Housing Demand and Remote Work*

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Abstract

We show that the shift to remote work explains over one half of the record 23.8 percent national house price increase from 2019 to 2021. Using variation in remote work exposure across U.S. metropolitan areas we estimate that an additional percentage point of remote work causes a 0.98 percent increase in house prices after controlling for negative spillovers from migration. This finding reflects an aggregate increase in demand for home space: remote work causes a similar increase in residential rents, a decline in commercial rents, a greater increase in prices for larger homes, and a decline in household size among movers. The cross-sectional effect on house prices combined with the aggregate shift to remote work implies that remote work raised aggregate U.S. house prices by 16.0 percent. Using a model of remote work and location choice we argue that this estimate is a lower bound on the aggregate effect. Our results argue for a fundamentals-based explanation for the recent increases in housing costs over speculation or financial factors, and that the evolution of remote work is likely to have large effects on the future path of house prices and inflation.

Keywords: work from home, remote work, housing, migration, aggregation
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1 Introduction

U.S. house prices have grown by 23.8 percent from December 2019 to November 2021, the fastest rate on record. At the same time, the COVID-19 pandemic has reshaped the way households work, with 42.8 percent of employees still working from home part or full time by November 2021 and evidence that a significant fraction of current remote work may become permanent (Barrero, Bloom, Davis, and Meyer, 2021; Bick, Blandin, and Mertens, 2021).[1] In this paper, we show that the shift to remote work accounts for at least one half of aggregate house price growth over this period. Our results suggests that 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. This implies that policy makers need to pay close attention to the evolution of remote work as an important determinant of future house price growth and inflation.[2]

We make three specific contributions. First, we identify a large effect of the shift to remote work on house price growth in the cross section of U.S. micro- and metropolitan areas (CBSAs) using exposure to the propensity for remote work. We measure exposure using the pre-pandemic remote work share, which we argue based on pre-trends and extensive controls is plausibly exogenous to other housing demand and supply shocks over the pandemic. We show similarly-sized effects of remote work on residential rent growth, much smaller effects on local inflation, and negative effects on commercial rents, consistent with remote work increasing the relative demand for housing.

Second, we isolate the share of the cross-sectional effect that represents an increase in housing demand and use it to extrapolate to the aggregate effect of remote work on house prices. Our initial estimate reflects both the increase in housing demand from working remotely and the reallocation of housing demand through migration towards areas suited

[1] Also see https://news.gallup.com/poll/355907/remote-work-persisting-trending-permanent.aspx.

[2] See Bolhuis, Cramer, and Summers (2022) on how pandemic house price and rental price growth are expected to increase inflation in 2022 and 2023.

for remote work. We show that we can isolate the aggregate increase in housing demand by adding high-quality controls for migration to our cross-sectional regression. We find that the increase in housing demand accounts for two thirds of the total effect of remote work on house prices. It also substantially increases the price of large homes relative to small homes and reduces household size among movers. The cross-sectional estimate conditional on migration implies that the aggregate shift to remote work accounts for at least one half of the total increase in aggregate house prices.

Third, we build a model of location choice, remote work, and housing demand as a laboratory to validate our aggregation approach. We calibrate the model to match the cross-sectional distribution of remote work before and during the pandemic, and show that it closely matches the distribution of house price growth over the pandemic. The model implies that our extrapolation from cross-sectional estimates after controlling for migration yields a lower bound on the aggregate effect of remote work on house prices. This shows that our approach of controlling for negative spillovers from migration is a novel solution to aggregating from cross-sectional estimates (Nakamura and Steinsson, 2018; Chodorow-Reich, 2019, 2020). The model also suggests that the vast majority of the increase in house prices will persist with remote work even as housing supply normalizes.

Our results rest on credibly identifying variation in remote work that is plausibly exogenous to housing demand and supply shocks from 2019 to 2021. 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 cheap housing and amenities. In this sense the pre-pandemic remote share summarizes a CBSA's exposure to the increasing availability of remote work. Consistent with this interpretation, we document that the pre-pandemic share of remote work is robustly correlated with the increase in remote work over the pandemic, even conditional on important local characteristics.

We then show that areas with more exposure to remote work saw significantly higher house price growth over the pandemic. Each additional percentage point of pre-pandemic remote work implies an additional 1.97 percentage points of house price growth. Exposure to remote work is uncorrelated with shocks to the labor market and important local characteristics, and there is no evidence of pre-pandemic trends in house prices correlated with exposure to remote work. All of this suggests that pre-pandemic exposure to remote work provides useful exogenous variation in remote work over the pandemic.

We use the pre-pandemic remote share as an instrument for the remote work share from the 2020 American Community Survey (ACS). More recent data on remote work with sufficient coverage at the CBSA level is not available, but we show that the 2020 ACS remote share correlates strongly with the broader remote work measures from Barrero, Bloom, Davis, and Meyer (2021) and Bick, Blandin, and Mertens (2021) at higher levels of aggregation in 2020 and 2021. We estimate that an additional percentage point of remote work in 2020 increases house price growth from December 2019 to November 2021 by 1.37 percentage points according to our preferred specification. Adding a broad range of controls, including density, demographic characteristics, labor market indicators, and stock market exposure leaves this estimate essentially unchanged. Additional evidence that this effect represents an increase in housing demand caused by remote work comes from the nearly identical effects on residential rents, negative and zero effect on commercial rents and inflation, and positive effects on the growth of residential building permits. These results rule out two kinds of alternative explanations. One in which a uniform demand shock, such as low interest rates, interacts with differential housing supply elasticities across CBSAs. And a second in which heterogenous demand shocks across CBSAs, such as labor market or financial wealth shocks, affect housing and non-housing prices similarly.

The total effect of remote work on house prices in the cross-section captures both the overall increase in housing demand and the reallocation of housing demand across regions resulting from migration. We cannot directly aggregate from this cross-sectional estimate because migration is a quantitatively important negative "spillover" in our setting:[3] House prices will grow faster in cities attractive to remote work as migrants move in, while house prices will grow slower 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 we can isolate the effect of remote work on housing demand by explicitly controlling for the effects of migration on house prices in our cross-sectional regression. This requires a precise measure of migration across CBSAs. 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 monthly frequency.

We find that the shift in demand accounts for around two thirds of the total cross-sectional effect of remote work on house price growth. Thus, an additional percentage point of remote work in 2020 increases house price growth from December 2019 to November 2021 by 0.98 percentage point, holding net migration fixed. 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 and a substantial decline in household size among movers.

To determine the effects of remote work on aggregate house prices, we combine our cross-sectional effect on house prices that controls for migration with the aggregate shift to remote work. This calculation implies that remote work increased house prices by 16.0 percentage points relative to the average increase of about 23.8 percentage points, or more than one half of the total increase. Remote work was therefore not only an important determinant of house price growth in the cross-section, but also for the aggregate U.S. economy.

We validate our novel approach to aggregation in a spatial general equilibrium model

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

of location choice, remote work choice, and housing demand.[4] We calibrate the model to match the cross-sectional distribution of remote work, occupations, migration, and our cross-sectional estimates. The model performs well in predicting house price growth in the cross-section of CBSAs, and its predicted effect of migration on house prices is very close to that in the data. Across a wide range of plausible parameterizations of the model, we show that our extrapolation from the cross-sectional estimate that controls for migration is a lower bound on the true aggregate effect of remote work on house prices. By contrast, extrapolating from cross-sectional estimates that do not control for migration would overstate the true aggregate effects of remote work. We then use the model to ask how much of the increase in house prices persists assuming that remote work persists but housing supply normalizes. Our baseline calibration implies that the vast majority of the increase in house prices—13.0 percentage points—will be permanent.

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

1.1 Related Literature

There is a rapidly growing literature on the feasibility and impact of remote work, particularly over the pandemic period. Bartik, Cullen, Glaeser, Luca, and Stanton (2020) and Dingel and Neiman (2020) study the actual and potential scope of remote work in the pandemic. Haslag and Weagley (2021) analyze the patterns of migration induced by remote work and Althoff, Eckert, Ganapati, and Walsh (2021) link the out-migration from large areas facilitated by remote work to job losses in local non-tradables. Davis, Ghent, and Gregory (2021) study the long-run implications of the shift to remote work.

Our work is closely related to several other studies of housing over the pandemic. Gamber,

[4]See Moretti (2011) for a textbook treatment. Seminal contributions include Rosen (1979) and Roback (1982).

Graham, and Yadav (2021) use variation in pandemic severity combined with occupational exposure to work from home to show that house prices increase when more time is spent at home. Using a general equilibrium heterogeneous agent model, they argue that this mechanism can explain a 3 percent increase in house prices from 2019-2021, about half of the total within-model increase in prices. We instead estimate the causal effect of the persistent shift to remote work on house price growth, show that this estimate aggregates to a lower bound when controlling for migration, and conclude that remote work explains at least one half of aggregate house price growth from 2019-2021. Brueckner, Kahn, and Lin (2021) focus on the reallocation of households across locations facilitated by the move to remote work, finding evidence that workers moved to cheaper locations from more expensive locations. Our work shuts down this effect of remote work in order to isolate the shift in demand for housing itself. Behrens, Kichko, and Thisse (2021) use a general equilibrium model of production with home work, allowing for remote work to affect housing demand, and argue that remote work has a non-linear effect on productivity and strictly increases inequality. Stanton and Tiwari (2021) study remote workers pre-pandemic and find that remote work is associated with higher housing expenditure shares, driven by demand for both larger and higher quality housing, consistent with the evidence we provide.

A number of related papers have documented the effects of the pandemic and remote work on housing and real estate markets within metro areas. Gupta, Mittal, Peeters, and Van Nieuwerburgh (2021) and Ramani and Bloom (2021) show that housing demand shifted from city centers to the periphery, similar to Liu and Su (2021) who show shifts in demand consistent with a reduced demand for density. These results are consistent with Delventhal, Kwon, and Parkhomenko (2021) who study the effects of work from home in a quantitative framework and find migration out of the city center. Gupta, Mittal, and Van Nieuwerburgh (2022) show that firms with a larger fraction of remote job postings reduced their demand for office space by more.

The rapid acceleration of house price growth in the pandemic has been interpreted by some observers as a sign of a new U.S. housing bubble (Coulter, Grossman, Martínez-García, Phillips, and Shi, 2022). We instead argue that the large increases in house prices over the pandemic reflect a fundamental increase in housing demand due to remote work. A related literature examines the role of fundamentals and bubbles in the evolution of house prices before the pandemic (see e.g., DeFusco, Nathanson, and Zwick, 2017; Kaplan, Mitman, and Violante, 2020; Chodorow-Reich, Guren, and McQuade, 2021, for recent contributions).

Finally, we contribute to the literature aggregating from micro estimates to macro effects(see e.g., Nakamura and Steinsson, 2014; Mondragon, 2018; Chodorow-Reich, 2019; Beraja, Fuster, Hurst, and Vavra, 2019; Herreño, 2020; Orchard, Ramey, and Wieland, 2022). Chodorow-Reich (2020) recommends using economic theory to sign the general equilibrium effects and bound the aggregate effect, thereby avoiding heavy dependency on model assumptions. In our analysis important general equilibrium forces pull in opposite directions: whereas migration has a negative spillover to regions with lower remote work share, the housing wealth effect imparts a positive spillover through trade linkages (Guren, McKay, Nakamura, and Steinsson, 2021; Stumpner, 2019). We propose to directly control for the negative general equilibrium spillover (migration in our case), making the resulting cross-sectional estimate a lower bound on the aggregate effect without the need of additional model structure. We verify this approach in a spatial equilibrium model with migration.[5]

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 (Ramani and Bloom, 2021; Gupta, Mittal, Peeters, and Van Nieuwerburgh, 2021), which is convenient for our purpose of estimating

[5] Adao, Arkolakis, and Esposito (2019) show how to discipline all general equilibrium effects by estimating a full set of spatial linkages. This approach requires specifying a structural model, but it allows one to recover an aggregate point estimate rather than a lower bound.

the effect of remote work on aggregate housing demand. Our final dataset has 895 CBSAs.

Zillow house price indices are our baseline measure of house prices.[6] We measure pre-pandemic price growth from December 2018 to December 2019, and pandemic price growth from December 2019 to November 2021. Table 1 shows that average house price growth increased from 4.1 percent pre-pandemic to 20.0 percent in the pandemic. Using population weights, house price growth increased from 3.0 percent to 23.8 percent. While the pandemic house price growth is measured over a longer period of time, the annualized growth rate of 11.3 percent is still well above the pre-pandemic growth rate.

We obtain rents for a subset of 178 CBSAs from Apartment List. Similar to house prices, rent growth accelerates dramatically from 2.4 percent in 2019 to 14.9 percent (7.2 percent annualized) from December 2019 through November 2021.

We rely on American Community Survey data from the 2015-2019 survey waves as well as the 2020 experimental run to measure remote work, where remote work is defined as an employed person that does not commute.[7] 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.[8]

Table 1 shows that the national (population-weighted) remote worker share increased from an average of 5.2 percent before the pandemic to 16.3 percent in 2020. Other surveys report higher remote shares: Bick, Blandin, and Mertens (2021) report more than 20 percent of workers are remote by late 2020 while Barrero, Bloom, and Davis (2021) report that 49 percent of paid working days were done remotely at the same time. These surveys are also able to capture the evolution of broader remote work trends such as hybrid work in subsequent years and so would be ideal to measure remote work adoption. But they are also relatively sparse below the state level, making them difficult to use for our analysis.

[6] See https://www.zillow.com/research/data/. Our results are also robust to using the proprietary S&P Corelogic Case-Shiller index.

[7] We exclude individuals in the armed forces when calculating remote work shares.

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

To determine the relationship between the 2020 ACS remote work share and these alternative surveys, we construct state-level measures of the fraction of days worked remotely for 2020, 2021, and 2022 if available from Barrero, Bloom, and Davis (2021) and Bick, Blandin, and Mertens (2021). 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 both survey measures of remote work in all years and this correlation is stable across years. Since these measures also explicitly capture hybrid work arrangements, our ACS measure will also pick up the effects of hybrid work. Second, the ACS measure tends to understate the prevalence of remote work by about 15-30 percentage points. This implies that our empirical estimates using the ACS estimates will end up being scaled up to reflect this discrepancy. Overall, these results show that the 2020 ACS gives a very accurate measure 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 the CBSA housing supply elasticity. Baum-Snow and Han (2019) 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 -0.62.

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 average in 2020, and the change in unemployment from November 2019 to November 2021. These choices avoid seasonality issues and use the most recent date available. The average change in unemployment over the full pandemic 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 wage growth from average wages per employee in the QCEW. The total wage bill is reported at quarterly frequency, so we measure pandemic wage growth from 2019Q4 to 2021Q4 and pre-pandemic wage growth from 2018Q4 to 2019Q4.

To capture exposure to the growth in stock market valuations over the pandemic, we construct the share of total dividend income in adjusted gross income by CBSA in 2019 (Chodorow-Reich, Nenov, and Simsek, 2021). This is the most recent geographically disaggregated data released by the IRS.

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:

$$\text{First Stage:} \qquad \text{Remote Work 2020}_i = \kappa + X_i'\theta + \gamma \text{Remote Work 2015-19}_i + \zeta_i \qquad (1)$$

$$\text{Second Stage:} \qquad \text{House Price Growth}_i = \alpha + X_i'\delta + \beta \widehat{\text{Remote Work}}\ 2020_i + \epsilon_i \qquad (2)$$

where Remote Work 2015-19$_i$ is the share of employed individuals working from home in the 2015-2019 ACS, Remote Work 2020$_i$ is the share of employed individuals working from home in the 2020 ACS, House Price Growth$_i$ is house price growth over the pandemic, 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 the pandemic likely affected remote work in 2020. In fact, if remote workers require more housing, then any shock that pushes up house prices will reduce remote work as remote workers 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 over 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 over the pandemic as reflecting local amenities, the cost 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 insight.[9] 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.[10] Similarly, amenities such as a mild winter climate and low summer temperature and humidity strongly predict remote work.[11] In contrast, density is only weakly correlated with remote work, suggesting a smaller role for housing cost in explaining the cross-sectional variation in remote work exposure.

Based on these results, we interpret the pre-pandemic remote work share as a sufficient statistic for how exposed a location is to the 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 both more immigration and more local workers electing

[9] Table A1 shows bivariate comparisons.

[10] 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.

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

to work remotely. This implies pre-pandemic remote work shares will be predictive of remote work shares over the pandemic, 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 and the stability of our estimates conditional on important local shocks and characteristics.

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. To the extent that the pre-pandemic remote share captures how suitable a location is for remote work, we would expect such locations to see 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 though aggregate housing demand is unaffected.

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

$$\text{Remote Work 2020}_i = \kappa + X_i'\theta + \gamma_1 \text{Remote Work 2015-19}_i + \gamma_2 \text{Net Migration}_i + \zeta_i \quad (3)$$

$$\text{House Price Growth}_i = \alpha + X_i'\delta + \beta_1 \widehat{\text{Remote Work}}\ 2020_i + \beta_2 \text{Net Migration}_i + \epsilon_i \quad (4)$$

Intuitively, controlling for net migration will collect any effects remote work has on house prices through net migration in the estimate of β_2. This means that β_1 will capture the direct effects of remote work on house prices only through the shift in housing demand. In Appendix 1.1 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.[12]

If we combine estimates of β_1 with the representative level of remote work in 2020, we obtain an estimate of the effect of remote work on aggregate house prices. In section 4 we argue using a model of remote work choice and location choice that this estimate is a lower bound on the aggregate effects of remote work.

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 3.6 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 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 2020 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 during the pandemic: areas at the top of the pre-pandemic distribution have more than 15 percent of workers at home in 2020 while areas at the bottom of the pre-pandemic distribution only have about 5 percent of workers at home in 2020. This is consistent with our argument that the same underlying fundamentals that made a city amenable to remote work in the pre-pandemic period continued to attract remote work during the pandemic.

Figure A1 shows heat maps of the distribution of remote work pre-pandemic and in 2020 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

[12] 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).

pre-pandemic and pandemic 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 1.74 percentage points of remote work during 2020. The estimate is very precise with the 95 percent confidence band ranging from 1.52 to 1.95. Since the aggregate remote share increases by a factor of 3, the first stage implies that low-remote areas get a slightly larger multiplicative treatment than high-remote areas. Still, the r-squared of 39 percent shows that the pre-pandemic remote share captures a substantial fraction of the variation in remote work over the pandemic, and therefore satisfies the relevance restriction.

Columns (2) through (4) of Table 3 sequentially introduce controls for important local observables. Column (2) adds pre-pandemic house price growth as a control. Column (3), additionally controls for racial/ethnic and age composition, and quintiles for CBSA density.[13] Column (4) controls for local labor market conditions before the pandemic and over the course of the pandemic and for the exposure to the stock market, proxied with the share of local income due to dividends. Introducing these controls raises the r-squared to 64 percent and has only small effects on the pre-pandemic remote work estimate, suggesting that this measure of remote work exposure is not correlated with other pandemic-related shocks to areas affecting levels of remote work.

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

[13] 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. 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.

the bottom of the distribution. However, house price growth was very high across the board; even the areas least exposed to remote work saw house prices grow by about 15 percent.

It is possible that the large apparent effect of remote work on house prices simply reflects pre-existing trends in house prices caused by differences in underlying fundamentals 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 negative, though the effects are small and not statistically significant. 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.

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 are not statistically distinguishable from zero at the 95 percent level before the pandemic begins, but the estimates rapidly increase by late 2020. 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 in Figure 4B. The corresponding build-up in expectations that remote work will be permanent documented by Barrero, Bloom, and Davis (2021) provides an explanation consistent with our hypothesis. In contrast, explanations that emphasize the pandemic, for example a revaluation of outdoor space, would imply stronger effects on house prices in 2020 when restrictions were more widespread and vaccines were not available.

To address other possible violations of the exclusion restriction we add controls to capture omitted variables 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 1.97 percent during the pandemic. This estimate is also fairly precise with the 95 percent confidence band ranging from 1.36 to 2.58. Columns (2) through (4) of Table 4 include the set of controls from Table 3. The net effect is a very slight increase in the our estimate to 1.98, even though many of the controls themselves are statistically significant and raise the r-squared from 17 to 39.

The stability of the remote work estimate reflects that the pre-pandemic remote share is only weakly correlated with many controls. For example, the correlation of remote work is -0.19 with the pre-pandemic unemployment rate, 0.06 with the change in unemployment from 2019 to 2020, and 0.15 with the change in unemployment from November 2019 through November 2021. Therefore, our estimate for remote work does not appear to be correlated with a shock to labor demand. More broadly, the U.S. economy had almost returned to full employment by November 2021, which suggests that there is limited scope for alternative explanations based on labor demand.

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 2020 implies a 1.14 percent faster house price growth during the pandemic. 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 also yields a fairly narrow 95 percent confidence band of 0.80 to 1.47.

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 1.37 that remains precisely estimated with the 95 percent confidence bands extending from 1.05 to

1.69. The IV coefficients are roughly twice as large as their OLS counterparts, which are displayed in Table A2. This suggests that there are significant shocks to pandemic housing demand that negatively affect remote work and/or there is substantial measurement error in the 2020 remote work share.[14]

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 over the pandemic. We now turn to the subsample of 178 CBSAs for which we have rental index data. 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 A3) and the reduced form and IV regressions Table A4.

Row (1) of Table 5 reports the IV estimate for pandemic rent growth. It implies that an additional percentage point of remote work in 2020 increases rent growth from December 2019 to November 2021 by 1.09 percentage points. This estimate is very close to our baseline estimate for house price growth (1.37 percentage points) and almost identical to the estimate for house price growth in this sub-sample (1.03 percentage points as shown in row (2) of Table 5).

A further way to assess whether there was a housing-specific increase in demand is to check whether 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 has 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 25 CBSAs and Bureau of Labor Statistics price indices for 22 CBSAs.[15] Given the small sample size we report reduced

[14] Rothbaum, Eggleston, Bee, Klee, and Mendez-Smith (2020) document the potential for nonresponse bias in the 2020 ACS production run due to the pandemic. To the extent that this measurement error is classical, our IV estimates are asymptotically unaffected. See also https://usa.ipums.org/usa/acspumscovid19.shtml

[15] We use the Reis commerical real estate effective rent index, now provided by Moody's CRE. This is a

form regressions with lagged dependent variables as controls and robust standard errors.

Row (3) of Table 5 shows the reduced form regressions for commercial rent growth. A one percentage point increase in remote work exposure predicts a marginally significant -0.26 percent decline in pandemic commercial rents. Row (4) reports the corresponding estimates for house price growth in the same sub-sample: a one percentage point increase in remote work exposure predicts a 2.37 percent increase in house prices, which is highly 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. The relatively small decline in the office rent index may reflect that 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 (5) of Table 5 displays the reduced form regression for the pandemic inflation rate excluding shelter on the remote worker share. The price level excluding shelter grows by 0.44 percent more over two years 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 2.98 percentage points (row 6). The insignificant inflation response is thus roughly one seventh the magnitude of the very significant house price responses. This corroborates our claim that remote work triggered a relative increase in the demand for housing and thus an increase in the relative price of housing.

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 (e.g., 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

quarterly, hedonic index intended to give the average "effective rent" per square foot for large-building office space in the metro area.

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. We provide further evidence against this explanation 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 (7) of Table 5 instead shows that housing permits grew faster in areas more exposed to remote work.

However, these additional permits are unlikely to have been completed by the end of our sample period: Row (8) of Table 5 shows that there is essentially no relationship between remote work exposure and the cumulative number of homes sold in 2020-21 relative to 2018-19. This suggests that the housing supply elasticity was essentially zero over the period we study.

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, climate characteristics, 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 A11 and Table A12. The IV estimates with all controls included is now 1.76 (95 percent confidence band from 1.20 to 2.32) versus 1.37 in our baseline regression. The first-stage is also less powerful indicating that the variation we use is less likely to be representative of the broader increase in remote work during the pandemic.

A closely-related question is whether specific subsets of the variation in pre-pandemic remote work ultimately accounts for the effect of remote work on house prices. In Table A10, we re-estimate our baseline equations using three distinct subsets of pre-pandemic remote work as our instrumental variable: (1) the remote work expected by the pre-pandemic distribution of occupations, (2) the average January and July temperatures (the most predictive climate variables we consider), and (3) the residual of pre-pandemic remote work after partialling out the occupation- and climate-driven variation. To ensure we isolate the particular source of variation, we include the remaining variation in the 2015-19 remote work share as a control. We find that each source of variation implies that remote work had economically large effects on housing markets over the pandemic. In relative terms the effects are largest for the residual variation and smallest for occupational variation, which is in large part explained by differential loadings on migration. The first stage F-statistics for these regressions, while reasonably strong, are uniformly lower than what we find using pre-pandemic remote work alone. This suggests that pre-pandemic remote work is more likely to capture variation that will reflect the true average treatment effect we need to plausibly aggregate our effects.

3.4 Decomposing The Effect of Remote Work

The large effects of remote work on house price and rent growth that we estimate in the cross-section reflect both an aggregate increase in 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. 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 over the pandemic. Table A8 reports specifications mirroring 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 the pandemic. These results suggest that migration may be a quantitatively important fraction of the estimated effect of remote work on housing demand in the cross-section of CBSAs.

To isolate the aggregate 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 1.31 (reduced form, column 1) and 0.73 (IV, column 5). This is roughly a one third 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 A9). 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 1.1 for details).[16]

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 pandemic net migration and pre-pandemic net migration. Our IV estimate of the effect of remote work on house prices remains almost unchanged at 0.98. This suggests that this effect is not driven by a non-linear migration response or measurement error in the migration variable.

[16] 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.

3.5 Supporting Evidence for the Mechanism

We have argued that the effect of remote work on house prices after controlling for migration represents an aggregate increase in the demand for home space. Here we present supporting evidence for this interpretation using data on price indexes for houses of different sizes and on household formation.

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 is roughly 40 percent larger for houses with at least three bedrooms compared to houses with only one bedroom. This is consistent with the argument that remote work increases the demand for space.

Another margin of adjustment is for remote workers to reduce the size of the household itself. For example, people who lived with roommates prior to the pandemic may decide to live alone with the onset of remote work, thus reducing their household size and increasing their total demand for home space. We construct measures of the number of adults in each primary sample member's household, defined as individuals with the same anonymized address, in the FRBNY/Equifax Consumer Credit Panel.[17] We then calculate the log change in

[17]For each primary sample member, the FRBNY/Equifax Consumer Credit Panel provides credit reports all individuals using the same address in their credit reports, even if they are not in the primary sample. To minimize erroneous household counts from non-traditional arrangements, like military bases or dorms, we drop any household with more than 7 adult members.

household size over the pre-pandemic and pandemic periods, distinguishing between households that moved or stayed in the same location.

Distinguishing between movers and stayers is important because of local general equilibrium effects: to the extent that the local housing supply is fixed in the short-run and we hold population fixed by controlling for net migration, we would not expect to see any change in average household size in a CBSA. We therefore test whether movers in high remote work areas choose to reduce their household size by more than movers in low remote work areas. Here we interpret movers as the marginal buyers or renters in the housing market and, in this sense, we test for a greater demand for space among the marginal buyer. In local general equilibrium, we expect that any such effect will at least be partly offset by stayers maintaining a correspondingly larger household size.

Rows (6)-(8) of Table 7 report the IV estimates of the effect of remote work on household size according to move status, again conditional on our full set of controls. Row (6) shows that remote work only modestly and insignificantly reduces household size over the pandemic, with the IV estimate showing an elasticity of -0.03. This is consistent with a relatively fixed housing supply and adult population in a CBSA conditional on our controls.

When we break these numbers up into households that moved (both into and within the CBSA) and households that did not move at all, we find that movers in areas more exposed to remote work reduce the sizes of their households. The estimate for movers in row (7), -0.18, implies that remote work reduced the average household size of movers by $-0.18\times16.3=-2.9$ percent.[18]

Row (8) shows that there is a positive but weaker relationship between remote work and changes in household size for households that did not move, consistent with stayers absorbing the remote work shock by increasing their household size. We also find that net inflows from December 2019 to November 2021 are strongly positively correlated with household size for all households, consistent with the view that housing supply was relatively fixed (Table A13).

[18] Table A14 shows that the negative effect on household size among movers reflects an increased probability of reducing household size.

These results support the claim that remote work increased housing demand. This increased demand translated into more house price growth for larger houses as well as reductions in household size, concentrated among households that moved across or within CBSAs.

3.6 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. For extrapolation we use the estimates in column (8) with all controls included.

The weighted 2020 remote worker share for the U.S. economy is 16.3 percent. Multiplying this value with our IV estimate in column (8), we obtain an aggregate effect of $16.3 \times 0.98 = 16.0$ percent. Since aggregate house prices grew by 23.8 percent from December 2019 to November 2021, our IV estimate implies that remote work can explain $16.0/23.8 = 67$ percent of the total increase in house prices over the pandemic. In the following section 4, we argue that this extrapolation represents a lower bound on the aggregate effect because we control for the effect of migration in the cross-section.

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 decile. Figure A2 shows that the treatment effects vary little by population. Using the population in each decile as weights, the weighted effect of remote work on house prices is 1.00, essentially identical to our baseline estimate.[19]

4 Model

We now use a model of housing demand, location choice, and work mode choice to show that extrapolating from our cross-sectional estimate that controls for migration provides a lower bound on the aggregate effect.

[19] We can also extrapolate by multiplying the ten treatment effects with the 2020 remote work share in each decile. This yields an aggregate effect of remote work on house prices of 16.6%.

The model deliberately omits channels that generate positive spillovers across locations, such as trade spillovers from positive housing wealth effects (Guren, McKay, Nakamura, and Steinsson, 2021; Stumpner, 2019). Adding such channels would only strengthen the argument that our cross-sectional estimate that controls for migration constitutes a lower bound on the aggregate effect. Our model also omits aggregate general equilibrium effects through monetary policy. The model therefore provides an estimate for the effect of remote work on house prices holding monetary policy fixed.

4.1 Firms and Workers

There is a set of locations $l = 1, ..., L$ and occupations $o = 1, ..., O$. Within each location there are firms indexed by f. Firms are immobile with $\ell(f)$ denoting the location of firm f. Firms only employ workers of a single occupation, $o(f)$, at an occupation-specific wage $z_{o(f)}$. Firms are perfectly competitive and production is linear in labor, so that $z_{o(f)}$ is also the value of output produced by a worker at firm f.

Workers have fixed occupations and a fixed attachments to firms. Each worker attached to firm f can choose to either work in the office of firm f (and thus in location $\ell(f)$), work remotely in location $\ell(f)$, or work remotely in another location $l \neq \ell(f)$. The work mode and location decision is an optimization problem based on location amenities, prices, and idiosyncratic tastes. Workers consume a non-housing good c and a housing good h.

4.2 Consumption Decision

We first solve for the optimal goods bundle (c, h) given attachment to firm f, and given the choices of location l and work mode w (remote r or office b).

The utility function is CES, with weight θ_w on the housing good and elasticity of substitution ζ between housing and non-housing:

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

Remote work implies a greater weight on housing expenditure, $\theta_r > \theta_b$.

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:

$$z_f = c_{flw} + p_l h_{flw}$$

The optimal consumption choices are

$$c_{flw} = \frac{(1-\theta_w) z_f}{P_{lw}^{1-\zeta}}, \qquad h_{flw} = \frac{\theta_w z_f}{p_l^{\zeta} P_{lw}^{1-\zeta}}$$

where the work-mode specific price level is $P_{lw} = \left[(1-\theta_w) + \theta_w p_l^{1-\zeta}\right]^{\frac{1}{1-\zeta}}$.

Utility for the combination f, l, w is then the wage deflated by the appropriate price index

$$U_{flw} = \frac{z_f}{P_{lw}}$$

4.3 Location and Work-mode Decision

We assume that workers have idiosyncratic tastes for location and work mode, a_{ifolw}. Each worker chooses the combination of location l and work mode w that maximizes total utility,

$$\max_{l,w} \{U_{flw} a_{ifolw}\}$$

To obtain closed form solutions for the distribution of workers, we assume that a_{ifolw} is drawn from a Frechet distribution with shape parameter κ and location parameter μ_{folw}. We parameterize μ_{folw} such that it is exactly identified by our empirical targets:

$$\mu_{folr} = \mu_{ol}\mu_{l=\ell(f)}\mu_{lr}, \qquad \mu_{folb} = \mu_{ol}\mu_{l=\ell(f)}\mu_{ob}.$$

Here μ_{ob} determines the propensity of occupations for remote work and μ_{lr} the propensity of a location for remote work. μ_{ol} captures occupation-specific amenities of location l and $\mu_{l=\ell(f)}$ determines the propensity to stay in the same location as the firm.

For workers attached to firm f who decide to stay in the location of their firm, $l = \ell(f)$, the probability of being remote is then,

$$s_{r|\ell(f)f} = \frac{\mu_{\ell(f)r} P_{\ell(f)r}^{-\kappa}}{\mu_{\ell(f)r} P_{\ell(f)r}^{-\kappa} + \mu_{o(f)b} P_{\ell(f)b}^{-\kappa}}. \tag{5}$$

The probability of choosing location l among workers of firm f is

$$s_{l|f} = \begin{cases} \frac{\mu_{o(f)l}\mu_{lr}P_{lr}^{-\kappa}}{\sum_{\tilde{l}\neq\ell(f)} \mu_{o(f)\tilde{l}}\mu_{\tilde{l}r}P_{\tilde{l}r}^{-\kappa} + \mu_{o(f)\ell(f)}\mu_{l=\ell(f)}\left(\mu_{\ell(f)r}P_{\ell(f)r}^{-\kappa} + \mu_{o(f)b}P_{\ell(f)b}^{-\kappa}\right)} & \text{if } l \neq \ell(f) \\ \frac{\mu_{o(f)l}\mu_{l=\ell(f)}\left(\mu_{o(f)b}\mu_{lr}P_{lr}^{-\kappa} + \mu_{lb}P_{lb}^{-\kappa}\right)}{\sum_{\tilde{l}\neq\ell(f)} \mu_{o(f)\tilde{l}}\mu_{\tilde{l}r}P_{\tilde{l}r}^{-\kappa} + \mu_{o(f)l}\mu_{l=\ell(f)}\left(\mu_{\ell(f)r}P_{\ell(f)r}^{-\kappa} + \mu_{o(f)b}P_{\ell(f)b}^{-\kappa}\right)} & \text{if } l = \ell(f) \end{cases}$$

4.4 Housing Demand, Housing Supply and Equilibrium

Total housing demand conditional on being in location l is:

$$H_l = \sum_f s_f s_{l|f} [s_{r|lf} h_{flr} + (1 - s_{r|lf}) h_{flb}]$$

where s_f is the distribution of workers across firms, $s_{r|lf}$ is given by (5) when $l = \ell(f)$, and $s_{r|lf} = 1$ whenever workers live away from the firm $l \neq \ell(f)$.

We assume that each location has a fixed net housing supply $\bar{H}_l$. A zero housing supply elasticity is consistent with our finding that home sales did not vary with remote work exposure (Table 5). We later show that this assumption is conservative: conditional on matching our cross-sectional estimates, the model with more elastic supply predicts larger aggregate effects of remote work on housing demand and house prices.

Housing is supplied by perfectly competitive investors who consume the proceeds $\sum_l p_l \bar{H}_l$ in the form of non-housing goods. Because both workers and investors consume all their income, the model features no aggregate housing wealth effect.

Markets clear when $H_l = \bar{H}_l$ in each location. The non-housing market clears by Walras's Law.

4.5 Calibration

Table 8 summarizes the calibration. We calibrate the model to the same $L = 895$ locations in the data. We sort occupations by their remote worker share in 2015-19, and aggregate them into $O = 8$ groups with equal population shares.

We set the demand elasticity to $\zeta = 0.67$ following Albouy, Ehrlich, and Liu (2016). McKay and Wieland (2021) argue that a range from 0.5 to 1 is supported by empirical evidence and we show later that our results are robust within this range.

We normalize the initial housing prices to $p_l = 1$ for all locations, so that the effect of prices on location choice will absorbed by the location parameter μ_{ol}. This has two advantages. First, the choice of κ does not affect the initial steady state. Indeed, κ plays little role in our results and so we set it to $\kappa = 1$. Second, the importance of housing in the utility function, θ_w, is exactly equal to the housing expenditure share. We therefore set $\theta_b = 0.24$ following Davis and Ortalo-Magné (2011). In our sensitivity analysis we show robustness to a wide range of values for κ and to letting the initial housing prices vary by location.

We calibrate occupation-specific wages based on the 2015-19 ACS, normalizing the lowest wage to 1. We later allow wages to vary by remote status.

4.6 Inference

The remaining parameters are jointly inferred from the data in five steps:

1. We fix a value for the remote housing share θ_r.
2. *Compute the steady state parameters*: We extract a set of location parameters $\mu_{ol}, \mu_{ob}, \mu_{lr}$, and $\mu_{l=\ell(f)}$ such that the model matches exactly the 2015-19 remote share in each location, the 2015-19 remote share in each occupation, the 2015-19 occupation-location

distribution (all from the ACS) and the pre-pandemic gross migration rate (3.8 percent) from the NYFRB/Equifax Consumer Credit Panel.[20] We also impose the restriction that net migration is zero pre-pandemic.

3. *Compute the 2021 parameters*: We infer a new set of remote-specific parameters μ'_{or}, μ'_{lr} to match the November 2021 distribution remote work shares by location and occupation. To create the November 2021 distribution we scale up the 2020 ACS remote shares in each location and occupation such that the aggregate remote share is 42.8 percent based on the November 2021 survey by Barrero, Bloom, Davis, and Meyer (2021). We later show that our aggregation approach is robust to different scaling.

   The new remote-specific parameters will induce migration across locations. We capture residual sources of migration through changes in the firm distribution: $s'_f = \alpha'_{l(f)} s_f$ where $\alpha_l > 1$ captures in-migration and $\alpha_l < 1$ captures out-migration. We then infer migration parameters $\mu'_{l=\ell(f)}$ and α'_l to match the pandemic gross migration rate (5.3 percent) and net migration rate from December 2019 to November 2019 in each location (both from the NYFRB/Equifax Consumer Credit Panel).

4. *Regression*: We compute the model-implied house prices growth from the steady state to November 2021 for each location. Then we run the same IV regression in the model as we do in the data (equations (3)-(4)).[21] If the IV estimate of remote work on house prices in the model is below 0.98 (column 8 of Table 6) then we raise θ_r, if it is above then we reduce θ_r.

5. Repeat steps 1-4 until convergence.

By construction, our model will closely match the first stage controlling for migration, column (1) of Table A9. This is because we exactly match the 2015-19 and 2020 remote shares by location and the pandemic net migration data as of November 2021. The model

[20]We adopt the following normalizations: $\frac{1}{L}\sum_l \mu_{ol} = 1$ and $\frac{1}{L}\sum_l \mu_{lr} = 1$.

[21]Running this regression also requires the 2020 remote share data. However, we do not need to explicitly solve the model for this period since this is the only information we use from 2020.

also matches the cross-sectional IV coefficient by construction—the required value for the remote housing share is θ_r =0.326.

4.7 Non-targeted Moments

The model is not calibrated to match the distribution of house price growth. However, Figure 6 shows that the model matches the distribution of house price growth across the 895 locations in the data very well. The correlation between model predicted house price growth and empirical house price growth is 0.50. This suggests that the model captures the bulk of the cross-sectional forces at work during the pandemic period.

The model also produces estimates for the effect of net migration on house prices that closely match those in the data. An additional percentage point of net migration raises house prices according to our IV regression by 1.30% in the model and 1.15% in the data (column 7, Table 4). In our reduced form regression the effect is 1.15% in the model and 1.00% in the data (column 3, Table 4). These estimates were not targeted in the calibration and suggest that the effect of housing demand on house prices in the model is reasonable.

4.8 Aggregation

Table 9 shows how our aggregation approach fares in the model laboratory. Row (1) corresponds to our approach in the data: we extrapolate from the IV regression that controls for migration. Column (1) of row (1) shows that this yields a 16.0% increase in aggregate house prices. The model matches the extrapolation from the data in Section 3.6 by construction: we target the IV coefficient with migration controls in the data (0.98) as well as the aggregate 2020 remote work share (16.3%). Therefore the model also matches their product.

In column (2) we report the "true" aggregate effect of the shift to remote work on house prices in the baseline model, 17.7%.[22] It is 10% larger than our extrapolated effect. This is because more remote-intensive areas have a higher initial housing expenditure share. In

[22]The true aggregate effect excludes the effect of the migration shocks α_l on house prices. Results are essentially identical when these shocks are included.

the simple case in which there is no migration and all wages are equal to 1, the equilibrium pandemic house price growth is given by

$$\frac{p_l'}{p_l} = \left[1 + \frac{(s_{r|l}' - s_{r|l})(\theta_r - \theta_b)}{\theta_b + s_{r|l}(\theta_r - \theta_b)}\right]^{\frac{1}{\xi}} \tag{6}$$

where $(s_{r|l}' - s_{r|l})(\theta_r - \theta_b)$ is change in housing demand from remote work and $\theta_b + s_{r|l}(\theta_r - \theta_b)$ is the initial housing expenditure share. High remote areas experience a larger housing demand shock, but the effect on house prices is discounted by the larger initial housing expenditure share. This force moderates the increase in house prices in high remote areas relative to low remote areas and therefore reduces the implied cross-sectional effect of remote work on house prices. Since we target a fixed cross-sectional coefficient, our calibration will compensate for this effect by increasing the housing expenditure share for remote workers θ_r until we match our target. And a larger inferred value for θ_r implies that the aggregate increase in housing demand and house prices is bigger. Thus, the model implies that our extrapolation in Section 3.6 understates the true aggregate effect. Consistent with this mechanism, Figure A3 documents that the housing expenditure share is larger in high remote areas.

In column (4) we show how much of the increase in house prices remains if the housing supply elasticity reverts to its pre-pandemic level. We assume that housing supply is $H_{lt}^S = \bar{H}_l p_l^{\phi_l}$, where $\phi_l > 0$ is the location-specific housing supply elasticity. We calibrate the long-run housing supply elasticities ϕ_l with the estimates from Baum-Snow and Han (2019). Because Baum-Snow and Han do not calculate supply elasticities for micropolitan areas, we extrapolate for those areas from an OLS regression of the housing supply elasticity on log density and its square.[23] We fix all other parameters at their 2021 level and calculate house prices, housing demand, location decisions, and work-mode decisions in general equilibrium.[24] We find that house prices will remain elevated by 13.0%. This is close to our extrapolation (16.0%) and suggests that remote work will have a significant, lasting effect on house prices.

[23]The average housing supply elasticity in our sample is 0.5 and the population-weighted average is 0.3.
[24]We assume that the income from housing production accrues to investors.

Row (2) of Table 9 shows that extrapolating from the reduced form significantly understates the true aggregate effects in the model. The reduced form coefficient multiplied by the mean remote share exposure predicts an aggregate effect of increased remote work on house prices of $1.763 \times 5.2 = 9.2$ percentage points. This is 48% smaller than the true effect in the model. The main reason for this difference is that the incidence of the remote work shock is uneven: whereas aggregate remote work increases by a factor of 3, the first stage coefficient is only 1.80. In effect, the reduced form does not take into account that the treatment scales imperfectly with the initial remote work exposure.

Row (3) shows that the model cautions against extrapolating from regressions that do not control for migration. The extrapolated effect in column (1) for this specification is $1.307 \times 16.3 = 21.3$ percentage points. This is 21% larger than the true effect in the model. Because households migrate from low remote towards the high remote areas, house prices in low remote areas will be lower and house prices in high remote areas will be higher, all else equal. This increases the cross-sectional coefficient on remote work and thus the extrapolated effect. But because migration does not increase aggregate housing demand, one should not extrapolate from this estimate to the aggregate effect.

4.9 Robustness

The remaining rows of Table 9 show that our aggregation approach works across a range of alternative parameterizations. For each parameterization we repeat the inference from row (1): we find the location parameters that are needed to match the distribution of remote work, occupation, and location choices, and the remote work housing share θ_r that is needed to match our IV estimate.

Row (4) sets a higher demand elasticity $\zeta = 1$, whereas row (5) sets a lower demand elasticity $\zeta = 0.5$. A higher demand elasticity implies that a given increase in housing demand will have a weaker effect on prices. But this would also lower the cross-sectional effect of the shift to remote work on house prices. To match our cross-sectional estimate the calibration compensates by increasing the housing demand of remote workers θ_r. The extrapolated aggregate effect is therefore unchanged by construction. If we pick a lower demand elasticity the same logic applies but the signs flip.

Column (2) shows that the aggregate effects are slightly larger with a higher demand elasticity and slightly smaller with a lower demand elasticity. This reflects the different inferred values for the remote housing share θ_r. A higher demand elasticity increases in the housing expenditure in high remote areas versus low remote areas (due to the higher inferred θ_r). This reinforces the dampening of the housing demand shock in high remote areas relative to low remote areas, and therefore implies an even larger aggregate effect for a given extrapolated effect. For both parameterizations the extrapolated effect remains a lower bound because the housing expenditure shares remain larger in the high remote areas.

By targeting the cross-sectional estimate of remote work on house prices, our aggregation approach is therefore robust to variations in parameters that affect the partial equilibrium shift in or slope of housing demand. Any change in parameter that implies a smaller shift in housing demand or flatter slope will be compensated for by a larger inferred value for remote housing demand θ_r. The cross-sectional effect of remote work on house prices is therefore highly informative about the aggregate effect in the spirit of Nakamura and Steinsson (2018).

In row (6) we allow housing supply to also respond in the short-run. For illustration, we assume that the location-specific housing supply elasticities in 2021 are one half of their long-run values in Baum-Snow and Han (2019). In order to match the same cross-sectional estimate with elastic housing supply, our model infers a larger housing demand shock. A higher value for the remote housing share θ_r in turn implies that the housing expenditure shares in high remote areas are larger than in our baseline model. Since this variation in expenditure shares dampens house price movements relatively more in high-remote areas, we obtain an even larger true effect on house prices than our baseline (column 2). Thus, our extrapolation again provides a lower bound.

If supply was already somewhat elastic during the pandemic, then there will be less additional supply forthcoming in the long-run. The implied long-run effect of remote work on house prices in this parameterization, 17.9%, is therefore substantially larger than in our baseline calibration (13.0%).

Rows (7) and (8) change the shape parameter of the Frechet distribution to $\kappa = 0.1$ and $\kappa = 8$ respectively, versus $\kappa = 1$ in our baseline model. This large range of values has essentially no effect on the aggregation through the model. Because we target the observed distributions of the population and remote work, any change in κ will be absorbed by the inferred location parameters μ_{folw} that are needed to rationalize the data.

In row (9) we assume that the remote work share in 2021 is 30 percent rather than 42.8 percent. Our calibration infers that if there are fewer remote workers, then each remote worker must have had a higher housing demand θ_r to match the cross-sectional estimates. This implies that the initial housing expenditure shares are higher in high-remote areas, which again dampens house price movements in high- versus low-remote areas. We therefore infer a larger aggregate effect and that our extrapolation again provides a lower bound.

The same result arises in row (10) when we allow the initial housing expenditure shares to differ based on the variation in relative house prices across location.[25] Since house prices are, on average, higher in high-remote areas, this specification also implies a higher housing expenditure share and in high-remote areas, and a larger "true" aggregate effect.

In row (11) we assume that remote work carries a productivity penalty of 10%. This penalty is large relative to the existing estimates that suggest unchanged or slight increases in productivity (Bloom, Liang, Roberts, and Ying, 2015; Bloom, Han, and Liang, 2022). In our model the penalty limits the increase in housing demand workers switching to remote, so to match our cross-sectional estimates our calibration compensates by raising the remote housing expenditure share θ_r. These two effects essentially cancel out on the "true" aggregate effect so that our extrapolation once more provides a lower bound.

In row (12) we add measurement error to the 2020 remote share distribution. We multiply

[25] Price data is from Redfin. For the 124 areas with missing median price per square foot, we extrapolate from a regression of the median price per square foot on log density and its square.

the observed shares with errors from a log-normal distribution such that the mismeasured 2020 remote share deviates from the empirical distribution by 16% on average. The true aggregate effect is essentially unaffected. This reflects that the IV estimate is asymptotically invariant to measurement error in the instrumented variable.

In summary, across a broad range of parameterizations the extrapolated aggregate effect from the IV regression with migration control is a lower bound on the true aggregate effect. This shows that our approach solves the aggregation problem in the context of negative spillovers from migration. We also find that the extrapolated aggregate effect is close to the model-implied long-run effect of remote work on house prices.

5 Conclusion

We show that the shift to remote work caused a large increase in housing demand. In turn, this increase in housing demand caused house prices and rents to increase sharply. Based on our cross-sectional estimates controlling for migration spillovers, we argue that remote work accounts for at least one half of the 23.8 percent increase in house prices from December 2019 to November 2021. 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 demand. Our results suggest that the increase in house prices over this period largely reflect fundamentals rather than a speculative bubble, and that lower interest rates and fiscal stimulus were of lesser importance.

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. If remote work persists, 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.

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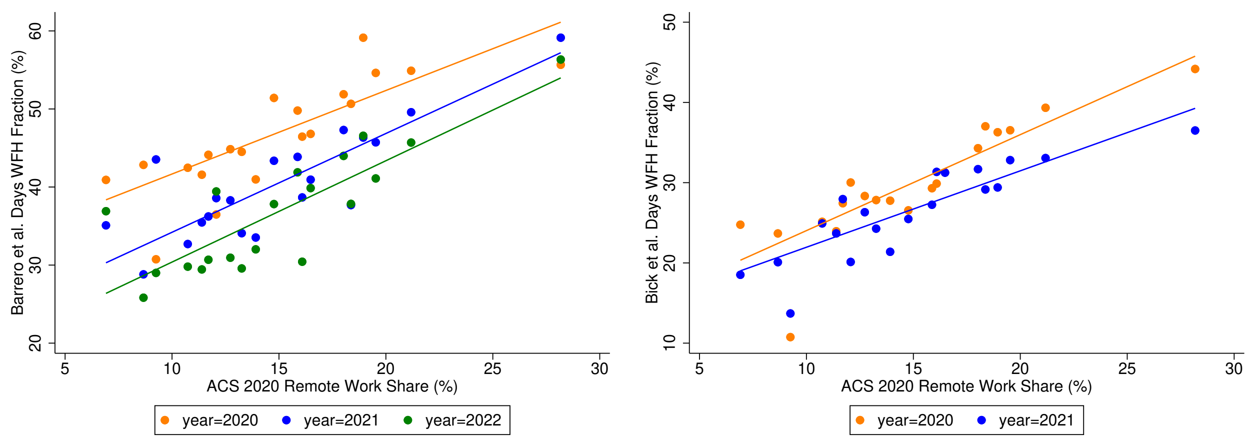
6 Figures

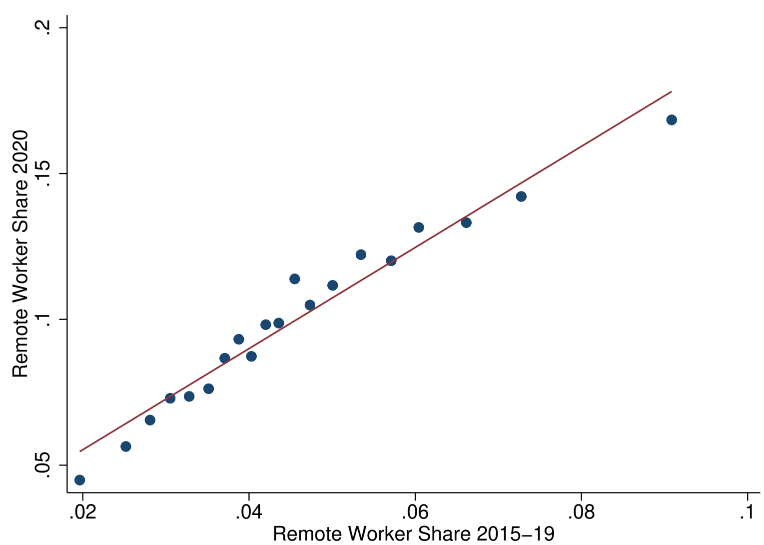
FIGURE 1
State-level Comparison of Survey Measures of Remote Work

A. Barrero, Bloom, and Davis (2021) and ACS Remote Work | **B. Bick, Blandin, and Mertens (2021) and ACS Remote Work**



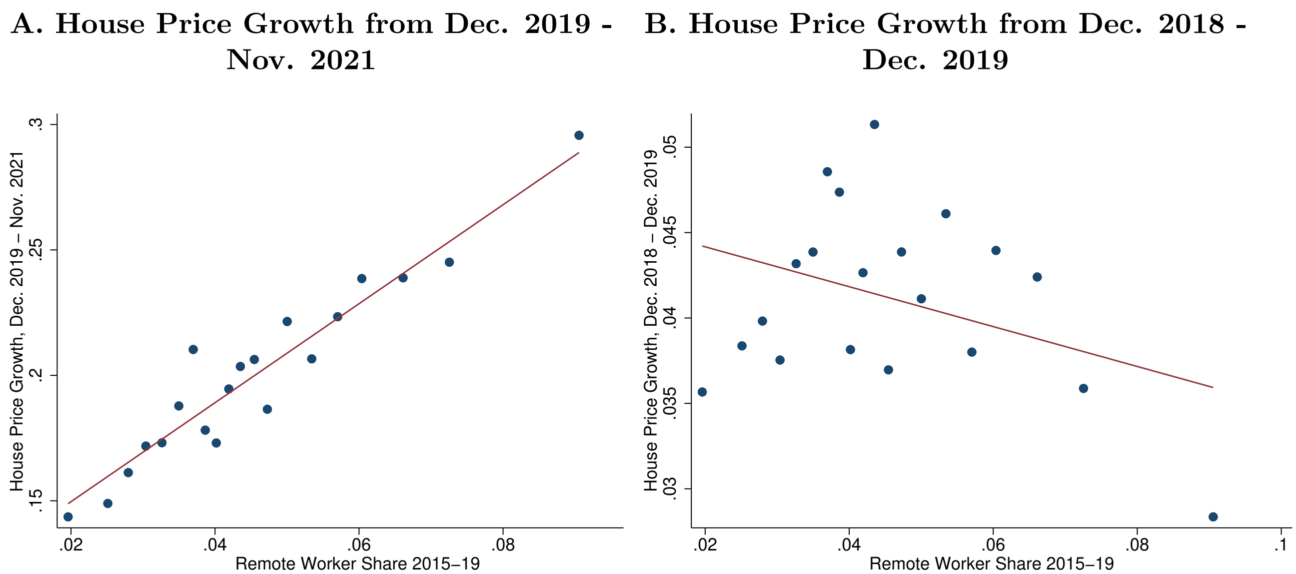
Sources: American Community Survey, Barrero, Bloom, and Davis (2021), Bick, Blandin, and Mertens (2021) and authors' calculations.

FIGURE 2
Binned Scatter Plot of Remote Worker Share 2020 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



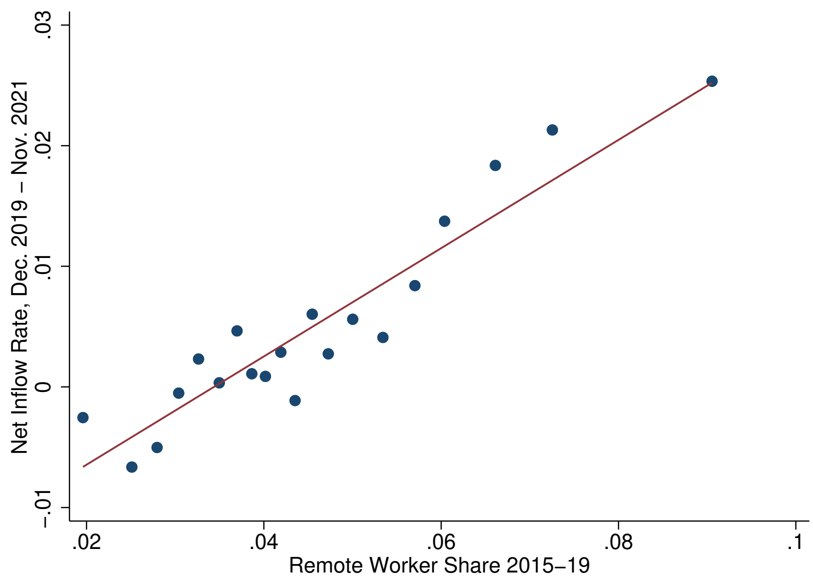
Sources: Zillow, American Community Survey, and authors' calculations.

FIGURE 4
Event Analysis of House Price Growth on Remote Worker Share 2015-19

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.

FIGURE 6

Model Predicted House Price Growth against Data House Price Growth

Sources: Zillow, American Community Survey, FRBNY/Equifax Consumer Credit Panel, and authors' calculations.

7 Tables

TABLE 1
Summary Statistics

	Mean	Weighted Mean	SD	Min	Max	N
Housing Demand						
House Price Growth	0.200	0.238	0.083	−0.133	0.498	895
HP Growth Pre-Pandemic	0.041	0.030	0.023	−0.074	0.138	895
Rent Growth	0.168	0.149	0.103	−0.402	0.392	178
Rent Growth Pre-Pandemic	0.025	0.024	0.032	−0.111	0.129	178
Remote Worker Shares						
Remote Worker Share 2015-19	0.046	0.052	0.017	0.011	0.140	895
Remote Worker Share 2020	0.100	0.163	0.048	0.014	0.297	895
Control Variables						
Net Inflow Rate Pandemic	0.005	−0.000	0.024	−0.068	0.192	895
Net Inflow Rate Pre-Pandemic	0.000	−0.000	0.012	−0.047	0.070	895
Log Density	3.728	5.828	1.235	−0.390	8.825	895
Δ Unemp. Rate 11/2019-11/2021	0.000	0.006	0.010	−0.069	0.044	895
Δ Unemp. Rate 2019-2020	0.034	0.046	0.015	0.002	0.161	895
Unemp. Rate 2019	0.040	0.036	0.014	0.016	0.208	895
Wage Growth Pandemic	0.155	0.172	0.045	−0.044	0.508	895
Wage Growth Pre-Pandemic	0.029	0.033	0.025	−0.101	0.191	895
Total Dividends / AGI	0.034	0.045	0.022	0.002	0.307	895
Share White	0.742	0.596	0.183	0.035	0.967	895
Share Black	0.086	0.129	0.113	0.002	0.584	895
Share Asian	0.020	0.062	0.037	0.001	0.505	895
Share Hispanic	0.117	0.185	0.153	0.004	0.954	895
Share College	0.169	0.238	0.058	0.061	0.405	895
Log Median Income	10.928	11.103	0.183	10.364	11.721	895
Share 65+	0.177	0.153	0.037	0.077	0.461	895

Sources: Zillow, Apartment List, American Community Survey, FRBNY/Equifax Consumer Credit Panel, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, 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	1.81***
	(0.19)
Share College	0.036
	(0.026)
Log Median Income	0.0046
	(0.0049)
Unemp. Rate 2019	−0.019
	(0.045)
Wage Growth Pre-Pandemic	−0.0075
	(0.015)
Total Dividends / AGI	0.046**
	(0.022)
Log Density	−0.00087
	(0.00072)
Share 65+	0.084***
	(0.021)
January Temperature	0.00037***
	(0.000092)
July Temperature	−0.00068***
	(0.00014)
July Humidity	−0.00019**
	(0.000073)
Race Controls	Yes
CBSA Clusters	50
R^2	0.67
Observations	895

Sources: Zillow, Apartment List, 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 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 House Price Growth Regressions

Dependent Variable:	Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.74^{***}	1.71^{***}	1.59^{***}	1.44^{***}
	(0.11)	(0.11)	(0.084)	(0.077)
HP Growth Pre-Pandemic		-0.21^{***}	-0.089^{*}	-0.068
		(0.063)	(0.050)	(0.043)
Nonparametric Density Control	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.39	0.40	0.63	0.64
Observations	895	895	895	895

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

Notes: This table reports a first stage regression. The dependent variables is the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 House Price Growth, Dec. 2019 - Nov. 2021

Dependent Variable:	House Price Growth, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.97***	2.05***	2.08***	1.98***				
	(0.31)	(0.29)	(0.26)	(0.24)				
Remote Worker Share 2020					1.14***	1.20***	1.30***	1.37***
					(0.17)	(0.16)	(0.16)	(0.16)
HP Growth Pre-Pandemic		0.62**	0.78***	0.74***		0.87***	0.89***	0.83***
		(0.24)	(0.22)	(0.22)		(0.22)	(0.23)	(0.22)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					248.11	222.47	356.56	346.96
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.17	0.20	0.34	0.39	0.11	0.15	0.19	0.23
Observations	895	895	895	895	895	895	895	895

Sources: Zillow, Apartment List, 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 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 - NOV. 2021

	Coeff.	S.E.	F-Stat	N
Residential Rent				
(1) Rent Growth	1.09***	(0.33)	120.7	178
(2) House Price Growth (Rent Sample)	1.03***	(0.28)	133.2	178
Commercial Rent (Reduced Form)				
(3) Commercial Rent Growth	−0.26*	(0.13)		25
(4) House Price Growth (Com. Rent Sample)	2.37***	(0.65)		25
Local Inflation (Reduced Form)				
(5) Inflation excl. Shelter	0.44	(0.33)		22
(6) House Price Growth (Inflation Sample)	2.98***	(0.70)		22
Housing Supply				
(7) Permit Growth	2.03**	(0.91)	352.1	714
(8) Growth of Cumulative Homes Sold	−0.16	(0.35)	260.8	544

Sources: Zillow, Apartment List, 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 indicated in the table row. The IV specifications in rows (1)-(2) and (7)-(8) 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 (3)-(6) 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 House Price Growth, Dec. 2019 - Nov. 2021

Dependent Variable:	House Price Growth, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.31***	1.38***	1.47***	1.43***				
	(0.23)	(0.23)	(0.17)	(0.15)				
Remote Worker Share 2020					0.73***	0.78***	1.00***	0.98***
					(0.11)	(0.11)	(0.11)	(0.11)
HP Growth Pre-Pandemic		0.38*	0.49***	0.46***		0.53***	0.55***	0.52***
		(0.20)	(0.16)	(0.16)		(0.18)	(0.17)	(0.16)
Net Inflow Rate Pandemic	1.16***	1.12***	1.06***		1.29***	1.25***	1.15***	
	(0.24)	(0.22)	(0.24)		(0.22)	(0.20)	(0.22)	
Net Inflow Rate Pre-Pandemic	0.93***	0.84***	0.81***		0.88***	0.74***	0.71***	
	(0.27)	(0.26)	(0.23)		(0.25)	(0.25)	(0.22)	
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	Yes	Yes	No	No	Yes	Yes
Stock Exposure Control	No	No	Yes	Yes	No	No	Yes	Yes
Nonparametric Migration Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					220.19	211.26	297.60	300.62
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.33	0.34	0.50	0.53	0.37	0.39	0.44	0.47
Observations	895	895	895	895	895	895	895	895

Sources: Zillow, Apartment List, 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 - Nov. 2021. 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 2020. Nonparametric migration controls include deciles of pandemic net migration and pre-pandemic net migration. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 and on Household Size by Moving Status, Dec. 2019 - Nov. 2021

	Coeff.	S.E.	F-Stat	N
House Price Growth				
(1) 1 Bedroom	0.66^{***}	(0.17)	224.9	666
(2) 2 Bedroom	0.78^{***}	(0.13)	226.4	666
(3) 3 Bedroom	0.93^{***}	(0.12)	217.3	666
(4) 4 Bedroom	0.84^{***}	(0.14)	215.7	666
(5) 5 Bedroom	0.90^{***}	(0.14)	216.7	666
Growth of Household Size				
(6) All Households	-0.03	(0.02)	277.8	895
(7) Movers	-0.18^{***}	(0.06)	294.9	895
(8) Stayers	0.03^{*}	(0.02)	288.2	895

Sources: Zillow, Apartment List, 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 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. Household size regressions also control for the log of the pre-pandemic average household size in a CBSA. 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.

TABLE 8
Model Calibration

	Name	Value / Time	Source / Target
Fixed Parameters			
N	Number of Locations	895	Number of Locations
O	Number of Occupations	8	Aggregate of 4-Digit Occupations
ζ	Housing Demand Elasticity	0.67	Albouy, Ehrlich, and Liu (2016)
κ	Frechet Shape Parameter	1	Convention
θ_b	Housing Share Office	0.24	Davis and Ortalo-Magné (2011)
θ_r	Housing Share Remote	0.33	Cross-Sectional IV Estimate
p_l	House Price by Location	1	Normalization
z_f	Wages by Firm	varies	2015-19 Occupation-Specific Wages
μ_{ol}	Location Amenities by Occupaton	varies	2015-19 Occupation-Location Distribution
Time-varying Parameters			
μ_{lr}	Remote Amenities by Location	Steady State	2015-19 Remote Share by Location
		2021	Scaled 2020 Remote Share by Location
μ_{ob}	Office Amenities by Occupation	Steady State	2015-19 Remote Share by Occupation
		2021	Scaled 2020 Remote Share by Occupation
$\mu_{l=\ell(f)}$	Amenity to Stay in Firm Location	Steady State	2018-19 Gross Migration Rate
		2021	2019-21 Gross Migration Rate
α_l	Firm Location Migration	Steady State	Normalized to 1
		2021	Pandemic Net Migration

This table reports the baseline calibration of the model in Section 4.

TABLE 9

Aggregate Effect of Remote Work on House Price Growth in the Model

Row	Extrapolating From	Extrapolated Aggregate Effect (1)	“True” Aggregate Effect (2)	$\frac{\text{Extrapolated}}{\text{“True”}}$ (3)	With Elastic Long-Run Supply (4)
	Baseline Parameterization				
(1)	IV, Migration Control	0.160	0.177	0.904	0.130
(2)	RF, Migration Control	0.092	0.177	0.521	0.130
(3)	IV, No Migration Control	0.213	0.177	1.206	0.130
	Alternative Parameterizations				
(4)	IV, Higher Demand Elasticity	0.160	0.184	0.869	0.144
(5)	IV, Lower Demand Elasticity	0.160	0.172	0.930	0.121
(6)	IV, Higher Supply Elasticity	0.160	0.204	0.782	0.179
(7)	IV, Lower Kappa	0.160	0.176	0.908	0.130
(8)	IV, Higher Kappa	0.160	0.182	0.880	0.134
(9)	IV, Smaller Remote Shift	0.160	0.220	0.725	0.163
(10)	IV, Heterogeneous Housing Shares	0.160	0.195	0.818	0.143
(11)	IV, Productivity Loss if Remote	0.160	0.178	0.897	0.132
(12)	IV, Error in 2020 Remote Share	0.160	0.180	0.890	0.133

This table reports regression results from the model in Section 4. The dependent variable is house price growth. The IV specifications use the remote work share from 2015-19 as instrument for the remote share in 2020. The reduced form (RF) specifications use the remote work share from 2015-19 as a regressor. All specifications, except row (3) control for net migration over the pandemic. The extrapolated aggregate effects in column (1) are based on the cross-sectional coefficient on remote work multiplied by the population mean of remote work. The true aggregate effect in column (2) is based on the true effects in the model and exclude the effects of the exogenous migration shocks α. The model has a zero housing supply elasticity (except in row 6). Column (4) shows the model-implied effect of remote work on house prices if the housing supply elasticity normalizes to pre-pandemic levels.

1 Appendix

1.1 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

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

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_2 is,

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

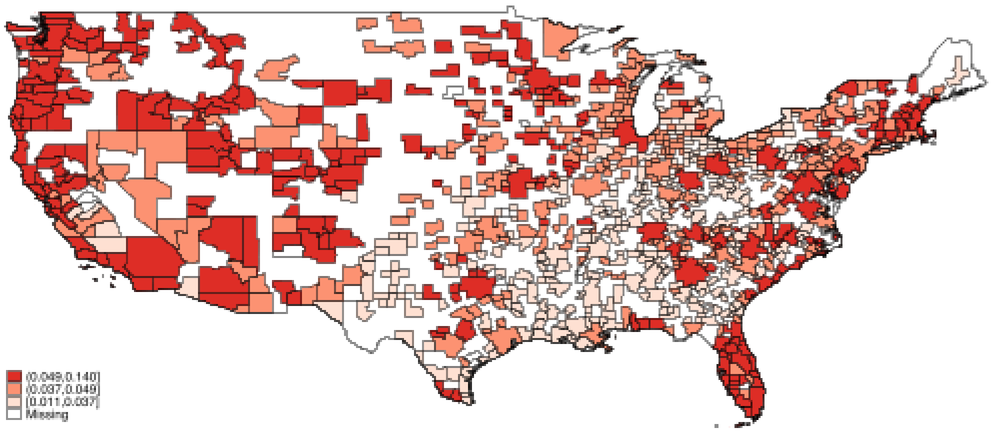
Line 1 uses $(I-P)'(I-P) = (I-P)$ and line 3 substitutes $(I-P)M = 0$. Line 4 uses Slutsky theorem and $E(RW'\epsilon_1) = 0$.

Empirically, we observe $E(RW'M) > 0$. OLS will then identify β_2 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_2^{OLS} will be biased downward.

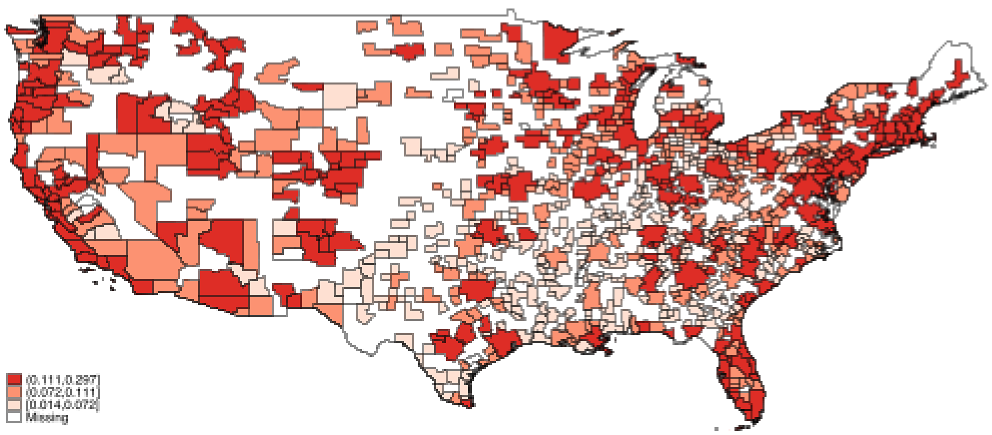
1.2 Figures

FIGURE A1
Geographic Distribution of Remote Worker Share

A. 2015-19 Average



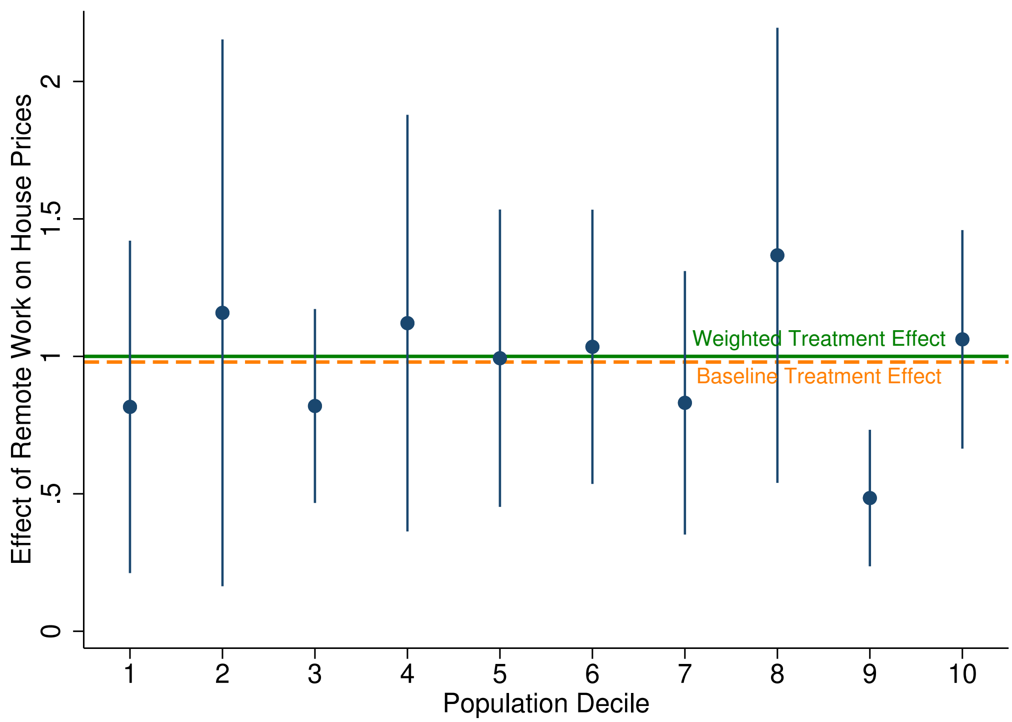
B. 2020



Sources: American Community Survey and authors calculations.

FIGURE A2

Variation in Effect of Remote Work on House Prices by Population

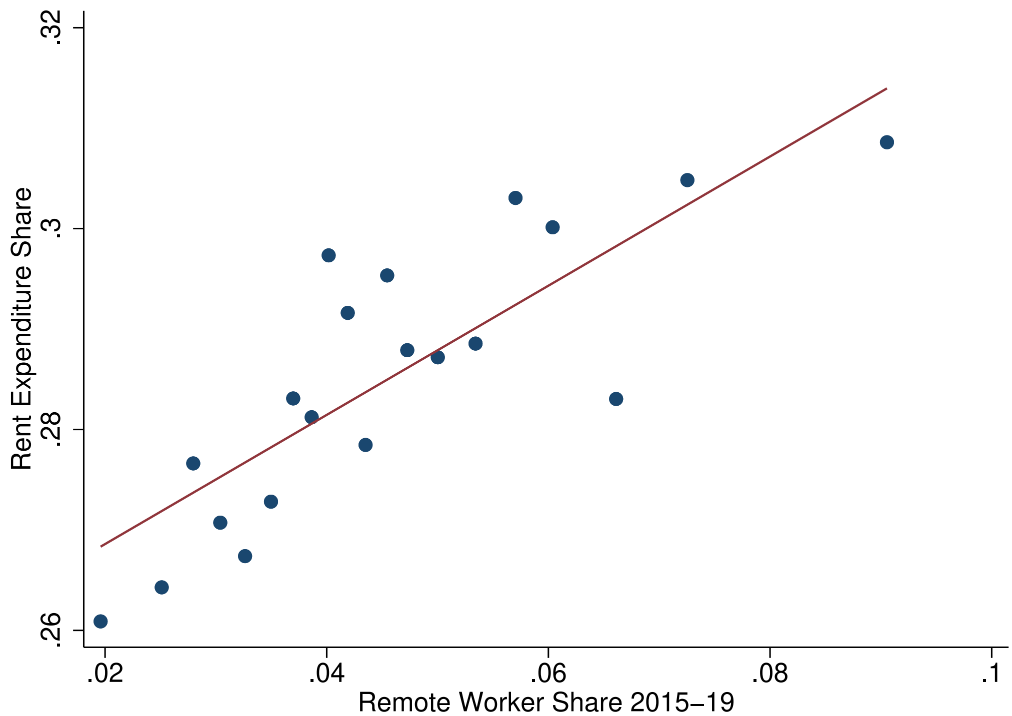


Sources: Zillow, Apartment List, American Community Survey, FRBNY/Equifax Consumer Credit Panel, Local Area Unemployment Statistics, QCEW, U.S. Census, IRS, and authors calculations.

Notes: The dependent variable is house price growth in a CBSA from Dec. 2019 - Nov. 2021. The error bars report the coefficients on the remote worker share in 2020 interacted with population decile instrumented by the remote worker share from 2015-19 interacted with population decline. The regression includes the full set of controls of column (8) in Table 6. The dashed orange line is the population weighted average of these 10 estimates. The solid green line is unweighted estimate from column (8) in Table 6. 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.

FIGURE A3

Binned Scatter Plot of Rent Expenditure Share on Remote Worker Share 2015-19



Sources: American Community Survey and authors calculations.

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)
Predicted Remote Worker Share 2015-19	2.36***									
	(0.17)									
Share College		0.16***								
		(0.018)								
Log Median Income			0.040***							
			(0.0066)							
Unemp. Rate 2019				−0.24**						
				(0.11)						
Wage Growth Pre-Pandemic					0.060*					
					(0.031)					
Total Dividends / AGI						0.31***				
						(0.064)				
Log Density							−0.00085			
							(0.00086)			
Share 65+								0.091***		
								(0.033)		
January Temperature									0.00026	
									(0.00017)	
July Temperature									−0.0015***	
									(0.00021)	
July Humidity									−0.00032***	
									(0.000085)	
Race Controls	No	No	No	No	No	No	No	No	No	Yes
CBSA Clusters	50	50	50	50	50	50	50	50	50	50
R^2	0.52	0.29	0.18	0.04	0.01	0.16	0.00	0.04	0.27	0.16
Observations	895	895	895	895	895	895	895	895	895	895

Sources: Zillow, Apartment List, 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 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 A2

EFFECT OF REMOTE WORK ON HOUSE PRICE GROWTH (OLS), DEC. 2019 - NOV. 2021

Dependent Variable:	House Price Growth, Dec. 2019 - Nov. 2021			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2020	0.71***	0.76***	0.63***	0.56***
	(0.092)	(0.085)	(0.100)	(0.087)
HP Growth Pre-Pandemic		0.73***	0.75***	0.72***
		(0.24)	(0.25)	(0.24)
Nonparametric Density Control	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.17	0.21	0.29	0.35
Observations	895	895	895	895

Sources: Zillow, Apartment List, 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 - Nov. 2021. The columns report an OLS regression on the remote worker share in 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

First Stage for Rent Growth Regressions

Dependent Variable:	Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	2.45***	2.47***	2.05***	1.94***
	(0.27)	(0.29)	(0.18)	(0.18)
Rent Growth Pre-Pandemic		−0.060	0.035	−0.013
		(0.10)	(0.081)	(0.076)
Nonparametric Density Control	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes
CBSA Clusters	45	45	45	45
R^2	0.46	0.46	0.72	0.74
Observations	178	178	178	178

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

Notes: This table reports a first stage regression. The dependent variables is the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

Effect of Remote Work on Rent Growth, Dec. 2019 - Nov. 2021

Dependent Variable:	Rent Growth, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.38	1.22	1.81**	2.11***				
	(0.86)	(0.83)	(0.68)	(0.58)				
Remote Worker Share 2020					0.56	0.50	0.88***	1.09***
					(0.35)	(0.35)	(0.34)	(0.33)
Rent Growth Pre-Pandemic		0.47	0.27	0.084		0.50	0.24	0.099
		(0.54)	(0.51)	(0.39)		(0.55)	(0.52)	(0.40)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					84.92	70.87	134.47	120.72
CBSA Clusters	45	45	45	45	45	45	45	45
R^2	0.04	0.06	0.28	0.43	-0.09	-0.05	0.17	0.25
Observations	178	178	178	178	178	178	178	178

Sources: Zillow, Apartment List, 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 rent growth in a CBSA from Dec. 2019 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 HOUSE PRICE GROWTH (RENT SAMPLE), DEC. 2019 - NOV. 2021

Dependent Variable:	House Price Growth (Rent Sample), Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	2.51^{***}	2.51^{***}	2.28^{***}	2.00^{***}				
	(0.68)	(0.63)	(0.66)	(0.53)				
Remote Worker Share 2020					1.02^{***}	1.02^{***}	1.11^{***}	1.03^{***}
					(0.26)	(0.24)	(0.30)	(0.28)
House Price Growth Pre-Pandemic		0.56^{*}	0.88^{**}	0.61^{*}		0.85^{**}	0.88^{**}	0.65^{**}
		(0.31)	(0.40)	(0.33)		(0.34)	(0.37)	(0.30)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					84.92	72.71	146.90	133.22
CBSA Clusters	45	45	45	45	45	45	45	45
R^2	0.23	0.26	0.38	0.55	-0.02	0.05	0.25	0.42
Observations	178	178	178	178	178	178	178	178

Sources: Zillow, Apartment List, 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 (rent sample) in a CBSA from Dec. 2019 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 Permit Growth, Dec. 2019 - Nov. 2021

Dependent Variable:	Permit Growth, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	2.19*	2.21*	2.84**	2.96**				
	(1.19)	(1.21)	(1.34)	(1.34)				
Remote Worker Share 2020					1.31*	1.33*	1.75**	2.03**
					(0.73)	(0.74)	(0.83)	(0.91)
Permit Growth Pre-Pandemic		−0.68***	−0.69***	−0.67***		−0.69***	−0.69***	−0.68***
		(0.13)	(0.13)	(0.13)		(0.13)	(0.13)	(0.13)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					181.77	180.58	315.46	352.05
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.00	0.14	0.17	0.20	-0.01	0.12	0.16	0.18
Observations	714	714	714	714	714	714	714	714

Sources: Zillow, Apartment List, 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 permit growth in a CBSA from Dec. 2019 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

Effect of Remote Work on Cumulative Homes Sold Growth, Dec. 2019 - Nov. 2021

Dependent Variable:	Cumulative Homes Sold Growth, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	−1.03	−0.36	−0.14	−0.23				
	(0.66)	(0.51)	(0.53)	(0.54)				
Remote Worker Share 2020					−0.60	−0.21	−0.080	−0.16
					(0.38)	(0.29)	(0.30)	(0.35)
Homes Sold Growth Pre-Pandemic		0.21***	0.21***	0.20***		0.21***	0.20***	0.20***
		(0.016)	(0.015)	(0.015)		(0.016)	(0.015)	(0.014)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					137.55	135.69	241.78	260.83
CBSA Clusters	47	47	47	47	47	47	47	47
R^2	0.01	0.43	0.46	0.47	0.03	0.44	0.47	0.48
Observations	544	544	544	544	544	544	544	544

Sources: Zillow, Apartment List, 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 cumulative homes sold growth in a CBSA from Dec. 2019 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 Net Inflow Rate Pandemic, Dec. 2019 - Nov. 2021

Dependent Variable:	Net Inflow Rate Pandemic, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	0.45***	0.28***	0.30***	0.25***				
	(0.11)	(0.064)	(0.065)	(0.058)				
Remote Worker Share 2020					0.26***	0.16***	0.18***	0.17***
					(0.065)	(0.035)	(0.041)	(0.039)
Net Inflow Rate Pre-Pandemic		1.11***	1.03***	1.01***		1.12***	1.03***	1.01***
		(0.15)	(0.13)	(0.11)		(0.16)	(0.13)	(0.12)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
F-Statistic					248.11	228.40	322.12	341.54
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.11	0.39	0.44	0.47	-0.14	0.29	0.37	0.41
Observations	895	895	895	895	895	895	895	895

Sources: Zillow, Apartment List, 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 the pandemic net inflow rate in a CBSA from Dec. 2019 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

First Stage for House Price Growth Regressions

Dependent Variable:	Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.80***	1.77***	1.46***	1.46***
	(0.12)	(0.12)	(0.085)	(0.084)
HP Growth Pre-Pandemic		−0.19***	−0.063	−0.065
		(0.057)	(0.042)	(0.041)
Net Inflow Rate Pandemic	−0.18*	−0.16*	−0.089	
	(0.098)	(0.091)	(0.062)	
Net Inflow Rate Pre-Pandemic	0.079	0.12	0.10	
	(0.13)	(0.13)	(0.090)	
Nonparametric Density Control	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	Yes	Yes
Stock Exposure Control	No	No	Yes	Yes
Nonparametric Migration Control	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.39	0.40	0.64	0.65
Observations	895	895	895	895

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

Notes: This table reports a first stage regression. The dependent variables is the remote worker share in 2020. 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. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 HOUSE PRICE GROWTH BY SOURCE OF VARIATION, DEC. 2019 - NOV. 2021

Source of Instrument Variation:	Occupation	Climate	Other	Occupation	Climate	Other
	No Migration Control			Migration Control		
	(1)	(2)	(3)	(4)	(5)	(6)
RHS variables:						
Remote Worker Share 2020	0.64***	1.05***	2.89***	0.68***	1.18***	1.59***
	(0.13)	(0.31)	(0.45)	(0.100)	(0.24)	(0.36)
Unused Remote Share Variation	1.79***	0.41	−5.03***	0.79***	−0.35	−1.94**
	(0.25)	(0.45)	(1.23)	(0.20)	(0.38)	(0.96)
HP Growth Pre-Pandemic	0.72***	0.81***	0.83***	0.49***	0.52***	0.55***
	(0.21)	(0.23)	(0.27)	(0.16)	(0.16)	(0.18)
Nonparametric Density Control	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wage & Unemployment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock Exposure Control	Yes	Yes	Yes	Yes	Yes	Yes
Nonparametric Migration Control	No	No	No	Yes	Yes	Yes
p-value Overidentification		0.00			0.37	
F-Statistic	125.78	65.48	47.21	138.60	67.42	44.83
CBSA Clusters	50	50	50	50	50	50
R^2	0.40	0.31	-0.29	0.51	0.43	0.34
Observations	895	895	895	895	895	895

Sources: Zillow, Apartment List, 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 - Nov. 2021. The columns report an instrumental variable regression in which the remote work share in 2020 is instrumented by predicted remote work share from 2015-19 (columns 1 and 3), the average January and July temperature (columns 2 and 4) 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. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

First Stage for House Price Growth Regressions

Dependent Variable:	Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	0.60***	0.60***	0.70***	0.72***
	(0.11)	(0.10)	(0.091)	(0.092)
HP Growth Pre-Pandemic		0.062*	0.016	0.026
		(0.034)	(0.029)	(0.030)
Share College	0.49***	0.50***	0.44***	0.46***
	(0.032)	(0.032)	(0.037)	(0.037)
Log Median Income	0.033***	0.035***	0.017	0.020*
	(0.010)	(0.011)	(0.011)	(0.011)
Nonparametric Density Control	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes
Climate Controls	Yes	Yes	Yes	Yes
CBSA Clusters	50	50	50	50
R^2	0.74	0.74	0.77	0.77
Observations	895	895	895	895

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

Notes: This table reports a first stage regression. The dependent variables is the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

EFFECT OF REMOTE WORK ON HOUSE PRICE GROWTH, DEC. 2019 - NOV. 2021

Dependent Variable:	House Price Growth, Dec. 2019 - Nov. 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	1.04***	1.13***	1.38***	1.26***				
	(0.34)	(0.29)	(0.25)	(0.24)				
Remote Worker Share 2020					1.74***	1.86***	1.97***	1.76***
					(0.46)	(0.41)	(0.30)	(0.29)
HP Growth Pre-Pandemic		0.81***	0.62***	0.60***		0.70***	0.59***	0.55***
		(0.18)	(0.15)	(0.15)		(0.15)	(0.15)	(0.16)
Share College	0.30***	0.34***	0.063	0.041	−0.56**	−0.59***	−0.79***	−0.77***
	(0.074)	(0.079)	(0.080)	(0.082)	(0.23)	(0.22)	(0.17)	(0.18)
Log Median Income	−0.045	−0.030	−0.028	−0.0079	−0.10**	−0.095**	−0.061	−0.042
	(0.037)	(0.037)	(0.029)	(0.028)	(0.044)	(0.043)	(0.038)	(0.033)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Demographic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Wage & Unemployment Controls	No	No	No	Yes	No	No	No	Yes
Stock Exposure Control	No	No	No	Yes	No	No	No	Yes
Region Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Climate Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Statistic					31.99	34.42	59.56	60.63
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.28	0.33	0.46	0.48	0.16	0.16	0.22	0.32
Observations	895	895	895	895	895	895	895	895

Sources: Zillow, Apartment List, 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 - Nov. 2021. 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 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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 Household Size by Moving Status, Dec. 2019 - Nov. 2021

Log Change HH Size:	All	Movers	Stayers	All	Movers	Stayers
	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
RHS variables:						
Remote Worker Share 2015-19	−0.049	−0.26***	0.047*			
	(0.032)	(0.082)	(0.027)			
Remote Worker Share 2020				−0.034	−0.18***	0.032*
				(0.022)	(0.055)	(0.019)
Log Change HH Size Pre-Pandemic	0.059	0.077**	−0.035	0.048	0.070*	−0.026
	(0.048)	(0.039)	(0.054)	(0.050)	(0.039)	(0.057)
Log HH Size Pre-Pandemic	0.040**	0.22***	−0.018*	0.039**	0.21***	−0.016*
	(0.017)	(0.034)	(0.0095)	(0.017)	(0.034)	(0.0094)
Net Inflow Rate Pandemic	0.12***	0.17	0.18***	0.12***	0.15	0.19***
	(0.030)	(0.10)	(0.022)	(0.030)	(0.10)	(0.023)
Net Inflow Rate Pre-Pandemic	−0.015	−0.35**	0.096***	−0.011	−0.33*	0.091**
	(0.052)	(0.17)	(0.037)	(0.053)	(0.17)	(0.037)
Nonparametric Density Control	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wage & Unemployment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock Exposure Control	Yes	Yes	Yes	Yes	Yes	Yes
F-Statistic				277.84	294.86	288.21
CBSA Clusters	50	50	50	50	50	50
R^2	0.15	0.14	0.28	0.15	0.13	0.27
Observations	895	895	895	895	895	895

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

Notes: The dependent variables are the log change in household size (columns 1 and 4), the log change in household size among households moving within and into the CBSA (columns 2 and 5), and the log change in household size among households staying at the same address in the CBSA (columns 3 and 6). Each column uses the average remote worker share from 2015-19 as an instrument for the remote worker share in 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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

EFFECT OF REMOTE WORK ON PROBABILITY OF REDUCING HOUSEHOLD SIZE BY MOVING STATUS, DEC. 2019 - NOV. 2021

Prob Reduce HH Size:	All	Movers	Stayers	All	Movers	Stayers
	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
RHS variables:						
Remote Worker Share 2015-19	0.048	0.11^{**}	-0.086^{***}			
	(0.059)	(0.043)	(0.030)			
Remote Worker Share 2020				0.033	0.075^{**}	-0.058^{***}
				(0.040)	(0.030)	(0.020)
Prob Reduce HH Size Pre-Pandemic	0.59^{***}	0.90^{***}	0.19^{***}	0.59^{***}	0.88^{***}	0.18^{***}
	(0.036)	(0.076)	(0.036)	(0.036)	(0.077)	(0.037)
Log HH Size Pre-Pandemic	0.074^{***}	-0.00071	0.13^{***}	0.075^{***}	0.0020	0.13^{***}
	(0.025)	(0.015)	(0.015)	(0.024)	(0.014)	(0.015)
Net Inflow Rate Pandemic	0.074^{**}	0.20^{***}	-0.13^{***}	0.077^{**}	0.21^{***}	-0.14^{***}
	(0.033)	(0.023)	(0.021)	(0.034)	(0.025)	(0.021)
Net Inflow Rate Pre-Pandemic	0.069	-0.075	0.034	0.066	-0.078^{*}	0.039
	(0.065)	(0.047)	(0.040)	(0.067)	(0.046)	(0.039)
Nonparametric Density Control	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes
Wage & Unemployment Controls	Yes	Yes	Yes	Yes	Yes	Yes
Stock Exposure Control	Yes	Yes	Yes	Yes	Yes	Yes
F-Statistic				302.09	290.34	310.26
CBSA Clusters	50	50	50	50	50	50
R^2	0.69	0.69	0.68	0.68	0.68	0.68
Observations	895	895	895	895	895	895

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

Notes: The dependent variables are the probability of reducing household size (columns 1 and 4), the probability of reducing household size among households moving within and into the CBSA (columns 2 and 5), and the probability of reducing household size among households staying at the same address in the CBSA (columns 3 and 6). Each column uses the average remote worker share from 2015-19 as an instrument for the remote worker share in 2020. Nonparametric density controls include quintiles of density. Demographic controls include the population shares identifying as white, black, asian, and hispanic respectively and quartiles of the population share above 65. Wage controls include quartiles of pandemic and pre-pandemic wage growth. Unemployment controls include the CBSA average unemployment rate in 2019, its change from 2019 to 2020, and its change from Nov. 2019 to Nov. 2021. The stock exposure control is total dividends relative to adjusted gross income 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. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$