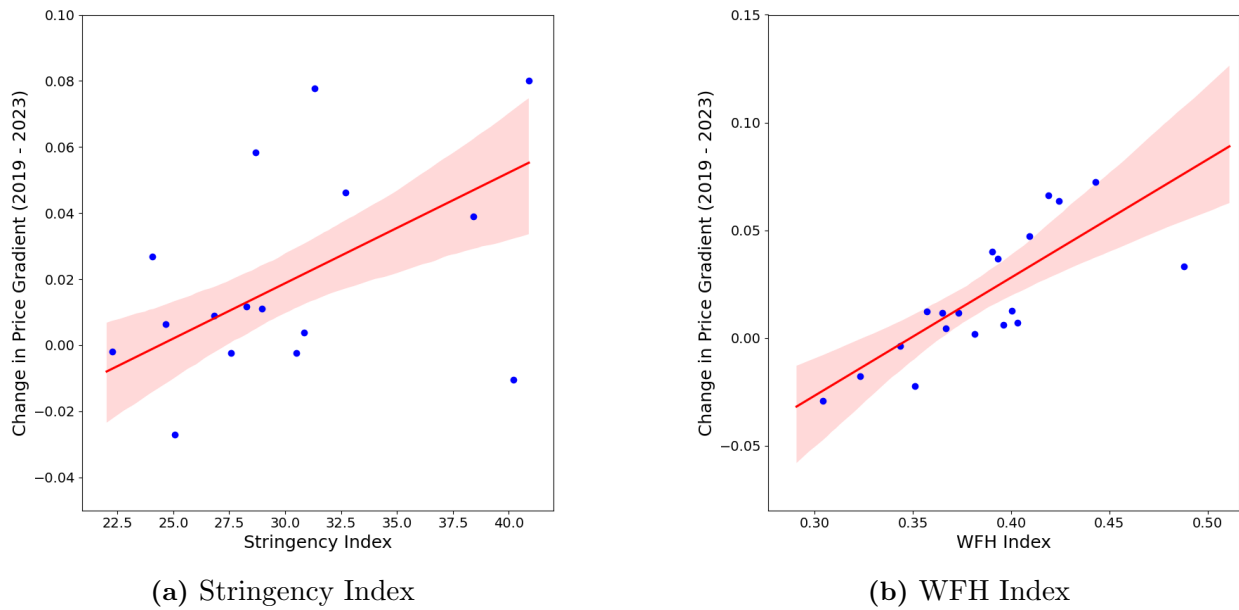
Figure 8: Price Gradient Estimates for Select Cities



captured by the [Dingel and Neiman \(2020\)](#) WFH Index.

In both cases we find strong positive relationships. In cities where stronger policy responses were implemented and the ability to transition to remote work was greater (e.g., more tech jobs), suburban housing rose faster relative to urban housing. This suggests that cities that saw stringent public health policies (e.g., lockdowns) and were able to transition to remote work saw a greater shift in preferences. The preference shock that drove lower demand for urban density was driven by the impact of COVID-19 on day-to-day life.

Figure 9: Binscatter of the change in the Price Gradient (2019 - 2023)



Notes: Percentage change in yearly average house prices between 2019 and 2023 at the ZIP-code level. Maps are constructed using zip code level data and include the respective city centers and their surrounding areas. House price data is the ZHVI from Zillow.

Table 2: Consumer Spending Growth per Category

Consumer Category	Consumption Growth
All	14.36%
Accommodation and Food Services	7.92%
Non-durables	17.56%
Durables	8.71%
Grocery and Food	18.09%
In-Person Services	8.76%
Remote Services	26.41%

Notes: This table presents the average change in Consumer Credit-Card Spending of different consumer categories in 2023 relative to January 2020 levels. The sample consists of the 47 US cities with available data. Data: Opportunity Insights (Chetty et al. 2023).

In the following section I ask, do we observe this shift in preferences in consumer behaviour? Has the shift in demand for urban density also brought a shift in the demand for urban amenities?

IV.B. Urban re-evaluation and Consumer preferences

Chetty et al. (2023) uses credit card spending data and finds exploratory evidence of a composition shift away from in-person services and urban amenities. Has this shift been persistent, and does it coincide with the preference shock on urban housing?

In table 2 I present the average consumer spending growth in 2023 relative to January 2020 levels by consumer categories for a sample of 47 cities. Remote services have experienced the largest growth, followed by grocery and food, and non-durables, whilst categories related to urban amenities - accommodation and food services and in-person services - have experienced much lower growth. This indicates a shift in consumption preferences towards remote services and away from urban amenities.

Since the data is defined based on the consumption of residents in the city, this phenomenon cannot be explained as a mechanical result of the pandemic-induced decline in tourism or commuters.

Out-migration is an unlikely explanation for the lack of recovery in urban amenities, as those who left the city are likely the people for the pandemic had the largest impact on their preferences, hence leading to them migrating away from the city. Those who remained in the city are likely to be those who value urban amenities the most and were less impacted by the shock. This should mechanically tilt the composition of consumption towards urban amenities rather than away from it, making the finding that these categories recovered slower even more striking.

To test whether the urban re-evaluation shock coincides with a shift in consumption categories I estimate the following regression:

$$\frac{C_{j,t} - C_{j,0}}{C_{j,0}} = \beta_0 + \beta_1 \Delta \delta_{j,t} + \gamma X_{j,t} + \epsilon_{j,t}. \quad (3)$$

Where $C_{j,t}$ is the consumption of area j at time t , $\Delta \delta_{j,t}$ is the change in the price

gradient in area j estimated from equation (2), and $X_{j,t}$ is a vector of controls. The intuition of estimating (3) is that, if $\beta_1 < 0$, lower demand for urban density - as captured by the re-evaluation shock ($\Delta\delta_t > 0$) - likely coincides with a lower demand for urban amenities.

I estimate equation (3) across the different consumption categories and present the results in table 3, where column (1) shows the results for total consumption, column (2) for food and accommodation, column (3) for in-person services, column (4) for groceries, and column (5) for remote services.¹⁹ All coefficients are negative, implying that lower consumption is observed in areas that experienced a larger re-evaluation of urban housing.²⁰

Table 3: Preference Results: Consumer Spending Growth

	(1)	(2)	(3)	(4)	(5)
$\Delta\delta_t$	-0.621	-1.296	-0.979	-0.406	-0.771
Standard Error	(0.322)	(0.300)	(0.328)	(0.337)	(0.484)
Observations	49	49	49	49	49
Category	Total	Food & Acc.	In-person	Grocery	Remote

Notes: This table presents the results from regression Consumer Spending Growth on the change in the City-level Price Gradient $\Delta\delta_{j,t}$ (2019-2023). Consumer spending growth is compute as the average level in 2023 relative to January 2020 levels. Data: Opportunity Insights, Zillow, Author's Calculations

However, out of the distinct categories, we only find statistically significant evidence for the categories related to urban amenities being affected by the re-evaluation of urban housing. Cities that experienced a larger fall in demand for urban density also experienced a lower consumption of urban amenities, whilst the consumption of other categories were not affected. As the re-evaluation shock captures the impact of the pandemic on demand for urban housing, this finding suggests a connection between the impact of the pandemic on the demand for urban density and the shift in consumption preferences. Whilst not directly causal, the results suggest that the pandemic has impacted how consumers value urban amenities.

In table 4 I estimate (3) using a notion of 'relative' consumption growth for each category. For consumption category k I define the dependent variable as $\frac{C_{j,t}^k - C_{j,0}^k}{C_{j,0}^k} - \frac{C_{j,t} - C_{j,0}}{C_{j,0}} = \Delta C_t^k$. The aim is to capture the change in consumption of category k whilst controlling for the general change in consumption.

Columns (1) and (2) show the results for food and accommodation and in-person services, and both are statistically significant at the 5% level and 10% level, respectively. Columns (3) and (4) show the results for groceries and remote services, and neither are statistically significant, with the former even showing a positive coefficient. This further supports the notion that the pandemic has caused a shift in consumption preferences away from urban

¹⁹In-person services includes: accomodation and food services; arts, entertainment, and recreation; PLS personal and laundry and more. Remote Services include finance and insurance, information, public administration, utilities, construction, and more. For full definitions, see Chetty et al. (2023).

²⁰Put differently, lower consumption in a given category is observed in cities where suburban house growth was higher than at the city core.

Table 4: Preference Results: Relative Spending Growth

	(1)	(2)	(3)	(4)
$\Delta\delta_t$	-0.675	-0.358	0.215	-0.150
Standard Error	(0.296)	(0.190)	(0.362)	(0.291)
Observations	49	49	49	49
Category	Food & Acc.	In-person	Grocery	Remote

Notes: This table presents the results from regressing relative Consumer Spending Growth ΔC_t^k on the change in the City-level Price Gradient $\Delta\delta_{j,t}$ (2019-2023). Consumer spending growth is compute as the average level in 2023 relative to January 2020 levels. Data: Opportunity Insights, Zillow, Author's Calculations

amenities. Relative to the general change in consumption, we find that the consumption of urban amenities has declined, whilst finding no evidence of such a shift for groceries or remote services.

If these shocks to consumption preferences coincide with consumption growth and house price growth, they may impact the estimation of consumption elasticities. This is discussed further in the following section.

V. Methodology

V.A. Empirical Specification

As argued in [section II.](#), the relationship between consumption and housing wealth can be expressed as a linear regression in growth rates as in (1). I estimate this relationship in both a panel setting and cross-sectional setting.

The cross-sectional follows the specification of [Mian et al. \(2013\)](#) is expressed as:

$$\frac{C_{i,t} - C_{i,0}}{C_{i,0}} = \beta_0 + \beta_1 W_{i,t} + \gamma X_i + \epsilon_i. \quad (4)$$

Where $C_{i,t}$ is the consumption level in county i at time t , $W_{i,t} = \Delta \ln p_{i,t} \times \frac{H_{i,t}}{H_{i,t} + A_{i,t} - D_{i,t}}$ is the housing net worth shock in county i at time t , and X_i is a vector of control variables. The coefficient of interest in (4) is β_1 , which captures the elasticity to consume out of housing wealth.

Complimentary to the cross-sectional approach, I also estimate a panel regression, which may address some of the endogeneity concerns related to estimating (4) due to the inclusion of fixed effects (discussed below). The specification is similar to [Guren et al. \(2021\)](#), and can be expressed as:

$$\Delta \ln C_{i,t} = \phi_0 + \phi_1 \Delta \ln p_{i,t} + \gamma X_{i,t} + \xi_c + \lambda_{s,t} + \epsilon_{i,t}. \quad (5)$$

Where $\Delta \ln C_{i,t}$ is the log difference in consumption in county i at time t , $\Delta \ln p_{i,t}$ is the log difference in house prices in county i at time t , and $X_{i,t}$ is a vector of time-varying

control variables, ξ_c are county fixed effects, and $\alpha_{s,t}$ are state-time fixed effects. Again, the coefficient of interest in (5) is ϕ_1 , however in this setting it captures the elasticity of consumption with respect to house prices rather than with respect to housing wealth. Due to the county-level data used to construct the wealth measure only being available at the annual level, I opt to use the house price shock directly as the consumption and house price data are defined at the quarterly level. Being a panel setting with county-level fixed effects, time-invariant county-specific controls are not valid, I therefore vary the set of controls between the cross-sectional and panel setting, and discuss this below.

V.B. Endogeneity concerns and motivation for controls

Estimating (4) and (5) may yield biased estimates for the coefficients of interest due to the likely endogenous relationship between housing wealth and consumption. House prices are equilibrium objects, any structural shocks to regional real estate markets is likely correlated with regional economic activity and consumption. Below I discuss key endogeneity concerns.

V.B.1. Productivity shocks. A key concern is that shocks to housing wealth coincide with unobserved shocks to permanent income and productivity.²¹ For example, regional variation in productivity (both current and expected future changes) may impact expected lifetime income (human capital), simultaneously drives consumption. Whether limited to the pandemic episode, or reflective of long-term differences in productivity growth across regions, these are problematic for the identification of β_1 . Moreover, the resulting higher economic activity and consumption, due to the higher productivity, may also give rise to reversely caused changes to house prices, as those who did not directly benefit from the productivity shock may still see their housing demand rise following the increased economic activity. These concerns motivate both a range of controls, and the use instrumental variable strategies.

Demographic controls (median income, median age, population density) and industry-share controls constructed at the 2-digit NAICS code level (construction, tradeable, non-tradeable, professional services, and others) are included (in-part) to control for differences productivity and human capital. Industry-shares also control for labour market exposure to the pandemic (discussed below). In the panel setting, I am able to control for long-term productivity trends via the inclusion of county fixed effects as in Guren et al. (2021) and Aladangady (2017).

V.B.2. Migration. As discussed above, the COVID-19 pandemic brought on large migration flows away from urban city centres, which would, if not controlled for, positively bias β_1 estimates. This would largely be mechanical, as a larger outflow of households implies lower consumption, but is also likely to be correlated with lower housing demand. Out-migration could also have knock-on effects on non-tradeable employment and income as economic activity recedes. I control for this by including the net migration rate in the set of controls.

²¹As argued above, discussions regarding this endogenous relationship can be traced back to discussions between Muellbauer and Murphy (1990) and King (1990) regarding the consumption and housing booms in the UK during the 1980's. The latter emphasized the endogenous relationship between house prices and consumption through permanent income.

V.B.3. Pandemic related. The COVID-19 pandemic introduces novel endogeneity concerns which may stem from both a differential sensitivity to the pandemic, but also due to cross-sectional variation in the severity of the pandemic and local policy responses. I address the first point via the inclusion industry share controls. By controlling for a county’s exposure to e.g., in-person services and the professional sector (which may have been more amenable to remote work), I control for a county’s exposure to the pandemic via its labour market structure. The pandemic may have brought about adoption of productive practices within industries such as more efficient teleworking, which these controls also capture.²²

Policy responses to the pandemic are likely to have driven both changes to consumption and housing demand. For example, severe lockdowns may have caused higher demand for housing but also impacted how households consume (as illustrated in [section IV.](#)). I control for the stringency of COVID-19 policy at the state-level using the OxCGRT policy tracker ([Hale et al. 2021](#)), as much of COVID-19 policy at the US level was set at the state-level²³.

V.B.4. Preferences. As argued in [section IV.](#), the house price shock likely coincides with a shock to preferences. If households do not substitute perfectly between goods, or substitute consumption for e.g., leisure, it may impact regional consumption growth. I control for this by (i.) including Google Mobility tracking data which measures the change in time spent in retail and restaurants during 2023 compared to January 2020 – this proxies for sustained shifts in preferences during the post-lockdown period, and (ii.) proxying for a region’s access to urban amenities by controlling for the log number of restaurants in the region in 2019, using data from the QCEW.

V.B.5. Spillover effects . As argued by [Chodorow-Reich \(2020\)](#), a key challenge when using regional data to study macroeconomic phenomena, is the presence of spillover effects. That is, the event of a shock to one region having a direct impact on the economic activity of another region. This is equivalent to the SUTVA assumption in applied microeconomics. These concerns are mitigated by the fact that consumer spending is measured based on the location of the consumer’s residence rather than the location of the business [Chetty et al. \(2023\)](#). This implies that even if a Connecticut based consumer who benefits from a wealth shock to their Connecticut home, chooses to spend their money in New York, the spending is still attributed to their Connecticut county. Housing wealth shocks may give rise to spillover effects via mobility, as households move across borders; as argued by [Mondragon and Wieland \(2022\)](#) and [Graham and Makridis \(2023\)](#) households move infrequently, and even so, this would be picked up by the migration controls.

²²The identifying assumption here is that firms within the same industry are similarly exposed to the pandemic, and are similarly equipped to adopt new practices in light of the pandemic shock. Any within-industry variation in a given region is assumed to be randomly distributed and not correlated with a latent variable which simultaneously drives house prices and consumption. Alternatively, I can use the WFH index by [Dingel and Neiman \(2020\)](#) to control for a county’s employees ability to work remotely. In unreported exercises I find that results are robust to controlling for the index in place of the industry-shares.

²³[Neelon et al. \(2021\)](#) study how partisanship policy at the Governor-level impacted variation in the spread and death rates of COVID-19.

V.C. Identification Strategy: Instrumental Variable Estimation

Motivated by the endogeneity concerns and potential measurement error, I employ instrumental variable strategies to estimate the relationship between housing wealth and consumption.

I estimate both (4) and (5) with 2SLS. I state the first and second stages in equations (6) and (7) respectively, where Z is the instrumental variable.²⁴ We require that the instrument is relevant, i.e., $cov(Z, W) \neq 0$, and that it is valid, i.e., that (8) is satisfied. Intuitively, the latter condition requires that the instrument is only correlated with consumption through the housing wealth shock, and not through any other channel.²⁵

$$\Delta W = \alpha_0 + \alpha_1 Z + \gamma X + \nu, \quad (6)$$

$$\Delta C = \beta_0 + \beta_1 \Delta \hat{W} + \gamma X + \epsilon, \quad (7)$$

$$0 = E[Z\epsilon|X]. \quad (8)$$

Finding valid instruments for the housing wealth shock is challenging, as house prices are equilibrium objects. Whilst the pandemic episode is a unique and arguably an exogenous event, I have above outlined potential endogeneity concerns associated with estimating (4). I address this by employing a range of research designs, each with their own set of instruments. Below I discuss the research designs and the instruments employed in each.

V.D. Research design 1: Housing Supply Elasticity

The first research design instruments housing wealth shocks using estimates of regional housing supply elasticity from [Saiz \(2010\)](#). This approach was pioneered by papers studying the relationship between housing and real economic activity (including housing wealth effects) during the great recession (see e.g., [Mian et al. 2013](#)). I implement this design in a novel setting as a test of the external validity of the use of the instrument.

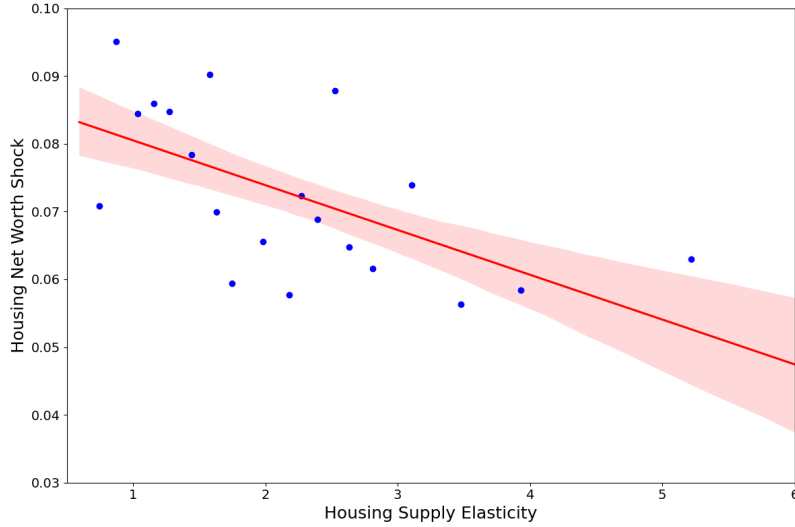
The housing supply elasticities measure the responsiveness of regional housing supply to changes in house prices, and are estimated from physical and regulatory constraints on housing supply. It draws on the idea that regions with a flatter topography, fewer bodies of water, and more habitable land are better able to expand housing supply in response to demand shocks; as such, they are attributed a higher elasticity. Large metropolitan areas, with more stringent land use regulations, more hilly terrain, and potentially located on the coast, are attributed a lower elasticity.

The instrument's relevance follows from the fact that regions with a higher housing supply elasticity are likely to have experienced a smaller house price shock in response to the pandemic; due to the fact that supply would have expanded faster in response to the demand shock. In [figure 10](#), I plot the relationship between the housing supply elasticity and the net worth shock during the pandemic, and show that the two are negatively correlated.

²⁴I omit subscripts from this section as the 2SLS approach is the same for both the cross-sectional and panel settings.

²⁵Or, equivalently, conditional on the observables, the instrument is orthogonal to the unobservables ϵ .

Figure 10: Relevance of the Saiz Instrument



Notes: This figure plots the binscatter relationship between the Saiz instrument and the Housing Net Worth Shock. The Saiz Instrument is sorted by size and split into twenty equally sized bins, the mean Net Worth Shock for each bin is plotted as blue scatter points. The best line of fit based on the underlying data is plotted in red.

V.D.1. Threats to identification The main threat to the validity of the instrument is that the housing supply elasticity may be correlated with long-term productivity trends and urban amenities. This criticism was raised by [Davidoff \(2016\)](#), who argue that cities with low housing supply elasticity tend to correspond with cities with high long-term productivity growth; for example, the San Francisco Bay Area and New York City both attract high-skilled workers yet are coastal cities with low elasticity.

The panel setting addresses these concerns directly, as the inclusion of county fixed effects controls for long-term productivity trends. As the elasticity is a time-invariant characteristic of a county, I adopt the approach of [Aladangady \(2017\)](#) and interact the elasticity with the ten-year real treasury rate, and use the interaction term as an instrument. This generates time variation in the instrument, and hence allows for the identification of the effect of the housing wealth shock on consumption. [Aladangady \(2017\)](#) provides indications of the instruments validity as it is uncorrelated with a range of variables that could proxy for productivity, such as wage growth, population growth, and employment growth.

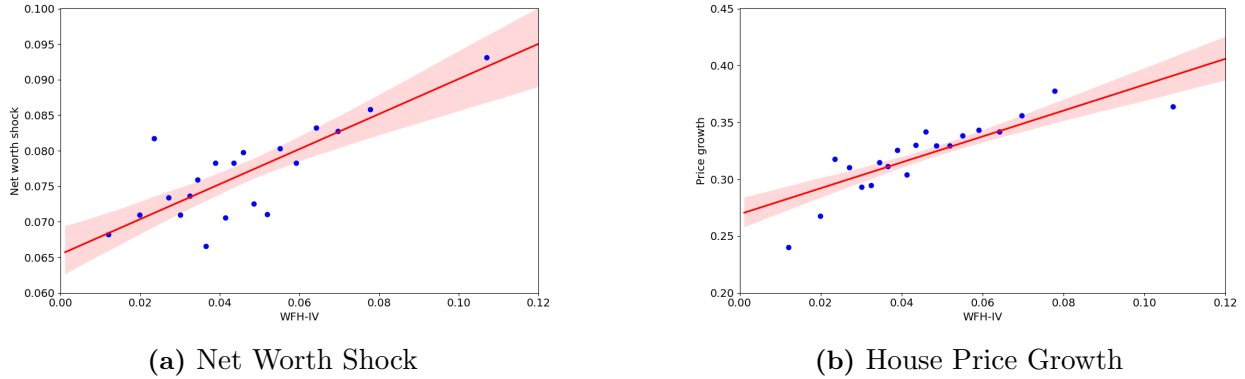
V.E. Research design 2: Pre-Pandemic Remote Work Share

My second and primary research design exploits the unique nature of the house price shock and instruments housing net worth shocks using the pre-pandemic (2015–2019) share of remote workers (henceforth WFH-IV).²⁶ As the share of remote workers is a time-invariant characteristic of a county, it is used in the cross-sectional setting. The identifying assumption is that, conditional on observables, the pre-pandemic share of remote workers in a county is orthogonal to any latent variables that correlate with consumption.

²⁶[Mondragon and Wieland \(2022\)](#) employ this instrument when studying the relationship between WFH during the pandemic and house prices.

Relevance follows from the fact that, the features which made a county more amenable to remote work prior to the pandemic, are likely to have made it more amenable to remote work during the pandemic. In [figure 11](#) I partition the WFH-IV into 20 bins sorted by size, compute the average price growth and housing net worth shock for each bin respectively, and plot these as scatter points. In red I plot the regression line of the underlying data accompanied by a shaded confidence interval range at the 95% level.

Figure 11: WFH-IV Relevance Binscatter Plots



Notes: This figure plots the binscatter relationship between the WFH-IV and the Net Worth Shock and House Price Growth respectively. The horizontal axis is sorted by size and divided into 15 equally sized bins. For each bin the mean value of the variable on the vertical axis is computed and plotted as blue scatter points. The red line is the regression line obtained from regressing the variable on the vertical axis on the horizontal axis (prior to binning the data)

Both plots exhibit a strong relationship between the two variables, indicating a strong relevance. Columns (1) and (2) in [table 5](#) report the regression estimates from a regression of the net worth shock and house price growth between 2019 and 2023 on the WFH-IV. Columns (1) and (3) report coefficients without controls and (2) and (4) include controls, where the controls are mirror those used in section (results). The coefficients are positive and strongly significant indicating that the instrument is strongly relevant. I also report the associated first stage excluded instruments F-statistics which strongly exceed the [Stock and Yogo \(2002\)](#) critical values at the ten percent level. Altogether these result suggest that the instrument is strongly relevant

V.E.1. Threats to Identification A higher share of remote workers could reflect an underlying corollary to consumption growth, such as productivity. For example, high-productivity areas with a degree of skilled workers such as software engineers may tend to work remotely at higher rates than other manual labour intensive regions. A way to check for this is to regress the WFH-IV on pre-pandemic consumption growth (retail employment growth) to discern whether the relationship simply reflects long-term regional heterogeneity in productivity.

In [figure 12](#) I plot the regression coefficients and their respective 95% confidence intervals from a regression of retail employment growth compared to the previous year on the WFH-IV. Besides 2019, all years prior to the pandemic are statistically insignificant with many estimates being very close to zero. The estimate for 2020 is strongly negative, as all counties

Table 5: Test of WFH-IV Relevance

<i>Dependent Variable:</i>	(1) Net Worth Shock	(2) Net Worth Shock	(3) Price Growth	(4) Price Growth
WFH-IV	0.247 (0.0324)	0.413 (0.0394)	1.138 (0.101)	1.093 (0.118)
Cragg-Donald Wald F-stat	126.92	85.284	57.99	108.92
Stock-Yogo C.V. (10%)	16.38	16.38	16.38	16.38
Controls	No	Yes	No	Yes
Observations	2,344	2,344	2,344	2,344

Notes: This table presents the regression coefficient estimates obtained from regressing the Net Worth Shock (columns 1-2) and House Price growth (columns 3-4) on the WFH-IV at the county-level. The standard errors are presented in parentheses and are clustered at the state level. The table also reports the Cragg-Donald Wald F-stat of the first-stage relevance of the excluded instrument, with the associated [Stock and Yogo \(2002\)](#) critical values at the 10% level. Columns (2) and (4) incorporate the same controls as in the main specification in [table 11](#), these include: per capita net migration, COVID-19 Policy Stringency Index, GPS Location Data, Industry Shares, log of median household income, log of net worth per capita, and log of population density.

experienced a negative shock this year.

Whilst this exercise does not prove the validity of the instrument, it does provide some indication that it is most likely not correlated with any underlying long-term trends in consumption and regional variation in growth; as was the concern with the Saiz instrument. What this result indicates, is that any potential confounding element is related to, or a consequence of, the pandemic shock. As argued above, I employ extensive controls to address these potentialities.

As argued above, the change in house prices is rooted in a preference shock caused by the pandemic episode, which also impacted the composition of consumption. Should they coincide, the preference shock would pose a threat to the validity of the WFH-IV.

In [table 6](#) I present the outcome from regressing the WFH-IV on the log of population density. The coefficient is strongly statistically insignificant, suggesting that there is no relationship between the WFH-IV and urban density.

Table 6: Regression of WFH-IV on log Population Density

Log Population Density	-0.000652 (0.000836)
Observations	2,344

Notes: The standard error in parenthesis are clustered at the state level.

Why would the preference shock coincide with the excluded instrument? For the preference shock to coincide with the WFH-IV we must have that these areas either coincide with the size distribution of preference shocks, or, that their consumption is more sensitive or exposed to preference shocks. Variation in the size of the preference shock may for example be

Figure 12: Pre-trend test of WFH-IV



Notes: This figure presents the coefficient estimates of regressing year to year growth in retail employment on the WFH-IV for each year respectively. The coefficient estimate is plotted in blue and the red error bars are the 95% confidence intervals. All regressions control for median household income, which is included as a control in the results section.

driven by differences in disruptions to everyday life brought-on by the pandemic ²⁷ ; whilst variation in exposure may be due to regional consumption being more (or less) reliant on urban amenities.²⁸

As preferences shift away from urban amenities, a natural prior is that exposure to the preference shock is higher in more urban areas. Presumably consumption and economic activity in these areas is more dependent on urban amenities. However, as demonstrated in [table 6](#), the WFH-IV does not correlate with urban density. To further ease any concerns of endogeneity I control for the number of restaurants per capita. This proxies for the exposure an area has to a preference shift away from urban amenities.

In [section IV](#), I show that the re-evaluation of urban housing varies widely across cities (see e.g., [figure 8](#)). Implying that within the cross-section of population dense areas we see great variation in the preference shock; making it unlikely that the size distribution of preference shocks coincides with urban density. Instead the size of the preference shock is likely a function of the pandemic's impact on the local community, as argued in [section IV](#).. To mitigate concerns that areas with a higher remote worker share were less impacted by the pandemic (and hence saw a smaller preference shock) I introduce a range of controls. Ideally, we would want to observe the regional composition shifts in consumption baskets. In lieu of this data I use Google location tracking data to control for the change in the time spent

²⁷A larger shock is interpreted as a larger shift away from city consumption which negatively impacts consumption. For example, denoting the preference shock as $\Delta\sigma < 0$, the prior under the OVB case outlined above is that $\text{cov}(\Delta\sigma, c) > 0$ and $\text{cov}(\Delta\sigma, \text{WFH-IV}) > 0$; the stronger the shock (the more negative the shock) the larger the decline in consumption, and, the larger the shock, the fewer remote workers.

²⁸Note that, no cross-sectional variation in the preference shock or consumption exposure to it is necessary to generate the house price shock. Implying that relevance does not necessitate the exclusion restriction is broken. A perfectly uniformly distributed housing preference shock would still generate cross-sectional variation in house prices.

Table 7: Regression of Change in Price Gradient on Various Controls

Log Restaurants	0.0106 (0.00571)
Mobility Tracker	-0.262 (0.0855)
Stringency Index	0.000534 (0.000886)
Observations	49
R-squared	0.476

Notes: This table presents the results from regressing the change in the price gradient ($\Delta\delta_t$) on various controls. Standard errors are reported in parentheses.

at in-person services such as retail and restaurants compared to the pre-pandemic level. If a county has seen a decline in time spent at restaurants, holding migration constant, it is interpreted as a preference shift away from consuming these services. I also use the OxCGRT policy stringency index to control for the state-level COVID-19 policy response to capture the impact of lockdown policy on consumption preferences.

In [table 7](#) I present the results from regressing the price gradient estimates from [\(3\)](#) on the above mentioned controls. The regression has an R-squared of 0.476, which suggests that – to the extent that the change in the price gradient is a suitable proxy for the change in consumption preferences – the set of variables explain at least half of its variation. Therefore, making them suitable proxies for the preference shock.

V.F. Research design 3: Sensitivity-design instruments

This design replicates [Guren et al. \(2021\)](#), who use the estimated price-sensitivity of house prices to regional house price changes as an instrument.²⁹ Motivated by the observed persistent differential sensitivity to regional house prices, this sensitivity is estimated and used as an instrument for house price growth in [\(5\)](#). The authors draw an analogy to a difference-in-difference design, whereby the regional house price shock is a differentially distributed treatment.

The instrument is obtained as $\hat{\gamma}_i\Delta P_{r,t}$ from estimating [\(9\)](#) where φ_i is a county fixed-effect, $\delta_i\Delta C_{i,r,t}$ is the consumption growth of county i in region r with a county-specific coefficient, $\mu_i\Delta C_{r,t}$ is the average consumption growth of region r , $\gamma_i\Delta P_{r,t}$ is the regional average house price growth³⁰, and $X_{i,r,t}$ is a vector of controls.

$$\Delta p_{i,r,t} = \varphi_i + \delta_i\Delta C_{i,r,t} + \mu_i\Delta C_{r,t} + \gamma_i\Delta P_{r,t} + \Psi X_{i,r,t} + v_{i,r,t} \quad (9)$$

Note that [\(9\)](#) is not the first stage equation, but rather the equation we estimate to obtain

²⁹Regions are the Census Regions and Divisions of the United States as defined by the Census Bureau. The US is divided into four regions (Northeast, Midwest, South, and West). For further information, see [here](#).

³⁰To prevent over-fitting, the regional average is computed once for each county, leaving out the respective county.

the instrument, $\hat{\gamma}_i \Delta P_{r,t}$. This makes the instrument county i 's estimated sensitivity to regional house price shocks. By controlling for county-level consumption when estimating γ_i we ensure that it is orthogonal to any local economic conditions, thereby controlling for potential reverse causation.

The inclusion of county fixed-effects controls for any time-invariant heterogeneity such as long-term differences in productivity growth. By controlling for county sensitivity to regional consumption $\mu_i \Delta C_{r,t}$ we ensure that the $\hat{\gamma}_i$ estimates are orthogonal to any local sensitivity to regional cyclicality which may coincide with the county-level sensitivity to house prices.

The identifying assumption is that there are no unobserved regional shocks Σ_r that simultaneously correlate with regional house price shocks in the time-series, whilst its differential county impact θ_i also correlates with $\hat{\mu}_i$ in the cross-section. Controlling for county sensitivity to regional consumption growth shocks implies that several potential candidates are ruled out, as this term would likely soak them up.

In addition to the above controls, I also include differential sensitivity to real mortgage rates and county industry-shares at the 2-digit NAICS-level with time specific dummies, similar to [Guren et al. \(2021\)](#).

VI. Results

VI.A. Cross-Sectional Results

In [table 8](#) I present the cross-sectional results. The first row presents the coefficient on the housing wealth shock, that is, β_1 in (4). The second row presents the estimated coefficient on the financial net worth shock. Columns (1) - (3) are estimated by OLS and columns (4) - (6) are estimated with 2SLS instrumenting the net worth shock with the WFH-IV. Columns (3) and (6) incorporate controls as discussed in the previous section.

Column (6) incorporates both 2SLS and extensive controls and is therefore interpreted as the key estimate. It is strongly positive and statistically significant at the 5% level. It tells us that a one percent increase to housing net worth causes a 0.839 percent increase to consumption. This implies that the risk-sharing hypothesis as discussed in [section II](#) is rejected. The size of the estimate is in line with [Mian et al. \(2013\)](#) who find estimates ranging between 0.634-0.869.

As discussed in [section V](#), the inclusion of the controls are important for deriving an accurate estimate, as their exclusion in column (4) is statistically insignificant. Conversely, the OLS estimate excluding controls in column (1) is significant at the 5% level and it loses significance when including controls in column (3). The inclusion of the financial asset shock in column (5) has a strong impact on the coefficient estimate, most likely due to their extremely high colinearity (see [figure 4](#)). The IV estimate being larger than the OLS estimate may be due to attenuation bias from measurement error and reverse causality which the 2SLS estimation strategy corrects for.

Because this estimate captures the effect of house prices scaled by the housing wealth to net worth ratio, deriving an interpretation in terms of USD is infeasible. [Table 9](#) presents the results from estimating (4) but using the log change in house prices instead of the net worth shock. The causally identified estimate in column (6) tells us that for a 1% increase

Table 8: Cross-Sectional Results (Net Worth Shock)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	WFH-IV	WFH-IV	WFH-IV
$\Delta \ln p \times \frac{H}{NW}$	0.262 (0.111)	0.324 (0.115)	0.152 (0.0967)	0.711 (0.620)	2.047 (2.018)	0.839 (0.415)
$\Delta \ln s \times \frac{F}{NW}$		0.286 (0.189)			2.013 (1.871)	
Controls	No	No	Yes	No	No	Yes
Observations	2,721	2,721	2,351	2,344	2,344	2,344

Notes: This table presents the regression coefficient estimates obtained from regressing retail employment growth between 2019 and 2023 on the net worth shock at the county-level. Columns 1–3 are estimated using OLS whilst columns 4–6 are estimated using 2SLS where the independent variable is instrumented by the WFH-IV. The standard errors are reported in parentheses and are clustered at the state level. Columns 4 and 6 include the following controls: per capita net migration, COVID-19 Policy Stringency Index, GPS Location Data, Industry Shares, log of median household income, log of net worth per capita, log number of restaurants, and log of population density.

in house prices, consumption increases by 0.316%. The OLS estimates in columns (1)–(3) range between 0.136 and 0.129, which is in line with [K. Case et al. \(2013\)](#) and [Berger et al. \(2018\)](#) who find estimates between 0.15–0.25. The 2SLS estimate is therefore larger than previous estimates, but not unreasonably so.

Following [Campbell and Cocco \(2007\)](#), I translate this estimate into an MPC in dollars by considering the pre-COVID median sale price of a US house which was \$320,000 and the 2019 average household expenditure was \$63,000, implying that for a 1% or \$3,200 house price increase, consumption increases by 0.316% or \$199, which is roughly 6.2 cents per dollar of the house value increase. This is in line with ([Aladangady 2017](#)) and ([Mian et al. 2013](#)) who estimate MPCHs in range of 5.1–7.2 cents per dollar increase.³¹

To understand if the results are sensitive to timing, I replicate [table 8](#) and [table 9](#) but I define the sample 2019–2022. I present the results in [table A.2](#) and [table A.3](#) respectively. The OLS cases are statistically significant, whilst the instrumented specifications are not. In column (7) of both tables I present the reduced form regression, i.e., the regression of retail employment growth on the WFH-IV. In both cases the reduced form relationship is strongly statistically insignificant. One explanation for the lack of a positive result is that the house price shock peaked in the second quarter of 2022 (see [figure 1](#)), implying a transmission to consumption is first picked up in 2023 and hence not present in [table A.2](#) and [table A.3](#).

VI.A.1. Housing Supply Instrument The house price shock studied in this paper is a useful opportunity to test the external validity of previously applied identification strategies, for example, the [Saiz \(2010\)](#) housing supply elasticity instrument utilized by [Mian et al. \(2013\)](#). In [table A.4](#) I present the result from estimating (4) using the housing supply elasticity

³¹Median house sale price from here: <https://fred.stlouisfed.org/series/MSPUS> and average annual consumption expenditure in 2019 from the BLS: <https://www.bls.gov/opub/reports/consumer-expenditures/2019/home.htm>. I derive the MPCH in dollar terms by simply dividing the dollar consumption increase by the house price increase, [Guren et al. \(2021\)](#) computes the same entity by taking elasticity / (H/C) = MPCH which yields an equivalent result.

Table 9: Cross-Sectional Results (House Price Growth)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	WFH-IV	WFH-IV	WFH-IV
$\ln \Delta p_t$	0.136 (0.0264)	0.137 (0.0261)	0.129 (0.0204)	0.154 (0.123)	0.187 (0.128)	0.316 (0.154)
$\Delta \ln s \times \frac{F}{NW}$		0.0533 (0.173)			0.232 (0.190)	
Controls	N	N	Y	N	N	Y
Observations	2,759	2,759	2,388	2,344	2,344	2,344

Notes: This table presents the regression coefficient estimates obtained from regressing retail employment growth between 2019 and 2023 on house price growth at the county-level. Columns 1–3 are estimated using OLS whilst columns 4–6 are estimated using 2SLS where the independent variable is instrumented by the WFH-IV. The standard errors are reported in parentheses and are clustered at the state level. Columns 4 and 6 include the following controls: per capita net migration, COVID-19 Policy Stringency Index, GPS Location Data, Industry Shares, log of median household income, log of net worth per capita, log number of restaurants, and log of population density.

estimates as an instrument for the net worth shock (columns 1–2) and house price growth (columns 3–4) respectively.

In both cases the estimates are statistically insignificant at the 5% level with negative coefficients estimated in columns 1–3. Multiple factors may contribute to why this identification strategy fails for this sample. First, the Saiz instrument is defined at the MSA-level implying that each county in the same MSA will be attributed the same elasticity value. Therefore, this instrument will not explain any within-MSA variation in house prices, which is likely detrimental as the house price shock is typified by variation within cities and their peripheries – as discussed in [section IV](#). Second, the sample is not only limited to counties in an MSA, but specifically for those that [Saiz \(2010\)](#) originally estimated the elasticity for. This substantially limits the sample size from near 2,400 observation to just below 500, potentially impacting inference.

VI.B. Panel results

Supplementary to the the cross-sectional results, I adopt identification strategies of previous studies to estimate the effects in a panel setting. Finding significant effects across several identification strategies puts greater requirements on any confounding element(s), as, to invalidate each approach, it must either (i) be simultaneously correlated with each of the instruments and the dependent variable, or, (ii) consist of multiple confounding elements which are correlated with at least one of the instruments. I therefore treat these estimates as complimentary to the cross-sectional evidence, and as an opportunity to test previously applied strategies in a new setting.

In [table 10](#) I present the results from the panel estimations. Column (1) is estimated using OLS, whilst (2) and (3) are estimated using 2SLS with the sensitivity instrument from [Guren et al. \(2021\)](#) and housing supply elasticity from [Saiz \(2010\)](#) and [Aladangady \(2017\)](#) respectively. The OLS estimate is near zero and statistically insignificant, whereas both instruments yield statistically significant estimates. As with the cross-sectional setting, this

Table 10: Panel Results

	(1)	(2)	(3)
	OLS	Sens.	Saiz
$\Delta \ln p$	0.001	0.278	0.789
Standard Error	(0.023)	(0.130)	(0.233)
Observations	12,033	8,574	6,114
Controls	Y	Y	Y
Fixed Effects	Y	Y	Y

Notes: This table presents the regression coefficient estimates obtained from regressing quarterly retail employment growth on house price growth at the county-level between 2020Q2-2023Q2. Column 1 is estimated using OLS whilst columns 2 and 3 are estimated using 2SLS where the independent variable is instrumented by the Sensitivity instrument and Saiz instrument respectively. The standard errors are reported in parentheses and are clustered at the state level. All specifications control for state $\times$ year and county fixed effects, and net migration per capita. Column 2 also controls for county-level sensitivity to mortgage rates and employment growth as discussed in [section V](#).

may due to attenuation bias caused by either measurement error, reverse causality, or both. The two 2SLS estimates are very different in magnitude as the sensitivity instrument yields an elasticity of 0.278 compared to 0.789 by the Saiz instrument.

The sensitivity estimate is much larger than [Guren et al. \(2021\)](#) who estimate an elasticity of 0.058 over a sample that covers years between 1978-2017. The differences may therefore be driven by the abnormally large and positive shock the elasticity in [table 10](#) is estimated over, compared to Guren et. al.’s 2021 sample which covers multiple housing and business cycle. This may also explain why estimates from the cross-sectional WFH-IV strategy are also on the larger side. A key finding of 2021, Guren et. al.’s analysis is that elasticities have changed over time, with estimates centred around the 1990’s reaching similar levels to column (2) in [table 10](#). However, it is worth noting that an increase in the elasticity would go against the trend of declining estimates found by the authors.

VI.C. Heterogeneity in Wealth and Income

[Carroll and Kimball \(1996\)](#) and [Holm \(2016\)](#) show that when households self-insure consumption, or face borrowing constraints, consumption is a concave function of wealth. This implies that the elasticity of consumption with respect to housing wealth should decrease in wealth. Similarly, [Berger et al. \(2018\)](#) argues that consumption elasticities should be hump shaped in wealth and income, as those with low income are unlikely to be homeowners, and those with high incomes are unlikely to be constrained. Finding these relationships in the data would emphasize the role of heterogeneity in explaining consumption dynamics and macro economic phenomena.

I investigate this hypothesis by estimating the following equation:

$$\frac{C_{i,t} - C_{i,0}}{C_{i,0}} = \beta_0 + \beta_1 W_{i,t} + \beta_2 NW_{i,t} + \beta_3 W_{i,t} \times NW_{i,t} + \epsilon_{i,t}. \quad (10)$$

Where $W_{j,t}$ is the housing wealth shock and $NW_{j,t}$ is the log of net worth per capita in thousands. The interaction term $W_{j,t} \times NW_{j,t}$ captures the effect of the housing wealth shock

Table 11: Cross-Sectional Wealth Results

	(1) OLS	(2) OLS	(3) WFH-IV	(4) WFH-IV
Interaction Term	-0.0690 (0.0264)	-0.0597 (0.0266)	0.175 (0.228)	0.0459 (0.120)
ln (Net Worth per Capita)	0.00149 (0.00146)	0.00123 (0.00127)	-0.0133 (0.0153)	-0.00432 (0.00756)
Net worth shock	0.469 (0.115)	0.425 (0.117)	0.0500 (1.889)	0.927 (1.023)
ln (Median Household income)		-0.00614 (0.0143)		-0.0219 (0.0215)
Net Migration per Capita		0.122 (0.0780)		0.211 (0.0777)
Stringency Index		-0.00144 (0.000602)		-0.00197 (0.000540)
Observations	2,377	2,377	2,156	2,156

Notes: This table presents the regression coefficient estimates obtained from regressing [equation \(10\)](#). Columns 1–2 are estimated using OLS whilst columns 3–4 are estimated using 2SLS where the independent variable and the interaction term are instrumented using the WFH-IV and its interaction with the log of Net Worth per Capita. The standard errors are reported in parentheses and are clustered at the state level.

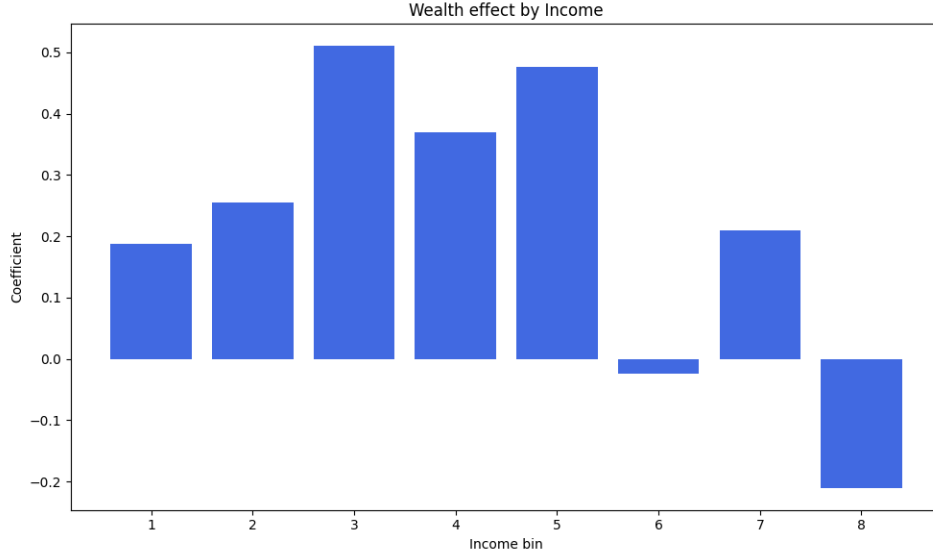
on consumption for different levels of net worth. If the elasticity to consume out of housing wealth is decreasing in wealth, we would expect this coefficient (β_3) to be negative, whilst the coefficient on the housing wealth shock (β_1) to be positive, this gives rise to a concave relationship between consumption and wealth for a given level of the housing wealth shock.

In column (1) of [table 11](#) I report the results from estimating [equation \(10\)](#) using OLS. As predicted, we find a concave relationship between consumption and housing wealth shocks, implying that wealthier counties are less sensitive in their consumption to housing wealth shocks. The coefficients on the interaction term and net worth shock are statistically significant at the 5% level. In column (2) I control for the log of median household income, net migration per capita, and the COVID policy stringency index, the estimates are robust to their inclusion.

I also estimate [\(10\)](#) with 2SLS using the WFH-IV in columns (3) and (4). As both the housing wealth shock and the interaction term are potentially endogenous, two instruments are required for just identification. Ideally one would implement two completely different instruments, however as valid instruments are difficult to find, I follow [Mian and Sufi \(2014\)](#) and use my main instrument along side the interaction of said instrument with the log of net worth per capita.³² Whilst the coefficients follow the same corollary structure, the coefficients are no longer significant. This may be due to the absence of a statistically significant relationship or the instrument strategy being inappropriate, e.g., the two excluded instrument will be highly colinear and explain roughly the same variation - as such, I carry

³²[Mian and Sufi \(2014\)](#) use the Saiz instrument and its interaction with wealth.

Figure 13: Wealth Effects by Income bins



Notes: This figure sorts log of median household income by size and splits the variable into eight bins. For each bin the coefficient from a regression of retail employment growth on the net worth shock is plotted.

out any further analysis with OLS.³³

As a further exercise, I sort the sample on median household income and split it into 8 equally sized bins, and estimate (4) for each bin respectively. Figure 13 presents the β_1 estimate for each bin respectively sorted from lowest to highest incomes. The near hump-shaped pattern provides further evidence of MPCH and elasticity heterogeneity in the data.

As argued by Berger et al. (2018), we expect MPCH's to follow a hump-shaped distribution. Households in the middle of the income distribution are those who are most likely to simultaneously have positive MPC's out labour income due to being constrained, whilst also being home-owners (i.e., the wealthy Hand-to-mouth households). Home-ownership is less prominent among those with lower incomes, hence consumption responses are lower in these bins. Those in the higher bins are unlikely to be constrained, and therefore do not respond to wealth shocks.³⁴

This result differs from Mian et al. (2013), who in a similar exercise, find a monotonically decreasing relationship between income bins and estimated MPCH's.

Mian et al. (2013) argue that between-county wealth inequality is fairly low, and is instead more present within counties, e.g., at the zip-code level. It could therefore be argued that my findings are due to low power caused by a lack of variation in the data. However, my sample is more than twice as large as Mian et al. (2013) and incorporates both urban and rural counties, whereas their paper is limited to counties within MSAs.

³³Mian et al. (2013) seemingly also carry out their further analysis of the distribution of MPCH's using OLS.

³⁴See figure 3 in their paper. The unevenness of the distribution of MPCs in figure 13 may be due to sampling error.

VI.D. The Role of Leverage

Cross-sectional variation in leverage may also generate heterogeneity in MPCHs. [Aladangady \(2017\)](#) and [Mian et al. \(2013\)](#) show empirically that households with a higher debt service ratio and leverage respond more strongly to house price shocks. A key reason for this result is that, if borrowing constraints bind, a house price shock functions as an increase in their collateral value, giving them better access to credit.

Simultaneously, from a self-insurance perspective, it could be argued that a lower net wealth position implies a lesser ability to self-insure consumption; hence generating greater responses to wealth shocks ([Berger et al. 2018](#)).

Home-equity extraction and mortgage refinancing may however be costly or entirely unavailable to highly levered or underwater borrowers [Ganong and Noel \(2020\)](#).³⁵ [Berger et al. \(2018\)](#) show that, in their housing model, incorporating long-term debt contracts and costly refinancing for households whose houses have declined in value, elasticities decline sharply when leverage exceeds the maximum level allowed at the origination of the mortgage. This suggests that as leverage reaches sufficiently high levels, elasticities should drastically drop off.

I am unable to compute the county-level loan-to-value (LTV) ratios because the debt data employed in this study is not limited to mortgages. Instead I define the leverage ratio as the ratio of household debt to gross wealth (financial and housing wealth), where household debt includes mortgages alongside other forms of debts such as autoloans and credit card debt.

In [table 12](#) I present the regression coefficients from estimating (10) where net worth per capita is substituted with the leverage ratio. Column (1) includes only the net worth shock, leverage ratio, and their interaction, and the interaction term is negative, contrary to what theory would predict. To ensure that this result is not driven by variations in wealth I control for net worth per capita and its interaction with the net worth shock in column (2). Column (3) incorporates further controls, and the interaction term is still negative. As the coefficients on the interaction term is statistically insignificant in all specification, it could be argued that the evidence points to leverage having no additional effect on the elasticity to consumption out of housing wealth.

As in the previous section, in [figure 14](#) I split the sample into bins based on the degree of leverage, and estimate the elasticity to consume for each bin. Keeping in mind that the housing net worth shocks mechanically factor in leverage, I produce heterogeneity graphs for both elasticity estimates of net worth shocks and house price shocks in the cross-section. In [figure 14a](#) the pattern is unstable, but reminiscent of that described by [Berger et al. \(2018\)](#), whereby the MPC increases in leverage but swiftly reaches zero for the most levered households. The MPC's in [figure 14b](#) are seemingly more stable and increase only to become much smaller at the highest levels of leverage.

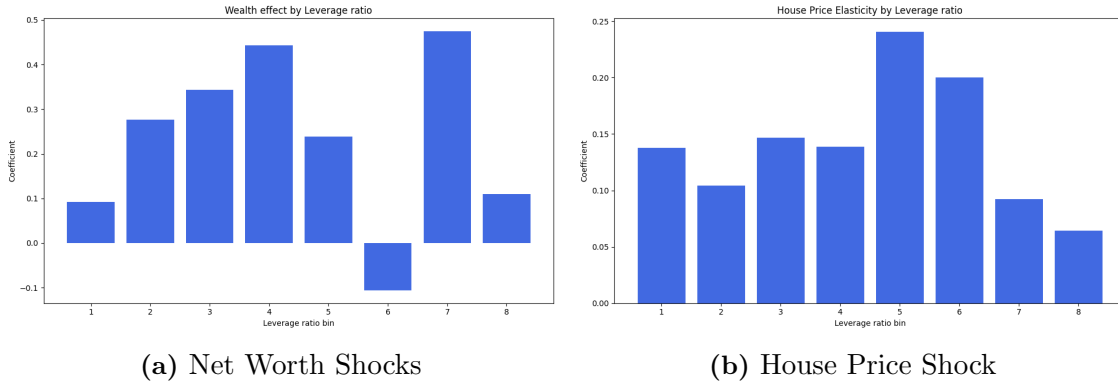
These results are mixed, as [figure 14](#) indicates that there may be a relationship between the MPC and leverage, but a simple cross-sectional regression fails to pick one up. This may be due to the measure use. Studies that have used leverage data at the household level, such as [Aladangady \(2017\)](#), estimate strong relationships between wealth effects and the leverage

³⁵An underwater borrower in the context of mortgage lending is a borrower who's home is worth less than at point of purchase.

Table 12: Cross-Sectional Results and Leverage

	(1)	(2)	(3)
Interaction term	-0.221 (1.783)	-1.932 (1.832)	-1.367 (1.733)
Leverage ratio	0.215 (0.159)	0.242 (0.171)	0.210 (0.158)
Net Worth Shock	0.239 (0.175)	0.600 (0.188)	0.505 (0.199)
Net Worth Shock $\times$ ln (Net worth per capita)		-0.0741 (0.0279)	-0.0612 (0.0275)
ln (Net Worth per Capita)		0.00205 (0.00173)	0.00174 (0.00156)
ln (Median Household Income)			-0.00846 (0.0148)
Net Migration per Capita			0.119 (0.0783)
Stringency Index			-0.00147 (0.000589)
Observations	2,721	2,721	2,721

Notes: This table presents the regression coefficient estimates obtained from regressing equation (9) where NW_{it} is substituted with the county level leverage. The interaction term is the interaction between the Net Worth Shock and Leverage ratio. The standard errors are reported in parentheses and are clustered at the state level

Figure 14: Wealth Effects by Leverage

Notes: This figure sorts the leverage ratio by size and splits the variable into eight bins. For each bin the coefficient from a regression of retail employment growth on the independent variable is plotted. The independent variables are Net Worth Shocks for [figure 14a](#) and House Price Shocks for [figure 14b](#) respectively.

ratio.

VII. Discussion and Policy Implications

As discussed in [section II.](#), finding evidence of housing wealth effects is indicative of an inability of households to perfectly insure and smooth their consumption, which may be rooted in credit constraints or incomplete markets ([Berger et al. 2018](#)). These are the same assumptions and modelling choices which lie at the foundation of modern heterogeneous macroeconomics ([Kaplan and Violante 2022](#)). The positive findings of this study therefore contribute to the motivation of the adoption of heterogeneous agent models in macroeconomic research. As consumption dynamics and the MPC (and its distribution) are instrumental in determining the propagation of economic shocks ([Achdou et al. 2022](#)), such modelling choices carry implications for the transmission of – and hence optimal decisions of – for example, monetary and fiscal policy.

There is a rich literature on the implications of heterogeneity for monetary policy, a fair summary of which is beyond the scope of this section. However, a few key points are worth mentioning. For example, heterogeneous agent models highlight the redistributive effects of monetary policy, which raises questions regarding its implications for the efficacy of policy in-itself, and, whether central banks’ mandates should be expanded to include distributional concerns ([Auclert 2019](#); [Moll 2020](#); [Violante 2021](#)). Moreover, these models emphasize the interplay between monetary and fiscal policy, as the former carries implications for government borrowing costs, and how governments make up this cost (either through taxation, borrowing, or inflation) have implications both on who carries the burden of these costs but also the efficacy of monetary policy.

The results of this paper also carry implications for the understanding of boom-bust dynamics in the macroeconomy. The results demonstrate a link between asset prices and real economic activity, and that fluctuations in asset prices can have effects on the real economy. Thus drawing a connection between these findings and the literature on asset price amplification. A fall in house prices would bring about a fall in spending and demand, which may further depress house prices, and so on.

The results on wealth and leverage indicate that those with more leverage and less net wealth tend to respond more to house price shocks, implying that distributions of wealth and leverage could exacerbate the boom-bust dynamics of the economy. Policies that limit leverage, such as loan-to-value (LTV) limits, may therefore be effective in dampening boom-bust dynamics and potential amplification effects. Conversely, lax lending standards can lead to a build-up of leverage in the economy, which can amplify the effects of a housing crisis.

This view is criticised by [Guren et al. \(2021\)](#) whose results suggest that the elasticity of consumption with respect to house prices has not changed with changes in LTV limits. Indicating that the boom-bust relationship between house prices and consumption is independent of such policy decision.

Debt forgiveness is another potentially effective policy tool in the event of a housing crisis, as it targets households with high leverage and low wealth, whose consumption would be most sensitive to a fall in house prices – as indicated by their larger MPCH’s estimated

in this study. An example of such a policy is the Obama administration’s Home Affordable Refinance Program (HARP), which was initiated in March 2009, and aimed to help homeowners to refinance their mortgages at lower rates and hence reduce their debt burden. [Kaplan et al. \(2020\)](#) evaluate the effectiveness of such debt forgiveness programs in a structural model and find that they are not effective in reducing the impact of house price fluctuations on aggregate demand. However, they do find that the policy is useful in reducing the number of underwater households and hence foreclosures. Implying that there are welfare gains associated with such policies, especially if the utility losses of foreclosures and avoided bank asset losses are accounted for.

VIII. Conclusion

The availability of public regional data provided by both public and private institutions enables the study of macroeconomic questions using methods from the causal identification literature. In this thesis I have studied the relationship between house price shocks and consumption during the COVID-19 pandemic. To my knowledge this is the first paper to exploit the housing demand and preference shock generated by the policy response and shift to remote work, in a study of housing wealth elasticities.

I find that for a one dollar increase in house prices during the pandemic, consumption increased by 6.2 cents. The propensity to consume out of housing wealth is found to vary in both wealth and leverage. Those in the middle of the income distribution – elsewhere referred to as the ‘wealthy hand-to-mouth’ ([Kaplan and Violante 2014](#)) – have the highest propensity to consume. Similarly, counties elasticities are found to increase in leverage, up until a point where they decrease, likely due to higher costs of equity withdrawals and mortgage refinancing.

I also find evidence indicative of a shift in consumption preferences. Cities that saw a greater increase in suburban house prices relative to those the city center also saw statistically significant decreases in consumption of urban amenities such as restaurants and bars. These effects are absent for in-person and remote services.

This thesis therefore contributes to the literature linking asset prices and real economic activity. It emphasises the role of heterogeneity, and thus provides additional motivation for the adoption of heterogeneous agent frameworks in macroeconomic modelling; which in turn carries implications for both monetary policy and fiscal policy ([Kaplan et al. 2018](#); [Broer et al. 2023](#)). Moreover, the estimated linkage between asset prices and consumption suggests a role for policy that addresses boom-bust cycles and amplification mechanisms generated by house price fluctuations and credit-cycles.

The re-evaluation of urban housing is seemingly persistent, having reached its peak in the second half of 2021 [figure 7](#). However it could be argued that we are yet to fully reach the post-COVID equilibrium. As such, future research may wish to study understand and disentangle the transitory and persistent components of this shock, and estimate their respective impacts on consumption ([Blundell et al. 2008](#)).

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A. Appendix

Table A.1: Summary Statistics

	Obs.	Mean	Std. Dev.	Min.	25th pct.	Median	75th pct.	Max.
Net Worth Shock (19-23)	2344	0.077	0.036	-0.121	0.055	0.076	0.098	0.303
House Price Growth (19-23)	2344	0.322	0.112	-0.262	0.262	0.330	0.394	0.875
Financial Asset Shock (19-23)	2344	0.287	0.018	0.167	0.278	0.290	0.299	0.424
Net Worth per Capita (\$000)	2344	3.365	2.559	0.476	2.174	2.860	3.757	70.356
Financial Assets per Capita (\$000)	2344	292.942	224.796	35.814	190.227	252.126	330.101	6628.618
log Net Worth per Capita (\$000)	2344	1.083	0.469	-0.742	0.777	1.051	1.324	4.254
Housing Wealth per Capita (\$000)	2344	78.496	56.287	18.212	50.017	64.060	87.574	1199.017
Household Debt per Capita (\$000)	2344	34.895	16.856	5.012	23.513	30.943	42.053	165.522
Leverage Ratio	2344	0.061	0.022	0.006	0.047	0.058	0.071	0.245
Median Household Income	2344	54754.885	14698.285	21504	45225.500	52502	60910	142299
Log Median Household Income	2344	10.878	0.250	9.976	10.719	10.869	11.017	11.866
Log Population Density	2344	3.427	1.434	-1.284	2.541	3.260	4.180	10.233
Net Migration per Capita	2344	-0.019	0.077	-1.270	-0.047	-0.019	0.009	0.634
Google Mobility Ret. & Rec.	2344	-0.086	0.117	-0.687	-0.137	-0.073	-0.017	0.611
Stringency Index	2344	34.816	6.217	14.690	31.030	34.884	37.855	58.096
Share Worked from Home (15-19)	2344	0.046	0.022	0.001	0.032	0.042	0.057	0.275
Home-ownership Rate	2344	0.588	0.089	0.187	0.538	0.594	0.646	0.869

A.1. Cross-sectional Robustness Results

Table A.2: Cross-Sectional Results (Net Worth Shock) - Robustness

	(1) OLS	(2) OLS	(3) OLS	(4) WFH-IV	(5) WFH-IV	(6) WFH-IV	(7) OLS
Net Worth Shock	0.240 (0.0908)	0.298 (0.0927)	0.127 (0.0822)	0.0863 (0.512)	0.416 (1.185)	0.233 (0.244)	
WFH-IV							0.0212 (0.0834)
Observations	2,723	2,723	2,352	2,345	2,345	2,345	2,345

Notes: This table presents the regression coefficient estimates obtained from regressing retail employment growth between 2019 and 2022 on the net worth shock at the county-level. Columns 1–3 are estimated using OLS whilst columns 4–6 are estimated using 2SLS where the independent variable is instrumented by the WFH-IV. Column 7 presents the reduced form regression without controls; that is, the regression of retail employment growth (19–22) on the WFH-IV. The standard errors are reported in parentheses and are clustered at the state level. Columns 2 and 5 control only for the financial assets shocks as in [table 8](#). Columns 4 and 6 include the following controls: per capita net migration, COVID-19 Policy Stringency Index, GPS Location Data, Industry Shares, log of median household income, log of net worth per capita, and log of population density.

Table A.3: Cross-Sectional Results (House Price Growth) - Robustness

	(1) OLS	(2) OLS	(3) OLS	(4) WFH-IV	(5) WFH-IV	(6) WFH-IV	(7) OLS
Price Growth	0.143 (0.0229)	0.144 (0.0233)	0.121 (0.0180)	0.0176 (0.103)	0.0360 (0.0982)	0.0816 (0.0848)	
WFH-IV							0.0212 (0.0834)
Controls	No	Yes	Yes	No	Yes	Yes	No
Observations	2,761	2,761	2,389	2,345	2,345	2,345	2,345

Notes: This table presents the regression coefficient estimates obtained from regressing retail employment growth between 2019 and 2022 on house price growth at the county-level. Columns 1–3 are estimated using OLS whilst columns 4–6 are estimated using 2SLS where the independent variable is instrumented by the WFH-IV. Column 7 presents the reduced form regression without controls; that is, the regression of retail employment growth (19–22) on the WFH-IV. The standard errors are reported in parentheses and are clustered at the state level. Columns 2 and 5 control only for the financial assets shocks as in [table 8](#). Columns 4 and 6 include the following controls: per capita net migration, COVID-19 Policy Stringency Index, GPS Location Data, Industry Shares, log of median household income, log of net worth per capita, and log of population density.

Table A.4: Cross-Sectional results (Saiz Instrument)

	(1)	(2)	(3)	(4)
Net Worth Shock	-1.411 (0.722)	-0.440 (0.376)		
Price Growth			-8.176 (51.85)	21.56 (668.6)
Controls	No	Yes	No	Yes
Observations	486	486	486	486

Notes: This table presents the regression coefficient estimates obtained from regressing retail employment growth between 2019 and 2023 on house price growth and net worth shocks respectively at the county-level. All columns are estimated with 2SLS where the house price growth and net worth shocks are instrumented with the [Saiz \(2010\)](#) instrument. The standard errors are reported in parentheses and are clustered at the state level. Columns 2 and 4 include the following controls: per capita net migration, COVID-19 Policy Stringency Index, GPS Location Data, Industry Shares, log of median household income, log of net worth per capita, and log of population density.