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Early effects of COVID-19 pandemic-related state policies on housing market activity in the United States

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ABSTRACT

I use daily and weekly data from 100 metropolitan areas in 2020 to investigate the effects of state-level policies to combat the COVID-19 pandemic on various indicators of U.S. housing market activity. Measures of housing market activity include change in new listings, total inventory, newly pending sales, median list price, web traffic to for-sale homes, and average number of days to pending sale status. Using event study and difference-in-differences models, I find that the closure of non-essential businesses in certain states was associated with up to an 11-percentage point decrease in new home listings and a 3.5 percentage point decrease in total inventory relative to the same period in 2019. I also find that school closures may affect some outcomes.

1. Introduction

COVID-19 is an infectious disease that had spread to 219 countries by April 2021. The World Health Organization (WHO) on March 11, 2020, declared the COVID-19 outbreak a global pandemic. The early cases of COVID-19 were confirmed in the United States by January 2020. As of April 2021, there were more than 135 million confirmed cases of COVID-19 globally, with approximately 3 million related deaths. Approximately 20 percent of all confirmed cases and deaths related to COVID-19 were reported in the United States.

Several recent studies document the negative short-run impact of the coronavirus pandemic on the U.S. economy (Baker et al., 2020; Atkeson, 2020; Lewis et al., 2020). This paper provides the first estimates of the short run impact of the state level policies to mitigate the spread of the coronavirus on various indicators of housing market activity in selected U.S. metropolitan areas.

During the early pandemic (2020), the majority of U.S. states implemented several policies to slow the spread of the COVID-19. Examples include shutting down certain businesses deemed non-essential, closing schools, ordering people to stay-at-home, and imposing restrictions on large gatherings. Early papers show that these policies were effective in slowing down the spread of the virus among the population (Martin-Calvo, et al., 2020) and reducing the movement and social interactions of individuals (Dave et al., 2021; Friedson et al., 2020; Gupta et al., 2020).

A first step towards understanding the short-run effects of these policies on U.S. economy requires a more detailed analysis of their effects on numerous areas of economic activity, including the housing market. To the best of my knowledge, no previous study has investigated the early short-run effects the state level policies to control the spread of the COVID-19 on housing market activity. This paper thus analyzes detailed data covering change in new listings, total inventory, newly pending sales, median list price, web traffic to for-sale homes, and average number of days to pending sale status. The results presented here can help us understand how the state level policies played out, along with providing evidence that could be relevant to other times and other places.

I use daily and weekly housing market data from 100 major U.S. metropolitan area housing markets from January 1, 2020, before any U.S. COVID-19 cases were confirmed, to December 12, 2020, when there were more than 16 million COVID-19 related cases and 297,000 deaths in the U.S.

The publicly-available aggregate data indicate that the early collapse and partial recovery of housing market activity was broad-based. Using...

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European Economic Association hosts a registry of research projects related to impacts of coronavirus pandemic on economic and social outcomes. In May 2020, when working paper version of this paper was first released (Yoruk, 2020), there was no other paper that investigate the effect of COVID-19 pandemic on real estate markets in the United States. Later, Anenberg and Ringo (2021) investigate the trends in housing market in the United States during the pandemic. D’Lima, Lopez, and Pradhan (2021) investigate the pricing effects in housing markets following government shutdown responses to COVID-19.

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event study and difference-in-differences models, I find that many of the policies that were introduced to combat the pandemic had no significant short-run impact on many indicators of housing market activity. The strongest effects stem from the closure of non-essential businesses. I find that the closure of non-essential businesses in certain states was associated with up to an 11-percentage point decrease in new home listings and a 3.5 percentage point decrease in total inventory relative to the same time period in 2019. I also find that school closures may affect some outcomes.
2. Background and existing literature

The coronavirus epidemic hit the United States relatively late. The first case of COVID-19 in the United States was confirmed on January 20, 2020, when parts of China were already under a quarantine order. The first COVID-19 related death in the United States was reported on February 29. Washington was the first state to issue a state of emergency on the same date, with New York and California following the next week. There were 100 cases by March 4, 2020, 10,000 cases by March 18, and more than one million cases by April 28.

Fig. 3. Change in newly pending sales for selected metropolitan areas.
Notes: Weekly data from January 4 to December 12, 2020. Percentage change in newly pending sales is relative to the same time period in the previous year. Author’s calculations from Zillow data (Zillow Research, 2020).

Fig. 4. Change in total inventory for selected metropolitan areas.
Notes: Weekly data from January 4 to December 12, 2020. Percentage change in total inventory is relative to the same time period in the previous year. Author’s calculations from Zillow data (Zillow Research, 2020).
From mid-March to early April 2020, many states adopted policies to mitigate the spread of the coronavirus. These included stay-at-home orders, non-essential business closures, bans on large gatherings, school closures, and closings of certain businesses such as restaurants and bars. By April 20, 2020, all states except Arkansas, Iowa, Nebraska, North Dakota, South Dakota, and Wyoming had issued some form of a stay-at-home order. By the same time, all but seven states had implemented some form of non-essential business closures. Initial implementation of these policies across different states occurred at different times. Kong and Prinz (2020) demonstrate that although announcement

Fig. 5. Average number of days to pending sale status for selected metropolitan areas.
Notes: Weekly data from January 4 to December 12, 2020. Author’s calculations from Zillow data (Zillow Research, 2020).

Fig. 6. Percentage change in web traffic to for-sale homes for selected metropolitan areas.
Notes: Daily data from March 1 to December 12, 2020. Percentage change in web traffic to for-sale homes is relative to the same time period in the previous year. Author’s calculations from Zillow data (Zillow Research, 2020).
Table 1
Event study estimates: restrictions on mass gatherings.

| No. of days to policy | Percentage change in new listings | Percentage change in page views |
|-----------------------|----------------------------------|--------------------------------|
|                       | (1)                              | (2)                            | (3)                              | (4)                              | (5)                              |
| -7                    | -0.031                           | -0.047**                        | -0.049**                         | -0.057**                         | 0.005                            |
|                       | (0.020)                          | (0.022)                         | (0.022)                          | (0.022)                          | (0.043)                          |
| -6                    | -0.036**                         | -0.041**                        | -0.045**                         | -0.053**                         | -0.036                           |
|                       | (0.018)                          | (0.016)                         | (0.017)                          | (0.016)                          | (0.028)                          |
| -5                    | -0.024                           | -0.028**                        | -0.031**                         | -0.038**                         | -0.028                           |
|                       | (0.017)                          | (0.016)                         | (0.016)                          | (0.016)                          | (0.026)                          |
| -4                    | -0.007                           | -0.010                          | -0.013**                         | -0.018**                         | -0.014                           |
|                       | (0.015)                          | (0.014)                         | (0.014)                          | (0.014)                          | (0.023)                          |
| -3                    | 0.002                            | 0.000                           | -0.001                           | -0.004                           | -0.006                           |
|                       | (0.011)                          | (0.010)                         | (0.011)                          | (0.012)                          | (0.019)                          |
| -2                    | 0.001                            | 0.000                           | -0.000                           | -0.000                           | -0.005                           |
|                       | (0.006)                          | (0.006)                         | (0.006)                          | (0.006)                          | (0.011)                          |
| -1                    | 0.003                            | 0.006                           | 0.006                            | 0.009                            | 0.009                            |
|                       | (0.009)                          | (0.008)                         | (0.009)                          | (0.009)                          | (0.017)                          |
| 0                     | 0.003                            | 0.006                           | 0.008                            | 0.012                            | 0.011                            |
|                       | (0.014)                          | (0.013)                         | (0.014)                          | (0.015)                          | (0.028)                          |
| 1                     | 0.003                            | 0.007                           | 0.009                            | 0.016                            | 0.016                            |
|                       | (0.018)                          | (0.017)                         | (0.017)                          | (0.019)                          | (0.037)                          |
| 2                     | 0.002                            | 0.007                           | 0.009                            | 0.020                            | 0.028                            |
|                       | (0.021)                          | (0.020)                         | (0.021)                          | (0.022)                          | (0.045)                          |
| 3                     | 0.003                            | 0.010                           | 0.011                            | 0.026                            | 0.039                            |
|                       | (0.025)                          | (0.024)                         | (0.025)                          | (0.027)                          | (0.055)                          |
| 4                     | 0.002                            | 0.007                           | 0.009                            | 0.020                            | 0.028                            |
|                       | (0.029)                          | (0.028)                         | (0.028)                          | (0.031)                          | (0.067)                          |
| 5                     | 0.003                            | 0.011                           | 0.012                            | 0.031                            | 0.047                            |
|                       | (0.067)                          | (0.061)                         | (0.061)                          | (0.068)                          | (0.128)                          |

Notes: The day prior the announcement of the policy is the excluded category. All models include city, state, and calendar date fixed effects. States that did not implement the policy are excluded from the sample. Standard errors are clustered at the state level and reported in parentheses. The signs *, **, and *** indicate statistical significance at 10, 5, and 1 percent levels.

There is also a broader literature which investigates the effects of health shocks and pandemics on housing markets. Ambrus et al. (2020) examine the impact on housing prices of a cholera epidemic in one neighborhood of nineteenth century London. They show that ten years after the epidemic, housing prices are significantly lower just inside the catchment area of the water pump that transmitted the disease. Moreover, differences in housing prices persist over the following 160 years. Francke and Korevaar (2020) investigate the effects of historical outbreaks of the plague in the 17th century Amsterdam and cholera in the 19th century Paris on housing prices. They find that outbreaks resulted in large declines in house prices, and smaller declines in rent prices, with large reductions in house prices during the first six months of an epidemic in heavily affected areas. They also show that price shocks were transitory, and both cities quickly reverted to their initial price paths. Wong (2008) finds that SARS epidemic led to a 1.6% fall in home values and 72% fall in transactions volume in Hong Kong. Davis (2004) investigates the effect of a health risk, a cluster of cases of pediatric leukemia in an isolated county of Nevada, on housing prices. He finds that compared with unaffected counties, housing prices in the affected county declined significantly during the period of maximum risk.

As a broad background to examining the 2020 COVID period, Figs. A1 and A2 provide summary trends for the U.S., comparing January 2020 through mid-December 2020 with the same period in the previous year. Figs. A1 indicates that there was considerable aggregate movement in numerous measures of housing market activity in 2020. On February 22, 2020, changes in new home listings and pending sales were considerably higher than in the same time period in 2019. By March 28, 2020, both indicators were below their 2019 levels. On April 18, changes in newly listed homes and pending sales in the United States were more than 30% below their 2019 levels. Both indicators started to exhibit an upward trend after May 9, 2020 and recovered by early July 2020. Not surprisingly, total inventory exhibited a decreasing trend throughout the year.

Changes in web traffic to for-sale listings initially declined, but then quickly rose to a considerably higher level for the rest of 2020 (Fig. B). Average number of days to pending sale status was declining until March 28. From that date until late November, it exhibited a relatively steady trend. Change in median list price of homes was mostly stable until early
cases per 10,000 people. On the other hand, several cities in my sample had less than 100 cases per 10,000 people on the same date. Figs. 1 – 6 present trends in various indicators of housing market activity relative to the same time period in 2019 for selected metropolitan areas.

Event study estimates: school closures.

Table 2

| No. of days to policy | Percentage change in new listings (1) | (2) | (3) | (4) | (5) | Percentage change in page views (1) | (2) | (3) | (4) | (5) |
|-----------------------|--------------------------------------|-----|-----|-----|-----|-------------------------------------|-----|-----|-----|-----|
| 0                     | 0.065 (0.055) 0.055 0.051           | 0.083 | -0.008 | 0.083 | 0.064 | 0.060                             | 0.075 | -0.077 |
| 1                     | 0.019 (0.013) 0.009 0.030           | -0.004 | 0.057 | 0.046 | 0.043 | 0.050                           | -0.040 |
| 2                     | 0.011 (0.036) 0.039 0.037           | 0.035 | 0.074 | 0.035 | 0.035 | 0.035                         | 0.033 | 0.044 |
| 3                     | 0.011 (0.029) 0.031 0.030           | 0.028 | 0.066 | 0.028 | 0.028 | 0.025                     | 0.037 |
| 4                     | 0.001 (0.001) 0.001 0.001           | -0.015 | 0.036* | 0.029 | 0.028 | 0.032                     | -0.023 |
| 5                     | -0.001 (0.015) 0.016 0.016          | 0.014 | 0.040 | 0.013 | 0.013 | 0.013                   | 0.018 |
| 6                     | -0.004 (-0.006) -0.006 -0.006      | -0.000 | 0.011* | 0.009 | 0.008 | 0.009*                  | -0.008 |
| 7                     | 0.000 (0.008) 0.009 0.009          | 0.008 | 0.021 | 0.006 | 0.006 | 0.005                   | 0.009 |
| 8                     | -0.000 (0.001) 0.001 0.001         | -0.004 | 0.011 | -0.010 | -0.007 | -0.007                   | -0.008 |
| 9                     | -0.004 (0.007) 0.007 0.007         | 0.000 | 0.029 | -0.017 | -0.012 | -0.011                   | -0.014 |
| 10                    | 0.004 (0.013) 0.013 0.014          | -0.003 | 0.012 | -0.013 | -0.012 | -0.014                   | -0.019 |
| 11                    | 0.004 (0.019) 0.020 0.020          | 0.003 | 0.038 | -0.025 | -0.018 | -0.016                   | -0.021 |
| 12                    | 0.004 (0.026) 0.026 0.026          | 0.005 | 0.046 | -0.034 | -0.024 | -0.022                   | -0.031 |
| 13                    | 0.000 (0.035) 0.035 0.035          | -0.000 | 0.009 | -0.007 | -0.005 | -0.005                   | -0.006 |
| 14                    | -0.004 (0.044) 0.044 0.044         | 0.006 | -0.003 | -0.002 | -0.003 | -0.003                   | -0.004 |
| 15                    | -0.009 (0.052) 0.052 0.052         | -0.010 | 0.040 | -0.047 | -0.031 | -0.027                   | -0.049 |
| 16                    | -0.018 (0.075) 0.075 0.075         | -0.002 | 0.055 | -0.062 | -0.038 | -0.033                   | -0.067 |
| N                     | 34.700 34.700 32.965 28.801 16.656 | 27.700 27.700 26.315 22.991 13.296 |

Daily new cases & deaths per capita

Notes: The day prior the announcement of the policy is the excluded category. All models include city, state, and calendar date fixed effects. States that did not implement the policy are excluded from the sample. Standard errors are clustered at the state level and reported in parentheses. The sign * indicates statistical significance at 10 percent level.

July 2020, but then exhibited a moderately increasing trend after that date, remaining positive relative to its level during the same time period in the previous year.

Intensity of the spread of the virus and total confirmed cases and deaths in 2020 differed considerably across metropolitan areas. For example, as of mid-December 2020, New York City had more than 427 cases per 10,000 people. On the other hand, several cities in my sample had less than 100 cases per 10,000 people on the same date. Figs. 1 – 6 present trends in various indicators of housing market activity relative to the same time period in 2019 for selected metropolitan areas.

3. Data

I use data on daily confirmed coronavirus cases and deaths for each state from the New York Times COVID-19 database (New York Times, 2020). I use state population data from 2019 Census estimates to calculate per capita number of cases and deaths for each state. Since recent papers show that residents in Republican states are less likely to comply with restrictive policies, I estimate several models that exclude states with Republican governors.2

There are few sources that track the social distancing policies at the state level. I use the data assembled by The Institute for Health Metrics and Evaluation (IHME) at the University of Washington because it is comprehensive and provides precise information on timing of the social distancing orders. In particular, I use information on the timing of the introduction and lifting of bans on mass gatherings; that is, any business closures that only affected certain businesses such as restaurants and bars, school closings, closures of all non-essential businesses, and stay-at-home orders.

Daily housing data come from Zillow, an online real estate database company. Zillow reported for 2020 the daily percentage change in the number of newly listed homes and web-traffic to for-sale homes relative to the same time period in the previous year for 100 major U.S. housing markets.3 Zillow also reported weekly data on the percentage change in newly pending sales, total inventory, median list price and average number of days to pending sale status for selected markets. The markets used in the empirical analysis, summary statistics of variables, and data availability period for each market are described in the appendix Tables A2 and A3.

4. Empirical models

In order to formally estimate the short run effects of the spread of the coronavirus-related state-level social distancing policies on housing market activity, I use two empirical models. First, I estimate an event study model that exploits differential policy announcement dates across different states. The main specification for this model for the metro-

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2 In an early working paper version of this paper (Yörük, 2020), I present results from models that include county level total and daily cases and deaths. I also present results from models that exclude cities in which Trump received the majority of votes in 2016 presidential election. These alternative models yield very similar results compared with those presented in this paper.

3 The data used in this paper were released by Zillow on December 17, 2000 (Zillow Research, 2000).
The coefficients of interest $\hat{\delta}_i$ estimate the differential change in outcome variables for each day $j$ relative to the day prior to the announcement date. Following Kong and Prinz (2020), for periods $j > 7$ and $j < -7$, I assign $j = 7$ and $j = -7$ respectively with the underlying assumption that the dynamic effects of the policy are constant 7 days before or after the policy announcement.\footnote{Conducting an event study with an extended time period would rely on comparisons between states that adopted the policy early and states that adopted the policy at least one week later. However, on many occasions, state level policies to mitigate the spread of coronavirus were introduced at the same time or within few days apart across different states. Therefore, separately identifying calendar date fixed effects for an extended time period would rely on increasingly selected set of treated and control states.} Eq. (1) not only enables one to estimate the daily effects of a particular policy on these outcomes during the first week of the policy announcement date but also acts as a falsification test since it includes controls for the periods before the policy change.

Next, I use a difference-in-differences model to estimate the joint impact of state policies on different indicators of housing activity, while controlling for the spread of the coronavirus in certain cases. The main specification for this model is

$$Y_{it} = \beta_{\text{Policy}} x_{it} + \alpha X_{it} + \gamma_t + \varsigma_s + \mu_i + \epsilon_{it}$$

where $\epsilon_{it}$ is a set of binary variables for policies adopted by state $s$ at calendar date $t$. These binary variables take the value of one for the calendar dates for which $day \geq 0$. In several models, I control for the spread of the coronavirus at the state level ($X_{st}$), either as total number of cases or deaths per 10,000 people. I present the summary statistics for these variables in Appendix Table A3. Unlike the event study model, I estimate Eq. (2) also for the outcome variables for which weekly data are available (change in total inventory, median list price, newly pending sales, and average number of days to pending sale status). The key identification assumption behind Eq. (2) is that alternative indicators of housing market activity follow a common trend for treatment and control cities (cities that adopted a particular policy at a given date vs. those did not) in the absence of the policies to control for the spread of coronavirus. The common trend assumption may fail when a policy endogeneity problem exists. However, it is not plausible that states that are experiencing relatively strong or weak housing markets are correspondingly more or less likely to adopt these policies. The event study model in Eq. (1) also provides a pre-trend analysis of each policy since it includes terms that control for 2 to 7 days before the true adoption dates. In these models, dummy variables that capture the time periods before dates.
implement the policy are excluded from the sample. Standard errors are clustered at the state level and reported in parentheses. The signs *, **, and *** indicate results compare a daily result with the same date one year earlier.

5. Results

The policy change should not be significant. 5

5. Results

5.1. Event study models

I estimate Eq. (1) for the outcome variables for which daily data are available and report the results under different model specifications in Tables 1–5. My purpose here is to estimate the short-run effects of certain state-wide policy changes. In each table, five columns of results are presented for a given policy restriction: (1) results without considering COVID activity; (2) results taking into account COVID activity; (3) dropping New York given that it was an early COVID state; (4) also dropping California and Washington as early COVID states; (5) dropping New York given that it was an early COVID state; (4) also dropping California and Washington as early COVID states; (5) dropping New York given that it was an early COVID state. All results appear to be substantive post-policy effects. However, for this particular policy itself. I thus cannot rule out the possibility that this is a case of a continuation of pre-existing trends rather than effects of the policy itself.

Table 2 demonstrates that school closures did not have a significant impact on the percentage change in new listings and page views for sale homes. The first specification in Table 3 indicates that the effects of closure of non-essential businesses on percentage change in new listings are negative and highly significant. In particular, column (1) indicates that one day after this policy adoption date, there was about a 3.6 percentage point decrease in the change in new listings relative to the same time period in the previous year. This effect grew during the week: after one week, this number was down by about 24.8 percentage points relative to its level in 2019. This effect is large, representing about a 1.2 standard deviation change from the mean of this variable. Importantly, there were no effects in the time periods before the adoption date. Results reported in specifications (2) – (4) are quite similar. Inclusion of total cases and deaths per capita (2), exclusion of New York (3), or inclusion of the first states where cases were seen (California and Washington) do not change the basic result. Specification 5 of Table 3, where states with a Republican governor are excluded from the sample, shows what appear to be substantive post-policy effects. However, for this particular specification, coefficient estimates for the pre-policy period are also larger but positive. They decline throughout the pre-policy period, insignificant. This result continues to hold during the following week. Table 1 also does not provide evidence of effects of restrictions on mass gatherings on web traffic to for sale homes. In this case, there are positive and increasing results after imposition of the policy (specifications 2 through 4). However, the coefficient estimates for the previous week are also increasing. I thus cannot rule out the possibility that this is a case of a continuation of pre-existing trends rather than effects of the policy itself.

Table 1 indicates that restrictions of mass gatherings did not have a significant impact on the percentage change in new listings relative to the same period in 2019. In particular, while the estimates suggest that introduction of this restriction was associated with a small (0.3 to 0.9 percentage points) increase in this impact, this result is statistically

5 The timing and strength of policy implementation may be endogenous to the expectations regarding the effectiveness of other social distancing strategies. Use of calendar day fixed effects to adjust for timing issues and use of controls for the number of total cases and deaths may account for how seriously a city would take the pandemic. Nonetheless, I cannot completely rule out this type of endogeneity.
implying that the post-policy decreases could well be a part of a continuing trend and not a response to the policy change.

The results presented in the second part Table 3, columns (2) and (3), show evidence of a negative impact of the closure of non-essential businesses on the percent change in web traffic to for-sale homes. For specifications 2 and 3, the closure of non-essential businesses led to a 13.3 to 13.5 percentage point decrease in page views to for-sale homes. Here, I find no significant effects.

In all models presented in Table 6, inclusion of controls for the spread of the coronavirus has only a modest impact on the change in newly pending sales: a one-point increase in the total number of cases per 10,000 people at the state level decreases the change in newly pending sales by only 0.1 percentage points. Table 6 also indicates that the control variables have no significant impact on the total inventory. Table 7 reports the results from a similar DD analysis for the percentage change in new listings and web traffic to for-sale homes. Since daily data are available for these outcome variables, the number of observations is large, yielding more precise estimates that can be compared to event study results. The benchmark model results reported in the first specification of panel A show that closure of non-essential businesses is associated with up to a 3.5 percentage point decrease in the total inventory, compared with the same time period in the previous year.

Although significance comes at a marginal 10% level. Closure of non-essential businesses is associated with up to a 5 to 9.1 percentage point decrease in this inventory measure relative to the same period in the previous year. This effect is robust under alternative models and large (0.3 to 0.7 standard deviation change from the mean of this variable) – although significance comes at a marginal 10% level. Closure of non-essential businesses is associated with up to a 3.5 percentage point decrease in the total inventory, compared with the same time period in the previous year.

In all models presented in Table 6, inclusion of controls for the spread of the coronavirus has only a modest impact on the change in newly pending sales: a one-point increase in the total number of cases per 10,000 people at the state level decreases the change in newly pending sales by only 0.1 percentage points. Table 6 also indicates that the control variables have no significant impact on the total inventory. Table 7 reports the results from a similar DD analysis for the percentage change in new listings and web traffic to for-sale homes. Since daily data are available for these outcome variables, the number of observations is large, yielding more precise estimates that can be compared to event study results. The benchmark model results reported in the first specification of panel A show that closure of non-essential businesses is associated with a 11 percentage point decrease in per 

### Table 5

| No. of days to policy | Percentage change in new listings (1) | Percentage change in new listings (2) | Percentage change in new listings (3) | Percentage change in new listings (4) | Percentage change in new listings (5) | Percentage change in page views (1) | Percentage change in page views (2) | Percentage change in page views (3) | Percentage change in page views (4) | Percentage change in page views (5) |
|-----------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|-------------------------------------|
| .7                    | 0.008                               | 0.002                               | -0.011                              | 0.047                               | 0.019                               | 0.006                               | -0.019                              | -0.010                              | -0.066*                             | 0.040                               |
| 1                     | 0.012                               | 0.012                               | 0.013                               | 0.015                               | 0.032                               | 0.008                               | 0.009                               | 0.010                               | 0.007                               | 0.021                               |
| 2                     | 0.016                               | 0.014                               | 0.005                               | 0.007                               | 0.014                               | 0.012                               | 0.007                               | 0.007                               | 0.012                               | 0.016                               |
| 3                     | 0.022                               | 0.020                               | 0.016                               | 0.025                               | 0.045                               | 0.012                               | 0.014                               | 0.011                               | 0.030                               | 0.032                               |
| 4                     | 0.044                               | 0.042                               | 0.023                               | 0.028                               | 0.046                               | 0.021                               | 0.011                               | 0.009                               | 0.022                               | 0.027                               |
| 5                     | -0.058*                             | -0.055*                             | -0.033                              | -0.034                              | -0.065                              | -0.022                              | -0.010                              | -0.008                              | 0.028                               | -0.029                               |
| 6                     | -0.073**                            | -0.069*                             | -0.044                              | -0.044                              | -0.076                              | -0.021                              | -0.006                              | -0.004                              | 0.036                                | -0.028                               |
| 7                     | -0.142**                            | -0.133**                            | -0.093**                            | -0.087                              | -0.084                              | -0.046                              | -0.018                              | -0.007                              | 0.046                                | -0.028                               |
| N                     | 29,148                              | 29,148                              | 27,413                              | 23,249                              | 14,921                              | 23,268                              | 23,268                              | 21,883                              | 18,559                               | 11,911                               |

Notes: The day prior the announcement of the policy is the excluded category. All models include city, state, and calendar date fixed effects. States that did not implement the policy are excluded from the sample. Standard errors are clustered at the state level and reported in parentheses. The signs * and ** indicate statistical significance at 10 and 5 percent levels.
study analysis, none of the policies that were introduced to mitigate the spread of the coronavirus had a statistically significant impact on the web traffic to for-sale homes. Furthermore, the effect of the other control variables on this outcome remains relatively small and statistically insignificant in most of the models.

In Table 8, the results reported in panel A indicate that while most of the state level policies did not have a significant impact on the median list price, school closures had a positive impact on this outcome. In particular, this policy is associated with approximately a 2 percentage point increase in median list price relative to the same period in the previous year. This effect corresponds to 0.33 standard deviation change from the mean of this variable. The positive impact of school closures on housing prices is surprising but can possibly be explained by the timing of the introduction of this policy. School closures were adopted by the majority of states between March 13 and 20. During that period, housing prices were increasing in the United States (appendix Figs. A1, panel B). Therefore, the coefficient on this policy could possibly be capturing the positive trend in housing prices during that time rather than the true effect of the policy. However, since daily data are not available for median list prices, it is not possible to estimate an event study model to formally test this hypothesis.

Nonetheless, I further investigate this possibility by estimating a model similar to the one presented in the fourth column of Table 8. In this model, I replace the actual school closures and other state level policy dummies with placebo dummies which take the value of one at one period (week) before the actual adoption date of the policy. If the coefficients on the school closures dummy in panel A of Table 8 represent the true effect of this policy on the percentage change in the median list prices, the coefficient on the placebo dummy should be insignificant. This is not the case, however. The coefficient on the placebo dummy is 0.025 (p<0.008), which is virtually the same as the coefficient presented in the fourth specification of Table 8. This provides further indication that the coefficients on the school closures dummy in panel A of Table 8 may be capturing the trend rather than representing the true effect of the policy itself.

The results in panel B of Table 8 indicate that under certain models, school closures are associated with up to a 4-day increase in average number of days to pending sale status relative to the same period in 2019. This effect corresponds to 0.3 standard deviation change from the mean of this variable, but marginally significant at 10 percent level. On the other hand, none of the remaining state level policies had a significant impact on the duration of the number of days to pending sale status.

### Table 6
The effect of state level policies on total inventory and newly pending sales.

| A. Percentage change in total inventory | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|----------------------------------------|-----|-----|-----|-----|-----|-----|-----|
| Ban on mass gatherings                 | 0.000 | -0.008 | 0.003 | -0.006 | -0.009 | -0.016 | 0.006 |
|                                        | (0.024) | (0.024) | (0.025) | (0.024) | (0.024) | (0.022) | (0.024) |
| School closures                        | -0.046 | -0.057* | -0.050* | -0.058* | -0.056* | -0.055* | -0.091* |
|                                        | (0.028) | (0.030) | (0.027) | (0.029) | (0.031) | (0.033) | (0.044) |
| Closure of any business                | 0.024 | 0.022 | 0.024 | 0.023 | 0.020 | 0.009 | 0.002 |
|                                        | (0.027) | (0.025) | (0.027) | (0.026) | (0.026) | (0.025) | (0.033) |
| Closure of non-essential business      | -0.035*** | -0.027*** | -0.034*** | -0.028*** | -0.024** | -0.021 | -0.025* |
|                                        | (0.011) | (0.010) | (0.011) | (0.009) | (0.010) | (0.016) | (0.014) |
| Stay at home order                     | 0.007 | 0.009 | 0.008 | 0.014 | 0.014 | 0.030 | 0.039 |
|                                        | (0.034) | (0.032) | (0.032) | (0.031) | (0.032) | (0.018) | (0.030) |
| Total cases per 10,000                 | -0.000** | -0.000* | -0.000* | -0.000* | -0.000 | -0.000 | -0.000 |
|                                        | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |

| B. Percentage change in newly pending sales |
|---------------------------------------------|
| Ban on mass gatherings                      | -0.010 | -0.022 | -0.009 | -0.028 | -0.023 | -0.039 | 0.009 |
|                                            | (0.047) | (0.050) | (0.047) | (0.050) | (0.053) | (0.060) | (0.053) |
| School closures                            | 0.032 | 0.002 | 0.031 | 0.001 | 0.005 | 0.006 | 0.155** |
|                                            | (0.072) | (0.064) | (0.075) | (0.058) | (0.061) | (0.058) | (0.069) |
| Closure of any business                    | 0.105 | 0.092 | 0.107 | 0.085 | 0.091 | 0.080 | 0.303*** |
|                                            | (0.065) | (0.068) | (0.065) | (0.067) | (0.070) | (0.077) | (0.099) |
| Closure of non-essential business          | -0.079 | -0.053 | -0.079 | -0.051 | -0.043 | -0.127 | -0.024 |
|                                            | (0.074) | (0.065) | (0.074) | (0.064) | (0.065) | (0.098) | (0.058) |
| Stay at home order                         | -0.092 | -0.087 | -0.094 | -0.083 | -0.084 | -0.141 | -0.101 |
|                                            | (0.074) | (0.068) | (0.075) | (0.070) | (0.075) | (0.105) | (0.082) |
| Total cases per 10,000                     | -0.001** | -0.001** | -0.001** | -0.001** | -0.001*** | (0.000) | (0.000) |
|                                            | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |

| Total deaths per 10,000                    | -0.003 | 0.006 | (0.007) | (0.008) |
|                                            | N     | 4719 | 4719 | 4719 | 4469 | 3919 | 2300 |
| Drop NY                                   | N     | N     | N     | N     | N     | N     | N     |
| Drop NY, WA, and CA                       | N     | N     | N     | N     | N     | N     | N     |
| Drop states with Republican Governors      | N     | N     | N     | N     | N     | N     | N     |

Notes: All models include city, state, and calendar date fixed effects. Standard errors are clustered at the state level and reported in parentheses. The signs *, **, and *** indicate statistical significance at 10, 5, and 1 percent levels.
Here can help us understand how the state level policies played out, the average number of days to pending sale status. The results presented newly pending sales, median list price, web traffic to for-sale homes, and December 12, 2020, this paper provides the first estimates of the short weekly data from 100 major U.S. metropolitan areas from January 1 to 2020, a majority of U.S. states adopted such policies. Using daily and measures of housing market activity. From mid-March to early April COVID pandemic to mitigate the spread of the virus on numerous activity in a large number of U.S. metropolitan areas.

6. Conclusion

This article explores the effects of policies adopted early in the COVID pandemic to mitigate the spread of the virus on numerous measures of housing market activity. From mid-March to early April 2020, a majority of U.S. states adopted such policies. Using daily and weekly data from 100 major U.S. metropolitan areas from January 1 to December 12, 2020, this paper provides the first estimates of the short run impact of these policies on various indicators of housing market activity in a large number of U.S. metropolitan areas.

Examples of these policies include shutting down certain businesses deemed non-essential, closing schools, ordering people to stay-at-home, and imposing restrictions on large gatherings. The changes in housing market activity covered are the change in new listings, total inventory, newly pending sales, median list price, web traffic to for-sale homes, and average number of days to pending sale status. The results presented here can help us understand how the state level policies played out, along with providing evidence that could be relevant to other times and other places.

Estimates from the event study and difference-in-differences models suggest that the response of housing market activity was complex. I find that the strongest policy effect stemmed from the closure of non-essential businesses. The closure of non-essential businesses was associated with up to an 11 percentage point decrease in new home listings and a 3.5 percentage point decrease in total inventory relative to the same period in 2019. This result is not surprising since majority of homes in the United States are sold by real estate agents who were considered as non-essential workers in many states that adopted the closure of non-essential businesses policy. Some states such as New York later relaxed their policy and allowed real estate agents to conduct their business. In reality, however, during the early months of the pandemic, real estate agents were not allowed to meet with their clients in person and only allowed to show properties virtually, especially if the state had a stay-at-home order in place. An increasing trend in the web traffic to for-sale listings after the majority of state level policies were adopted by late March 2020 may also be explained by this policy and stay-at-home orders.

Under certain specifications, I show that school closures led to a 5 to 9.1 percentage point decrease in the change in total inventory relative to the same period in the previous year. This effect is economically large but marginally statistically significant. School closures also led to an increase in the average number of days to pending sale.

Future research should investigate the long-run effects of COVID-19 on U.S. housing market activity when housing market data for the extended post-pandemic period become available.
Table 8
The effect of state level policies on median list price and days to pending sale status.

A. Percentage change in median list price

| Policy                                      | (1)     | (2)     | (3)     | (4)     | (5)     | (6)     | (7)     |
|---------------------------------------------|---------|---------|---------|---------|---------|---------|---------|
| Ban on mass gatherings                      | -0.011  | -0.008  | -0.013  | -0.012  | -0.010  | -0.007  | -0.019 *|
| School closures                             | 0.020** | 0.023** | 0.024***| 0.025***| 0.023** | 0.022** | 0.021*  |
| Closure of any business                     | 0.009   | 0.010   | 0.009   | 0.009   | 0.009   | 0.011   | 0.018   |
| Closure of non-essential business          | -0.004  | -0.006  | -0.004  | -0.006  | -0.008  | 0.004   | -0.017**|
| Stay at home order                          | -0.002  | -0.003  | -0.001  | -0.001  | -0.003  | 0.008   | -0.010  |
| Total cases per 10,000                      | 0.000*  | 0.000   | 0.000   | 0.000   | 0.000   | 0.000   | 0.000   |
| Total deaths per 10,000                     | 0.003***| 0.003*  | 0.003   | 0.003   | 0.003   | 0.003   | 0.003   |
| N                                           | 4750    | 4750    | 4750    | 4750    | 4750    | 4750    | 4750    |

B. Average number of days to pending sale status

| Policy                                      | (1)     | (2)     | (3)     | (4)     | (5)     | (6)     | (7)     |
|---------------------------------------------|---------|---------|---------|---------|---------|---------|---------|
| Ban on mass gatherings                      | 0.380   | 0.253   | 1.013   | 1.292   | 0.898   | 0.459   | 3.206** |
| School closures                             | 3.969*  | 3.798*  | 2.921   | 3.151*  | 3.852*  | 4.001*  | -4.123  |
| Closure of any business                     | 0.442   | 0.422   | 0.612   | 0.661   | 0.781   | 0.498   | 0.612   |
| Closure of non-essential business          | 0.123   | 0.248   | 0.288   | 0.074   | 0.164   | 1.308   | 2.530***|
| Stay at home order                          | 1.569   | 1.579   | 1.062   | 1.003   | 1.173   | 0.029   | 3.134*  |
| Total cases per 10,000                      | -0.004  | 0.008   | 0.008   | 0.008   | 0.008   | 0.008   | 0.008   |
| Total deaths per 10,000                     | -0.863***| -0.930***|         |         |         |         |         |
| N                                           | 4624    | 4624    | 4624    | 4624    | 4729    | 3829    | 2240    |

Notes: All models include city, state, and calendar date fixed effects. Standard errors are clustered at the state level and reported in parentheses. The signs *, **, and *** indicate statistical significance at 10, 5, and 1 percent levels.
Data Availability

Data will be made available on request.

Acknowledgments

An early version of this paper which covers a shorter time period has been circulated as CESifo Working Paper No. 8333 (Yorük, 2020). I thank the Editor for insightful comments.

Appendix

Table A1
State adoption and end dates for sample states.

| State | Mass gatherings ban | School closures | Closure of non-essential business | Any business closures | Stay at home orders |
|-------|---------------------|-----------------|-----------------------------------|-----------------------|-------------------|
| AL    | 3/19                | 3/19            | 3/28 - 4/30                       | 3/19 - 6/15           | 4/4 - 4/30        |
| AR    | 3/27 - 6/18         | 3/17 - 8/24     |                                   | 3/19                  |                   |
| AZ    | 3/30 - 5/16         | 3/16            |                                   | 3/30 - 5/16           | 3/20 - 5/16       |
| CA    | 3/11                | 3/19            | 3/19 - 9/2                        | 3/17                  | 3/19              |
| CO    | 3/20                | 3/23            | 3/26 - 5/9                        | 3/17                  | 3/26 - 5/9        |
| CT    | 3/12                | 3/17            | 3/23 - 5/20                       | 3/16                  |                   |
| DC    | 3/13                | 3/16            | 3/25 - 5/29                       | 3/16                  | 3/30 - 5/29       |
| FL    | 4/3 - 6/5           | 3/17 - 10/5     |                                   | 3/17 - 9/25           | 4/3 - 5/18        |
| GA    | 3/24                | 3/18            |                                   | 3/24                  | 4/3              |
| HI    | 3/17                | 3/19            | 3/25 - 5/1                        | 3/17                  | 3/25 - 6/10       |
| IA    | 3/17 - 6/12         | 4/4             | 3/17 - 5/8                        | 3/17 - 10/16          |                   |
| ID    | 3/25 - 5/1          | 3/23            | 3/25 - 5/1                        | 3/25 - 6/13           | 3/25 - 5/1        |
| IL    | 3/13                | 3/17            | 3/21 - 5/1                        | 3/16                  | 3/21 - 5/9        |
| IN    | 3/12                | 3/19            | 3/24 - 5/18                       | 3/16                  | 3/25 - 5/18       |
| KS    | 3/17 - 5/22         | 3/17            |                                   | 3/30 - 6/8            | 3/30 - 5/4        |
| KY    | 3/19                | 3/20            | 3/26 - 5/11                       | 3/16                  | 3/23 - 5/15       |
| LA    | 3/13 - 5/15         | 3/16            | 3/22 - 5/1                        | 3/17                  |                   |
| MA    | 3/13                | 3/17            | 3/24 - 5/18                       | 3/16                  | 3/30 - 5/15       |
| MD    | 3/16 - 6/10         | 3/16            | 3/23 - 5/15                       | 3/16                  | 3/24 - 6/1        |
| MI    | 3/13                | 3/16            | 3/23 - 5/7                        | 3/16                  | 3/26 - 6/1        |
| MN    | 3/28                | 3/18            |                                   | 3/17                  | 3/28 - 5/18       |
| MO    | 3/23 - 5/4          | 3/23            |                                   | 3/23 - 6/16           | 4/6 - 5/15        |
| MS    | 3/24                | 3/19            | 4/3 - 4/27                        | 3/24 - 6/1            | 4/3 - 4/27        |
| NC    | 3/14                | 3/14            | 3/30 - 5/8                        | 3/17                  | 3/30 - 5/8        |
| NE    | 3/16                | 4/2             |                                   | 3/19 - 7/6            |                   |
| NM    | 3/12                | 3/13            | 3/24 - 5/15                       | 3/16                  |                   |
| NV    | 3/24                | 3/16            | 3/21 - 5/9                        | 3/18                  | 3/31 - 5/9        |
| NY    | 3/12                | 3/18            | 3/22 - 6/8                        | 3/16                  | 3/32 - 6/8        |
| OH    | 3/12                | 3/16            | 3/23 - 5/4                        | 3/15                  | 3/23 - 5/20       |
| OK    | 3/24 - 5/24         | 3/17            | 4/1 - 4/24                        | 4/1 - 6/1             |                   |
| OR    | 3/12                | 3/16            |                                   | 3/17                  | 3/23 - 6/19       |
| PA    | 4/1 - 9/14          | 3/17            | 3/23 - 5/8                        | 3/18 - 7/3            | 4/1 - 6/5         |
| RI    | 3/17                | 3/16            |                                   | 3/17                  | 3/28 - 5/9        |
| SC    | 3/18                | 3/16            | 3/22 - 6/8                        | 3/18 - 8/3            | 4/7 - 5/4         |
| TN    | 3/23                | 3/20            | 4/1 - 5/26                        | 3/23                  | 4/2 - 5/26        |
| TX    | 3/21 - 6/4          | 3/19            |                                   | 3/21                  | 4/2 - 5/1         |
| UT    | 3/19 - 5/1          | 3/16            |                                   | 3/19                  |                   |
| VA    | 3/15                | 3/16            | 3/24 - 5/15                       | 3/17                  | 3/30 - 6/5        |
| WA    | 3/11                | 3/13            | 3/25 - 7/3                        | 3/16                  | 3/23 - 7/3        |
| WI    | 3/17                | 3/18            | 3/25 - 5/11                       | 3/17                  | 3/25 - 5/13       |

Table A2
List of sample housing markets.

Akron, OH       Dayton Beach, FL*       Melbourne, FL*       Salt Lake City, UT+
Albany, NY      Denver, CO            Milwaukee, WI+       San Antonio, TX+
Albuquerque, NM Des Moines, IA+       Minneapolis-St. Paul, MN San Diego, CA
Allentown, PA+  Detroit, MI           Nashville, TN+       San Francisco, CA
Atlanta, GA     El Paso, TX+          New Haven, CT        Scranton, PA*+       Seattle, WA
Augusta, GA     Fort Myers, FL+       New Orleans, LA       Spokane, WA*+       Springfield, MA
Austin, TX      Fresno, CA           Oklahoma City, OK     St. Louis, MO
Bakersfield, CA Grand Rapids, MI      North Port-Sarasota-Bradenton, FL+ Stockbridge, GA
Baltimore, MD   Greensboro, NC        Ogden, UT+           Stamford, CT
Baton Rogue, LA+ Greenville, SC        Orlando, FL+         Toledo, OH
Birmingham, AL  Harrisburg, PA        Phoenix, AZ          Tucson, AZ
Boise City, ID  Hartford, CT          Syracuse, NY         Tulsa, OK
Boston, MA      Houston, TX           Raleigh, NC          Urban Honolulu, HI+
Buffalo, NY     Indianapolis, IN       Portland, OR         Ventura, CA
Charleston, SC  Jackson, MS+          Providence, RI
Charlotte, NC   Jacksonville, FL      Chattanooga, USA      West Palm Beach, FL
Chattanooga, TN+ Kansas City, MO       Charleston, SC        Washington, DC
Chicago, IL     Knoxville, TN         Cincinnati, OH        Chicago, IL
Cincinnati, OH  Lakeland, FL+         Cleveland, OH         Cleveland, OH

(continued on next page)


Table A2 (continued)

| Outcome variables | No. of Obs. | Mean    | Standard Deviation |
|-------------------|------------|---------|--------------------|
| Percentage change in new listingsa | 34,700 | -0.010  | 0.205              |
| Percentage change in newly pending salesb | 3323 | 0.109   | 0.366              |
| Percentage change in total inventoryc | 4719 | -0.228  | 0.144              |
| Percentage change in web traffic to for-sale homesd | 27,700 | 0.243   | 0.205              |
| Percentage change in median list pricee | 4750 | 0.069   | 0.061              |
| Average number of days to pending sale statusf | 4624 | 18.19   | 13.87              |
| Total number of cases (per 10,000 people) | 34,700 | 119.12  | 137.99             |
| Total number of deaths (per 10,000 people) | 34,700 | 3.32    | 4.12               |

Notes: The sign * indicates the markets with no data on days to sale pending status, newly pending listings, total for sale inventory, and median listing price. The sign + indicates the markets with no data on newly pending listings.

Table A3

Summary statistics.

| Outcome variables | No. of Obs. | Mean    | Standard Deviation |
|-------------------|------------|---------|--------------------|
| Bans on mass gatherings | 34,700 | 0.583   | 0.493              |
| Any business closures | 34,700 | 0.647   | 0.478              |
| School closures | 34,700 | 0.752   | 0.432              |
| Closure of non-essential business | 34,700 | 0.131   | 0.337              |
| Stay-at-home orders | 34,700 | 0.200   | 0.400              |

Notes: Percentage changes in outcome variables are relative to the same time period in the previous year. Summary statistics for outcome variables: Data available for (a) Daily from January 1 to December 2, 2020 for 100 markets. (b) Weekly from January 4 to December 12, 2020 for 70 markets. (c) Weekly from January 4 to December 12, 2020 for 94 markets. (d) Daily from March 1 to December 2, 2020 for 100 markets.

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