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HOUSING DEMAND AND REMOTE WORK

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ABSTRACT

What explains record U.S. house price growth since late 2019? We show that the shift to remote work explains over one half of the 23.8 percent national house price increase over this period. Using variation in remote work exposure across U.S. metropolitan areas we estimate that an additional percentage point of remote work causes a 0.93 percent increase in house prices after controlling for negative spillovers from migration. This cross-sectional estimate combined with the aggregate shift to remote work implies that remote work raised aggregate U.S. house prices by 15.1 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 imply 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.

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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 some evidence that a significant fraction of current remote work may be permanent (Barrero, Bloom, Davis, and Meyer, 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 pre-existing exposure to the propensity for remote work. We argue based on pre-trends and extensive controls that this exposure 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 and much smaller or negative effects on local inflation and 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 aggregate increase in housing demand. Our initial estimate reflects both the increase in housing demand from working remotely as well as the relocation of housing demand through migration towards areas suited for remote work. Only the first effect represents an increase in aggregate housing demand. We show we can isolate this component by controlling for high-quality

[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.

measures of migration and find that migration accounts for one third of the total effect of remote work on house prices. Extrapolating from the remaining two thirds implies that the 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 cross-sectional distribution of house price growth over the pandemic. The model implies that controlling for migration is important to get a valid lower bound on the aggregate effect of the shift to remote work on house prices. Migration acts as a negative spillover in that it increases housing demand in areas with more remote work and reduces housing demand elsewhere, which raises the cross-sectional estimate. However, extrapolating from this estimate to aggregate house price growth would clearly be inappropriate because migration may have zero effect on aggregate housing demand. More generally, our results suggests that controlling for negative spillovers is necessary and, in some contexts, may even be sufficient in order to extrapolate from cross-sectional estimates to aggregate effects.

We begin by estimating the effect of remote work on housing demand and house prices using variation in exposure to the shift in remote work across CBSAs. 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 show the pre-pandemic share reflects the local distribution of occupations and their propensity for remote work as well as local 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.

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 in 2020, the latest available data from the American Community Survey. We estimate that an additional percentage point of remote work in 2020 increases house price growth from December 2019 to November 2021 by 1.47 percentage points according to our preferred specification.

If the shift to remote work represents a broad increase in housing demand, then we should expect to see effects on rents as well as prices. We find that the effects of remote work on rent growth are almost identical to the effects on house prices within the same sample, consistent with a broad increase in housing demand. We also show, in a more limited sample, that greater exposure to remote work predicts a decline in commercial rents, consistent with reduced demand for office space. The negative effect on commercial rents is significantly smaller in magnitude than the increase in house prices, indicating an asymmetric response of prices to increased remote work that may ultimately be inflationary. We then show that local inflation excluding shelter only weakly increases with exposure to remote work, consistent with remote work increasing housing demand relative to other expenditures. Finally, we estimate a positive effect of remote work exposure on the growth of building permits, which indicates that our results can not be explained by a relatively low housing supply elasticity in more exposed CBSAs.

It is not straightforward to extrapolate from cross-sectional estimates to aggregate effects (Nakamura and Steinsson, 2017; Chodorow-Reich, 2019). Cross-sectional effects capture spillovers across regions that may cancel out in the aggregate. These are important in our setting as we document an increase in migration towards areas pre-disposed for remote work. House prices will grow faster in cities attractive to remote work as migrants move in, while house prices will slow 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.

To isolate the aggregate increase in housing demand from remote work we must remove the effects of migration on house prices from our cross-sectional estimate. We show we can isolate the effect of remote work on housing demand by explicitly controlling for the effects of migration on house prices, although this requires a precise measure of migration across CBSAs. We use address changes in the FRBNY / Equifax Consumer Credit Panel, a 5% 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 migration accounts for around one third of the total cross-sectional effect of remote work exposure on house price growth. Thus, after controlling for net migration, an additional percentage point of remote work in 2020 increases house price growth from December 2019 to November 2021 by 0.93 percentage point. This implies that the majority of the increase in house prices caused by remote work is due to the shift in housing demand. Extrapolating from this cross-sectional estimate suggests that the shift to remote work increased house prices by 15.1 percentage points relative to the average increase of about 23.8 percentage points, or more than one half of the total increase.

We validate our approach to aggregation in a spatial general equilibrium model of location choice, remote work choice, and housing demand. We calibrate the model to match the cross-sectional distribution of remote work, occupations, migration, and our cross-sectional estimates.[3] 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. The model implies 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 housing demand and house prices. By contrast, extrapolating from cross-sectional estimates that do

[3] See Moretti (2010) for a textbook treatment. Seminal contributions include Rosen (1979) and Roback (1982).

not control for migration would overstate the true aggregate effects of remote work. The model also shows that extrapolating from instrumental variables regressions is more reliable than from reduced form because the instrumental variable estimator accounts for systematic variation in treatment intensity across locations.

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, 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% 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 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.

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; Chodorow-Reich, 2014; Beraja, Hurst, and Ospina, 2019; Chodorow-Reich, 2019; Herreño, 2020). 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 remainder of the 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.[4]

2 Data

We use core-based statistical areas (CBSAs) as our unit of observation in our analysis. A CBSA collects counties into economically-connected units, which gives a sensible level of aggregation for measuring migration across regions and house prices. Our final dataset has 895 CBSAs.

We use Zillow house price indexes for our baseline measure of house prices.[5] 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 12 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 (7.5 percent annualized) from December 2019 through November 2021.

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

[4] 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.

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

as the 2020 experimental run to measure remote work, where remote work is defined as an employed person that does not commute.[6] 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.

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 studies report higher remote shares: the Current Population Survey reports about 22% of workers by late 2020 (Dey, Frazis, Piccone Jr, and Loewenstein, 2021) while Barrero, Bloom, Davis, and Meyer (2021) report that 49% of paid working days were done remotely at the same time. We suspect the difference arises primarily because of remote work being measured through commute time in the ACS. Workers who are currently working remotely but expecting a return to the office in some capacity may still report their commute time to the ACS, whereas they will report working remotely in the other surveys. While this difference does not pose a problem for identification or extrapolation, it does affect the interpretation of our estimates as we discuss below.

Our analysis requires a high-quality measure of migration in and out of CBSAs. Here we rely on the FRBNY/Equifax Consumer Credit Panel.[7] These data, built from an anonymized 5% 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

[6] Rothbaum, Eggleston, Bee, Klee, and Mendez-Smith (2020) document the significant potential for non-response bias in the 2020 ACS production run due to the pandemic, which is likely to introduce additional measurement error in our estimates. See also https://usa.ipums.org/usa/acspumscovid19.shtml. We exclude individuals in the armed forces when calculating remote work shares.

[7] See Lee and Van der Klaauw (2010).

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 -0.54.

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. However, the aggregate increase in unemployment rates is still about 25% of the pre-pandemic level of unemployment.

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:

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

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

where Remote Work 2015-19$_i$ is the share of employed individuals working from home in the 2015-2019 ACS, Remote Work 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 affect remote work in 2020. In fact, if remote workers require more housing, then any shock that pushes up house prices will actually reduce remote work as remote workers move to cheaper housing or return to office work. Therefore, 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. The share of the employed with a college degree and the predicted remote share based on local occupation shares and the probability of being remote in an occupation at the national level are strong predictors of pre-pandemic remote work.[8] Similarly, amenities such as a mild winter climate and low summer temperature and humidity strongly predict remote work.[9] We also find that high density predicts less remote work all else equal, which likely reflects the availability of larger and / or less expensive homes as captured by density.

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 to work remotely. This implies that the pre-pandemic remote work shares will be predictive

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

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

of remote work shares over the pandemic. 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 of remote workers 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, the cross-sectional causal effect of remote work would be greater due to migration, even though aggregate house prices are likely unaffected.

In order to isolate the component of remote work that shifts aggregate housing demand, we estimate the following regression in which we explicitly 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 aggregate housing demand. Econometrically, it is straightforward to 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.[10] See Appendix 1.1 for the full details.

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 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.

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 1 separates remote work share in 2015-2019 into 20 bins 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% of workers at home in 2020 while areas at the bottom of the pre-pandemic distribution only have about 5% of workers at home in 2020. This is consistent with the 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 2 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 pre-pandemic and pandemic remote work shares.

Column (1) of Table 3 reports the corresponding regression. For each percentage point of the remote work share in 2015-19 we expect 1.74 percentage points of remote work during

[10] 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).

2020. The estimate is very precise with the 95% confidence band ranging from 1.51 to 1.96. Since the aggregate remote share increases by a factor of 3, the first stage implies that low-remote area get a slightly larger multiplicative treatment than high-remote areas. Still, the r-squared of 39% 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)-(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 composition, and quintiles for CBSA density.[11] Column (4) controls for local labor market conditions before the pandemic and over the course of the pandemic. Introducing these controls does not affect 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. The first stage coefficient remains precisely estimated with the 95% confidence band ranging from 1.35 to 1.72.

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 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%.

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

[11] We control non-parametrically for density because we found that the effect of density on remote work is convex, while the effect of density on house price and rent growth is concave.

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 4 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 5 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% 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 in with a low pre-pandemic remote share. For example, to the extent that remote work is more common in areas with low housing supply elasticity, then the monetary accommodation during the pandemic may have raised prices more in areas with a high initial remote shares. Labor demand may have also increased relatively more (or dropped relatively less) in areas with a greater share of remote workers, causing a similar omitted variable bias.

To address this concern 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% 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 estimates of remote work to 2.25, even though many of the controls themselves are statistically significant and raise the r-squared from 0.17 to 0.36.

The stability of the remote work estimate reflects that the pre-pandemic remote share is only weakly correlated with the controls. For example, the correlation between log density and remote share is -0.06 since very dense metro areas such as New York and Boston have average remote shares, similar to low density areas like Minot (ND). This suggests that heteoregeneity in housing supply elasticities is unlikely to drive our results. The pre-pandemic remote share is also only weakly correlated with labor market indicators, such as the pre-pandemic unemployment rate (-0.19), the change in unemployment from 2019 to 2020 (0.06), or the change in unemployment from November 2019 through November 2021 (0.15). Therefore, our estimate for remote work does not appear to capture a differential labor demand shock.

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 (Andrewsi, Stocki, and Sun, 2018). This is consistent with the very precise estimate and high r-squared first stage results in Table 3, and also yields a fairly narrow 95% confidence band of 0.80 to 1.47. Note that our IV estimate is not strictly an elasticity of house prices to remote work since our measurement of remote work stops in 2020, whereas prices are measured through November 2021. For example, if the remote work share had proportionally increased to 40 percent by November 2021, then the appropriate elasticity would be $1.14 \times 16.3\% \times \frac{1}{40\%} = 0.46$.

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 1.47 that remains precisely estimated with the 95% confidence bands extending from 1.11 to 1.82.

The IV coefficients are roughly twice as large as their OLS counterparts, which are displayed in Table 5. This suggests that there are significant shocks to pandemic housing demand that negatively affect remote work and/or there is significant measurement error in the 2020 remote work share.[12]

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. Table 6 re-estimates the first-stage relationships between exposure to remote work and actual remote work in 2020 on the subsample of CBSAs with rent data. Without controls, the instrument is even more strongly correlated with remote work over the pandemic in this subsample, but with controls the estimate is generally consistent with the results in Table 3.

Table 7 reports the reduced form and IV estimate with pandemic rent growth as dependent variable. The estimates without controls in columns (1) and (5) are positive but imprecise. One cannot reject that they are zero, but one also cannot reject that they are the same as our estimate for house price growth in the same sub-sample (Table 8). Once we add controls in columns (2)-(4) and (6)-(8), the effect on rent growth increases in size and is statistically significant.

For example, the IV estimate in column (8) implies that an additional percentage point of remote work in 2020 increases rent growth from December 2019 to November 2021 by 1.15 percentage points. This estimate is both very close to our baseline estimate for house price growth (1.47 percentage points) and the estimate for house price growth in this sub-sample (1.29 percentage points). However, even with controls the estimates for rent growth are somewhat imprecise. The 95% confidence band ranges from 0.33 to 1.98. The comparison with house prices in the same sub-sample shows that the precision is lower both because the sub-sample with rent data is smaller and because the rent data are more noisy.

[12] The 2020 measure of remote work may be prone to unusually high levels of measurement error due to the unusual survey conditions caused by the pandemic (Rothbaum, Eggleston, Bee, Klee, and Mendez-Smith, 2020).

A final 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 housing 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.[13] Given the small sample size we report reduced form regressions with lagged dependent variables as controls and robust standard errors.

Table 9 shows the reduced form regressions for commercial rent growth in columns (1) and (2). A one percentage point increase in remote work exposure predicts a -0.25 percent decline in pandemic commercial rents, consistent with the substitution of office space for home work space. Columns (3) and (4) report the corresponding estimates for house price growth in the same sub-sample: a one percentage point increase in remote work exposure predicts a 2.48 percent increase in house prices, which are consistent with the broader sample and statistically significant despite the small 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, while the data provide evidence for substitution from office space to home space, the relative magnitudes of our estimates suggest this effect is not symmetric. This is suggestive evidence that the shift to remote work may have been inflationary for the U.S. economy. At the minimum, these results show that exposure to remote work is not correlated with a broader expansion in real estate values.

A related concern is that increasing housing costs are reflective of a broader increase in prices faced by consumers. Table 10 displays the reduced form regression for the pandemic

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

inflation rate excluding shelter on the remote worker share. According to column (1) the price level excluding shelter grows by 0.43 percent more over two years for every percentage point of initial remote work exposure. For comparison, the effect of remote work exposure on house price growth in the same sub-sample is 3.51 percentage points (column 3). The inflation responses are thus roughly one seventh of the magnitude of the house price responses. This corroborates our premise that remote work triggered a relative increase in the demand for housing and thus an increase in the relative price of housing, rather than a broad-based increase in demand correlated with remote work exposure.

Another explanation for these patterns is that remote work exposure proxies for a low housing supply elasticity, so that even a uniform increase in housing demand would increase house prices more in more exposed CBSAs. The fact that our estimates in Table 4 were insensitive to controlling for density was evidence against this hypothesis. We provide further evidence against this explanation by estimating the response of building permit growth from 2019 - 2021 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 as construction becomes constrained. Table 11 shows that building permits grow more in areas more exposed to remote work. While permits may not represent completed housing units given the pandemic supply constraints, they suggest that supply is responding more strongly in areas with more remote work.

There are a number of additional local observables that are correlated with the increase in remote work over the pandemic: the share of individuals with college education, the log median income, and census region fixed effects. We do not include these controls in our baseline regressions because they absorb significant valid variation in remote work, leaving the remaining variation at risk of not being representative of the true 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 our aggregation exercise. Similarly, Figure 2 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 A2 and Table A3. The IV estimates with all controls included is now 2.32 (95% confidence band from 1.54 to 3.09) versus 1.47 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.

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 6 shows that exposure to remote work is strongly correlated with net inflows over the pandemic. Table A1 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 13 reports the reduced form and IV estimates. Comparing columns (1) and (5) with our baseline estimate in Table 4, we see that the migration controls enter positively and reduce the cross-sectional estimate 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 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 12). 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 mechanism by which remote work exposure can affect housing demand across locations (see Appendix 1.1 for details).

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 0.93. This suggests that this effect is not driven by a non-linear migration response or measurement error in the migration variable.

3.5 Aggregation

We now use our cross-sectional estimates cleansed of the migration channel to estimate the aggregate effect of the shift to remote work on house prices. For extrapolation we use our baseline estimates in columns (4) and (8) with all controls included.

The weighted pre-pandemic remote worker share for the U.S. economy is 5.2 percent. Multiplying this value with our reduced-form estimate in column (4) of Table 13 yields an aggregate effect of the shift to remote work on house prices of $5.2 \times 1.47 = 7.7$ percent. If we instead apply the 2020 remote worker share (16.3 percent) to the IV estimate in column (8), we obtain an aggregate effect of $16.3 \times 0.93 = 15.1$ percent.

The reduced form and IV estimate imply different aggregate effects because the aggregate remote share increased by a factor of 3, whereas the from first stage of Table 12 the 2020 remote share is only 1.59 times as large as the pre-pandemic remote share. This difference reflects that areas with lower initial remote shares saw a larger proportional increase in remote work. The reduced form does not take into account heterogeneity in treatment intensity, whereas the IV scales the estimate appropriately. Therefore, the larger IV coefficient yields is a more accurate estimate of the aggregate effects—an insight we confirm in the model as well.

Since aggregate house prices grew by 23.8 percent from December 2019 to November 2021, our IV estimates imply that remote work can explain 15.1/23.8=64 percent of the total increase in house prices over the pandemic. In the following section 4, we argue that this represents a lower bound on the aggregate effect because we control for the effect of migration in the cross-section. This implies that the shift to remote work was responsible for at least one half of the increase in house prices over the pandemic.

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 to the aggregate effect provides a lower bound. We also use the model to confirm that extrapolating from the IV estimate yields more accurate aggregate effects than extrapolating from the reduced form.

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. Since the shift to remote work appears to be very persistent (Barrero, Bloom, Davis, and

Meyer, 2021), we adopt a medium-run perspective in which monetary policy, and more generally aggregate demand shocks, would be neutral.

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 $l(f)$ denoting the location of firm f. Firms only employ workers of a single occupation, $o(f)$, at an occupation-specific wage $z_{o(f)}$. Production is linear in labor and perfectly competitive, 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 $l(f)$), work remotely in location $l(f)$, or work remotely in another location $l \neq l(f)$. 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. Remote work shifts the consumption bundle towards the housing good.

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_{ilw}, so that utility from choosing location l and work mode w is $U_{flw} a_{ilw}$. We assume that a_{ilw} follows a Frechet distribution with shape parameter κ to obtain closed form solutions for the distribution of workers.

Specifically, for workers who decide to stay in the location of their firm, $l(f)$, the probability of being remote is,

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

where $\mu_{o(f)b}$ and $\mu_{l(f)b}$ are location parameters of the Frechet distribution. $\mu_{o(f)b}$ determines the propensity of occupations for remote work and $\mu_{l(f)r}$ the propensity of a location for remote work.

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

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

where μ_{ol} is location parameter capturing occupation-specific amenities of location l and μ_{stay} is a location parameter that determines the propensity to stay in the same location as the firm.

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, and $s_{r|lf} = 1$ whenever $l \neq l(f)$.

Perfectly competitive investors supply a location-specific net quantity of housing $\bar{H}_l$. Investors 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 (Buiter, 2010).

We assume a zero housing supply elasticity because home building was subject to significant supply-chain delays over the pandemic. Consistent with this assumption, in unreported results we found that the interaction of remote work exposure with density was not economically or statistically significant. While the response of building permits (Table 11) scales with remote work, it is unclear how much of this prospective supply was completed by the end of our sample.

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

Law.

4.5 Calibration

We calibrate the model to the $L = 895$ locations in the data. We sort occupations by their remote worker share in 2015-19, and aggregate them into $O = 4$ groups with equal population shares.[14] We extract a new set of location parameters $\mu_{ol}, \mu_{ob}, \mu_{lr}, \mu_{stay}$ such that the model matches exactly the remote share in each location, the remote share in each occupation, the occupation-location distribution (all from the 2015-19 ACS) and the pre-pandemic gross migration rate (3.8%) from the NYFRB/Equifax Consumer Credit Panel.[15] We impose the restriction that net migration is zero pre-pandemic. We calibrate occupation-specific wages based on the 2015-19 ACS, normalizing the lowest wage to 1.

We normalize the initial housing prices to $p_l = 1$, so that the effect of prices on location choice is absorbed by the location parameters μ_{lr} and μ_{lb}, which capture work-mode-specific amenities across locations. This has two advantages. First, the choice of κ does not affect the initial steady state and thus this part of our inference. Indeed, κ plays little role in our results and so we set it to $\kappa = 1$.[16] 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). Stanton and Tiwari (2021) show that pre-pandemic, remote workers' home value is up to 21.5% higher than non-remote workers. We scale this parameter by $\frac{1}{0.44+\frac{1}{2}0.56}$ to account for the fact that 56% of remote households in their sample are mixed remote. This yields $\theta_r = \theta_b \times 1.3 = 0.312$.

This leaves the housing demand elasticity ζ as the remaining parameter. We pick it to match the estimated IV coefficient of remote work on house prices controlling for migration, 0.93 (column 7 of Table 13). This step is done jointly with inferring the shift to remote work,

[14] Adding more occupations has almost no effect on our results.

[15] As the system is overdetermined we adopt the following normalizations: $\frac{1}{L}\sum_l \mu_{ol} = 1$ and $\frac{1}{L}\sum_l \mu_{lr} = 1$.

[16] Results are virtually identical for $\kappa = 0$ and for $\kappa = 8$. This is because the effects through κ are almost completely absorbed by changes in location parameters as we match the pandemic distribution of remote work and occupations across locations.

discussed below.

4.6 Shift to Remote Work

We capture the shift to remote work at two points in time: first, in 2020 when the ACS measures the pandemic remote share that we use in our first stage. Second, in November 2021, which is the end point of our sample and when we measure house prices.

For 2020, we extract a new set of location parameters μ'_{or}, μ'_{lr} such that the model matches exactly the remote share in each location and the remote share in each occupation (both from the 2020 ACS). Since the only information we use from 2020 is the remote share by location we can remain agnostic about migration happening over this period: for any new distribution of s'_f, we can always find a unique set of location parameters μ'_{or}, μ'_{lr} that exactly match the data.

The aggregate remote share in the 2020 ACS is 16.7% and our model matches this exactly. However, since other sources document much higher remote work shares in November 2021 (e.g., Barrero, Bloom, Davis, and Meyer, 2021; Dey, Frazis, Piccone Jr, and Loewenstein, 2021), we believe that many respondents in 2020 considered their remote work arrangement temporary.[17] To account for the fact that permanent remote work has increased from 2020 to November 2021, we scale up the 2020 remote shares in each location and occupation such that the aggregate remote share is 42.8% based on the November 2021 survey by Barrero, Bloom, Davis, and Meyer (2021). We use these data for calibrating the model to November 2021.[18]

Specifically, we obtain a final location parameters $\mu''_{or}, \mu''_{lr}, \mu''_{stay}$ and migration shocks α''_l, β''_o, to match these remote share data, the pandemic gross migration rate (5.3%) and the full pandemic net migration rate in each location (both from the NYFRB/Equifax Consumer

[17] In addition, the 2020 data understates pandemic remote work because it includes pre-pandemic months.

[18] Scaling to a higher or lower remote share in November 2021 primarily affects the estimate of the housing demand elasticity ζ that is needed to match the cross-sectional estimates. Conditional on choosing ζ in this way, the remote work share in November 2021 matters little for our model-based estimates of the aggregate effects of remote work.

Credit Panel). Since we do not have data on net migration by occupation, we impose the restriction that the new firm distribution solves $s'_f = \alpha'_{l(f)} s_f + \beta'_{o(f)}$.

By construction, our model will closely match the first stage controlling for migration, column (1) of Table 12.[19] This is because we exactly match the 2020 ACS remote shares by location and the pandemic net migration data as of November 2021. The model also matches the cross-sectional IV coefficient by construction—the required value for the demand elasticity is ζ =0.52.

The model is not calibrated to match the distribution of house price growth. However, Figure 7 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 0.5. This suggests that the model captures the bulk of the cross-sectional forces at work during the pandemic period.

4.7 Regression

Table 14 reports the reduced form and instrumental variable estimates from estimating (3)-(4) in the model. The model regressions closely reproduce those in the data. The IV coefficient on remote work in column (2) fits its empirical counterpart by construction (column 7, Table 4), and the reduced form coefficient in column (1) also fits closely because we almost exactly match the first stage (column 3, Table 4). The model also produces estimates for the effect of net migration on house prices in columns (1) and (2) that closely match those in the data. These estimates were not targeted in the calibration and are therefore another source of model validation.[20]

The table also displays the aggregate effect of remote work on house prices calculated in the same way as we extrapolated in the previous section. It then compares this extrapolation to the true aggregate effect in the model.

[19] The first stage coefficient in the model is equal to 1.801 compared to 1.80 in the data. We do not match it exactly because there is no pre-pandemic migration in our model.

[20] The model matches net migration by construction, but its correlation with house prices is not directly targeted.

Column (1) shows that the extrapolating from 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 1.667×5.2=8.7 percentage points. The true effect in the model is 17.6 percentage points, 2.0 times larger than the extrapolation.[21] 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 1.801. In effect, the reduced form does not take into account that the treatment scales imperfectly with the initial remote work exposure.

Column (2) shows that the instrumental variable regression gets much closer to the true aggregate effect. The extrapolated effect is 0.926×16.3=15.1 percentage points, so that the true effect is only 1.2 times larger. The IV specification does much better than the reduced form because it appropriately scales the reduced form coefficient with the first stage coefficient.

The extrapolation from the IV still understates the true aggregate effect. This is because areas with more remote work consume more housing, which implies that the general equilibrium effects through prices act more strongly in those regions. This force moderates the increase in house prices in high remote areas relative to low remote areas. Since this general equilibrium effects amplifies the aggregate response of house prices to remote work, the model suggests that our extrapolation from the data in section 3 is a lower bound to the true aggregate effect.

By contrast, the model cautions against extrapolating from regressions that do not control for migration. Column (4) shows that extrapolating from an IV specification that does not control for migration yields a predicted house price growth of 1.295×16.3=21.1 percentage points. The true effect is smaller, equal to 0.8 times this extrapolation. 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

[21] The true aggregate effect excludes the effects of the migration shocks α_l'' and β_o''. Results are virtually identical when these shocks are included.

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.

The model suggests that controlling for migration successfully deals with this issue and allows one to bound the aggregate effect without specifying a full general equilibrium model. This result corroborates our approach to bounding aggregate effects from below by controlling for negative spatial linkages.

5 Conclusion

In this paper 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 24% 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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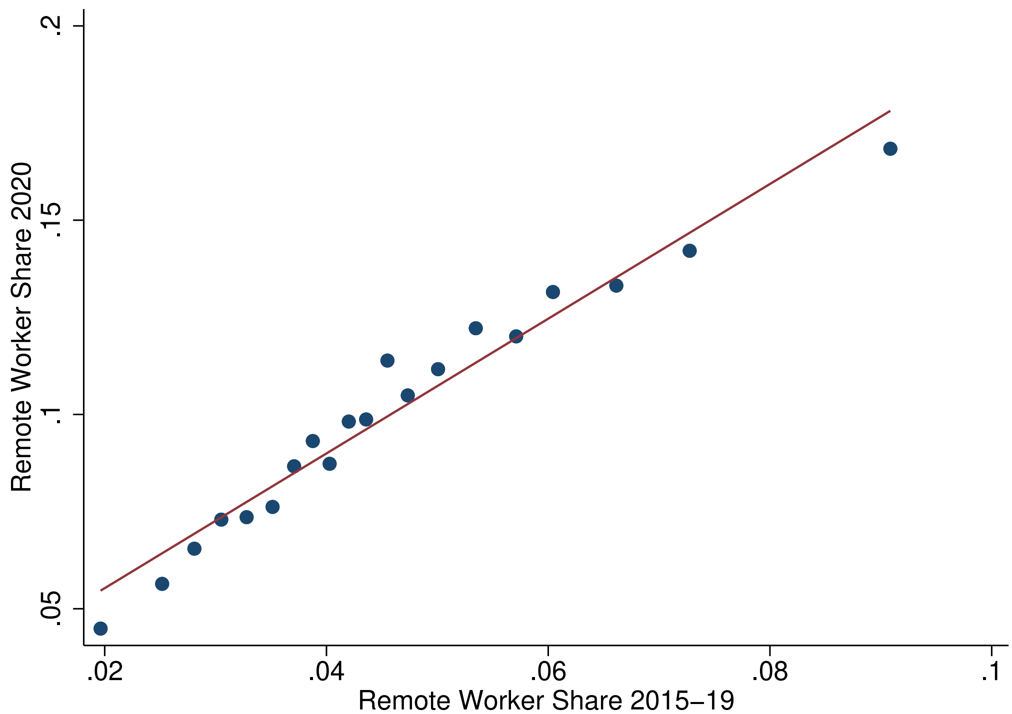
6 Figures

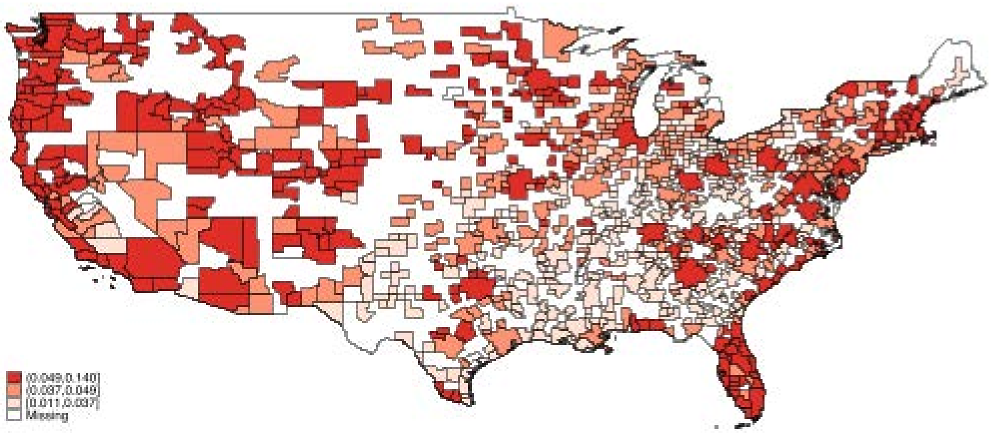
FIGURE 1
Binned Scatter Plot of Remote Worker Share 2020 on Remote Worker Share 2015-19



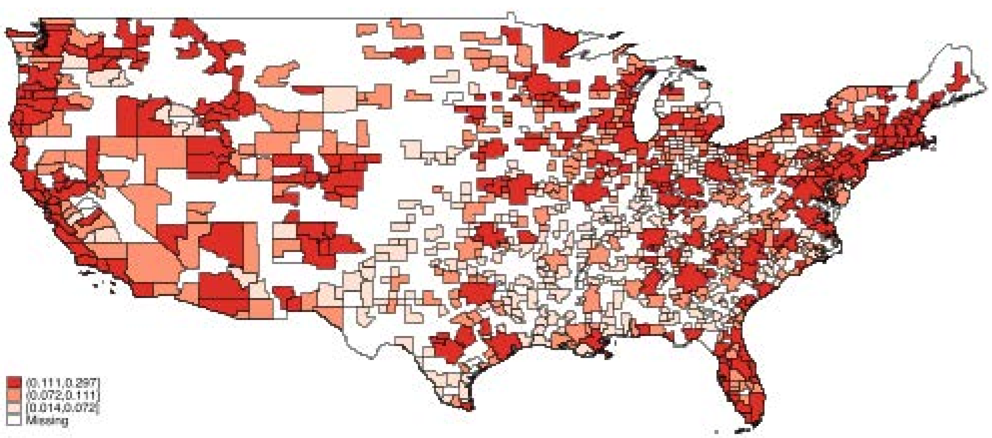
Sources: American Community Survey and authors calculations.

FIGURE 2
Geographic Distribution of Remote Worker Share

A. 2015-19 Average



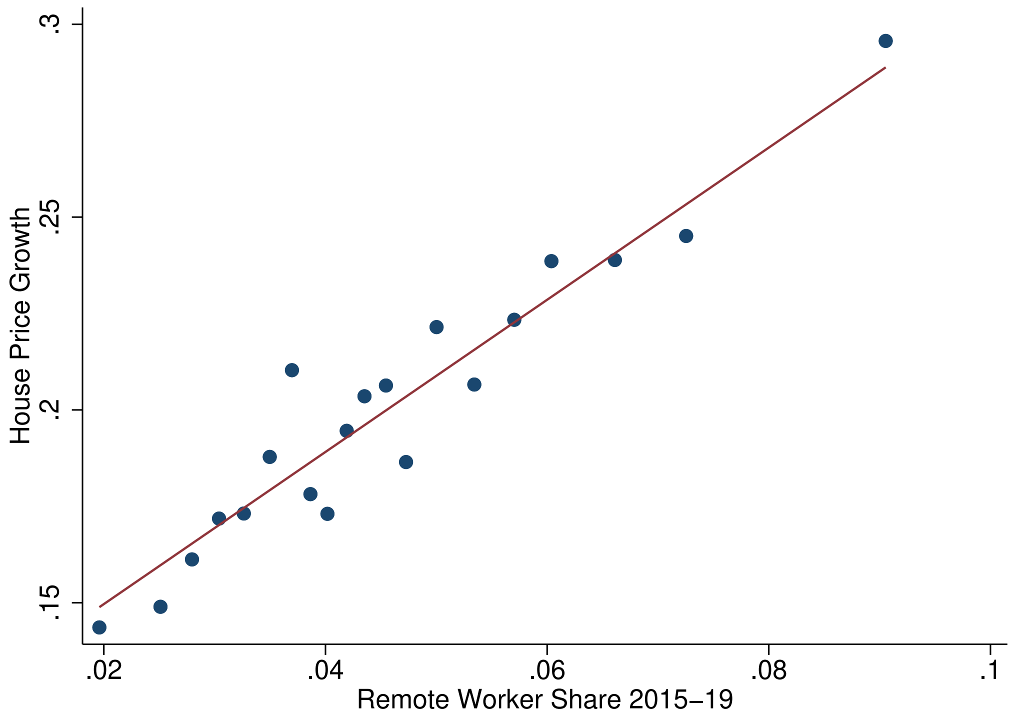
B. 2020



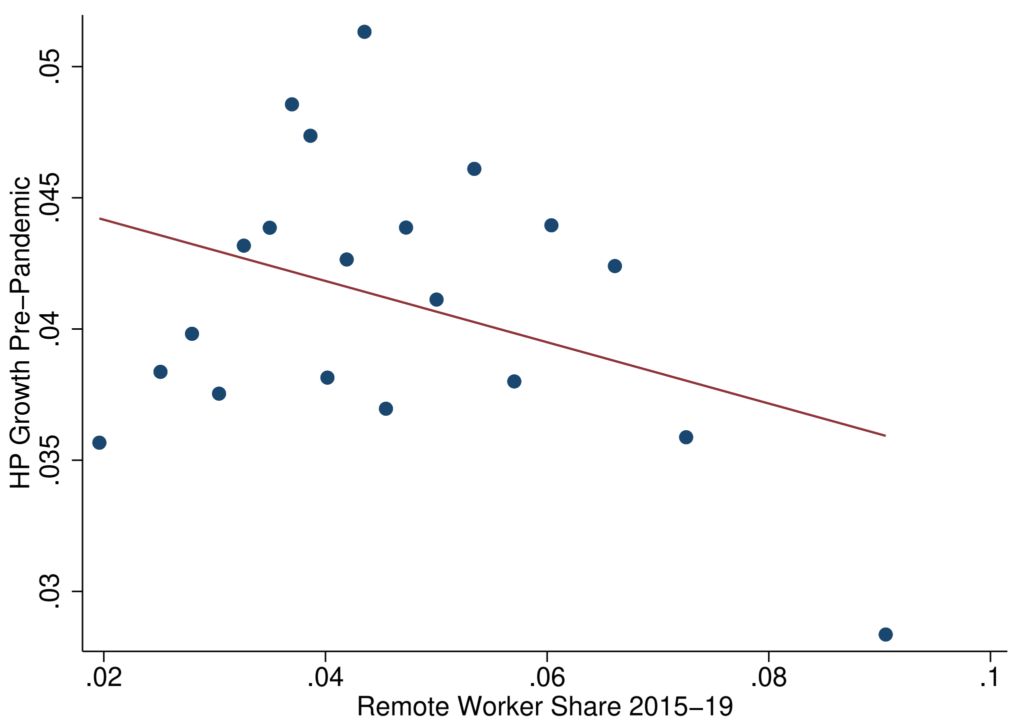
Sources: American Community Survey and authors calculations.

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

A. House Price Growth from Dec. 2019 - Nov. 2021

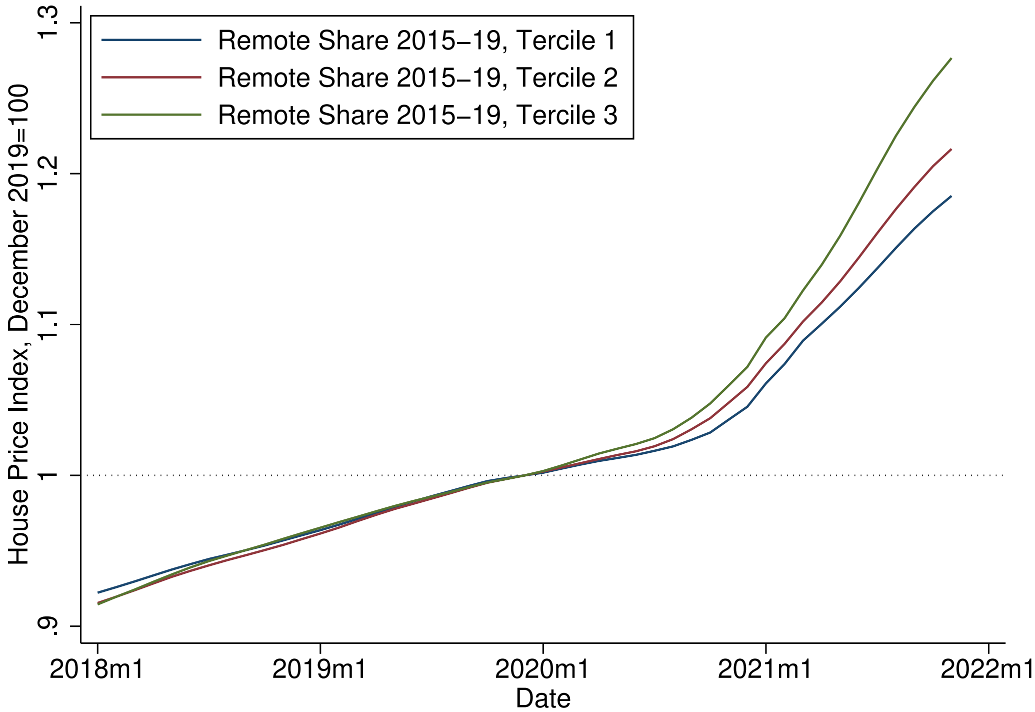


B. House Price Growth from Dec. 2018 - Dec. 2019



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

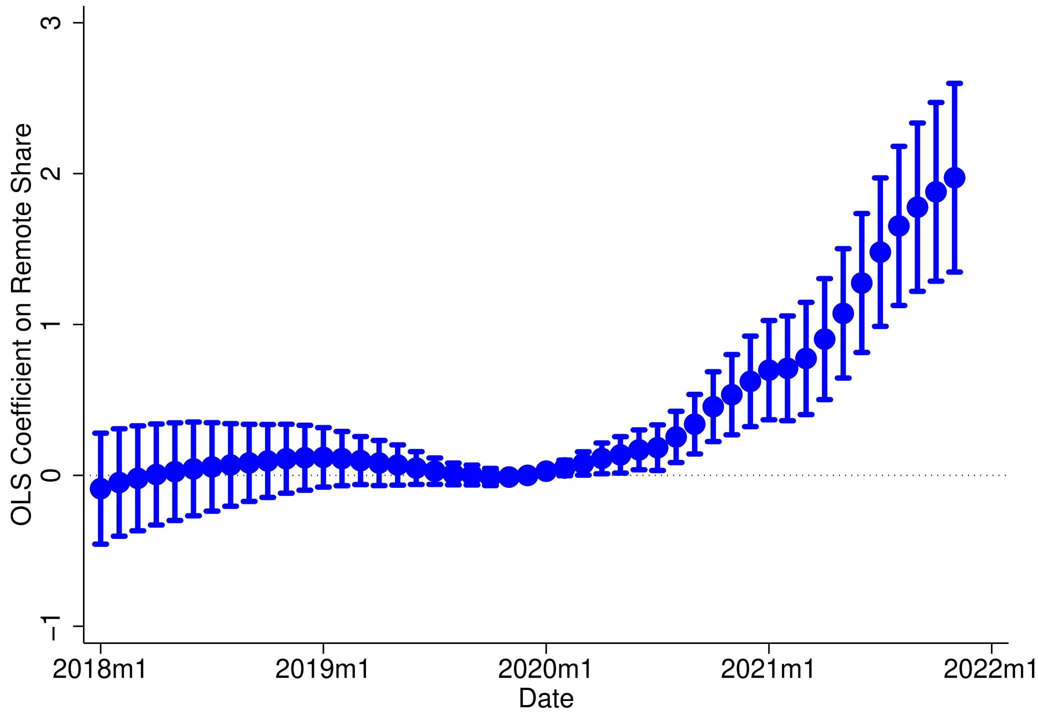
FIGURE 4
Event Analysis of House Price Growth on Remote Worker Share 2015-19



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

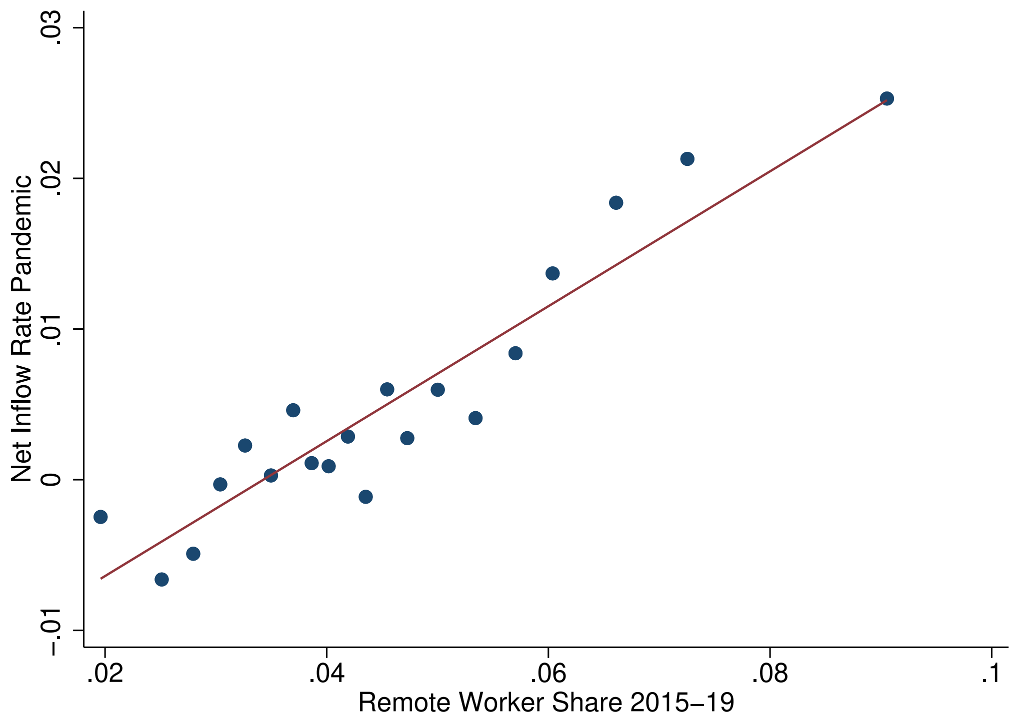
Notes: The average 2015-19 remote worker shares in quartiles 1 through 3 are: $[2.9\%, 4.3\%, 6.5\%]$.

FIGURE 5
Regression Event Analysis of House Price Growth on Remote Worker Share 2015-19



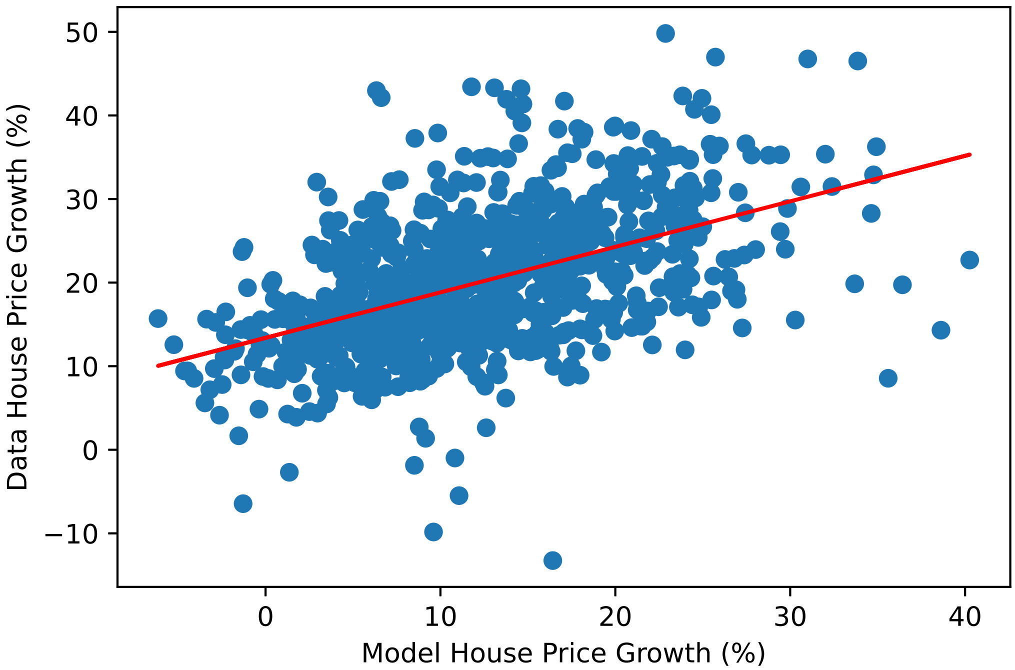
Sources: Zillow, American Community Survey, and authors calculations.
Notes: Error bars represent the 95% pointwise confidence interval, with standard errors clustered at the state level. When a CBSA straddles multiple states we assign it to the state that accounts for the majority of the CBSA population.

FIGURE 6
Binned Scatter Plot of Net Inflow Rate Pandemic on Remote Worker Share 2015-19



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

FIGURE 7
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
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

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	1.93***
	(0.20)
Share College	0.054**
	(0.026)
Log Median Income	−0.0015
	(0.0044)
Unemp. Rate 2019	0.0089
	(0.043)
Log Density	−0.0015**
	(0.00071)
Share White	−0.011
	(0.013)
Share Black	−0.038**
	(0.016)
Share Asian	−0.076**
	(0.035)
Share Hispanic	−0.026*
	(0.014)
January Temperature	0.00050***
	(0.00012)
July Temperature	−0.00078***
	(0.00015)
July Humidity	−0.00015*
	(0.000076)
CBSA Clusters	50
R^2	0.64
Observations	895

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. 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

	Dep. Variable: Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.74***	1.71***	1.55***	1.54***
	(0.11)	(0.11)	(0.092)	(0.093)
HP Growth Pre-Pandemic		−0.21***	−0.084	−0.073
		(0.063)	(0.052)	(0.049)
Δ Unemp. Rate 11/2019-11/2021				0.34*
				(0.18)
Δ Unemp. Rate 2019-2020				−0.30**
				(0.11)
Unemp. Rate 2019				0.051
				(0.11)
Nonparametric Density Control	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes
CBSA Clusters	50	50	50	50
R^2	0.39	0.40	0.61	0.62
Observations	895	895	895	895

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

Notes: This table reports first stage regression. The dependent variables are the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. 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 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***	2.11***	2.25***				
	(0.31)	(0.29)	(0.26)	(0.25)				
Remote Worker Share 2020					1.14***	1.20***	1.36***	1.47***
					(0.17)	(0.16)	(0.18)	(0.18)
HP Growth Pre-Pandemic		0.62**	0.76***	0.71***		0.87***	0.87***	0.82***
		(0.24)	(0.21)	(0.21)		(0.22)	(0.21)	(0.21)
Δ Unemp. Rate 11/2019-11/2021				−1.25*				−1.75**
				(0.74)				(0.69)
Δ Unemp. Rate 2019-2020				0.71*				1.14***
				(0.42)				(0.42)
Unemp. Rate 2019				0.49				0.42
				(0.36)				(0.36)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes	No	No	Yes	Yes
F-Statistic					248.11	222.47	283.89	272.80
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.17	0.20	0.33	0.36	0.11	0.15	0.14	0.16
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, U.S. Census, and authors calculations.

Notes: The dependent variable is house price growth in a CBSA from Dec. 2019 - Nov. 2021. The first four columns report an OLS 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. 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 5

Effect of Remote Work on House Price Growth (OLS), Dec. 2019 - Nov. 2021

	Dependent Variable: House Price Growth			
	OLS			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2020	0.71***	0.76***	0.61***	0.67***
	(0.091)	(0.084)	(0.096)	(0.093)
HP Growth Pre-Pandemic		0.73***	0.70***	0.66***
		(0.24)	(0.24)	(0.23)
Δ Unemp. Rate 11/2019-11/2021				−1.27*
				(0.77)
Δ Unemp. Rate 2019-2020				0.87**
				(0.43)
Unemp. Rate 2019				0.33
				(0.44)
Nonparametric Density Control	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes
CBSA Clusters	50	50	50	50
R^2	0.17	0.21	0.26	0.29
Observations	895	895	895	895

Sources: Zillow, Apartment List, American Community Survey, FRBNY/Equifax Consumer Credit Panel, Local Area Unemployment Statistics, U.S. Census, 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. 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 6

First Stage for Rent Growth Regressions

	Dep. Variable: Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	2.45***	2.47***	2.01***	1.99***
	(0.27)	(0.29)	(0.22)	(0.23)
Rent Growth Pre-Pandemic		−0.060	−0.0047	−0.057
		(0.10)	(0.090)	(0.087)
Δ Unemp. Rate 11/2019-11/2021				0.35
				(0.57)
Δ Unemp. Rate 2019-2020				−0.70**
				(0.31)
Unemp. Rate 2019				0.28
				(0.35)
Nonparametric Density Control	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes
CBSA Clusters	45	45	45	45
R^2	0.46	0.46	0.68	0.70
Observations	178	178	178	178

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

Notes: This table reports first stage regression. The dependent variables are the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. 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 7
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	1.82**	2.29***				
	(0.86)	(0.83)	(0.85)	(0.71)				
Remote Worker Share 2020					0.56	0.50	0.91*	1.15***
					(0.35)	(0.35)	(0.47)	(0.42)
Rent Growth Pre-Pandemic		0.47	0.37	0.14		0.50	0.37	0.20
		(0.54)	(0.53)	(0.40)		(0.55)	(0.56)	(0.43)
Δ Unemp. Rate 11/2019-11/2021				−4.21**				−4.61***
				(1.57)				(1.71)
Δ Unemp. Rate 2019-2020				0.46				1.26
				(0.97)				(1.13)
Unemp. Rate 2019				2.46**				2.14*
				(1.10)				(1.23)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes	No	No	Yes	Yes
F-Statistic					84.92	70.87	81.82	76.18
CBSA Clusters	45	45	45	45	45	45	45	45
R^2	0.04	0.06	0.23	0.37	-0.09	-0.05	0.06	0.13
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, U.S. Census, and authors calculations.
Notes: The dependent variable is rent growth in a CBSA from Dec. 2019 - Nov. 2021. The first four columns report an OLS 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. 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 8

Effect of Remote Work on House Price Growth (Rent Sample), Dec. 2019 - Nov. 2021

	Dependent Variable: House Price Growth (Rent Sample)							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	2.51***	2.51***	2.37***	2.54***				
	(0.68)	(0.63)	(0.70)	(0.57)				
Remote Worker Share 2020					1.02***	1.02***	1.18***	1.29***
					(0.26)	(0.24)	(0.37)	(0.31)
House Price Growth Pre-Pandemic		0.56*	0.79*	0.59		0.85**	0.78*	0.62*
		(0.31)	(0.40)	(0.37)		(0.34)	(0.40)	(0.38)
Δ Unemp. Rate 11/2019-11/2021				−3.16***				−3.58***
				(0.89)				(1.07)
Δ Unemp. Rate 2019-2020				0.079				0.95
				(0.62)				(0.72)
Unemp. Rate 2019				0.75				0.47
				(0.81)				(0.87)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes	No	No	Yes	Yes
F-Statistic					84.92	72.71	96.87	92.01
CBSA Clusters	45	45	45	45	45	45	45	45
R^2	0.23	0.26	0.36	0.46	-0.02	0.05	0.16	0.21
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, U.S. Census, 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 an OLS 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. 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 9

EFFECT OF REMOTE WORK ON COMMERCIAL RENT GROWTH PANDEMIC AND HOUSE PRICE GROWTH, DEC. 2019 - NOV. 2021

Dependent Variable:	Commercial Rent Growth Pandemic		House Price Growth	
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	-0.25^{*}	-0.26^{*}	2.48^{***}	2.37^{***}
	(0.13)	(0.13)	(0.70)	(0.64)
Commercial Rent Growth Pre-Pandemic		0.18		
		(0.15)		
HP Growth Pre-Pandemic				1.39^{**}
				(0.56)
R^2	0.13	0.15	0.31	0.47
Observations	25	25	25	25

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

Notes: The dependent variables are Commercial Rent Growth Pandemic and House Price Growth in a CBSA from Dec. 2019 - Nov. 2021. Robust standard errors are given in parenthesis. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE 10

Effect of Remote Work on Inflation excl. Shelter and House Price Growth, Dec. 2019 - Nov. 2021

Dependent Variable:	Inflation excl. Shelter		House Price Growth	
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	0.43*	0.44	3.51***	2.98***
	(0.23)	(0.30)	(0.97)	(0.65)
Inflation Pre-Pandemic		−0.018		
		(0.34)		
HP Growth Pre-Pandemic				1.53**
				(0.60)
R^2	0.18	0.18	0.40	0.57
Observations	22	22	22	22

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

Notes: The dependent variables are Inflation excl. Shelter and House Price Growth in a CBSA from Dec. 2019 - Nov. 2021. Robust standard errors are given in parenthesis. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE 11
Effect of Remote Work on Permit Growth, 2019 - 2021

	Dependent Variable: Permit Growth, 2019 - 2021							
	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	2.67**	2.00*	2.55*	2.89**				
	(1.20)	(1.10)	(1.31)	(1.32)				
Remote Worker Share 2020					1.54**	1.16*	1.63*	1.87**
					(0.69)	(0.63)	(0.84)	(0.85)
Permit Growth Pre-Pandemic		−0.37***	−0.37***	−0.37***		−0.36***	−0.37***	−0.37***
		(0.050)	(0.050)	(0.049)		(0.049)	(0.049)	(0.048)
Δ Unemp. Rate 11/2019-11/2021				−2.59				−3.31
				(3.53)				(3.57)
Δ Unemp. Rate 2019-2020				−2.36				−1.80
				(1.95)				(1.97)
Unemp. Rate 2019				0.55				0.47
				(2.04)				(2.01)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes	No	No	Yes	Yes
F-Statistic					251.08	249.44	274.23	271.03
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.01	0.11	0.12	0.13	-0.01	0.10	0.10	0.11
Observations	879	878	878	878	879	878	878	878

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

Notes: The dependent variable is permit growth in a CBSA from 2019 - 2021. The first four columns report an OLS 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. 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 12

First Stage for House Price Growth Regressions

	Dep. Variable: Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	1.80***	1.61***	1.58***	1.59***
	(0.12)	(0.10)	(0.10)	(0.10)
Net Inflow Rate Pandemic	−0.18*	−0.13**	−0.11*	
	(0.098)	(0.061)	(0.059)	
Net Inflow Rate Pre-Pandemic	0.073	0.078	0.085	
	(0.13)	(0.088)	(0.094)	
HP Growth Pre-Pandemic		−0.072	−0.065	−0.067
		(0.053)	(0.049)	(0.047)
Δ Unemp. Rate 11/2019-11/2021			0.32*	0.27
			(0.18)	(0.17)
Δ Unemp. Rate 2019-2020			−0.28**	−0.26**
			(0.11)	(0.11)
Unemp. Rate 2019			0.058	0.032
			(0.12)	(0.10)
Nonparametric Density Control	No	Yes	Yes	Yes
Race Controls	No	Yes	Yes	Yes
Nonparametric Migration Controls	No	No	No	Yes
CBSA Clusters	50	50	50	50
R^2	0.39	0.61	0.62	0.63
Observations	895	895	895	895

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

Notes: This table reports first stage regression. The dependent variables are the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. Race controls include the population shares identifying as white, black, asian, and hispanic respectively. Nonparametric migration controls include deciles of pandemic net migration and pre-pandemic net migration. 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 13

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***	1.35***	1.50***	1.47***				
	(0.23)	(0.19)	(0.17)	(0.16)				
Remote Worker Share 2020					0.73***	0.84***	0.95***	0.93***
					(0.11)	(0.12)	(0.11)	(0.11)
Net Inflow Rate Pandemic	1.17***	1.16***	1.08***		1.30***	1.27***	1.19***	
	(0.23)	(0.25)	(0.23)		(0.22)	(0.22)	(0.21)	
Net Inflow Rate Pre-Pandemic	0.93***	0.68***	0.76***		0.87***	0.62***	0.68***	
	(0.27)	(0.24)	(0.24)		(0.25)	(0.23)	(0.23)	
HP Growth Pre-Pandemic		0.53***	0.50***	0.48***		0.59***	0.57***	0.54***
		(0.16)	(0.16)	(0.16)		(0.16)	(0.16)	(0.15)
Δ Unemp. Rate 11/2019-11/2021			−0.90*	−1.04**			−1.20***	−1.29***
			(0.52)	(0.49)			(0.44)	(0.42)
Δ Unemp. Rate 2019-2020			0.46	0.51*			0.73**	0.75***
			(0.32)	(0.29)			(0.30)	(0.27)
Unemp. Rate 2019			0.49*	0.51*			0.44	0.48*
			(0.28)	(0.26)			(0.28)	(0.26)
Nonparametric Density Control	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Race Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Nonparametric Migration Controls	No	No	No	Yes	No	No	No	Yes
F-Statistic					220.39	256.94	236.79	243.66
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.33	0.47	0.49	0.51	0.38	0.42	0.43	0.46
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, U.S. Census, and authors calculations.

Notes: The dependent variable is house price growth in a CBSA from Dec. 2019 - Nov. 2021. The first four columns report an OLS 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. Race controls include the population shares identifying as white, black, asian, and hispanic respectively. Nonparametric migration controls include deciles of pandemic net migration and pre-pandemic net migration. 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 14

MODEL RESULTS: CROSS-SECTIONAL AND AGGREGTE EFFECT OF REMOTE WORK ON HOUSE PRICE GROWTH

Dependent Variable:	House Price Growth			
	Migration Control		No Migration Control	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: Regression coefficients:				
Remote Worker Share 2015-19	1.667		2.243	
Remote Worker Share 2020		0.926		1.295
Net Inflow Rate Pandemic	1.318	1.464		
Panel B: Aggregation Results:				
Extrapolated Aggregate Effect	0.087	0.151	0.117	0.211
True Aggregate Effect	0.176	0.176	0.176	0.176
Ratio: True / Extrapolated Effect	2.016	1.162	1.498	0.831

This table reports regression results from the model in Section 4.The dependent variable is house price growth.Columns (1) and (3) report reduced form regressions and columns (2) and (4) IV specification.The IV specifications use the remote work share from 2015-19 as instrument.The extrapolated aggregate effects are based on the cross-sectional coefficient on remote work multiplied by its population mean.The true aggregte effects are based on the true effects in the model and exclude the effects of the exogenous migration shocks α and β.

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. First, assume that the true relationship between these is given as (ignoring constants for simplicity)

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

Also assume that net migration depends on remote work plus a disturbance

$$M = \gamma RW + \epsilon_2.$$

We can then estimating the following model of the relationship between house price growth and remote work with OLS (this model corresponds to (2)):

$$H = \rho_1 RW + e_1.$$

Assuming there is no correlation between the remote work shock and the residuals ϵ_1 and ϵ_2, this estimate recovers

$$\hat{\rho}_1 = \beta_1 \gamma + \beta_2.$$

This is the total effect of the remote work shock on house prices, which includes both the migration channel and the demand channel. Because this coefficient contains both mechanisms, it is not directly useful for learning about the size of the shift in demand caused by remote work.

To isolate the demand effect, we propose estimating the following regression that controls for net migration

$$H = \rho_1 M + \rho_2 RW + e_2.$$

It is straightforward to evaluate the conditional OLS estimate $\hat{\rho}_2 = \frac{Cov(H,RW|M)}{Var(RW|M)}$ to determine what we recover. First, notice that $RW = (M - \epsilon_2/\gamma)$ so that $E[RW|M] = M/\gamma$. Then the covariance term in the numerator collapses to the following

$$\begin{aligned} Cov(H, RW|M) &= E[H * RW|M] - E[H|M] * E[RW|M] \\ &= \frac{\beta_2}{\gamma^2}\sigma^2_{\epsilon_2} - \frac{1}{\gamma}E[\epsilon_1\epsilon_2], \end{aligned}$$

where $\sigma^2_{\epsilon_2}$ is the variance of the residual. Similar calculations of the conditional variance give

$$Var(RW|M) = \frac{1}{\gamma^2}\sigma^2_{\epsilon_2}.$$

Therefore, the recovered OLS estimate is

$$\hat{\rho}_2 = \beta_2 - \frac{E[\epsilon_1\epsilon_2]}{\gamma\sigma^2_{\epsilon_2}}.$$

If the disturbances to migration and house price growth are uncorrelated, then we will recover exactly the true effect of remote work on housing demand β_2. However, if they are positively correlated, then we will actually understate the true demand effect, and if they are negatively correlated then we will overstate the true demand effect. [22] It seems most plausible that shocks that increase migration into a city also increase that city's house prices (for example, a large employer opening a local plant), so that we are more likely to understate the true effect. This would be an example of "over-controlling."

These conclusions extend to the setting where we use an IV estimator except that the necessary exclusion restriction and relevance assumptions are now conditional on migration.

1.2 Tables

[22] Assuming that $\gamma > 0$, as is the case empirically.

TABLE A1
EFFECT OF REMOTE WORK ON NET INFLOW RATE PANDEMIC, DEC. 2019 - NOV. 2021

	Dependent Variable: Net Inflow Rate Pandemic, Dec. 2019 - Nov. 2021							
	OLS							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RHS variables:								
Remote Worker Share 2015-19	0.45^{***}	0.28^{***}	0.33^{***}	0.35^{***}				
	(0.053)	(0.042)	(0.048)	(0.050)				
Remote Worker Share 2020					0.26^{***}	0.16^{***}	0.21^{***}	0.23^{***}
					(0.034)	(0.026)	(0.033)	(0.035)
Net Inflow Rate Pre-Pandemic		1.10^{***}	1.09^{***}	1.09^{***}		1.12^{***}	1.11^{***}	1.10^{***}
		(0.093)	(0.092)	(0.089)		(0.096)	(0.094)	(0.092)
Δ Unemp. Rate 11/2019-11/2021				-0.24^{**}				-0.32^{***}
				(0.095)				(0.10)
Δ Unemp. Rate 2019-2020				0.080				0.15^{**}
				(0.066)				(0.071)
Unemp. Rate 2019				0.074				0.062
				(0.064)				(0.065)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes	No	No	Yes	Yes
F-Statistic					397.97	372.76	352.65	332.67
CBSA Clusters								
R^2	0.11	0.39	0.41	0.42	-0.14	0.29	0.31	0.31
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, U.S. Census, and authors calculations.

Notes: The dependent variable is net inflow rate pandemic in a CBSA from Dec. 2019 - Nov. 2021. The first four columns report an OLS 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. Race controls include the population shares identifying as white, black, asian, and hispanic respectively. Robust standard errors are given in parenthesis. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE A2

First Stage for House Price Growth Regressions

	Dependent Variable: Remote Worker Share 2020			
	(1)	(2)	(3)	(4)
RHS variables:				
Remote Worker Share 2015-19	0.58***	0.58***	0.67***	0.67***
	(0.12)	(0.12)	(0.094)	(0.095)
Share College	0.50***	0.50***	0.44***	0.45***
	(0.029)	(0.028)	(0.035)	(0.035)
Log Median Income	0.033***	0.033***	0.021**	0.025**
	(0.010)	(0.011)	(0.0097)	(0.0099)
HP Growth Pre-Pandemic		0.030	0.015	0.027
		(0.038)	(0.025)	(0.024)
Δ Unemp. Rate 11/2019-11/2021				0.28*
				(0.16)
Δ Unemp. Rate 2019-2020				−0.15
				(0.11)
Unemp. Rate 2019				0.29***
				(0.10)
Nonparametric Density Control	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
CBSA Clusters	50	50	50	50
R^2	0.73	0.73	0.76	0.77
Observations	895	895	895	895

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

Notes: This table reports first stage regression. The dependent variables are the remote worker share in 2020. The instrument is the average share of remote workers from 2015-19 in a CBSA. 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 A3

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	1.01**	1.06***	1.53***	1.56***				
	(0.38)	(0.36)	(0.29)	(0.27)				
Remote Worker Share 2020					1.76***	1.83***	2.29***	2.32***
					(0.50)	(0.47)	(0.42)	(0.40)
Share College	0.28***	0.31***	−0.016	0.0081	−0.60**	−0.60**	−1.02***	−1.02***
	(0.081)	(0.088)	(0.081)	(0.079)	(0.26)	(0.25)	(0.22)	(0.22)
Log Median Income	−0.044	−0.031	−0.047	−0.038	−0.10**	−0.092**	−0.095**	−0.096**
	(0.038)	(0.040)	(0.034)	(0.034)	(0.045)	(0.045)	(0.043)	(0.041)
HP Growth Pre-Pandemic		0.68***	0.55***	0.53***		0.62***	0.52***	0.47***
		(0.20)	(0.14)	(0.14)		(0.15)	(0.15)	(0.15)
Δ Unemp. Rate 11/2019-11/2021				−1.26**				−1.90***
				(0.61)				(0.62)
Δ Unemp. Rate 2019-2020				0.74*				1.09**
				(0.39)				(0.44)
Unemp. Rate 2019				−0.13				−0.80**
				(0.30)				(0.39)
Nonparametric Density Control	No	No	Yes	Yes	No	No	Yes	Yes
Race Controls	No	No	Yes	Yes	No	No	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Statistic					22.50	22.97	50.49	50.76
CBSA Clusters	50	50	50	50	50	50	50	50
R^2	0.26	0.30	0.44	0.45	0.15	0.16	0.08	0.11
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, U.S. Census, and authors calculations.

Notes: The dependent variable is house price growth in a CBSA from Dec. 2019 - Nov. 2021. The first four columns report an OLS 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. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$