Housing Demand and Remote Work*

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Abstract

We show that the shift to remote work explains over one-half of the 18.9 percent increase in U.S. real house prices from 2019 to 2023. Using variation in remote work exposure across metropolitan areas, we estimate that an additional percentage point of remote work causes a 0.92 percent increase in house prices after controlling for spillovers from migration. This finding reflects an increase in demand for home space: remote work causes an increase in residential rents, a decline in commercial rents, and a greater increase in prices for larger homes. The cross-sectional effect on house prices combined with the aggregate shift to remote work implies that remote work raised real house prices by 11.9 percent. We show that our cross-sectional estimate is a sufficient statistic for extrapolation to the true aggregate effect in a wide class of models. Our results argue for a fundamentals-based explanation for the recent increases in housing costs over speculation or financial factors, and introduce an empirical solution to the aggregation problem of cross-sectional estimates when it is possible to control for spillovers.

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1 Introduction

U.S. real house prices have grown by 18.9 percent from December 2019 to December 2023. At the same time, the COVID-19 pandemic reshaped the way households work, with 30 percent of employees working from home part or full time in December 2023, up from 7 percent in 2019 (Barrero, Bloom, and Davis, 2021; Bick, Blandin, and Mertens, 2023). In this paper, we use estimates based on cross-sectional data to show that the shift to remote work accounts for at least one-half of aggregate real house price growth over this period. Our results suggests that real house price growth over the pandemic reflected a change in fundamentals rather than a speculative bubble, and that fiscal and monetary stimulus were less important factors.[1] This implies that the evolution of remote work may be an important determinant of future house price growth and shelter inflation.[2]

We make three contributions. First, we use a novel measure of exposure to remote work to identify the causal effect of the shift to remote work on house price growth in the cross section of U.S. micro- and metropolitan areas (CBSAs). We measure exposure to remote work with the pre-pandemic remote work share and we show that it is strongly correlated with post-pandemic remote work and plausibly uncorrelated with other housing demand and supply shocks. In addition to finding large effects of remote work on house prices, we find that remote work has comparable effects on residential rent growth, much smaller effects on local inflation, negative effects on commercial rents, and that it increases permit growth, all consistent with remote work increasing local housing demand. Relative to the pioneering work by Brueckner, Kahn, and Lin (2023), Gupta, Mittal, Peeters, and Van Nieuwerburgh (2021), Liu and Su (2021), and Ramani and Bloom (2021), who show that remote work caused economic activity and housing demand to shift from the city core to the periphery, we document that remote work also had large effects on housing demand and house prices across cities. Two related studies estimate inter-city effects on house prices with a different focus from ours: Brueckner, Kahn, and Lin (2023) show that remote work had a negative effect on housing prices in high- compared to low-productivity cities; and Gamber, Graham, and Yadav (2023) estimate inter-city effects of time spent at home, which combines both remote work and pandemic intensity.[3]

Second, we isolate the share of the cross-sectional effect that represents an increase in

[1] Coulter, Grossman, Martínez-García, Phillips, and Shi (2022) discuss risks of a housing bubble in 2022.

[2] See Bolhuis, Cramer, and Summers (2022).

[3] Gupta, Mittal, and Van Nieuwerburgh (2022) estimate the effects of remote work on demand for office space, showing large effects on commercial real estate outcomes in more exposed cities, consistent with our results on commercial rents. Van Nieuwerburgh (2023) surveys and summarizes recent research on the effects of remote work and housing and commercial real estate.

per capita housing demand. Our initial estimate of the effect of remote work on house prices reflects two distinct effects: first, the increase in per capita housing demand caused by an individual working remotely and so demanding more or higher quality space; and second, the reallocation of per capita housing demand across CBSAs through migration towards areas suited for remote work. We separate these two effects because only the first effect has direct implications for aggregate house prices, while the reallocation of housing demand nets out across regions when supply is inelastic everywhere in the short-run (Howard, Liebersohn, and Ozimek, 2023).[4] We show that we can isolate the increase in per capita housing demand by adding high-quality controls for migration constructed from credit bureau data to our cross-sectional regression. The increase in per capita housing demand accounts for one-half of the total effect of remote work on house prices and substantially increases the price of large homes relative to small homes. Relative to prior work by Stanton and Tiwari (2021) who use household level data to show that remote workers consume more housing, we estimate the importance of this channel for the 2019-2023 housing boom. Our focus on isolating the increase in demand for home space from the effect of remote work on migration is also distinct from Althoff, Eckert, Ganapati, and Walsh (2022), Bick, Blandin, Mertens, and Rubinton (2024), Brueckner, Kahn, and Lin (2023), Dalton, Dey, and Loewenstein (2022), De Fraja, Matheson, and Rockey (2021), Gupta, Mittal, Peeters, and Van Nieuwerburgh (2021), Haslag and Weagley (2024), Ramani and Bloom (2021), and Ramani, Alcedo, and Bloom (2024) who focus on how remote work facilitated migration and the shift in economic activity away from city centers and large, expensive cities.

The cross-sectional estimate conditional on migration implies that the aggregate shift to remote work accounts for at least one-half of the increase in aggregate real house prices. Our third contribution is to show that this extrapolation from the cross-section yields the correct aggregate effect of remote work on house prices, or a tight lower bound thereof, in a wide class of equilibrium housing models. This is because the cross-sectional effect of remote work on house prices holding fixed migration is a sufficient statistic for the shift in aggregate housing demand. Intuitively, by controlling for migration in the cross-section, we isolate how the increased demand for home space affects house prices, which we can then aggregate across cities due to the local scope of housing markets. The combination of cross-sectional empirical estimates and minimal structure to elicit the aggregate effect of remote work on housing markets distinguishes this paper from work that uses structural models to determine the effects of remote work on housing markets and city structure (Behrens, Kichko, and Thisse, 2024; Brueckner, Kahn, and Lin, 2023; Brueckner, 2025; Davis, Ghent, and Gre-

[4] Louie, Mondragon, and Wieland (2025) show that supply elasticities are also similar across cities in the long-run, in which case the reallocation of housing demand also nets out in the long-run.

gory, 2024; Delventhal, Kwon, and Parkhomenko, 2021; Delventhal and Parkhomenko, 2024; Duranton and Handbury, 2023; Gamber, Graham, and Yadav, 2023; Howard, Liebersohn, and Ozimek, 2023; Kyriakopoulou and Picard, 2023; Monte, Porcher, and Rossi-Hansberg, 2023; Richard, 2024). The large aggregate effect of remote work on house prices that we estimate is consistent with the models of Davis, Ghent, and Gregory (2024) and Delventhal and Parkhomenko (2024), and larger than that reported in Gamber, Graham, and Yadav (2023) and Howard, Liebersohn, and Ozimek (2023). More generally, we demonstrate a novel, empirical solution to the broader problem of aggregating from cross-sectional estimates for the case where "spillovers" across treated units can be directly controlled for (Nakamura and Steinsson, 2018; Chodorow-Reich, 2020; Adao, Arkolakis, and Esposito, 2019).[5]

We now describe our contributions in more detail. Our results rest on using variation in remote work exposure that is plausibly exogenous to other housing demand and supply shocks from 2019 to 2023. We measure exposure to the remote work shock using the pre-pandemic 2015-2019 remote work share from the American Community Survey (ACS). We show that variation in pre-pandemic remote work reflects the local distribution of occupations and their propensity for remote work as well as characteristics that make remote work attractive, such as low density and amenities. Thus, the pre-pandemic remote share summarizes a CBSA's exposure to the shift to remote work. This empirical variation in remote work is therefore distinct from purely occupation-based measures such as Dingel and Neiman (2020).

We document that the pre-pandemic share of remote work is robustly correlated with the increase in remote work from 2019 to 2023 even conditional on important local characteristics. While our ACS-based measure primarily captures full-time remote status (Bick, Blandin, Caplan, and Caplan, 2024b; Buckman, Barrero, Bloom, and Davis, 2025), the 2023 ACS remote work share is highly correlated with broader measures of remote work from Barrero, Bloom, and Davis (2021), Bick, Blandin, and Mertens (2023), and the Census Household Pulse Survey in the cross-section of U.S. states (see also Kmetz, Mondragon, and Wieland, 2023; Bick, Blandin, Caplan, and Caplan, 2024a).

We then show that areas with more exposure to remote work saw significantly higher house price growth from 2019 to 2023. Each additional percentage point of pre-pandemic remote work implies an additional 2.89 percentage points of house price growth. Critically, house price growth prior to the pandemic is unrelated to remote work exposure, ruling out alternative explanations based on differential trends. Instead, the main threat to identification is that other shocks caused a revaluation of housing exactly in those regions more exposed to remote work. For example, social distancing and lockdowns may have caused

[5]A spillover occurs when the treatment of one unit affects the potential outcome of another unit (Chodorow-Reich, 2020).

households to value outdoor amenities and lower density more, both of which are correlated with the initial remote work share. However, controlling for differences in density or climate has no discernible effects on our estimates. Another explanation is that local labor markets fared better in more exposed areas precisely because workers could switch to remote work. But our estimates are also not sensitive to controlling for local labor market outcomes. Our estimates are also robust to controlling for stimulus payments, stock market exposure, and mortgage payments, suggesting that alternative explanation based on fiscal shocks, stock market valuation shocks, or monetary shocks do not account for our results.

The timing of our effects is also consistent with the shift to remote work being the primary causal factor driving our results. While the effect of remote work on house prices rises from 2020 through mid-2022, it is then nearly constant through the end of 2023. Barrero, Bloom, and Davis (2021) show that the early period coincides with the increasing adoption of permanent remote work policy by employers, and, by mid-2022 employer plans converged to realized work from home rates so that employees could reasonably expect current remote work arrangements to persist. By contrast, other shocks such as pandemic lock-downs and social distancing, loose monetary and fiscal policy, the stock market boom, and the labor market boom had substantially reversed from 2022-2023. One would expect to see similar reversals in our estimates if these alternatives were important confounding factors.

The fact that we find similarly-sized effects on residential rents, negative and zero effects on commercial rents and inflation, and positive effects on the growth of residential building permits provides further evidence that remote work caused an increase in housing demand. Specifically, these results are not consistent with two other kinds of alternative explanations: one in which heterogeneous demand shocks across CBSAs that are not specific to housing, such as labor market, fiscal, monetary, or financial wealth shocks, affect housing and non-housing prices similarly; and a second in which a uniform demand shock, such as low interest rates, interacts with differential housing supply elasticities across CBSAs (Howard, Liebersohn, and Ozimek, 2023; Louie, Mondragon, and Wieland, 2025). All of these results suggest that pre-pandemic exposure to remote work provides useful exogenous variation in the shift to remote work from 2019 to 2023.

The total effect of remote work on house prices in the cross-section captures both the increase in per capita housing demand and the reallocation of this demand across regions through migration. This means we cannot directly aggregate this cross-sectional estimate because migration is a quantitatively important negative spillover in our setting: house prices will grow more in cities attractive to remote work as migrants move in, while house prices will grow less in the cities losing migrants. Thus, our cross-sectional estimate of remote work will be inflated by this negative spillover across CBSAs, and give a misleadingly large estimate of the aggregate effects of remote work.

We show that we can isolate the effect of remote work on housing demand as long as we have a precise measure of migration across CBSAs by explicitly controlling for the effects of migration on house prices in our cross-sectional regression. To this end, we use address changes in the FRBNY / Equifax Consumer Credit Panel, a 5 percent sample from the universe of consumer credit reports, which allows us to observe anonymized addresses down to the census block at a quarterly frequency. Our baseline estimate without controlling for migration implies that an additional percentage point of remote work in 2023 increases real house price growth from December 2019 to December 2023 by 1.72 percentage points. After controlling for migration, we find that the shift in per capita demand accounts for around one-half of the total cross-sectional effect of remote work on house price growth. Thus, an additional percentage point of remote work in 2023, holding net migration fixed, increases house price growth from December 2019 to December 2023 by 0.92 percentage point. Supporting our interpretation that this effect is capturing a large increase in the demand for home space, we also show that remote work causes a 40 percent greater house price appreciation for large houses relative to small houses.

To determine the effects of remote work on aggregate house prices, we combine our cross-sectional effect that controls for migration with the aggregate shift to remote work. This calculation implies that remote work increased house prices by 11.9 percentage points relative to the total real increase of about 18.9 percentage points, or more than one-half of the total increase. Remote work was therefore an important determinant of house price growth in the cross-section and for the aggregate U.S. economy.

We validate this aggregation approach by showing that our cross-sectional estimate of remote work conditional on migration is a sufficient statistic for aggregation in a wide class of equilibrium housing models. In particular, extrapolating from this estimate yields either the correct aggregate effect or a lower bound. We show this argument exactly in a simple static model with log utility, homogeneous income among remote and non-remote workers, and homogeneous supply elasticities. We then extend the argument to models with dynamic housing demand, CES utility, heterogeneous income, and heterogeneous supply elasticities. The aggregation result continues to hold because the cross-sectional estimate, holding fixed migration, captures exactly the increased demand in home space caused by remote work, which aggregates across cities.

Aggregation from micro estimates fails in other settings because spillovers cause the control group to no longer represent a "no-shock" counterfactual (Chodorow-Reich, 2020). However, in our setting it is feasible to control for spillovers and thereby obtain the right counterfactual for aggregation. For housing demand, we directly observe and control for spillovers from migration. Housing supply is nontradable and cannot move across regions, so there are no spillovers on the supply side.[6] While our approach is attractive for aggregation in this setting, a disadvantage relative to structural models is that the minimal structure does not allow us to calculate counterfactuals other than the ones we measure in the data nor can we do welfare analysis.

We conclude that the shift to remote work induced by the pandemic caused a large increase in per capita housing demand. This suggests a fundamentals-based explanation for the rapid increase in real house prices from 2019 to 2023, and that the future of remote work may be critical for the path of housing demand and house prices going forward.

2 Data

We use core-based statistical areas (CBSAs) as our unit of observation. A CBSA collects counties into economically-connected units, including both the urban core(s) and associated periphery. This unit of observation already aggregates the effect of remote work on shifting housing demand between the core and the periphery (Brueckner, Kahn, and Lin, 2023; Liu and Su, 2021; Ramani and Bloom, 2021), which is convenient for our purpose of estimating the effect of remote work on aggregate housing demand. Our final dataset has 893 CBSAs.

Zillow house price indices are our baseline measure of house prices.[7] We also report results using the S&P Corelogic Single Family Home Price index. We measure pre-pandemic price growth from December 2018 to December 2019, and pandemic and post-pandemic price growth from December 2019 to December 2023. We deflate nominal house price using the national CPI excluding shelter. Table 1 shows that average real house price growth increased from 3.8 percent pre-pandemic to 15.4 percent (3.9 percent annualized) from 2019 to 2023. Nationally, real house price growth increased from 2.9 percent to 18.9 percent (4.7 percent annualized). The 2019-23 average masks the unprecedented real house price boom from December 2019 through December 2021 (7.3 percent annualized), followed by below-normal growth through December 2023 (2.2 percent annualized).

We obtain single family rents for a subset of 233 CBSAs from Zillow and for 80 CBSAs from S&P Corelogic. Similar to house prices, Zillow real rent growth increased from 2.1 percent in 2019 to 12.3 percent (3.1 percent annualized) from December 2019 through December 2023. The latter consists of a 4.4 percent annualized rate during the 2019-21 boom followed by a deceleration to a 2.1 percent annualized growth rate from 2021-23.

We rely on American Community Survey data from the 2015-2019 survey waves and the

[6] For tradable goods higher supply in one region could come at the expense of lower supply in another.
[7] See https://www.zillow.com/research/data/.

2023 survey to measure remote work, where remote work is defined as an employed person that does not commute.[8] We also use the ACS to measure local demographic characteristics. These data are available at the individual level with public-use microdata area (PUMA) geographic identifiers, which we aggregate to CBSAs using the person-level weights and PUMA-CBSA area correspondence files.[9]

Table 1 shows that the national (population-weighted) remote worker share increased from an average of 4.5 percent in 2015-19 to 12.9 percent in 2023. Other surveys report higher remote shares in 2023: Bick, Blandin, and Mertens (2023) report more than 24 percent of workers are remote, Barrero, Bloom, and Davis (2021) report that 29 percent of paid working days were done remotely at the same time, and the Census Household Pulse Survey suggests that 25 percent of work days were remote.[10] Unlike the ACS, these surveys are able to capture the evolution of broader remote work trends such as hybrid work and therefore are ideal to measure remote work adoption (Bick, Blandin, Caplan, and Caplan, 2024b; Buckman, Barrero, Bloom, and Davis, 2025). But they are also relatively sparse below the state level, making them difficult to use for our analysis.

To determine the relationship between the 2023 ACS remote work share and these alternative surveys, we construct state-level measures of the fraction of days worked remotely for 2022-2023 from Barrero, Bloom, and Davis (2021), Bick, Blandin, and Mertens (2023), and the Census Household Pulse Survey. Figure 1 shows binned scatter plots of these measures against our measure of remote work from the ACS. There are two important takeaways. First, our ACS measure is strongly correlated with all three survey measures of remote work. Since these measures also explicitly capture hybrid work arrangements, our ACS measure will also pick up the effects of hybrid work in the cross-section. Second, the ACS measure tends to understate the prevalence of remote work by about 15 percentage points. This implies that our empirical estimates using the ACS estimates will end up being scaled up to reflect this discrepancy. However, when we extrapolate from the cross section to the aggregate we multiply by a smaller level of remote work, which exactly compensates for this bias. Overall, these results show that the 2023 ACS gives a very accurate measure of remote work in the cross-section. Kmetz, Mondragon, and Wieland (2023) and Bick, Blandin, Caplan, and Caplan (2024a) provide more detailed analysis that shows that the ACS remote work is

[8]We exclude individuals in the armed forces, agriculture, personal care, child care, and those living in student housing when calculating remote work shares.

[9]See https://mcdc.missouri.edu/geography/PUMAs.html.

[10]Following Barrero, Bloom, and Davis (2021), in the Household Pulse Survey we keep any respondent that reports employment in the last seven days, an annual income of at least $25000, is between the age of 20 and 64, and we assign 30 percent of days worked remotely to those responding working remotely 1-2 days, 70 percent for working remotely 3-4 days, and 100 percent to those responding working remotely for 5 days. We aggregate these responses using person weights.

highly correlated with other measures of remote work in the cross-section.

Our analysis requires a high-quality measure of migration in and out of CBSAs. Here we rely on the FRBNY/Equifax Consumer Credit Panel (Lee and Van der Klaauw, 2010). These data, built from an anonymized 5 percent sample of the universe of consumer credit reports, provide information on an individual's reported address on their credit files down to the census block level at a monthly frequency. We track an individual's reported CBSA across the same periods used to measure house price changes. We then aggregate these moves into gross in- and outflows, and define net migration for CBSA i between t and $t+1$ as $\text{Net Migration}_{i,t,t+1} \equiv \frac{\text{Gross Inflow}_{i,t,t+1} - \text{Gross Outflow}_{i,t,t+1}}{N_{i,t}}$, where N is the number of individuals in that CBSA in the pre-period. On an annualized basis, the dispersion of migration reported in Table 1 is quite similar in the pre- and post-pandemic periods.

We use population density (population per square mile) from the U.S. Census as a measure of density-based amenities (Brooks, Hoxie, and Veuger, 2023) and of the CBSA housing supply elasticity. Baum-Snow and Han (2024) extract housing supply elasticities for new and existing housing in large metropolitan areas using labor demand shocks at the census tract level. For the subsample in which both measures are available, the correlation between log density and the first principal component of their supply elasticities is -0.47.

We collect unemployment rates from the Local Area Unemployment Statistics (LAUS) to measure shocks to the local labor market. We calculate the pre-pandemic unemployment rate as the average in 2019, the pandemic unemployment rate as the change from 2019 to 2020, and the post-pandemic change in unemployment from December 2019 to December 2023. The average change in unemployment over the full period is actually quite low, despite the extremely rapid increase in early 2020, due to the very rapid recovery in labor markets. As an alternative labor market indicator, we calculate Bartik predicted wage growth using four-digit occupation shares in the CBSAs and national per capita wage growth by occupation from the QCEW. These data are reported at quarterly frequency, so we measure wage growth from 2019Q4 to 2023Q4 and pre-pandemic wage growth from 2018Q4 to 2019Q4.

To capture exposure to the growth in stock market valuations, we construct the share of total dividend income in adjusted gross income by CBSA in 2019 (Chodorow-Reich, Nenov, and Simsek, 2021). We use the same source to determine total income per filer in 2019 and fiscal stimulus payments in 2020-21. Fiscal stimulus is the sum of the three economic impact payments, the recovery rebate credit, and the refundable child care credit. Finally, we obtain the average debt-to-income (DTI) ratio by CBSA for new mortgages issued in 2019 from the Home Mortgage Disclosure Act (HMDA) dataset.

3 Empirical Results

3.1 Research Design

We use cross-sectional data to recover the effect of increased remote work on housing demand as measured by house prices. Our baseline regression is an instrumental variables regression of the form:

$$\text{First Stage:} \qquad \text{Remote Work 2023}_i = \kappa + X_i'\theta + \gamma\text{Remote Work 2015-19}_i + \zeta_i \tag{1}$$

$$\text{Second Stage:} \qquad \text{House Price Growth}_i = \alpha + X_i'\delta + \beta\widehat{\text{Remote Work}}\text{ 2023}_i + \epsilon_i \tag{2}$$

where Remote Work 2015-19$_i$ is the share of employed individuals working from home in the 2015-2019 ACS, Remote Work 2023$_i$ is the share of employed individuals working from home in the 2023 ACS, House Price Growth$_i$ is house price growth from 2019 to 2023, and X is a vector of controls.

We use an IV approach as a benchmark since unobserved shocks to housing demand and house prices during and after the pandemic likely affected remote work take up. In fact, if remote workers require more housing, then any shock that pushes up house prices will reduce remote work as potential remote workers in an area migrate to cheaper housing or return to office work. Our instrument must be plausibly uncorrelated with other shocks to house prices while still being correlated with actual remote work after the pandemic.

We use pre-pandemic remote work share, calculated over 2015-2019, as our instrument. Intuitively, we can think of the concentration of remote work prior to the pandemic as reflecting local amenities, the cost and type of housing, and the distribution of occupations amenable to remote work. Table 2, where we regress pre-pandemic remote work on local observables, confirms this intuition.[11] The predicted local remote share based on local occupation shares interacted with the probability of being remote in an occupation at the national level is a very strong predictor of pre-pandemic remote work.[12] Similarly, amenities such as a mild winter climate and low summer temperature and humidity strongly predict remote work.[13] In contrast, density is only weakly correlated with remote work, suggesting a smaller or conflicting roles for housing cost or amenities affiliated with density in explaining the cross-sectional variation in remote work exposure. Our measure contrasts with Dingel

[11] Table A1 shows bivariate comparisons.

[12] To construct predicted remote work share we measure remote work shares for 4-digit occupation codes in the 2015-19 ACS and then weight these occupational shares by the share of workers in that occupation in each CBSA.

[13] We draw our climate measures from https://www.ers.usda.gov/data-products/natural-amenities-scale/.

and Neiman (2020), who isolate variation in remote work caused by occupations, by incorporating the local determinants of remote work take up. This gives us greater empirical power in the cross-section and more likely isolates variation that is representative of the average changes in remote work adoption.

Based on these results, we interpret the pre-pandemic remote work share as a sufficient statistic for how exposed a location is to the changing availability of remote work. Once remote work becomes available more broadly during the pandemic, these same locations will see relatively more remote work due to more immigration and more local workers electing to work remotely. This implies pre-pandemic remote work shares will be predictive of remote work shares over the pandemic and post-pandemic period, satisfying the relevance assumption. We also check that the exclusion restriction, that exposure to remote work is uncorrelated with other shocks to house prices, likely holds by examining pre-trends, the stability of our estimates conditional on important local shocks and characteristics, the dynamics of our estimates, and other outcomes.

If the exclusion restriction holds, then equations (1)-(2) estimate a valid causal effect of remote work on house prices in the cross-section. However, because this causal effect contains the effects of net migration induced by remote work across CBSAs, it may not be appropriate for quantifying how remote work affects aggregate house prices. If pre-pandemic remote share reflects a location's suitability for remote work, then places with more remote work ex ante will tend to see larger net inflows of remote workers. Such migration would raise housing demand and house prices in high remote share locations, while at the same time lowering house prices in low remote share areas, all else equal. Therefore, migration would raise the cross-sectional causal effect of remote work, even if aggregate housing demand is unaffected.

In order to isolate the component of remote work that reflects a shift in per capita housing demand, we control for the effects of migration on remote work and house prices,

$$\text{Remote Work 2023}_i = \kappa + X_i'\theta + \gamma_1 \text{Remote Work 2015-19}_i + \gamma_2 \text{Net Migration}_i + \zeta_i \quad (3)$$

$$\text{House Price Growth}_i = \alpha + X_i'\delta + \beta_1 \widehat{\text{Remote Work}}\ 2023_i + \beta_2 \text{Net Migration}_i + \epsilon_i. \quad (4)$$

Intuitively, controlling for net migration will allow the estimate of β_2 to absorb any effects of remote work on house prices through net migration. This means that β_1 will capture the direct effects of remote work on house prices only through the shift in per capita housing demand. In Appendix A1 we show that β_1 will recover the intended effects if unobserved shocks to migration and house prices are uncorrelated. If unobserved shocks to migration and house prices are positively correlated, which is the more likely case, then we will understate the true effect of remote work on house prices.[14]

If we combine estimates of β_1 with the representative level of remote work in 2023, we obtain an estimate of the effect of remote work on aggregate house prices. In Section 4 we argue that this extrapolation yields the correct aggregate effect because our cross-sectional estimate is a sufficient statistic for the aggregate effects of remote work in a broad class of equilibrium housing models. We also use the model framework to validate our empirical approach in equations (3)-(4).

We conservatively cluster standard errors at the state level in all specifications. CBSAs often cross state borders, so we allocate a CBSA to the state which contains the largest share of population. Following the recommendations by Solon, Haider, and Wooldridge (2015), we estimate unweighted regressions and in Section 4.1 we show that treatment effects vary little with population size.

3.2 Remote Work and House Prices

We next argue that the remote work share in 2015-2019 satisfies the relevance and exclusion restrictions necessary for it to be a valid instrumental variable for post-pandemic remote work.

Figure 2 separates CBSAs into 20 bins based on their remote work share in 2015-2019 and then plots the average remote work share in 2023 within each bin, along with the linear regression line from the underlying data. Areas that had large shares of remote work prior to the pandemic also had significantly larger shares of remote work in the post-pandemic period: in 2023, areas at the top of the pre-pandemic distribution have more than 15 percent of workers at home while areas at the bottom of the pre-pandemic distribution only have about 5 percent of workers at home. This is consistent with our argument that the same underlying fundamentals that made a place amenable to remote work in the pre-pandemic period continued to attract remote work during and after the pandemic. Figure A1 shows heat maps of the distribution of remote work pre-pandemic and in 2023 across CBSAs, grouping CBSAs into terciles in both periods. The stability of the tercile membership across the two maps is suggestive of a strong first stage relationship between pre-pandemic and 2023 remote work shares.

Column (1) of Table 3 reports the corresponding regression. For each percentage point of remote work share in 2015-19 we expect 1.79 percentage points of remote work during 2023. The estimate is very precise with the 95 percent confidence band ranging from 1.59 to 2.00. Since the aggregate remote share increases by a factor of 3, the first stage implies

[14] A simple example of a shock that would induce a positive correlation would be firm creation, which induces immigration for new jobs as well as higher incomes and so house prices.

that there is a level shift in remote work along with a multiplicative treatment effect. Still, the r-squared of 55 percent shows that the pre-pandemic remote share captures a substantial fraction of the variation in 2023 remote work, and therefore satisfies the relevance restriction.

Columns (2) through (4) of Table 3 sequentially introduce controls for important local observables. Column (2) controls for pre-pandemic house price growth, racial/ethnic and age composition, and quintiles for CBSA density.[15] Column (3) adds controls for January temperature, July temperature, and average July humidity, which are important predictors of the initial level of remote work. Column (4) controls for local labor market conditions before the pandemic, during, and after the pandemic and for the exposure to the stock market using total dividends per filer, exposure to fiscal policy using stimulus payments per filer, and exposure to monetary policy using DTI ratios. Introducing these controls raises the r-squared to 72 percent and has only small effects on the pre-pandemic remote work estimate, suggesting our estimated effects are not caused by other pandemic-related shocks.

We next provide evidence that exposure to remote work satisfies the exclusion restriction that it only affects house prices through its effect on post-pandemic levels of remote work. We begin by documenting the robust and stable relationship between exposure to remote work and house price growth from 2019 to 2023 (the reduced form). In Figure 3A we plot real house price growth over the pandemic against the remote work share in 2015-19. This shows that real house price growth is strongly positively correlated with exposure to remote work. The areas most exposed to remote work saw real house prices grow by ten times as much as areas at the bottom of the distribution. The cities least exposed to remote work experienced real house price growth below the historical national average.

It is possible that the large apparent effect of remote work on house prices simply reflects pre-existing trends in house prices caused by factors unrelated to remote work. In Figure 3B we plot pre-pandemic house price growth from December 2018 to December 2019 against the remote work share in 2015-19. The relationship between remote work and pre-pandemic house price growth is weakly negative. In Figure 4A we plot average house prices indexed to December 2019 for terciles of exposure to remote work. House price growth across these groups was indistinguishable leading up to the pandemic, but then began to diverge in 2020, with the gap widening throughout 2021 as house prices continued to grow rapidly and stabilizing in mid-2022.

As a more formal test for pre-trends, Figure 4B plots the regression coefficient of house price growth relative to December 2019 against the 2015-19 remote work share. The estimates

[15] The relationship between log density and remote share is U-shaped: high-density metro areas have similar remote shares to low-density metros, and higher remote shares than medium-density areas (Brooks, Hoxie, and Veuger, 2023). There is a similar relationship between the share of the population above 65 and remote work. To capture these patterns we control for density and age composition nonparametrically.

are not statistically distinguishable from zero at the 95 percent level before the pandemic begins, but the estimates increase sharply in late 2020 and stabilize around mid-2022. This shows that the differences in house price growth correlated with exposure to remote work are not reflective of differential trends prior to the pandemic.

The absence of pre-trends does not rule out the possibility that other shocks during the pandemic may have increased housing demand in locations with a high pre-pandemic remote share relative to locations with a low pre-pandemic remote share. However, any explanation of our estimates must also be consistent with the build-up of the treatment effect from 2020-2021 and its stability starting in mid-2022 seen in Figure 4B.

The dynamics of our estimated treatment effect are consistent with a causal explanation based on remote work. Barrero, Bloom, and Davis (2021) show that in 2020, future employer plans consistently pointed to less remote work going forward, suggesting that current arrangements may be reversed. Over the course of 2021 and early 2022, employer plans converged to the higher levels of remote work currently in place, giving employers increasing confidence that working from home will "stick" (Barrero, Bloom, and Davis, 2021, 2023). This period coincides with the sharply increasing treatment effects from remote work in Figure 4B. Finally, by mid-2022 employer remote work plans fully converged to those currently in place and displayed little further change. Over this period our estimates suggest stable housing demand from remote work.

In contrast, other pandemic-era phenomena that may have stimulated housing markets have substantially reversed since mid-2022: pandemic lock-downs and social distancing are no longer enforced; the large-scale pandemic fiscal stimulus has ended; monetary policy reversed course and mortgage rates are higher than at any time in the 2000s; and the stock market and labor market normalized in late 2022 following an extraordinary boom. To the extent that one believes that our estimates largely capture these other shocks, one would also expect a reversal in our estimates. Yet this is not what we find. Remote work, however, is consistent with our empirical patterns as it has remained largely unchanged since mid-2022 (Barrero, Bloom, and Davis, 2021; Bick, Blandin, and Mertens, 2023).

As another approach to assess violations of the exclusion restriction, we add controls to capture these other shocks and examine the stability of our estimate on the effect of remote work on house prices. Column (1) of Table 4 reports the regression without controls. A location with a one percentage point higher remote work share in 2015-19 should expect an increase in house price growth of 2.89 percent from 2019 to 2023. Columns (2) through (4) of Table 4 include the set of controls from Table 3. In columns (2) and (3) we add controls for density and climate variables respectively. These will capture revaluation of density and outdoor space due to the pandemic. While these variables are also correlated with remote work, adding these controls has very small effects on our estimates. This suggests that the uncorrelated part of remote work has essentially the same effect on the housing market, and that our estimates are not driven by a revaluation of amenities. This interpretation is also consistent with our dynamic estimates in Figure 4B, as one would expect some of the amenity effect to recede with the end of lockdown and social distancing enforcement. In column (4) we add the labor market controls, as well as controls for stock market exposure, fiscal stimulus payments, and mortgage rate exposure. Our estimates change minimally, which suggests we are not loading on these other shocks. Again this result is consistent with our dynamic treatment effects, which show little change since mid-2022, even though the labor market, the stock market, monetary policy, and fiscal policy displayed large swings. In the next section we show additional evidence that we are measuring a housing-specific demand shock, rather than a general demand shock that differentially affects regions.

The net effect of adding these controls is a very slight change in the our estimate to 2.55, even though many of the controls themselves are statistically significant and raise the r-squared from 12 to 34 percent. This estimate is also fairly precise with the 95 percent confidence band ranging from 1.38 to 3.72 percent.

3.3 The Total Effect of Remote Work

Column (5) of Table 4 reports the IV coefficient from estimating equations (1)-(2). Each additional percentage point of remote work in 2023 implies a 1.61 percent increase in house prices. The Kleibergen-Paap weak identification F-statistic is extremely high, suggesting that the risk of weak instrument issues is low (Andrews, Stock, and Sun, 2019). This is consistent with the high r-squared in the first stage results in Table 3, and the fairly narrow 95 percent confidence band of 0.82 to 2.41.

Columns (6) through (8) of Table 4 add our set of control variables. With our full set of controls in column (8), we obtain a slight increase in the overall estimate to 1.72 that remains precisely estimated with the 95 percent confidence bands extending from 1.04 to 2.41. The IV coefficients are roughly 50 percent larger than their OLS counterparts, which are reported in Table A2. This suggests that there are shocks to pandemic housing demand that negatively affect remote work and/or there is measurement error in the 2023 remote work share.

We next show a set of broader regional outcomes that support the interpretation that our estimates captures an increase in housing demand from remote work, and therefore that our instrument satisfies the exclusion restriction. We do this in the context of the IV, as other outcomes are generally only available in smaller samples and so any variation in reduced-form estimates could be due to differences in the first stage.

Row (1) in Table 5 replaces the Zillow house price index with the proprietary Corelogic house price index and finds essentially identical effect on house price growth as in our baseline results. Thus, our estimates are not driven by the particularities of the algorithm used to generate the Zillow house price index.

If remote work causes an increase in overall demand for housing services then we should also expect to see an effect of remote work on rents. We now turn to the subsample of 233 CBSAs for which we have rental index data from Zillow and 80 CBSAs for which we have rental data from CoreLogic. For brevity we only report the IV estimate with all controls included — equivalent to column (8) of Table 4. In the appendix we include a full set of regressions of the first stage (Table A4) and the reduced form and IV regressions Table A5.

Row (2) of Table 5 reports the IV estimate for rent growth from Zillow. It implies that an additional percentage point of remote work in 2023 increases rent growth from December 2019 to December 2023 by 0.37 percentage points. This estimate is a quarter of the estimate for house price growth in this sub-sample (1.29 percentage points as shown in row (3) of Table 5). Row (4) shows that the effect on CoreLogic rent growth, 0.87 percentage points, is essentially as large as the effect on house price growth in that subsample, 1.16. The difference between CoreLogic and Zillow is primarily due to measurement rather than the sample. In the overlapping sub-sample, Zillow implies a causal effect on rents that is only one-third as large as CoreLogic.

An additional way to assess whether there was a housing-specific increase in demand is to check if the price of housing has increased relative to non-residential real estate or the broader bundle of consumer expenditures. If demand for residential real estate partly reflected substitution away from commercial real estate, then we should expect to see lower commercial real estate prices in areas with more remote work. Alternatively, if the growth in house prices is driven by some common shock to real estate values (such as accommodative monetary policy), then we should expect to see similar trends across all types of real estate. We test this prediction using commercial rent data from REIS for 24 CBSAs and Bureau of Labor Statistics price indices for 22 CBSAs.[16] Given the small sample size we report reduced form regressions with just lagged dependent variables as controls and robust standard errors.

Row (6) of Table 5 shows the reduced form regressions for commercial rent growth. A one percentage point increase in remote work exposure predicts a negative but statistically insgnificant percent decline in commercial rents. Row (7) reports the corresponding estimates for house price growth in the same sub-sample: a one percentage point increase in remote

[16] We use the Reis commerical real estate effective rent index, now provided by Moody's CRE. This is a quarterly, hedonic index intended to give the average "effective rent" per square foot for large-building office space in the metro area.

work exposure predicts a percent increase in house prices, which is statistically significant and consistent with the broader sample. The house price growth effects are roughly ten times larger in magnitude and have the opposite sign as the effects on commercial rents. Thus, the data provide evidence for substitution from office space to home space, consistent with Gupta, Mittal, and Van Nieuwerburgh (2022). The relatively small decline in the office rent index may be because it captures both existing and new leases, whereas our housing price and residential rent series only reflect new purchases and leases.

A related concern is that increasing housing costs are reflective of a broader increase in prices faced by consumers. Row (8) of Table 5 gives the reduced form regression for the inflation rate on remote worker share and row (9) gives the same for inflation excluding shelter. While the overall price level shows a positive and statistically significant relationship, the price level excluding shelter grows by 0.41 percent more from 2019 to 2023 for every percentage point of initial remote work exposure, a statistically insignificant effect. For comparison, the effect of remote work exposure on house price growth in the same sub-sample is 2.96 percentage points (row 10). The insignificant inflation response is thus roughly one seventh the magnitude of the very significant house price responses. This supports our claim that we capture a relative increase in the demand for housing from remote work and thus an increase in the relative price of housing, rather than a generic demand shock.

Together with our results on residential and commercial rents, this finding rules out alternative explanations for our results based on broad-based increase in demand correlated with remote work (such as an increase in financial wealth). However, another possible explanation is that remote work exposure proxies for a low housing supply elasticity, so that even a uniform increase in housing demand would increase house prices more in less elastic/higher remote work CBSAs. The fact that our estimates in Table 4 are insensitive to controlling for density is evidence against this hypothesis, but we also check this by estimating the response of building permit growth and cumulative home sales to remote work exposure. If differential supply constraints explain our price results, then we should expect to see a negative relationship between remote work and permits or home sales. Row (11) of Table 5 instead shows that housing permits grew faster in areas more exposed to remote work. These results are consistent with Louie, Mondragon, and Wieland (2025), who show that the effects of remote work on local housing prices and quantities do not vary with measures of local housing supply constraints.

There are a number of additional local observables that are correlated with the increase in remote work over the pandemic: the share of individuals with college education, the log median income, and census region fixed effects. We do not include these controls in our baseline regressions because they absorb significant valid variation in remote work, leaving the remaining variation at risk of not being representative of the true average treatment. For example, the share of college-educated workers is an important predictor of remote work, as occupations that disproportionately employ college-educated workers tend to be more amenable to remote work. By including this control we would restrict attention to potentially unrepresentative variation, which would be problematic for aggregation. Similarly, Figure A1 shows that remote work is more common in the West and less common in the South, but this is largely explained by the geographic distribution of occupations and amenities and it is not obvious that we want to exclude this variation.

However, we check how sensitive our estimates are to including these controls. We report the first stage, reduced form, and IV estimates with these conservative controls in Table A14 and Table A15. The IV estimates with all controls included is now 1.83 (95 percent confidence band from 0.89 to 2.77) versus 1.72 in our baseline regression. The first-stage is also less powerful indicating that this variation is less likely to be representative of the broader increase in remote work relative to our baseline approach.

A closely-related question is whether specific subsets of the variation in pre-pandemic remote work ultimately account for the effect of remote work on house prices. In Table A13, we re-estimate our baseline equations using two subsets of pre-pandemic remote work as our instrumental variable: (1) the remote work expected by the pre-pandemic distribution of occupations, (2) the residual of pre-pandemic remote work after partialling out the occupation-driven variation, and (3) both pieces of variation as two separate instruments. To ensure we isolate the particular source of variation for (1) and (2), we include the remaining variation in the 2015-19 remote work share as a control. We find that both sources of variation imply large effects of remote work on housing markets. The effect due to the residual variation is larger than that for the occupational variation, which suggests that purely occupational-based measures understate the importance of remote work on housing markets. However, we do not reject the overidentification restriction when both instruments enter separately, especially after we control for net migration, so statistically they imply similar effects. The first stage F-statistics for these regressions, while reasonably strong, are uniformly lower than what we find using pre-pandemic remote work alone. Pre-pandemic remote work is therefore more likely to capture variation that will reflect the true average treatment effect we need to plausibly aggregate our effects.

3.4 The Effect of Remote Work on Per Capita Housing Demand

The large effects of remote work on house price and rent growth that we estimate in the cross-section reflect both an increase in per capita housing demand, as remote work requires more housing, as well as a relocation of housing demand towards areas that are better suited for remote work. We expect only the former to significantly affect aggregate housing demand and house prices.[17] Therefore, to determine the aggregate effects of remote work we need to separate the total effect of remote work on housing demand into these two components.

We first document that remote share exposure is a quantitatively important determinant of net migration. The binned scatter plot Figure 5 shows that exposure to remote work is strongly correlated with net inflows from 2019 to 2023. Table A11 reports specifications with migration as the outcome that mirror our primary results and we find that areas exposed to remote work saw much higher net inflows of residents. These inflows were also strongly correlated with pre-pandemic inflows, suggesting pre-existing migration patterns may have been amplified over and after the pandemic. These results indicate that migration may be a quantitatively important driver of the effects of remote work on housing demand that we find in the cross-section of CBSAs.

To isolate the per capita increase in housing demand we estimate equations (3)-(4), in which we directly control for net migration into a CBSA. Table 6 reports the reduced form and IV estimates. The migration controls enter positively and reduce the cross-sectional estimate of remote work on house price growth to 1.46 (reduced form, column 1) and 0.83 (IV, column 5). This is roughly a one-half drop compared to our baseline estimates in Table 4 that do not control for migration. Mechanically, the reduced form and IV results move in similar magnitude because the first stage is essentially unchanged (Table A12). Note that this attenuation of the remote work effect does not reflect an omitted variable bias, but rather is an instance of a "bad control." By controlling for migration we deliberately shut down one of the mechanisms by which remote work exposure can affect housing demand across locations (see Appendix A1 for details).[18]

Our estimate increases slightly after including the full set of controls (columns 3 and 7). In columns (4) and (8) we control for migration non-parametrically by including deciles of 2019-23 net migration and pre-pandemic net migration. Our IV estimate of the effect of remote work on house prices remains almost unchanged at 0.92. This suggests that this effect is not driven by a non-linear migration response or measurement error in the migration variable.

We next show our final supporting evidence that the effect of remote work represents an increase in the the demand for home space using data on price indexes for houses of

[17]This holds especially in the short-run when housing supply is inelastic everywhere (Howard, Liebersohn, and Ozimek, 2023). However, the evidence in Louie, Mondragon, and Wieland (2025), that housing supply elasticities across cities are also similar in the long-run, suggests this argument may also extend to the long-run.

[18]We have also run specifications in which we controlled separately for net migration among individuals with high credit scores or originating from zip codes with high incomes. These controls had very little additional explanatory power over our baseline net migration control.

different sizes. If housing is perfectly divisible then an increase in demand would raise the price per square foot uniformly. However, since housing is indivisible housing demand may be segmented across subsets of the housing stock (Piazzesi, Schneider, and Stroebel, 2020), and the remote work shock may not fall uniformly across these segments. In particular, an increase in the demand for more space caused by remote work likely increases the demand for large houses more than the demand for small houses. We test this hypothesis using Zillow price indices that are broken out according to the number of bedrooms, available for a subset of our CBSAs. Rows (1) through (5) of Table 7 display IV estimates of the effect of remote work on house price indices ranging from 1 to 5 bedrooms. These regressions include our full set of controls from column (7) of Table 6. The estimates show that the effect of remote work on house price growth grows as the number of bedrooms increases, with the effect on 5-bedroom houses being about 50 percent larger than the effect on two-bedroom houses, and with no significant effect on one-bedroom houses. This is consistent with the argument that remote work increases the demand for space.

These results support the claim that remote work increased housing demand. This increased demand translated into more house price growth for larger houses. Combined with our previous results on the dynamic effects of remote work, the insensitivity to controlling for other channels, and broader regional outcomes we find consistent evidence that our estimates capture a causal effect of remote work on housing demand.

4 The IV Estimate as A Sufficient Statistic for Aggregation

We now use our cross-sectional effects on house prices cleansed of the migration channel (Table 6) to estimate the aggregate effect of the shift to remote work on house prices. We show that this extrapolation recovers the true aggregate effect for a wide class of models. This is because the cross-sectional effect of remote work on house prices, holding fixed migration, is a sufficient statistic for the aggregate effect of remote work. We lay out the argument in a simple model, with extensions in Appendix A2.

4.1 Extrapolating Using the IV Estimate

For extrapolation we use the estimates in column (8) with all controls included, $\hat{\beta}_1^{IV}$ =0.92. The weighted 2023 remote worker share for the U.S. economy is 12.9 percent. Multiplying this value with our estimate $\hat{\beta}_1^{IV}$, we obtain an aggregate effect of 12.9×0.92=11.9 percent. Since aggregate real house prices grew by 18.9 percent from December 2019 to December

2023, our IV estimate implies that remote work can explain 11.9/18.9=63 percent of the total increase in house prices over this period. In the following section, we argue that this extrapolation represents a tight lower bound on the true aggregate effect.

For the purpose of aggregation it is important that our treatment effects are nationally representative. Following Solon, Haider, and Wooldridge (2015) we estimate the effects of remote work on house prices by population quartile. Figure A2 shows that the treatment effects vary little by population. Using the population in each quartile as weights, the weighted effect of remote work on house prices is 0.95, essentially identical to our baseline estimate. We can also extrapolate by multiplying the four treatment effects with the 2023 remote work share in each quartile, which yields an aggregate effect of remote work on house prices of 12.2%.

4.2 Housing Demand, Supply, and Equilibrium

We next show why this aggregation approach works in a simple cross-sectional model of housing demand and remote work, which we extend in Appendix A2 to address potential complications.

We assume log utility over non-housing consumption c and housing consumption h. Conditional on being in location l, the utility from work-mode w is

$$U_{wl} = (1 - \theta_w) \ln c_{lw} + \theta_w \ln h_{lw}$$

Work mode w is either remote r or in-office b. Remote work implies a greater weight on housing expenditure, $\theta_r > \theta_b$. In Appendix A2.3 we extend the model to a CES utility function, and in Appendix A2.4 to a dynamic model of housing demand. These add additional parameters to the model—the elasticity of substitution and the speed of adjustment respectively—but do not otherwise change the aggregation argument.

We assume that the non-housing good costs the same in all locations and normalize its price to 1. The relative price of housing in a location is p_l. The budget constraint is then:

$$z_l = c_{lw} + p_l h_{lw}$$

where z_l is per-capita income in location l. We assume that remote and office workers earn the same income z_l. We extend the model to permanent income differences between remote and non-remote workers (Pabilonia and Vernon, 2022) and imperfect substitutability of remote and non-remote work (Davis, Ghent, and Gregory, 2024) in Appendix A2.2, and we show that our aggregation argument continues to hold.

Optimal per capita housing demand in location l by work-mode w is

$$h_{wl}^d = \frac{\theta_w z}{p_l},$$

which we aggregate to total housing demand,

$$H_l^d = N_l \left(s_{r|l} \frac{\theta_r z_l}{p_l} + (1 - s_{r|l}) \frac{\theta_b z_l}{p_l} \right).$$

Here N_l is total population in location l and $s_{r|l}$ is the share of remote workers in location l.

We are not taking a stance on the determinants of local population N_l, nor do we assume that it is exogenous. In this sense, our framework captures a wide range of spatial equilibrium models.

We assume the housing supply function in location l is given by,

$$H_l^s = \bar{H}_l p_l^{\phi}$$

where ϕ is the common housing supply elasticity and $\bar{H}_l$ is a housing supply shifter. A uniform supply elasticity is consistent with the evidence in Louie, Mondragon, and Wieland (2025). In Appendix A2.5 we allow for heterogeneous supply elasticities and show that our IV estimate is a sufficient statistic for a lower bound on the true aggregate effect.[19]

Equating housing demand and supply yields the equilibrium housing price

$$p_l = \left[\frac{N_l}{\bar{H}_l} \left(s_{r|l} \theta_r + (1 - s_{r|l}) \theta_b \right) z_l \right]^{\frac{1}{1+\phi}}.$$

4.3 Aggregation and the IV Estimate as a Sufficient Statistic

Denote by x' the value of variable x after a series of shocks, including a remote work shock. We denote the change in the variable by $\Delta x = x' - x$. Then the growth rate of house prices

[19] Intuitively, areas with large remote work shares are measured to be relatively more elastic, which is picked up by the IV estimator. However, a given remote worker is more likely to live in areas measured to be less elastic location due to their larger population size, which determines the aggregate effect. Since the weighted supply elasticity for the IV estimator is higher than what the weighted supply elasticity for aggregation, the price effects we measure are a lower bound.

from before to after the shocks is

$$\begin{aligned}\Delta \ln p_l &= \frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{s_{r|l}\theta_r+(1-s_{r|l})\theta_b}\Delta s_{r|l} + \frac{1}{1+\phi}\Delta \ln N_l - \frac{1}{1+\phi}\Delta \ln \bar{H}_l + \frac{1}{1+\phi}\Delta \ln z_l \\ &\approx \frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}\Delta s_{r|l} + \frac{1}{1+\phi}\Delta \ln N_l + -\frac{1}{1+\phi}\Delta \ln \bar{H}_l + \frac{1}{1+\phi}\Delta \ln z_l\end{aligned}$$

House prices increase with an increase in the share of remote workers ($\Delta s_{r|l} > 0$), in-migration ($\Delta \ln N_l > 0$), a negative shock to housing supply ($\Delta \ln \bar{H}_l < 0$), and an increase in income ($\Delta \ln z_l > 0$). The approximation in the second line follows from the fact that the initial remote work shares are small. In Appendix A2.1 we show that the approximation error is small.[20]

Our objective is to measure the aggregate effect of remote work from the increased demand for home space:

$$G_{\text{true}} = \sum_l s_l \frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}\Delta s_{r|l} = \left[\frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}\right]\Delta s_r \tag{5}$$

where s_l is the population weight of location l. The aggregate change in remote work $\Delta s_r = \sum_l s_l s_{r|l}$ is observable, but the term in square brackets is not directly observable.

In structural approaches, the elements of the bracketed term are directly calibrated. This requires evidence on the housing expenditure shares across work-modes, the housing supply elasticity, and, in our extensions, the elasticity of substitution across housing and non-housing goods, relative income of remote and non-remote workers, the elasticity of substitution between remote and non-remote work, the speed of housing adjustment, and the distribution of supply elasticities across locations and its correlation with other factors. We next show that our IV estimate holding fixed migration encodes this information as needed for aggregation.

Define differences from aggregate variables as $\tilde{x} = x_l - x$. Then, in the cross-section we estimate,

$$\Delta \ln \tilde{p}_l = \beta_1 \Delta \tilde{s}_{r|l} + \beta_2 \Delta \ln \tilde{N}_l + \epsilon_l$$

where $\beta_1 = \frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}$ is our coefficient of interest for aggregation, $\beta_2 = \frac{1}{1+\phi}$, and the error term is $\epsilon_l = -\frac{1}{1+\phi}\Delta \ln \tilde{\bar{H}}_l + \frac{1}{1+\phi}\Delta \ln \tilde{z}_l$. Below we discuss the model-implied advantages and disadvantages of extrapolating from the IV rather than the reduced form, and from estimating the first stage in levels or differences.

[20]Specifically, it is less than 0.15% of the aggregate effects of remote work G_{true} (defined in (5)).

We obtain β_1 through an IV using the local remote share $s_{r|l}$ as instrument. The first stage is,

$$\Delta s_{r|l} = c + (\lambda - 1)s_{r|l}, \tag{6}$$

where c captures a level shift and λ a proportional shift in remote work.

In Section 3 we argue that $s_{r|l}$ satisfies the exclusion restriction $Cov(s_{r|l}, \epsilon_l) = 0$. By also controlling for migration, our IV estimator yields

$$\begin{aligned}\beta_1^{IV} &= \frac{Cov(\Delta \ln \tilde{p}_l, \tilde{s}_{r|l} | \Delta \tilde{N}_l)}{Cov(\Delta \tilde{s}_{r|l}, \tilde{s}_{r|l} | \Delta \tilde{N}_l)} \\ &= \left[\frac{1}{1+\phi} \frac{(\theta_r - \theta_b)}{\theta_b}\right]\end{aligned}$$

which is the term needed for G_{true} in (5).

Extrapolating using β_1^{IV} therefore yields the correct answer for the aggregate effect of remote work:

$$G_{\text{IV}} = \beta_1^{IV} \Delta s_r = G_{\text{true}} \tag{7}$$

This completes the argument that our cross-sectional empirical strategy isolates a sufficient statistic for the aggregate effect of remote work on house prices. Intuitively, remote work exposure in the cross-section captures both the increased demand for home space and induced migration, but only the former has an aggregate effect on house prices. So we cannot directly extrapolate from the cross-sectional estimate. But once we control for migration, then we isolate the increased the demand for home space and recover a sufficient statistic for aggregation. This argument makes no reference to the nature of housing demand. Consequently, it is straightforward to show that the aggregation argument extends unchanged to the cases with CES utility, dynamic housing demand, and heterogeneous income among remote and non-remote workers (Appendix A2).

4.4 Using the Model to Inform the Empirical Strategy

We next use the model structure to validate our empirical approach in Section 3. First, the model is informative about whether one should aggregate using the IV or the reduced form. Aggregating using the IV estimate is preferred to aggregating using the reduced form because the latter will not capture the aggregate effects of the level shift in remote work c. The extrapolation using the reduced form yields $G_{RF} = \left[\frac{1}{1+\phi}\frac{(\theta_r - \theta_b)}{\theta_b}\right](\Delta s_r - c) \leq G_{true}$

whenever $c \geq 0$. So while reduced form estimates are useful in checking the validity of an empirical design, their usefulness in extrapolating to aggregate effects will depend on the structure of the first stage relationship.

Second, we can use the model to clarify the implications of different specifications of the first stage and IV. In practice we run the first stage in levels, where we instrument for the 2023 level of remote work, instead of instrumenting for the change in remote work as in equation (6). The advantage of the level specification is that it allows for robust inference even when λ is close to one. When λ is close to one the second stage may become poorly behaved. This is because the IV estimator in the finite sample recovers the following (Wooldridge, 2002)[21]

$$\begin{aligned}\beta_1^{IV,Finite} &= \left[\frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}\right] + \frac{Cov(\epsilon_l, \tilde{s}_{r|l}|\Delta\tilde{N}_l)}{Cov(\Delta\tilde{s}_{r|l}, \tilde{s}_{r|l}|\Delta\tilde{N}_l)} \\ &= \left[\frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}\right] + \frac{Cov(\epsilon_l, \tilde{s}_{r|l}|\Delta\tilde{N}_l)}{\lambda-1}.\end{aligned}$$

While asymptotically the last term will go to zero with a valid instrument, it is possible that in any finite sample the denominator is small enough to significantly bias the resulting estimate. In practice, our estimates suggest $\lambda - 1 = 0.36$ (see column (4) of Table A12), implying that any finite sample bias is being inflated by a factor of three.

By contrast, when we estimate the specification in levels we recover

$$\beta_1^{IV,lvls,Finite} = \beta_2\frac{\lambda-1}{\lambda} + \frac{Cov(\Delta\epsilon_l, \tilde{s}_{r|l}|\Delta\tilde{N}_l)}{\lambda}$$

and extrapolate using $\beta_1^{IV,lvls}s_r'$. With a valid instrument the bias from the second term will be small so long as λ is not very small. Since $\lambda = 1.36$, the finite sample bias term is being reduced when estimated in levels, as opposed to being magnified when estimated in differences. While we no longer recover exactly β_1, the quantity we do recover is a sufficient statistic for a lower bound on the aggregate effect of remote work whenever $c \geq 0$. Specifically, the extrapolation using the level specification yields $G_{IV,lvls} = \left[\frac{1}{1+\phi}\frac{(\theta_r-\theta_b)}{\theta_b}\right]\left(\Delta s_r - \frac{c}{\lambda}\right) \leq G_{true}$ whenever $c \geq 0$. Because we are dividing the bias term $c = 0.068$, by $\lambda = 1.36$, this lower bound is tight and suggests that the true aggregate effect is about five percentage points larger than our lower bound estimate of 11.9 percentage points. This calculation puts us close to explaining almost the entire aggregate growth in real house prices from 2019-2023.

[21]Note that this is not a problem of the instrument being invalid, but simply a reflection that in finite samples the correlation between the residual and the instrument may not be exactly zero.

4.5 Discussion

Our approach yields estimates of the aggregate effects of remote work on housing demand with minimal assumptions on the model structure. In particular, we do not need to take a stance on the exact tradeoffs governing migration decisions or the cause of the remote work shift, and we can capture a broad range of housing demand functions. A disadvantage of our approach is that we cannot analyze counterfactuals beyond the one we identify in the data nor can we discuss the welfare implications of remote work.[22] In this sense, our work is distinct from and complementary to structural approaches where deeper assumptions about structural parameters and relationships are made and which facilitate a broader set of counterfactual calculations and welfare analysis (Behrens, Kichko, and Thisse, 2024; Brueckner, Kahn, and Lin, 2023; Brueckner, 2025; Davis, Ghent, and Gregory, 2024; Delventhal, Kwon, and Parkhomenko, 2021; Delventhal and Parkhomenko, 2024; Duranton and Handbury, 2023; Gamber, Graham, and Yadav, 2023; Howard, Liebersohn, and Ozimek, 2023; Kyriakopoulou and Picard, 2023; Monte, Porcher, and Rossi-Hansberg, 2023; Richard, 2024).

Our estimates of the aggregate effect of remote work on house prices in 2023 are in between the scenarios in Davis, Ghent, and Gregory (2024) (13-29%) and Delventhal and Parkhomenko (2024) (16%) and those reported by Gamber, Graham, and Yadav (2023) (3%) and Howard, Liebersohn, and Ozimek (2023) (1.5-7%).[23] To the extent that our estimate is a lower bound, it is consistent with and provides external validation to the first set of models.

Our framework also yields additional insights into when one can directly aggregate from the cross-section in this manner. Aggregation from the cross-section is challenging whenever spillovers are present because the untreated regions do not measure a "no-shock" counterfactual (Chodorow-Reich, 2020). In our setting we are able to directly control for spillovers on the demand side. And since housing is a non-traded good, there are no spillovers on the supply side. Therefore our estimates do correspond to the the "no-shock" counterfactual and aggregate. In contrast, in settings with tradable goods part of local supply may come

[22]Intuitively, the specific aggregate effect we quantify reflects the constellation of correlations between housing demand, remote work, and housing supply relevant at that particular moment. A reversal in remote work would likely differ in these correlations and so alter the aggregate effect, requiring re-estimation with additional identifying variation.

[23]The range for Davis, Ghent, and Gregory (2024) is based on their short-run scenario and their "most likely" long-run scenario with Baum-Snow and Han (2024) supply elasticities; for Delventhal and Parkhomenko (2024) it is based on their short-run scenario with fixed housing supply; and for Gamber, Graham, and Yadav (2023) includes all time spent at home and is thus an upper bound for the remote work effect.

from reduced supply in other regions.[24] In these settings, spillovers on the supply side must also be controlled for.

5 Conclusion

We show that the shift to remote work caused a large increase in per capita housing demand. In turn, this increase in housing demand caused house prices and rents to increase sharply and persistently. Based on our cross-sectional estimates controlling for migration spillovers, we argue that remote work accounts for at least one-half of the 18.9 percent increase in house prices from December 2019 to December 2023. While remote work also facilitated migration across cities and this migration was correlated with house price growth, the majority of the effect of remote work on house prices across CBSAs is due to the direct effects of the shift in per capita demand. Our results suggest that the increase in house prices over this period largely reflects fundamentals rather than a speculative bubble, and that lower interest rates and fiscal stimulus were less important.

Our results also imply that the future path of housing costs may depend critically on the path of remote work. If remote work reverses, then there may be a general reversal in housing demand and potentially house prices, although the adjustment my occur only in real terms and not require a fall in nominal prices. If remote work persists or even expands, then we may expect important repercussions as increased housing costs feed into inflation and so affect the response of monetary policy. Given the macroeconomic importance of either outcome, policy makers need to pay close attention to the future evolution of remote work.

Finally, we have shown how cross-sectional effects, in certain situations, may be directly informative about aggregate effects if researchers are able to directly control for spillovers that might otherwise complicate the aggregation (Chodorow-Reich, 2020). This approach combines the sharp identification that is available with cross-sectional data with minimal structure in which the cross-sectional estimate, after controlling for spillovers, is a sufficient statistic for aggregation.

[24] To see this, consider an economy with N cities that each have demand curves for a perfectly tradable good $\widehat{q}_i = a_i - b\widehat{p}$ so that they all face the same price (hats indicate percentage changes). The good has an aggregate supply curve $\frac{1}{N}\sum_i \widehat{q}_i = \widehat{Q} = \psi\widehat{p}$. In this case the perfectly identified estimates of local supply elasticities (normalized to the correct scale) give $\frac{\partial \widehat{q}_i}{\partial a_i}\frac{1}{N}/\frac{\partial \widehat{p}}{\partial a_i} = \psi + b\left(1 - \frac{1}{N}\right)$. With just one location the local and aggregate supply curves are identical, but as the number of locations increases the local supply curve also reflects trade with other locations, which is pinned down by the price elasticity of demand.

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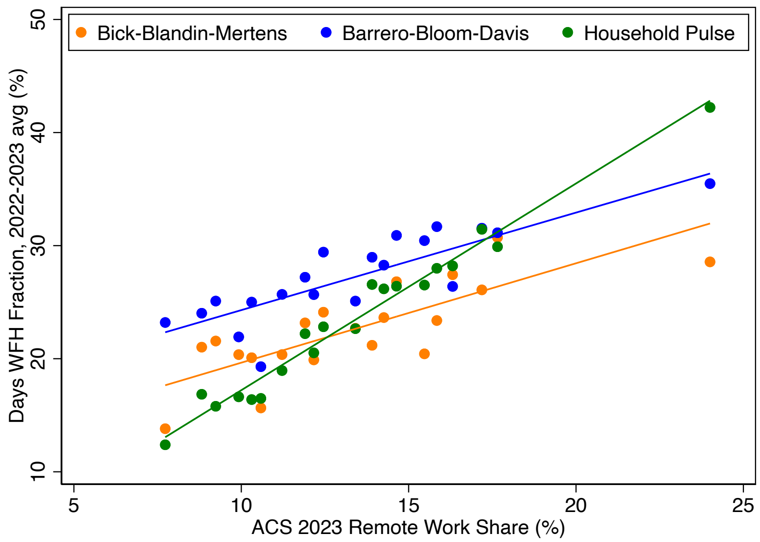
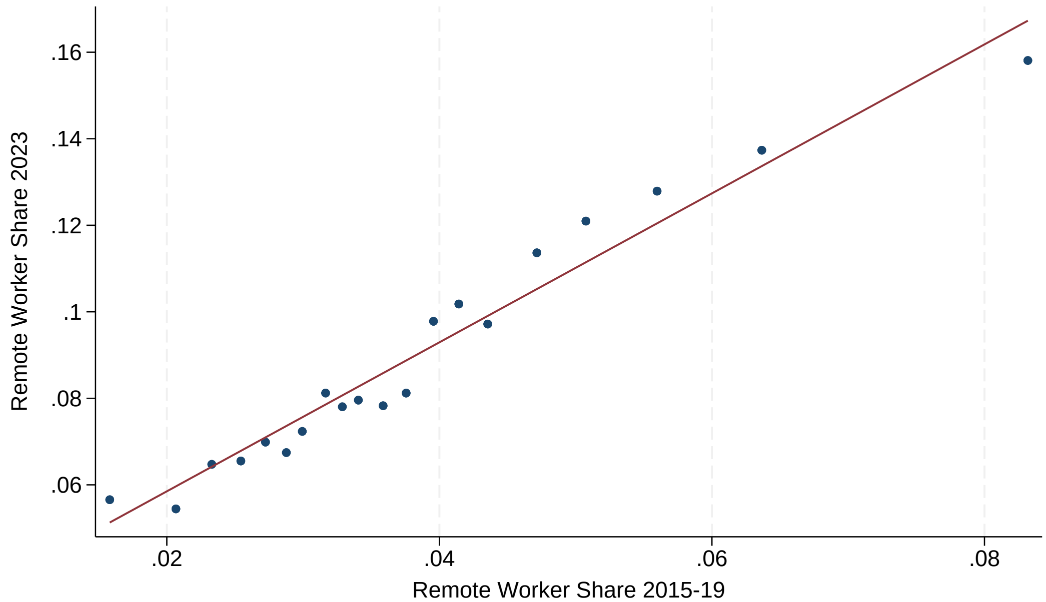
6 Figures

FIGURE 1
State-level Comparison of Survey Measures of Remote Work



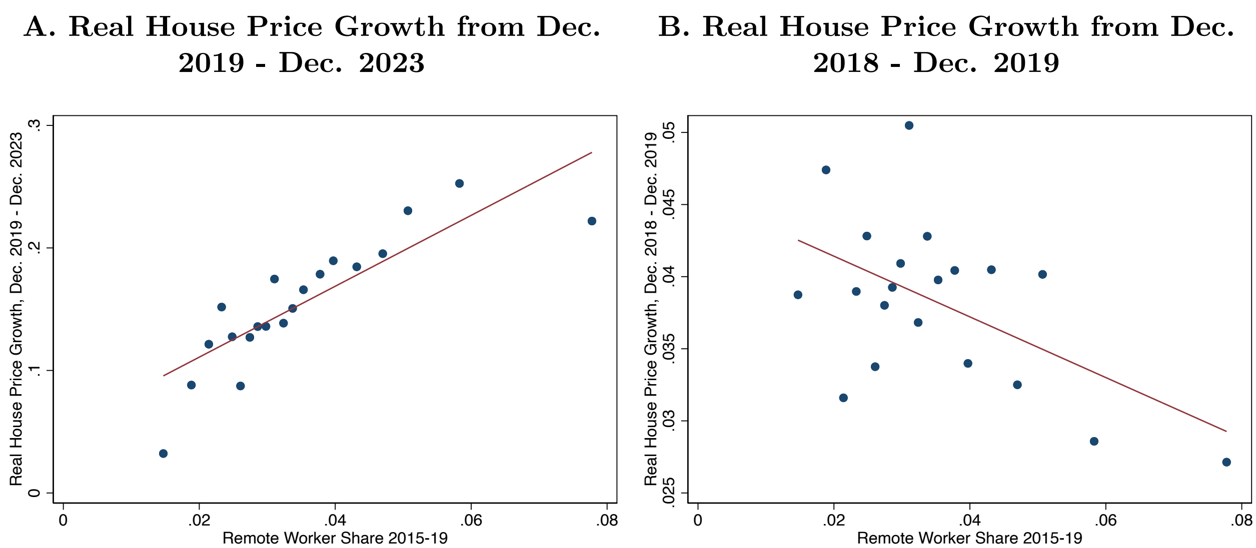
Sources: American Community Survey, Barrero, Bloom, and Davis (2021), Bick, Blandin, and Mertens (2023), Census Household Pulse Survey, and authors' calculations.

FIGURE 2
Binned Scatter Plot of Remote Worker Share 2023 on Remote Worker Share 2015-2019



Sources: American Community Survey and authors' calculations.

FIGURE 3
Binned Scatter Plot of House Price Growth on Remote Worker Share 2015-19



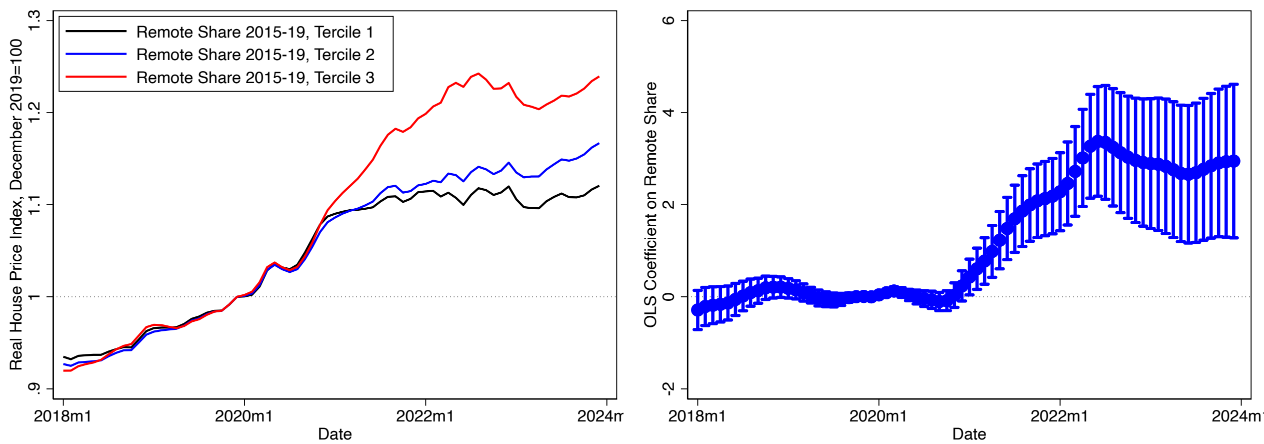
Sources: Zillow, American Community Survey, and authors' calculations.

FIGURE 4

Event Analysis of House Price Growth on Remote Worker Share 2015-19

A. Event Analysis of House Price Growth on Remote Worker Share 2015-2019

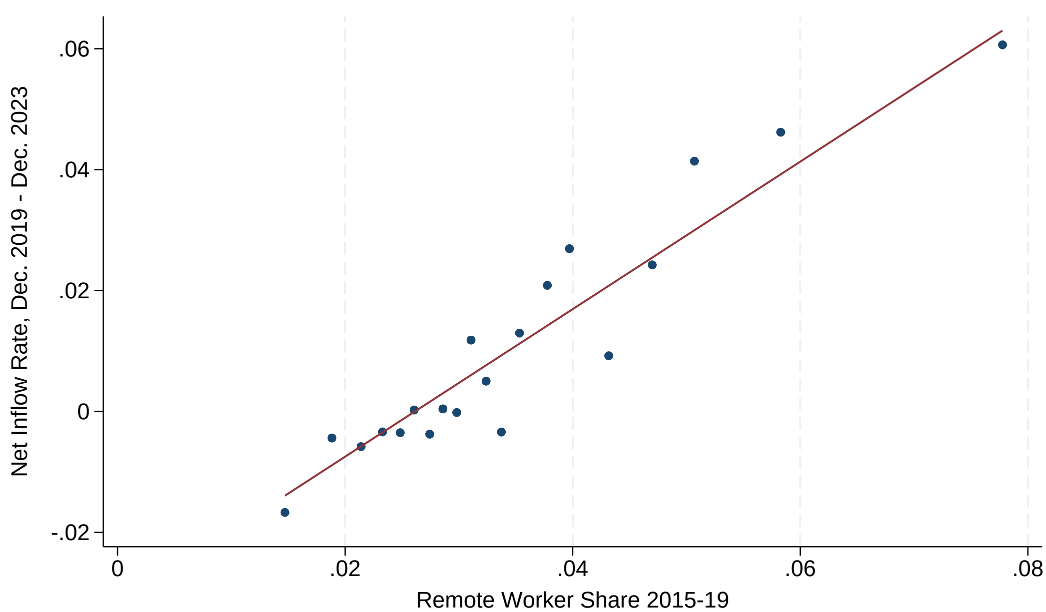
B. Regression Event Analysis of House Price Growth on Remote Worker Share 2015-2019



Sources: Zillow, American Community Survey, and authors' calculations.

FIGURE 5

Binned Scatter Plot of Net Inflow Rate on Remote Worker Share 2015-2019



Sources: American Community Survey and authors' calculations.

7 Tables

TABLE 1
SUMMARY STATISTICS

	Mean	Weighted Mean	SD	Min	Max	N
Housing Demand						
Real House Price Growth	0.154	0.167	0.124	−0.414	0.431	893
HP Growth Pre-Pandemic	0.038	0.023	0.031	−0.113	0.150	893
Rent Growth (Zillow)	0.141	0.123	0.073	−0.163	0.378	237
Rent Growth Pre-Pandemic	0.022	0.021	0.017	−0.054	0.064	233
Remote Worker Share 2015-19	0.035	0.045	0.015	0.008	0.126	893
Remote Worker Share 2023	0.084	0.129	0.036	0.018	0.235	893
Control Variables						
Net Inflow Rate Pandemic	0.011	−0.001	0.045	−0.117	0.446	893
Net Inflow Rate Pre-Pandemic	0.000	−0.000	0.012	−0.047	0.072	893
Log Density	3.710	5.832	1.250	−0.390	8.825	893
Δ Unemp. Rate 12/2019-12/2023	−0.000	0.003	0.009	−0.052	0.043	893
Δ Unemp. Rate 2019-2020	0.034	0.046	0.015	0.003	0.155	893
Unemp. Rate 2019	0.040	0.036	0.014	0.017	0.207	893
Pred. Wage Growth Pandemic	0.130	0.128	0.005	0.102	0.145	893
Pred. Wage Growth Pre-Pandemic	0.037	0.037	0.002	0.028	0.043	893
Log Total Dividends / AGI	−3.531	−3.220	0.476	−5.917	−1.189	893
Log Income / Filer	4.028	4.281	0.212	3.507	5.349	893
Stimulus / Filer	4.879	4.340	0.469	1.660	6.039	893
DTI 2019	35.043	36.043	1.764	28.627	40.897	893
Share White	0.736	0.595	0.187	0.035	0.967	893
Share Black	0.088	0.130	0.119	0.002	0.711	893
Share Asian	0.020	0.062	0.037	0.001	0.505	893
Share Hispanic	0.120	0.186	0.156	0.006	0.954	893
Share College	0.168	0.238	0.058	0.061	0.405	893
Log Median Income	10.925	11.103	0.185	10.364	11.721	893
Share 65+	0.176	0.153	0.036	0.077	0.461	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.
Notes: Weighted mean is weighted by average 2015-19 CBSA employment.

TABLE 2

Sources of Pre-Pandemic Remote Work Variation

Dependent Variable:	Remote Worker Share 2015-19
	(1)
RHS variables:	
Predicted Remote Worker Share 2015-19	1.97***
	(0.20)
Share College	0.0096
	(0.021)
Log Median Income	0.0034
	(0.0043)
Unemp. Rate 2019	−0.014
	(0.041)
Pred. Wage Growth Pre-Pandemic	0.90**
	(0.36)
Log Total Dividends / AGI	0.0016
	(0.0011)
Log Income / Filer	−0.00054
	(0.0030)
Stimulus / Filer	0.0015
	(0.0015)
DTI 2019	0.00085***
	(0.00030)
Log Density	−0.0016*
	(0.00078)
Share 65+	0.089***
	(0.018)
January Temperature	0.00027***
	(0.000069)
July Temperature	−0.00053***
	(0.000096)
July Humidity	−0.00022***
	(0.000067)
Race Controls	Yes
CBSA Clusters	50
R^2	0.69
Observations	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.
Notes: The dependent variable is the average share of remote workers from 2015-19 in a CBSA. The predicted remote worker share is based on the local occupation distribution and the national propensity for remote work in each occupation. Race controls include the population shares identifying as white, black, asian, and hispanic respectively. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE 3
First Stage for Real House Price Growth Regressions

Dependent Variable:	Remote Worker Share 2023			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.79***	1.65***	1.65***	1.48***
	(0.10)	(0.082)	(0.087)	(0.077)
HP Growth Pre-Pandemic		−0.014	−0.0076	0.033
		(0.022)	(0.022)	(0.027)
Density & Demographic Controls	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.55	0.68	0.68	0.72
Observations	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: This table reports a first stage regression. The dependent variables is the remote worker share 2023. The instrument is the average share of remote workers from 2015-19 in a CBSA. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 4

Effect of Remote Work on Real House Price Growth, Dec. 2019 - Dec. 2023

Dependent Variable:	Real House Price Growth, Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	2.89^{***}	2.64^{***}	2.54^{***}	2.55^{***}				
	(0.81)	(0.62)	(0.48)	(0.59)				
Remote Worker Share 2023					1.61^{***}	1.60^{***}	1.54^{***}	1.72^{***}
					(0.40)	(0.35)	(0.27)	(0.35)
HP Growth Pre-Pandemic		0.30	0.29	0.16		0.32	0.30	0.11
		(0.21)	(0.21)	(0.19)		(0.20)	(0.19)	(0.16)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					295.64	406.03	364.37	365.20
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.12	0.24	0.24	0.34	0.12	0.22	0.23	0.34
Observations	893	893	893	893	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is real house price growth in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE 5

Effect of Remote Work on Broader Outcomes, Dec. 2019 - Dec. 2023

	Coeff.	S.E.	F-Stat	N
Real House Prices and Rents				
(1) CoreLogic House Price Growth	1.45***	(0.27)	395.2	893
(2) Rent Growth (Zillow)	0.37**	(0.17)	122.9	233
(3) House Price Growth (Zillow Rent Sample)	1.29***	(0.29)	124.6	233
(4) Rent Growth (CoreLogic)	0.87***	(0.23)	48.5	80
(5) House Price Growth (CoreLogic Rent Sample)	1.16***	(0.30)	48.1	80
Real Commercial Rent (Reduced Form)				
(6) Commercial Rent Growth	−0.21	(0.22)		24
(7) House Price Growth (Com. Rent Sample)	2.51*	(1.46)		24
Local Inflation (Reduced Form)				
(8) Inflation	1.39*	(0.76)		22
(9) Inflation ex. Shelter	0.41	(0.33)		22
(10) House Price Growth (Inflation Sample)	2.96*	(1.58)		22
Housing Supply				
(11) Cumulative Permit Growth	1.99**	(0.87)	323.6	843

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is indicated in the table row. The IV specifications in rows (1)-(5) and (11) include the full set of controls from column (8) in Table 4 with pre-pandemic house price growth replaced with the lagged dependent variable. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. The reduced form specifications in rows (6)-(10) include the lagged dependent variable as control. For the reduced form specifications robust standard errors are given in parenthesis.

TABLE 6

Effect of Remote Work on Real House Price Growth, Dec. 2019 - Dec. 2023

Dependent Variable:	Real House Price Growth, Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.46**	1.22**	1.44***	1.25**				
	(0.63)	(0.53)	(0.50)	(0.50)				
Remote Worker Share 2023					0.83**	0.78**	1.05***	0.92***
					(0.34)	(0.33)	(0.34)	(0.34)
HP Growth Pre-Pandemic		0.024	−0.022	−0.063		0.048	−0.037	−0.064
		(0.17)	(0.17)	(0.16)		(0.17)	(0.15)	(0.14)
Net Inflow Rate Pandemic	1.19***	1.08***	1.02***		1.17***	1.04***	0.91***	
	(0.23)	(0.22)	(0.26)		(0.22)	(0.21)	(0.25)	
Net Inflow Rate Pre-Pandemic	−0.071	0.26	0.77*		−0.11	0.22	0.70	
	(0.69)	(0.50)	(0.45)		(0.67)	(0.50)	(0.45)	
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	Yes	Yes	No	No	Yes	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	Yes	Yes	No	No	Yes	Yes
Nonparametric Migration Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					281.77	436.93	399.14	434.12
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.27	0.36	0.43	0.47	0.29	0.35	0.42	0.47
Observations	893	893	893	893	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is real house price growth in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Nonparametric migration controls include deciles of pandemic net migration and pre-pandemic net migration. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE 7

Effect of Remote Work on House Price Growth by Number of Bedrooms, Dec. 2019 - Dec. 2023

	Coeff.	S.E.	F-Stat	N
Real House Price Growth				
(1) 1 Bedroom	0.72	(0.47)	328.6	610
(2) 2 Bedroom	0.67*	(0.36)	328.6	610
(3) 3 Bedroom	0.68**	(0.30)	328.6	610
(4) 4 Bedroom	0.83***	(0.31)	328.6	610
(5) 5 Bedroom	0.98***	(0.31)	328.6	610

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.
Notes: The dependent variable is indicated in the table row. All specifications include the full set of controls from column (7) in Table 6 with pre-pandemic house price growth replaced with the lagged dependent variable. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population.

Online Appendix

A1 Regression Decomposition

Here we work out the exact assumptions justifying the regression specification including both remote work and net migration in a simplified setting. Let H stand for house price growth, M be net migration, and RW be remote work. Assume that the true relationship between these variables is given as

$$H = \beta_1 RW + \beta_2 M + \epsilon_1.$$

We assume RW is uncorrelated with the error ϵ_1.

To isolate the remote work effect, we propose estimating a regression that controls for net migration. Let $P = M(M'M)^{-1}M'$ be the annihilator matrix for migration. Then, the OLS estimate for β_1 is,

$$\begin{aligned}
\beta_1^{OLS} &= [RW'(I-P)RW]^{-1}[RW'(I-P)H] \\
&= [RW'(I-P)RW]^{-1}[RW'(I-P)(\beta_1 RW + \beta_2 M + \epsilon_1)] \\
&= \beta_1 + [RW'(I-P)RW]^{-1}[RW'(I-P)\epsilon_1] \\
&\xrightarrow{p} \beta_1 - [E(RW'(I-P)RW)]^{-1}[E(RW'M)E(M'M)^{-1}E(M'\epsilon_1)].
\end{aligned}$$

Line 1 uses $(I-P)'(I-P) = (I-P)$ and line 3 substitutes $(I-P)M = 0$. Line 4 uses Slutsky theorem and $E(RW'\epsilon_1) = 0$.

Empirically, we observe $E(RW'M) > 0$. OLS will then identify β_1 exactly if migration is uncorrelated with house price shocks. If instead migration is positively correlated with house price shocks—the more likely empirical case—then $E(M'\epsilon_1) > 0$ and β_1^{OLS} will be biased downward.

A2 Model Appendix

A2.1 Validating the Approximation

The aggregate effect of remote work is now

$$G_{\text{true}} \equiv \sum_l s_l \frac{1}{1+\phi} \frac{(\theta_r - \theta_b)}{s_{r|l}\theta_r + (1-s_{r|l})\theta_b} \Delta s_{r|l} = \frac{\theta_r - \theta_b}{1+\phi} \sum_l s_l \frac{\Delta s_{r|l}}{d_l}$$

where $d_l = s_{r|l}\theta_r + (1-s_{r|l})\theta_b$ is the local housing expenditure share.

Using $\Delta s_{r|l} = \Delta \tilde{s}_{r|l} + \Delta s_r$ we get,

$$\begin{aligned}
G_{\text{true}} &= \frac{\theta_r - \theta_b}{1+\phi} \left[\sum_l s_l \frac{\Delta \tilde{s}_{r|l}}{d_l} + \left(\sum_l s_l \frac{1}{d_l} \right) \Delta s_r \right] \\
&= \frac{\theta_r - \theta_b}{1+\phi} \left[E_s \left(\frac{\Delta \tilde{s}_{r|l}}{d_l} \right) + E_s \left(\frac{1}{d_l} \right) \Delta s_r \right] \\
&= \frac{\theta_r - \theta_b}{1+\phi} \left[Cov \left(\Delta \tilde{s}_{r|l}, \frac{1}{d_l} \right) + E_s \left(\frac{1}{d_l} \right) \Delta s_r \right] \\
&= \frac{\theta_r - \theta_b}{1+\phi} \left[(\lambda - 1) Cov \left(\tilde{s}_{r|l}, \frac{1}{d_l} \right) + E_s \left(\frac{1}{d_l} \right) \Delta s_r \right]
\end{aligned}$$

where $E_s(x) = \sum_l s_l x_l$ is a population weighted mean and the last line follows from the first stage.

In the cross-section we estimate:

$$\Delta \ln \tilde{p}_l = \beta_{1l} \Delta \tilde{s}_{r|l} + \beta_{2l} \ln \tilde{N}_l + \epsilon_l$$

where $\beta_{1l} = \frac{1}{1+\phi}\frac{(\theta_r - \theta_b)}{d_l}$, $\beta_{2l} = \frac{1}{1+\phi}\Delta$, and $\epsilon_l = -\frac{1}{1+\phi}\Delta \ln \tilde{\tilde{H}}_l + \frac{1}{1+\phi}\Delta \ln \tilde{z}_l$

The IV coefficient conditional on controls is

$$\beta_1^{IV} = \frac{\theta_r - \theta_b}{1+\phi} \frac{Cov\left(\frac{\Delta \tilde{s}_{r|l}}{d_l}, \tilde{s}_{r|l}\right)}{Cov\left(\Delta \tilde{s}_{r|l}, \tilde{s}_{r|l}\right)}$$

From the first stage we get

$$\begin{aligned}
Cov \left(\frac{\Delta \tilde{s}_{r|l}}{d_l}, \tilde{s}_{r|l} \right) &= (\lambda - 1) Cov \left(\frac{\tilde{s}_{r|l}}{d_l}, \tilde{s}_{r|l} \right) \\
Cov \left(\Delta \tilde{s}_{r|l}, \tilde{s}_{r|l} \right) &= (\lambda - 1) Var \left(\tilde{s}_{r|l} \right)
\end{aligned}$$

Therefore, the covariance can be expressed as

$$\frac{Cov\left(\frac{\Delta\tilde{s}_{r|l}}{d_l}, \tilde{s}_{r|l}\right)}{Cov\left(\Delta\tilde{s}_{r|l}, \tilde{s}_{r|l}\right)} = \frac{Cov\left(\frac{\tilde{s}_{r|l}}{d_l}, \tilde{s}_{r|l}\right)}{Var\left(\tilde{s}_{r|l}\right)} = \sum_l \underbrace{\frac{\tilde{s}^2_{r|l}}{\sum_j \tilde{s}^2_{rj}}}_{\equiv\omega_l} \frac{1}{d_l} = E_\omega\left(\frac{1}{d_l}\right)$$

where $E_\omega(x) = \sum_l \omega_l x_l$.

The extrapolated effect is

$$\begin{aligned} G_{\text{IV}} &\equiv \beta_1^{IV}\Delta s_r \\ &= \frac{\theta_r - \theta_b}{1+\phi} E_\omega\left(\frac{1}{d_l}\right)\Delta s_r \end{aligned}$$

The difference in the true and extrapolated effect is

$$G_{\text{true}} - G_{\text{IV}} = \frac{\theta_r - \theta_b}{1+\phi}\left[(\lambda - 1)Cov_s\left(\tilde{s}_{r|l}, \frac{1}{d_l}\right) + \left\{E_s\left(\frac{1}{d_l}\right) - E_\omega\left(\frac{1}{d_l}\right)\right\}\Delta s_r\right]$$

Write $d_l = \theta_b + \delta_l$ with $\delta_l = (\theta_r - \theta_b)s_{r|l} \leq (\theta_r - \theta_b)\max_l\{s_{r|l}\} = |\delta_l|$. Using a first order Taylor expansion we get,

$$\frac{1}{d_{rl}} = \frac{1}{\theta_b}\left[1 - \frac{\delta_l}{\theta_b} + O\left(\frac{\delta_l^2}{\theta_b^2}\right)\right]$$

Therefore the first term can be written as

$$Cov_s\left(\tilde{s}_{r|l}, \frac{1}{d_l}\right) = -\frac{\theta_r - \theta_b}{\theta_b^2}Var_s\left(s_{r|l}\right) + O\left(\frac{(\theta_r - \theta_b)^2}{\theta_b^3}Var_s\left(s_{r|l}\right)^{\frac{3}{2}}\right)$$

And the second term is

$$\begin{aligned} E_s\left(\frac{1}{d_l}\right) - E_\omega\left(\frac{1}{d_l}\right) &\approx -\frac{1}{\theta_b^2}\left[E_s\left(\delta_l\right) - E_\omega\left(\delta_l\right)\right] + \frac{1}{\theta_b^3}\left[E_s\left(\delta_l^2\right) - E_\omega\left(\delta_l^2\right)\right] \\ &= -\frac{\theta_r - \theta_b}{\theta_b^2}\left[E_s\left(s_{r|l}\right) - E_\omega\left(s_{r|l}\right)\right] + \frac{(\theta_r - \theta_b)^2}{\theta_b^3}O\left(s^2_{r|l}\right) \\ &= -\frac{\theta_r - \theta_b}{\theta_b^2}\left[s_r - E_\omega\left(s_{r|l}\right)\right] + \frac{(\theta_r - \theta_b)^2}{\theta_b^3}O\left(s^2_{r|l}\right) \\ &= \frac{\theta_r - \theta_b}{\theta_b^2}E_\omega\left(\tilde{s}_{r|l}\right) + \frac{(\theta_r - \theta_b)^2}{\theta_b^3}O\left(s^2_{r|l}\right) \\ &= \frac{\theta_r - \theta_b}{\theta_b^2}Skew(\tilde{s}_{r|l})\sqrt{Var(\tilde{s}_{r|l})} + \frac{(\theta_r - \theta_b)^2}{\theta_b^3}O\left(s^2_{r|l}\right) \end{aligned}$$

The error is then bounded by

$$\frac{|G_{\text{true}} - G_{\text{IV}}|}{G_{\text{true}}} \leq \frac{\theta_r - \theta_b}{\theta_b} \left| (\lambda - 1) Var_s\left(s_{r|l}\right) + Skew(\tilde{s}_{r|l})\sqrt{Var(s_{r|l})}\Delta s_r \right|$$

Using the highest estimates from Stanton-Tiwari we get $\frac{\theta_r - \theta_b}{\theta_b} \leq 0.2$. In the data, $Var_s(s_{r|l}) = 0.013^2$, $Var(s_{r|l}) = 0.015^2$ and $Skew(s_{r|l}) = 1.59$ from 2015-2019. From the first stage we get $|\lambda - 1| < 1$ and survey evidence suggests $\Delta s_r \leq 0.3$. Plugging these values into the formula we get an upper bound on the error of 0.15%.

$$\frac{|G_{\text{true}} - G_{\text{IV}}|}{G_{\text{true}}} \leq 0.2 \times (1 \times 0.013^2 + 0.015 \times 1.59 \times 0.3) = 0.0014648 = 0.14648\%.$$

Intuitively, because remote shares are small before the remote work shock, the variation in initial housing expenditure share d_l induced by remote work is small as well. Hence, one can treat $d_l \approx \theta_b$.

A2.2 Variation 1: Different income for remote workers

We study two different versions with heterogeneous income. One in which remote workers have different permanent income (Pabilonia and Vernon, 2022). Another in which remote and non-remote work are imperfect substitutes (Davis, Ghent, and Gregory, 2024).

Remote workers have different average income

There are two types of workers, high productivity and low productivity. Denote their productivities by $z_H z_l$ and $z_L z_l$, with $z_H \geq 1$ and $z_L \leq 1$. Only high productivity workers have the option to be remote.

Total housing demand is

$$H_l^d = N_l \left(s_{r|l} \frac{\theta_r z_H z_l}{p_l} + (1 - s_{r|l} - s_{Ll}) \frac{\theta_b z_H z_l}{p_l} + s_{Ll} \frac{\theta_b z_L z_l}{p_l} \right)$$

and the equilibrium housing price is

$$p_l = \left[\frac{N_l}{\bar{H}_l} \left(s_{r|l}\theta_r z_H + (1 - s_{r|l} - s_{Ll})\theta_b z_H + s_{Ll}\theta_b z_L \right) z_l \right]^{\frac{1}{1+\phi}}$$

The gross growth rate of house prices across periods is

$$\frac{p_l'}{p_l} = \left[\frac{N_l'}{N_l} \frac{\bar{H}_l}{\bar{H}_l'} \frac{s_{r|l}'\theta_r z_H + (1 - s_{r|l}' - s_{Ll}')\theta_b z_H + s_{Ll}'\theta_b z_L}{s_{r|l}\theta_r z_H + (1 - s_{r|l} - s_{Ll})\theta_b z_H + s_{Ll}\theta_b z_L} \frac{z_l'}{z_l} \right]^{\frac{1}{1+\phi}}$$

In logs, using Δ to denote the difference between periods:

$$\begin{aligned}
\Delta \ln p_l &= \frac{1}{1+\phi} \ln \left(\frac{s_{r|l}'\theta_r z_H + (1 - s_{r|l}' - s_{Ll}')\theta_b z_H + s_{Ll}'\theta_b z_L}{s_{r|l}\theta_r z_H + (1 - s_{r|l} - s_{Ll})\theta_b z_H + s_{Ll}\theta_b z_L} \right) + \frac{1}{1+\phi}\Delta \ln N_l - \frac{1}{1+\phi}\Delta \ln \bar{H}_l + \frac{1}{1+\phi}\Delta \ln \\
&= \frac{1}{1+\phi} \frac{(\theta_r - \theta_b) z_H}{s_{r|l}\theta_r + (1 - s_{r|l})\theta_b} \Delta s_{r|l} + \frac{1}{1+\phi} \frac{\theta_b (z_L - z_H)}{s_{r|l}\theta_r + (1 - s_{r|l})\theta_b} \Delta s_{Ll} + \frac{1}{1+\phi}\Delta \ln N_l - \frac{1}{1+\phi}\Delta \ln \bar{H}_l + \\
&\approx \underbrace{\frac{1}{1+\phi} \frac{(\theta_r - \theta_b) z_H}{\theta_b}}_{\equiv \beta_1} \Delta s_{r|l} + \frac{z_L - z_H}{1+\phi} \Delta s_{Ll} + \frac{1}{1+\phi}\Delta \ln N_l - \frac{1}{1+\phi}\Delta \ln \bar{H}_l + \frac{1}{1+\phi}\Delta \ln z_l
\end{aligned}$$

Nothing in the aggregation problem changes. The coefficient β_1 reflects the higher income of remote workers $z_H \geq z_L$. The error term in the regression contains an additional term from the composition of H and L workers.

Imperfect substitutability of remote work with office work

Following Davis, Ghent, and Gregory (2024) we assume that wages of remote and non-remote workers are a function of the overall amount of remote work in a location,

$$z_{r|l} = f(s_{r|l}) z_l, \qquad z_{b|l} = g(1 - s_{r|l}) z_l$$

In the case that remote work and office work are imperfect substitutes, we expect $f' < 0$ and $g' < 0$: more remote work reduces wages for remote workers and increases wages for non-remote workers. [25] On the other hand, complementarities in work-from-home adoption push towards $f' > 0$. We normalize $g(1) = 1$, so z_l is the wage in a location with zero remote work, but do not restrict the wage functions otherwise.

Total housing demand is then

$$H_l^d = N_l \left(s_{r|l}\theta_r f(s_{r|l}) + (1 - s_{r|l})\theta_b g(1 - s_{r|l}) \right) \frac{z_l}{p_l}$$

[25] In a competitive labor market wages equal marginal product, but we do not need to take a stance on the competitiveness of the labor market.

and the equilibrium housing price is

$$p_l = \left[\frac{N_l}{\bar{H}_l} \left(s_{r|l} \theta_r f(s_{r|l}) + (1 - s_{r|l}) \theta_b g(1 - s_{r|l}) \right) z_l \right]^{\frac{1}{1+\phi}}$$

The gross growth rate of house prices across periods is

$$\frac{p_l'}{p_l} = \left[\frac{N_l'}{N_l} \frac{\bar{H}_l}{\bar{H}_l'} \frac{s_{r|l}' \theta_r f(s_{r|l}') + (1 - s_{r|l}') \theta_b g(1 - s_{r|l}')}{s_{r|l} \theta_r f(s_{r|l}) + (1 - s_{r|l}) \theta_b g(1 - s_{r|l})} \frac{z_l'}{z_l} \right]^{\frac{1}{1+\phi}}$$

In logs, using Δ to denote the difference between periods:

$$\begin{aligned}
\Delta \ln p_l &= \frac{1}{1+\phi} \ln \left(\frac{s_{r|l}' \theta_r f(s_{r|l}') + (1 - s_{r|l}') \theta_b g(1 - s_{r|l}')}{s_{r|l} \theta_r f(s_{r|l}) + (1 - s_{r|l}) \theta_b g(1 - s_{r|l})} \right) + \frac{1}{1+\phi} \Delta \ln N_l - \frac{1}{1+\phi} \Delta \ln \bar{H}_l + \frac{1}{1+\phi} \Delta \ln z_l \\
&\approx \frac{1}{1+\phi} \frac{\theta_r f(s_{r|l}) + s_{r|l} \theta_r f'(s_{r|l}) - \theta_b g(1 - s_{r|l}) - (1 - s_{r|l}) \theta_b g'(1 - s_{r|l})}{s_{r|l} \theta_r f(s_{r|l}) + (1 - s_{r|l}) \theta_b g(1 - s_{r|l})} \Delta s_{r|l} + \frac{1}{1+\phi} \Delta \ln N_l - \frac{1}{1+\phi} \\
&= \frac{1}{1+\phi} \frac{\theta_r f(s_{r|l})(1 + \epsilon_f) - \theta_b g(s_{r|l})(1 + \epsilon_g)}{s_{r|l} \theta_r f(s_{r|l}) + (1 - s_{r|l}) \theta_b g(s_{r|l})} \Delta s_{r|l} + \frac{1}{1+\phi} \Delta \ln N_l - \frac{1}{1+\phi} \Delta \ln \bar{H}_l + \frac{1}{1+\phi} \Delta \ln z_l \\
&\approx \underbrace{\frac{1}{1+\phi} \frac{\theta_r f(0)(1 + \epsilon_f) - \theta_b (1 + \epsilon_g)}{\theta_b}}_{\equiv \beta_1} \Delta s_{r|l} + \frac{1}{1+\phi} \Delta \ln N_l - \frac{1}{1+\phi} \Delta \ln \bar{H}_l + \frac{1}{1+\phi} \Delta \ln z_l
\end{aligned}$$

where $\epsilon_f = \frac{f'(s)s}{f(s)}$ and $\epsilon_g = \frac{g'(s)s}{g(s)}$ are the elasticities of the wage functions for remote and non-remote workers. The last line uses the approximation that initial remote shares are small and the normalization $g(1) = 1$.

As before β_1 encodes the necessary information for aggregation. In this setting, it encapsulates any initial remote work penalty ($f(0) \leq 1 = g(1)$) as well as any complementarities and substitutabilities in production as remote work increases through the elasticities of the wage functions ϵ_f and ϵ_g.

A2.3 Variation 2: CES Utility

The utility function is now,

$$U_{wl} = \left[(1 - \theta_w)^{\frac{1}{\zeta}} c_{lw}^{\frac{\zeta-1}{\zeta}} + \theta_w^{\frac{1}{\zeta}} h_{lw}^{\frac{\zeta-1}{\zeta}} \right]^{\frac{\zeta-1}{\zeta}}$$

Then housing demand in location l is

$$h_{rl}^d = \frac{\alpha_r(p_l)z}{p_l}$$
$$h_{bl}^d = \frac{\alpha_b(p_l)z}{p_l}$$

where $\alpha_w(p_l) = \frac{\theta_w p_l^{1-\zeta}}{1-\theta_w+\theta_w p_l^{1-\zeta}}$.

The implicit housing market equilibrium is

$$\bar{H}_l p_l^{1+\phi} = N_l \left(s_{r|l}\alpha_r(p_l) + (1-s_{r|l})\alpha_b(p_l)\right) z_l.$$

Define the expenditure share function $A(p,s) \equiv s\alpha_r(p) + (1-s)\alpha_{bl}(p_l)$.

We totally differentiate this expression around $p_l = 1$ to get

$$(1+\phi+\eta)\Delta \ln p_l \approx \frac{\theta_r - \theta_b}{\theta_b}\Delta s_{r|l} + \Delta \ln N_l - \Delta \ln \bar{H}_l + \Delta \ln z_l$$

where

$$\eta = \frac{\partial \ln A(p,s)}{\partial \ln p}\Big|_{p=1,s=0} = (1-\zeta)(1-\theta_b)$$

Then the aggregation argument proceeds unchanged from here.

Note that our baseline model assumed $\zeta = 1$ and therefore $\eta = 0$. Thus, the CES utility introduces an additional parameter—the elasticity of the expenditure share with respect to the house price η—but does not otherwise affect the aggregation logic of the baseline model.

A2.4 Variation 3: Dynamic Housing Demand

The desired (long-run) stock for work-mode $w \in \{r, b\}$ in location l at time t is

$$h_{wlt}^{*,d} = \frac{\theta_w z_{lt}}{p_{lt}},$$

identical to the static model. Actual holdings adjust sluggishly:

$$h_{wlt} = (1-\delta_h)h_{wl,t-1} + \kappa\left[h_{wlt}^{*,d} - (1-\delta_h)\, h_{wl,t-1}\right],$$

where $0 < \delta_h < 1$ is the physical depreciation rate and $0 < \kappa \leq 1$ measures the speed of adjustment. This model reduces to the static model when the adjustment speed is instantaneous, $\kappa = 1$.

Aggregating across remote and office workers gives

$$H_{lt}^{d} \;=\; N_{lt}\Big[s_{rlt}\,h_{rlt} + (1-s_{rlt})\,h_{blt}\Big].$$

Housing supply is increasing in the price

$$H_{lt}^{s} = \bar{H}_{lt} p_{lt}^{\phi},$$

Equilibrium requires the stock households actually hold after adjustment to equal the stock available after developers build:

$$H_{lt}^{s} = H_{lt}^{d}.$$

Linearizing around a steady state with a small remote share yields

$$\begin{aligned}
\hat{h}_{wlt} &= (1-\delta_h)\hat{h}_{wl,t-1} + \kappa\big[\hat{h}_{wlt}^{*,d} - (1-\delta_h)\hat{h}_{wl,t-1}\big] \\
\hat{h}_{wlt}^{*,d} &= \hat{z}_{lt} - \hat{p}_{lt} \\
\hat{H}_{lt}^{d} &= \hat{N}_{lt} + \frac{\theta_r - \theta_b}{\theta_b}\hat{s}_{rlt} + \hat{h}_{blt} \\
\hat{H}_{lt}^{s} &= \phi\hat{p}_{lt} + \hat{\bar{H}}_{lt}
\end{aligned}$$

The equilibrium price is then:

$$\phi\hat{p}_{lt} + \hat{\bar{H}}_{lt} = \hat{N}_{lt} + \frac{\theta_r - \theta_b}{\theta_b}\hat{s}_{rlt} + (1-\delta_h)\hat{h}_{bl,t-1} + \kappa\big[\hat{z}_{lt} - \hat{p}_{lt} - (1-\delta_h)\hat{h}_{bl,t-1}\big]$$

The aggregation argument now proceeds unchanged from the baseline model. In particular, the IV estimates $\beta_1 = \frac{1}{\phi+\kappa}\frac{\theta_r-\theta_b}{\theta_b}$ and

$$G_{\text{IV}} = \beta_1^{\text{dyn}}\Delta s_r = \frac{1}{\phi+\kappa}\,\frac{\theta_r - \theta_b}{\theta_b}\Delta s_r = G_{\text{true}}.$$

For the special case $\kappa = 1$, the model reduces to the static model.

A2.5 Variation 4: Heterogenous Supply Elasticities

We assume the elasticity of the housing supply function may vary by location l,

$$H_l^s = \bar{H}_l p_l^{\phi_l}$$

Following the same derivations as the baseline model, the evolution of the local house price is,

$$\Delta \ln p_l \approx \frac{1}{1+\phi_l}\frac{(\theta_r-\theta_b)}{\theta_b}\Delta s_{r|l} + \frac{1}{1+\phi_l}\Delta \ln N_l - \frac{1}{1+\phi_l}\Delta \ln \bar{H}_l + \frac{1}{1+\phi_l}\Delta \ln z_l.$$

Per location the effect of remote work on house price growth is therefore

$$\beta_{1l} = \frac{\theta_r-\theta_b}{\theta_b}\frac{1}{1+\phi_l}, \qquad \frac{\partial \beta_{1l}}{\partial \phi_l} < 0.$$

Define the exposure–weighted average

$$\overline{\beta}_1 \;=\; \frac{\sum_l s_l \beta_{1l} \Delta s_{r|l}}{\Delta s_r}, \qquad \beta_1^{\mathrm{IV}} \;=\; \sum_l \omega_l \beta_{1l}, \quad \omega_l = \frac{\tilde{s}_{r|l}\,\Delta \tilde{s}_{r|l}}{\sum_j \tilde{s}_{r|j}\,\Delta \tilde{s}_{r|j}}.$$

Then

$$\begin{aligned}\overline{\beta}_1 - \beta_1^{\mathrm{IV}} &= \sum_l \left(s_l \frac{\Delta s_{r|l}}{\Delta s_r} - \omega_l \right) \beta_{1l} \\ &= \frac{\theta_r-\theta_b}{\theta_b} \sum_l \left(s_l \frac{\Delta s_{r|l}}{\Delta s_r} - \omega_l \right) \frac{1}{1+\phi_l}\end{aligned}$$

The difference between the true and IV–extrapolated aggregate effects is

$$\begin{aligned}\frac{G_{\mathrm{true}} - G_{\mathrm{IV}}}{G_{\mathrm{true}}} &= \frac{\sum_l s_l(\beta_{1l} - \beta^{IV})\Delta s_{rl}}{\sum_l s_l \beta_{1l} \Delta s_{rl}} \\ &= \frac{\bar{\beta}_1 - \beta_1^{IV}}{\bar{\beta}_1} \\ &\approx \sum_l \left(s_l \frac{\Delta s_{r|l}}{\Delta s_r} - \omega_l \right) \frac{1+\phi}{1+\phi_l}\end{aligned}$$

When the change in remote work is independent of the local supply elasticity, $\Delta s_{r|l} \perp\!\!\!\perp \tilde{\phi}_l$, then $\sum_l \left(s_l \frac{\Delta s_{r|l}}{\Delta s_r} - \omega_l \right) \frac{1+\phi}{1+\phi_l} = \sum_l E\left(s_l \frac{\Delta s_{r|l}}{\Delta s_r} - \omega_l \right) E\frac{1+\phi}{1+\phi_l} = 0$ and there is no bias in either the estimated coefficient or in the aggregation from the cross-section.

Using the Baum-Snow and Han (2024) supply elasticity measures we can also directly calculate the bias. We obtain $\sum_l s_l \frac{\Delta s_{r|l}}{\Delta s_r} \frac{1+\phi}{1+\phi_l} = 1.39$ and $\sum_l \omega_l \frac{1+\phi}{1+\phi_l} = 1.22$. Thus,

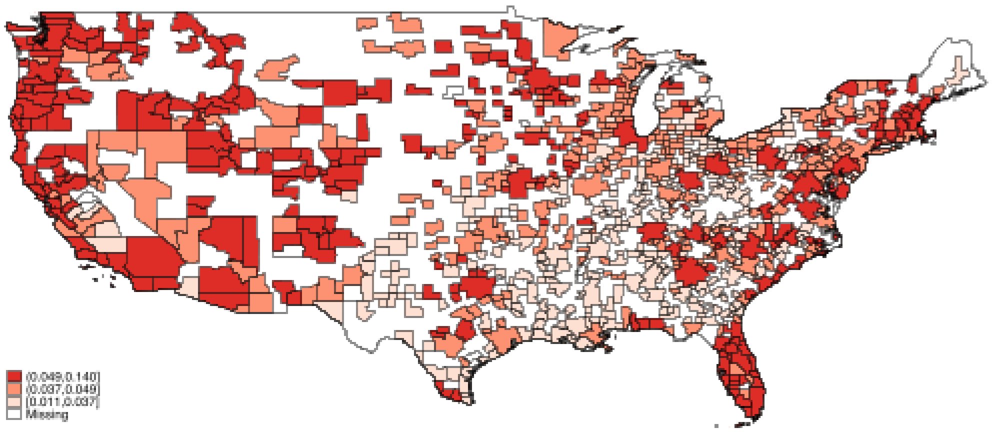
$$\frac{G_{\mathrm{true}} - G_{\mathrm{IV}}}{G_{\mathrm{true}}} = 0.17 = 17\%$$

and our IV understates the true aggregate effect. This reflects the fact that relative increases in remote work, which implicitly weigh the IV estimates, occur primarily in relatively elastic areas. In contrast, an average remote worker is more likely to be located in a relatively inelastic area because these tend to be larger.

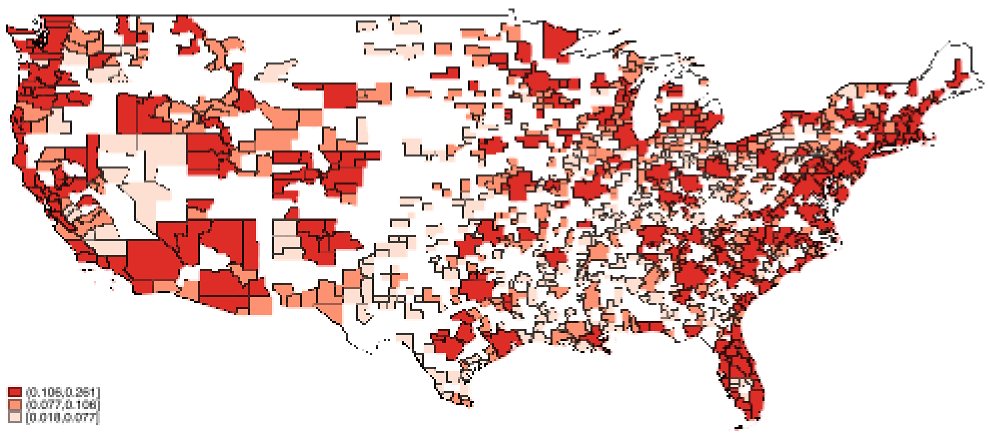
A2.6 Figures

FIGURE A1
Geographic Distribution of Remote Worker Share

A. 2015-19 Average



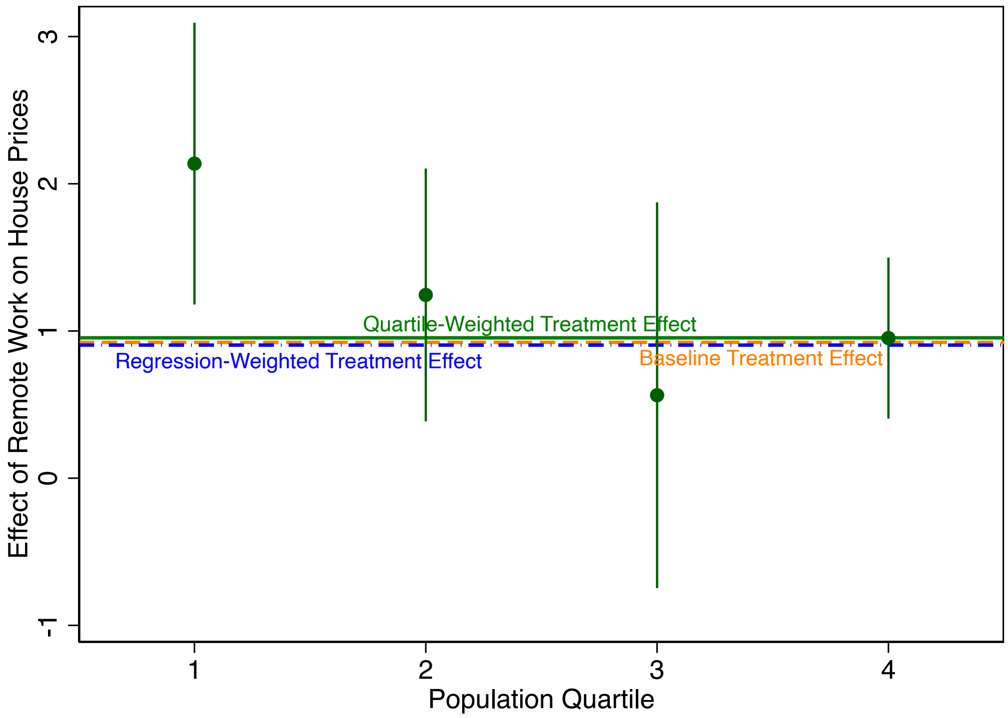
B. 2023



Sources: American Community Survey and authors calculations.

FIGURE A2

Variation in Effect of Remote Work on House Prices by Population



Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax Consumer Credit Panel, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, and authors calculations.

Notes: The dependent variable is house price growth in a CBSA from Dec. 2019 - Dec. 2023. The error bars report the coefficients on the remote worker share in 2023 interacted with the population quartile instrumented by the remote worker share from 2015-19 interacted with the population quartile. The regression includes the full set of controls of column (8) in Table 6, in which migration controls are also interacted with population quartiles. The dashed orange line is the population weighted average of these 4 estimates. The solid green line is unweighted estimate from column (8) of Table 6. The dashed-dotted blue line is the population-weighted estimate of the regression in column (8) of Table 6. Standard errors are clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population.

A2.7 Tables

TABLE A1

Sources of Pre-Pandemic Remote Work Variation: Bivariate Comparisons

Dependent Variable:	Remote Worker Share 2015-19												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Predicted Remote Worker Share 2015-19	1.69***												
	(0.23)												
Share College		0.16***											
		(0.018)											
Log Median Income			0.033***										
			(0.0059)										
Unemp. Rate 2019				−0.20**									
				(0.081)									
Pred. Wage Growth Pre-Pandemic					0.17								
					(0.41)								
Log Total Dividends / AGI						0.014***							
						(0.0019)							
Log Income / Filer							0.035***						
							(0.0048)						
Stimulus / Filer								−0.014***					
								(0.0032)					
DTI 2019									0.0025**				
									(0.00093)				
Log Density										0.0016**			
										(0.00077)			
Share 65+											0.090***		
											(0.033)		
January Temperature												0.00046***	
												(0.00012)	
July Temperature												−0.0014***	
												(0.00017)	
July Humidity												−0.00022**	
												(0.000085)	
Race Controls	No	No	No	No	No	No	No	No	No	No	No	No	Yes
CBSA Clusters	50	50	50	50	50	50	50	50	50	50	50	50	50
R^2	0.36	0.37	0.17	0.03	0.00	0.21	0.25	0.20	0.09	0.02	0.05	0.23	0.09
Observations	893	893	893	893	893	893	893	893	893	893	893	893	893

TABLE A2

Effect of Remote Work on Real House Price Growth (OLS), Dec. 2019 - Dec. 2023

Dependent Variable:	Real House Price Growth, Dec. 2019 - Dec. 2023			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2023	1.24***	1.09***	1.02***	1.17***
	(0.29)	(0.20)	(0.17)	(0.23)
HP Growth Pre-Pandemic		0.30	0.26	0.13
		(0.20)	(0.20)	(0.17)
Density & Demographic Controls	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.13	0.23	0.24	0.35
Observations	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is real house price growth in a CBSA from Dec. 2019 - Dec. 2023. The columns report an OLS regression on the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A3

Effect of Remote Work on House Price Growth (Core-Logic), Dec. 2019 - Dec. 2023

Dependent Variable:	House Price Growth (Core-Logic), Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.29**	1.23***	1.56***	2.09***				
	(0.49)	(0.42)	(0.40)	(0.38)				
Remote Worker Share 2023					0.72***	0.74***	0.94***	1.45***
					(0.26)	(0.26)	(0.26)	(0.27)
House Price Growth Pre-Pandemic		0.65***	0.73***	0.56***		0.63***	0.70***	0.42***
		(0.19)	(0.18)	(0.17)		(0.18)	(0.17)	(0.16)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					295.64	425.87	363.15	395.17
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.04	0.15	0.21	0.28	0.04	0.14	0.19	0.26
Observations	893	893	893	893	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is house price growth (core-logic) in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A4

First Stage for Rent Growth (Zillow) Regressions

Dependent Variable:	Remote Worker Share 2023			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.93***	1.88***	1.98***	1.77***
	(0.18)	(0.13)	(0.16)	(0.16)
Rent Growth Pre-Pandemic		0.025	0.059	0.21*
		(0.11)	(0.097)	(0.11)
Density & Demographic Controls	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes
CBSA Clusters	48	48	48	48
R^2	0.57	0.73	0.74	0.76
Observations	233	233	233	233

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: This table reports a first stage regression. The dependent variables is the remote worker share 2023. The instrument is the average share of remote workers from 2015-19 in a CBSA. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A5

Effect of Remote Work on Rent Growth (Zillow), Dec. 2019 - Dec. 2023

Dependent Variable:	Rent Growth (Zillow), Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	0.74	0.50	0.52**	0.65*				
	(0.65)	(0.36)	(0.24)	(0.35)				
Remote Worker Share 2023					0.38	0.26	0.26**	0.37**
					(0.34)	(0.18)	(0.12)	(0.17)
Rent Growth Pre-Pandemic		1.43***	1.57***	1.29***		1.43***	1.56***	1.21***
		(0.38)	(0.39)	(0.26)		(0.36)	(0.37)	(0.25)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					111.46	212.04	162.83	122.91
CBSA Clusters	48	48	48	48	48	48	48	48
R^2	0.02	0.38	0.42	0.48	0.00	0.38	0.42	0.49
Observations	233	233	233	233	233	233	233	233

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is rent growth (zillow) in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A6

Effect of Remote Work on House Price Growth (Rent Sample), Dec. 2019 - Dec. 2023

Dependent Variable:	House Price Growth (Rent Sample), Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.93**	1.86***	1.88***	2.37***				
	(0.80)	(0.62)	(0.58)	(0.60)				
Remote Worker Share 2023					1.00**	1.00***	0.96***	1.29***
					(0.43)	(0.34)	(0.29)	(0.29)
House Price Growth Pre-Pandemic		−0.18	−0.022	−0.081		−0.089	0.059	−0.046
		(0.44)	(0.55)	(0.35)		(0.42)	(0.51)	(0.29)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					111.46	198.72	163.53	124.55
CBSA Clusters	48	48	48	48	48	48	48	48
R^2	0.11	0.25	0.31	0.45	0.03	0.24	0.31	0.48
Observations	233	233	233	233	233	233	233	233

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is house price growth (rent sample) in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A7

First Stage for Rent Growth (Core-Logic) Regressions

Dependent Variable:	Remote Worker Share 2023			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	2.14***	1.93***	2.14***	1.98***
	(0.44)	(0.24)	(0.26)	(0.28)
Rent Growth Pre-Pandemic		0.11	0.15*	0.18
		(0.086)	(0.081)	(0.11)
Density & Demographic Controls	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes
CBSA Clusters	28	28	28	28
R^2	0.59	0.75	0.78	0.81
Observations	80	80	80	80

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: This table reports a first stage regression. The dependent variables is the remote worker share 2023. The instrument is the average share of remote workers from 2015-19 in a CBSA. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A8

Effect of Remote Work on Rent Growth (Core-Logic), Dec. 2019 - Dec. 2023

Dependent Variable:	Rent Growth (Core-Logic), Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.57	1.40**	1.56**	1.72***				
	(1.19)	(0.56)	(0.65)	(0.55)				
Remote Worker Share 2023					0.74	0.73**	0.73***	0.87***
					(0.68)	(0.30)	(0.28)	(0.23)
Rent Growth Pre-Pandemic		−0.50	−0.50	−0.20		−0.59	−0.61	−0.36
		(0.55)	(0.56)	(0.44)		(0.50)	(0.51)	(0.40)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					23.50	65.58	68.20	48.45
CBSA Clusters	28	28	28	28	28	28	28	28
R^2	0.07	0.54	0.57	0.68	-0.14	0.50	0.54	0.65
Observations	80	80	80	80	80	80	80	80

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is rent growth (core-logic) in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A9

Effect of Remote Work on House Price Growth (Core-Logic Rent Sample), Dec. 2019 - Dec. 2023

Dependent Variable:	House Price Growth (Core-Logic Rent Sample), Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	2.89**	2.10***	2.08***	2.13***				
	(1.16)	(0.58)	(0.63)	(0.67)				
Remote Worker Share 2023					1.35*	1.10***	0.97***	1.16***
					(0.78)	(0.35)	(0.28)	(0.30)
House Price Growth Pre-Pandemic		1.24**	1.11**	1.24**		0.88	0.75	0.57
		(0.50)	(0.54)	(0.58)		(0.55)	(0.55)	(0.69)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					23.50	67.68	67.07	48.14
CBSA Clusters	28	28	28	28	28	28	28	28
R^2	0.15	0.63	0.66	0.76	-0.12	0.58	0.63	0.73
Observations	80	80	80	80	80	80	80	80

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is house price growth (core-logic rent sample) in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A10

Effect of Remote Work on Cumulative Permit Growth, Dec. 2019 - Dec. 2023

Dependent Variable:	Cumulative Permit Growth, Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.79	2.86*	2.34*	2.92**				
	(1.57)	(1.46)	(1.33)	(1.33)				
Remote Worker Share 2023					1.01	1.74**	1.41*	1.99**
					(0.86)	(0.84)	(0.77)	(0.87)
Permit Growth Pre-Pandemic		0.12**	0.11**	0.11**		0.12***	0.11***	0.11***
		(0.046)	(0.045)	(0.044)		(0.045)	(0.044)	(0.043)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					285.53	373.81	321.85	323.58
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.00	0.05	0.07	0.15	0.00	0.05	0.07	0.16
Observations	843	843	843	843	843	843	843	843

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is cumulative permit growth in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\, p < 0.1$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$

TABLE A11

Effect of Remote Work on Net Inflow Rate Pandemic, Dec. 2019 - Dec. 2023

Dependent Variable:	Net Inflow Rate Pandemic, Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.22***	0.66***	0.68***	0.68***				
	(0.31)	(0.15)	(0.15)	(0.13)				
Remote Worker Share 2023					0.68***	0.41***	0.42***	0.48***
					(0.15)	(0.083)	(0.084)	(0.083)
Net Inflow Rate Pre-Pandemic		2.05***	1.80***	1.62***		1.99***	1.70***	1.50***
		(0.27)	(0.25)	(0.21)		(0.28)	(0.25)	(0.22)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					295.64	432.58	384.96	450.38
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.16	0.52	0.56	0.61	0.06	0.51	0.55	0.61
Observations	893	893	893	893	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is net inflow rate pandemic in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A12

First Stage for Real House Price Growth Regressions

Dependent Variable:	Remote Worker Share 2023			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.76***	1.57***	1.37***	1.36***
	(0.10)	(0.075)	(0.068)	(0.065)
HP Growth Pre-Pandemic		−0.031	0.015	0.0012
		(0.020)	(0.024)	(0.025)
Net Inflow Rate Pandemic	0.020	0.056**	0.10***	
	(0.029)	(0.025)	(0.029)	
Net Inflow Rate Pre-Pandemic	0.042	0.058	0.069	
	(0.080)	(0.073)	(0.074)	
Density & Demographic Controls	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes
Labor Market Controls	No	No	Yes	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	Yes	Yes
Nonparametric Migration Control	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.55	0.68	0.73	0.74
Observations	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: This table reports a first stage regression. The dependent variables is the remote worker share 2023. The instrument is the average share of remote workers from 2015-19 in a CBSA. Nonparametric migration controls include deciles of pandemic net migration and pre-pandemic net migration. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\, p < 0.1$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$

TABLE A13

Effect of Remote Work on Real House Price Growth by Source of Variation, Dec. 2019 - Dec. 2023

Source of Instrument Variation:	Occupation	Residual	Both	Occupation	Residual	Both
	No Migration Control			Migration Control		
	(1)	(2)	(3)	(4)	(5)	(6)
RHS variables:						
Remote Worker Share 2023	1.24***	2.32***	1.46***	0.73**	1.50**	0.86***
	(0.34)	(0.54)	(0.32)	(0.32)	(0.60)	(0.30)
HP Growth Pre-Pandemic	0.12	0.086	0.12	−0.038	−0.036	−0.038
	(0.17)	(0.16)	(0.16)	(0.16)	(0.15)	(0.15)
Net Inflow Rate Pandemic				0.98***	0.82***	0.95***
				(0.25)	(0.27)	(0.25)
Net Inflow Rate Pre-Pandemic				0.74*	0.65	0.72
				(0.44)	(0.47)	(0.44)
Density & Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Climate Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wage & Unemployment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock Exposure, Stimulus, & DTI Controls	Yes	Yes	Yes	Yes	Yes	Yes
p-value Overidentification			0.07			0.25
F-Statistic	333.89	45.62	282.59	326.96	36.56	278.48
CBSA Clusters	50	50	50	50	50	50
R^2	0.35	0.30	0.35	0.43	0.41	0.43
Observations	893	893	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is Real House Price Growth in a CBSA from Dec. 2019 - Dec. 2023. The columns report an instrumental variable regression in which the remote work share in 2023 is instrumented by predicted remote work share from 2015-19 (columns 1 and 3) and the remaining variation in the remote work share from 2015-19 (columns 3 and 6). The unused variation of the instrument is inlcuded as a control. Nonparametric migration controls include deciles of pandemic net migration and pre-pandemic net migration. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A14

First Stage for Real House Price Growth Regressions

Dependent Variable:	Remote Worker Share 2023			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.14***	1.09***	1.09***	1.12***
	(0.11)	(0.11)	(0.10)	(0.087)
HP Growth Pre-Pandemic		0.041*	0.042*	0.042**
		(0.021)	(0.021)	(0.020)
Share College	0.29***	0.23***	0.23***	0.25***
	(0.020)	(0.023)	(0.023)	(0.031)
Log Median Income	0.0024	0.0065	0.0067	−0.0017
	(0.0073)	(0.0053)	(0.0055)	(0.0075)
Density & Demographic Controls	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes
CBSA Clusters	50	50	50	50
R^2	0.69	0.73	0.73	0.75
Observations	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: This table reports a first stage regression. The dependent variables is the remote worker share 2023. The instrument is the average share of remote workers from 2015-19 in a CBSA. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A15

EFFECT OF REMOTE WORK ON REAL HOUSE PRICE GROWTH, DEC. 2019 - DEC. 2023

Dependent Variable:	Real House Price Growth, Dec. 2019 - Dec. 2023							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	3.34***	2.49***	2.63***	2.05***				
	(0.85)	(0.69)	(0.60)	(0.55)				
Remote Worker Share 2023					2.94***	2.28***	2.42***	1.83***
					(0.65)	(0.65)	(0.57)	(0.48)
HP Growth Pre-Pandemic		0.22	0.22	0.14		0.13	0.12	0.067
		(0.21)	(0.21)	(0.18)		(0.18)	(0.18)	(0.16)
Share College	0.057	0.010	−0.030	0.22	−0.78**	−0.51*	−0.59**	−0.24
	(0.23)	(0.16)	(0.14)	(0.20)	(0.33)	(0.27)	(0.24)	(0.28)
Log Median Income	−0.076	−0.071	−0.073	−0.017	−0.083	−0.086	−0.089	−0.014
	(0.083)	(0.057)	(0.055)	(0.059)	(0.071)	(0.059)	(0.058)	(0.058)
Density & Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Climate Controls	No	No	Yes	Yes	No	No	Yes	Yes
Labor Market Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure, Stimulus, & DTI Controls	No	No	No	Yes	No	No	No	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Statistic					101.69	100.67	108.73	164.33
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.14	0.26	0.27	0.35	0.09	0.21	0.21	0.34
Observations	893	893	893	893	893	893	893	893

Sources: Zillow, CoreLogic, American Community Survey, FRBNY/Equifax CCP, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, HMDA, and authors calculations.

Notes: The dependent variable is real house price growth in a CBSA from Dec. 2019 - Dec. 2023. The first four columns report the reduced form regression on the average share of remote workers from 2015-19 in a CBSA. The next four columns use the average remote worker share from 2015-19 as an instrument for the remote worker share in 2023. Density & Demographic Controls include quintiles of density, the population shares identifying as white, black, asian, and hispanic respectively, and quartiles of the population share above 65. Climate controls include the average January temperature, the average July temperature, and average July humidity. Labor Market Controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, its change from Dec. 2019 to Dec. 2023, and Bartik-predicted 2019Q4-2023Q4 and 2018Q4-2019Q4 growth based on 4-digit OCC codes. Stock Exposure, Stimulus, & DTI Controls include stock exposure based on the log of total dividends relative to AGI in 2019, the log of AGI per filer in 2019, fiscal stimulus payments per filer, and the average debt-to-income ratio on a new mortgage in 2019. Standard errors are given in parenthesis and clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.