Department of Economics

MSc Extended Essay Cover Sheet

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COURSE CODE	E	C	4	7	5

TITLE

What are the Macroeconomic Impacts of Evictions? Evidence from the COVID-19 Pandemic

WORD COUNT	6	3	5	5

Double spaced, typed. Maximum 6,000 words (as a guideline this is 14-21 double spaced pages). Abstract, footnotes, references and appendices do not count toward the word count, provided such additions are brief and do not contain information that rightly belongs in the body of the essay. Equations are included in the word count and counted as the page equivalent (i.e., as the number of words that would occupy the same amount of space in text).*

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What are the Macroeconomic Impacts of Evictions? Evidence from the COVID-19 Pandemic

Candidate Number: 51104

Abstract

At the beginning of the COVID-19 pandemic, 43 US states implemented eviction moratoria to protect vulnerable renters. This paper studies the economic impacts of evictions using a difference-in-difference design that exploits the ends of these moratoria, which varied by state. My results suggest that evictions (and the threat of evictions) were effective in reducing the unemployment rate by almost 1%. I also find some evidence that long-term output and number of labor force participants rose following moratorium end. These results point to the importance of eviction moratoria as economic policy alongside lockdowns, which are presently considered the major drivers of economic changes during this period.

1 Introduction

In the United States, millions of households every year are under the threat of eviction. In the years 2000-2018, just prior to the COVID-19 pandemic, an average of 3.6 million cases were filed each year (Garnham et al. (2022)). These filings represent more than 1% of the US population being under threat of eviction in a given year. Because evictions can be incredibly taxing for individuals, it's possible that these cases have large aggregate effects on the labor force that have henceforth gone unnoticed in macroeconomic research. The major problem with studying the eviction effect on the labor force, however, is the potential for reverse causality. Whereas previous literature has overcome this issue by estimating these effects in micro settings, there is little research on the labor effects of evictions in the aggregate. I overcome this challenge using a novel setting: the incidence of eviction moratoria in the United States during the COVID-19 pandemic. By exploiting quasi-random variation in eviction moratorium expiration in different US states following the start of the COVID-19 pandemic in March 2020, I find strong evidence that following moratorium expiry, the unemployment rate in counties with evictions fell by around 0.8 to 1%. This drop is consistent with unemployed people returning to work rather than exiting the labor force. Finally, I find weak evidence for a rise in industrial added value, my measure for county-level economic output, around 0.5%.

My research expands upon previous literature in two important ways. First, previous literature on the labor effect of evictions tends to exploit micro-level variation in policy within a city or a state. This paper exploits variation at the county level, allowing for estimation of the macroeconomic effects of eviction policy. Secondly, previous literature on the economic effects of pandemic lockdowns describe them as a primary driver for the unique evolution of unemployment and GDP during this time. Although this is undeniably true, my paper shows that eviction policy additionally returned unemployment and GDP to their pre-pandemic levels. From a policy perspective, this paper also demonstrates that workers respond immediately to evictions. In other words, eviction moratoria do not have sticky effects on unemployment.

The rest of the paper is organized as follows: section 2 presents the institutional context, section 3 reviews related literature, and section 4 discusses data used. Section 5 presents the empirical strategy and section 6 presents main (6.1) and heterogeneity (6.2) results. Finally, section 7 analyzes robustness checks and section 8 concludes.

2 Institutional Context

In March 2020, the World Health Organization declared COVID-19 a pandemic, and nations around the world instituted large-scale social protection orders to prevent the spread of the virus. In March 2020, 43 states in the US imposed some form of lockdown, including measures like stay-at-home orders, mandatory closure of certain businesses, and restrictions on social gatherings. These lockdowns, alongside similar shutdowns worldwide, wrought rapid economic deterioration in the United States. The unemployment rate rose from just 4.4% in March to its peak in April at 14.8%, and quarterly US GDP contracted by more than 8% in quarter 2 according to the U.S. Bureau of Labor Statistics (2025) and U.S. Bureau of Economic Analysis (2025), respectively. Although US GDP nearly returned to

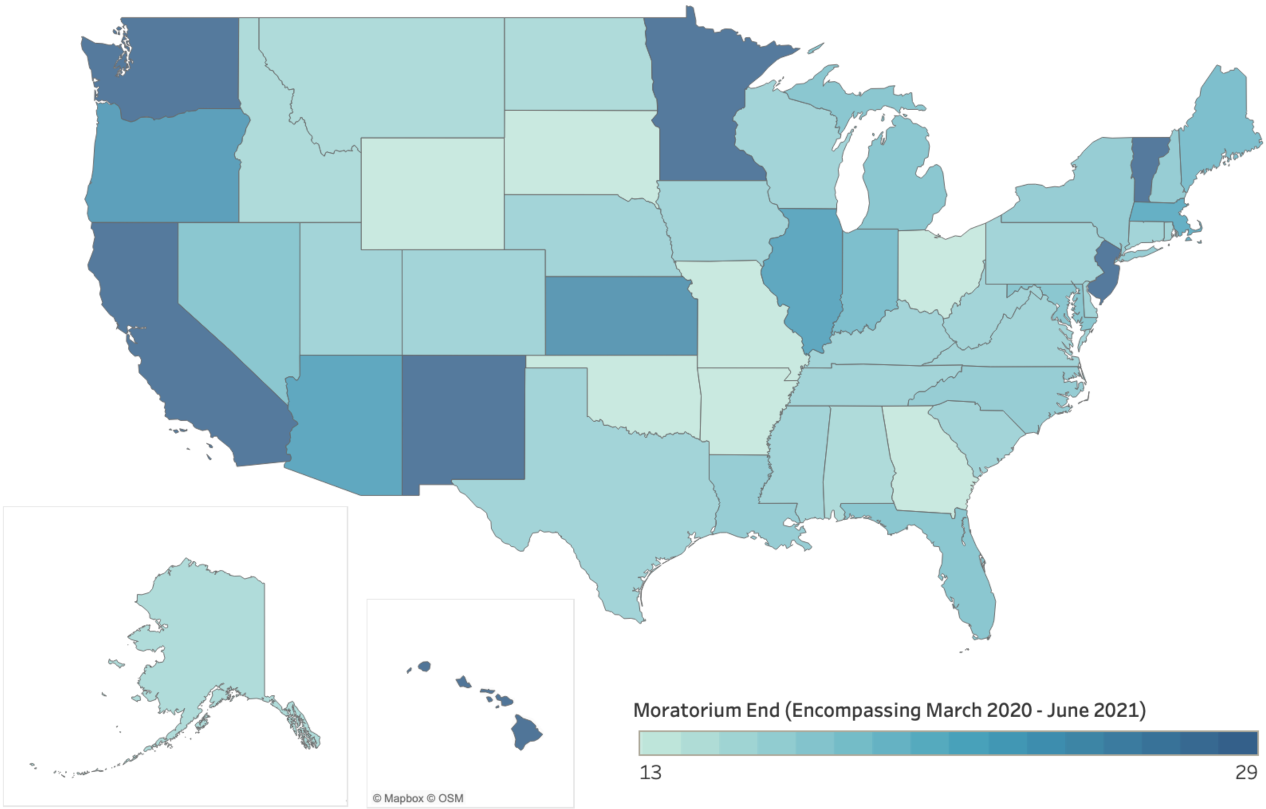

Figure 1: States by month of moratorium end, centered at January 2019 (month 0). States with no moratorium end (Arkansas, Georgia, Missouri, Ohio, Oklahoma, South Dakota and Wyoming) are labeled March 2020, which is month 13. States with moratoria that ended on or after June 2021 (California, Hawaii, Minnesota, New Jersey, New Mexico, Vermont and Washington) are labeled June 2021, which is month 29.

pre-pandemic levels by quarter 3, it took until November 2021 for the US unemployment rate to return to below that of March 2020. In response to wide-scale job loss and to prevent the further spread of COVID-19, the 43 lockdown states also imposed eviction moratoria in March. In this setting, eviction moratorium refers to some form of stay on eviction proceedings in the courts, allowing affected renters to remain in their homes. Although moratorium strength varied by state, they immediately reduced eviction filings. According to the The Eviction Lab (2025), a public provider of nationwide eviction data, eviction filings fell from 90,391 in February to 50,497 in March and just 6,859 in April, the month in which moratoria in all 43 states were in effect. Although these states began their moratoria at the same time, they ended them at different times, ranging from April 2020 to June 2021, providing an interesting setting for experimentation. Figure 1 demonstrates end month by state. Note that the seven lightest states did not impose lockdowns or eviction moratoria, whereas the seven dark states (including Hawaii) ended their moratoria on or after June 2021. I consider March 2020, the month in which most states started their moratoria, as the start of the "moratorium" regime in which 43 states are considered control. Over time, states become "treated" by moratorium end, leading to an immediate resumption in evictions. Provided there is quasi-random assignment of treatment, this is the perfect setting for difference-in-difference (DiD) estimation of the various economic effects of eviction treatment.

3 Literature Review

There is a large and growing body of research on the economic effect of the 2020 COVID-19 lockdowns. Besley and Stern (2020) presents the main trade-off of lockdown policy: public health and lives saved versus protecting incomes. The authors argue that the best economic policy relaxes this trade-off and takes into account the advice of different disciplines. Previous literature also models the shock and subsequent ripple effects lockdown policies have on an economy. Auray and Eyquem (2020) model lockdown shocks through a separation shock or a labor utilization shock and their effects on output, consumption and unemployment in a heterogeneous agent model. Buera et al. (2021) model lockdowns as a one-period shock to a fraction of nonessential firms and consider both larger lockdown fractions and longer lockdown periods. The authors find a temporary recessionary spike followed by long-term economic scarring from the lockdown effect on young nonessential firms. Articles around this period also sought to describe the material effects of lockdowns on labor, consumption and output. Coibion et al. (2020) use survey data to estimate the economic effects of localized lockdowns. The authors find individuals in a county under lockdown had 1.9% lower labor force participation, a 2.4% higher unemployment rate, and a 31% drop in consumption spending. Deb et al. (2022) use Nitrogen Dioxide emissions as an indicator for country-level economic activity and find that lockdown measures are consistent with a 30-day reduction in industrial production of 10%.

There is also substantial literature on the health and mortality effects of eviction moratoria during the COVID-19 pandemic. Similar difference-in-difference designs to my own estimate the moratorium treatment effect on health outcomes. Leifheit et al. (2021) uses an event study specification with moratorium end, but explores the effect of evictions on COVID-19 transmission and mortality. In the sixteen weeks after moratorium end, the authors find 2.1 times higher COVID-19 transmission and 5.4 times higher mortality relative to moratorium states. Benfer et al. (2021) demonstrate that evictions lead to increased COVID-19 transmission, are more likely to target populations with chronic conditions, and increase health inequities for minority groups. Because moratoria are effective in reducing evictions, the authors argue for moratorium adoption as a means of preventing COVID-19 transmission and mortality. An et al. (2022) use the same analysis to quantify the effects of eviction moratoria on household well-being, including consumer spending. The authors find that in high renter share areas, the imposition of an eviction moratorium led to a 16% increase in consumption spending. Using Google Search trend data, the authors also find that both food insecurity and depression indicators fall in areas under an eviction moratorium. It is important to note, however, that the above results could be biased by heterogeneous treatment effects in this staggered adoption setting. My results are robust in the presence of heterogeneous treatment effects, discussed further in section 5.2. Nande et al. (2021) uses an epidemiological model to quantify the health effects of evictions during the pandemic in a metropolitan area of 1 million people. The authors find that a 1% increase in evictions over a (moratorium) baseline without evictions leads to a 4% increase in infections. In a simulated city with high and low socioeconomic status neighborhoods, a more plausible model, this increase in infections rises to 5.4%.

Finally, there is robust literature on the health and economic effects of evictions in a more normal (non-pandemic) setting. Matthew Desmond (the founder of the Eviction Lab) has a large body of work analyzing the effects of evictions in Milwaukee, including Desmond (2012), Desmond and Shollenberger (2015) and Desmond and Gershenson (2016). These articles find worse economic outcomes for low-income renters: evictions are more likely to create and reproduce urban poverty, evicted renters are more likely to relocate to poorer or high-crime neighborhoods, and almost 20% of forcibly moved renters experienced subsequent job loss. Desmond and Kimbro (2015) focuses on mothers and finds that recently evicted mothers are more likely to experience material hardship, parental stress, and maternal depression. These negative results remain at least two years after the eviction, demonstrating their long-lasting effects. Hatch and Yun (2020) finds similar results for young adults. One year and 7-8 years after eviction, young adults are more likely to report having poor mental and general health. With respect to physical health, Currie and Tekin (2015) finds that increased foreclosures are linked to an increase in nonelective and preventable hospital and ER visits. Allen et al. (2019) finds that the inverse also holds true: California counties treated by Medicaid expansion saw a relative drop in evictions. Recent work in Collinson and Reed (2018) and Collinson et al. (2024) overcomes reverse causality between evictions and job loss by instrumenting judge leniency in an eviction order. Both articles find that evictions lead to an increase in hospitalization and homelessness, while the latter finds around a 1.5% reduction in employment in the first 4 quarters after eviction filings.

My research expands upon the previous literature on both COVID-19 lockdowns and evictions. Previous literature focuses on the health and mortality effects of eviction moratoria during the COVID-19 pandemic but does not account for their general effects on labor and output. Although moratoria clearly improved health outcomes by slowing the transmission of COVID-19, my research suggests they also prolonged the high unemployment of this period, illustrating the health-economy tradeoff in Besley and Stern (2020). My research also expands upon previous literature on evictions by offering a county-level analysis of eviction effects on output and labor. My results are a stark departure from previous literature, which largely suggests that evictions not only lead to worse health outcomes, but also job loss and increased unemployment. The fact that I find a *reduction* in unemployment in no means discredits previous research, but instead points to the specialty of the pandemic setting.

4 Data

My main dataset consists of monthly data for 2,965 US counties (of 3,144 total) for the 37 months encompassing January 2019 through January 2022. This includes 24 monthly observations following March 2020, the month in which the World Health Organization declared COVID-19 a pandemic according to the Centers for Disease Control and Prevention (2022). This dataset is the combination of four data sources. Monthly, county-level unemployed persons and civilian labor force count data comes from the US Current Population Survey, sponsored jointly by the US Census Bureau and the Bureau of Jobs and Labor Statistics, and was accessed through FRED (Federal Reserve Economic Data). This source contains observations of 2,988 US counties. Data on added value is provided by Smith et al. (2024) under the National Economic Resilience & Research Center (NERRC), which is sponsored jointly by the Economic Development Administration and Argonne National Laboratory. The NERRC defines added value as the sum total of the value added by each industry, where value added represents the total output of an industry minus its inputs. This serves as a measure of county-level economic activity, since increases in value added represent rising industrial output or falling input costs. This dataset contains observations for 3,141 US counties, where observations are monthly, county-level total estimated added value in billions USD. For statewide timing of the eviction moratorium end, I use the Covid-19 Housing Policy Scorecard from The Eviction Lab (2021). I independently verified this data using COVID-era press releases and news reports to ensure their accuracy. Additionally, to allow for the possibility of interaction between lockdowns and eviction moratoria, I use lockdown order dates from Ballotpedia (2020). Figure 2 provides pre-COVID summary statistics and illustrates the relative similarity between always treated, staggered treatment and control counties in the final sample chosen in section 5.3. Full sample summary statistics are provided in appendix figure A.1. I use two other datasets. In section 5.1, I verify treatment effect on evictions using eviction filing counts. Although county-level evictions filings data are scarce, the Legal Services Corporation, a congressionally funded independent nonprofit, reports monthly county and state-level data for 1,250 counties and municipalities in 30 states. Because county-level filing counts often lack data for short periods, I use monthly state-level eviction filing counts. Finally, in section 6.2, I restrict my analysis to counties with the largest cities. I use SimpleMaps (2024) to find the FIPS codes of all counties ranked by population size.

5 Empirical Strategy

5.1 Channels of Evictions

I explore two opposite channels through which evictions could affect individuals. Following eviction, people are forced to find new housing and face additional stressors like choosing immediate

	Mean	SD	Min	Max
Always Treated (601 counties)				
Population	60,384	136,164	917	1,323,807
State homelessness	4,754	2,684	427	7696
Unemployment	.039	.012	.013	.155
Added Value	3.534	11.434	.035	153
Unemployed persons	1,093	2,594	11	32,074
Labor force persons	29,446	70,240	494	715,904
Lockdown Start	14.799	.401	14	15
Lockdown End	15.463	.499	15	16
Treatment Time	13	0	13	13
Staggered Treatment (1,203 counties)				
Population	90,043	242,896	399	4,420,568
State homelessness	5,504	9,042	371	43,283
Unemployment	.038	.016	.009	.221
Added Value	5.316	15.647	.023	304
Unemployed persons	1,571	4,632	7	102,165
Labor force persons	44,982	123,401	215	2,276,458
Lockdown Start	14.258	.437	14	15
Lockdown End	15.992	.71	15	17
Treatment Time	17.181	2.392	15	24
Control (156 counties)				
Population	113,940	208,880	657	1,281,565
State homelessness	4,147	1,563	738	6,413
Unemployment	.042	.019	.011	.171
Added Value	6.971	16.109	.034	148
Unemployed persons	2,086	3,664	11	23,512
Labor force persons	58,656	108,724	253	720,694
Lockdown Start	14	0	14	14
Lockdown End	15.91	.286	15	16
Treatment Time	29	0	29	29

Figure 2: Summary Statistics broken down by treatment type. Observations are for the 13 months spanning January 2019 through January 2020 inclusive. Always Treated refers to the 601 counties that did not have eviction moratoria, Staggered Treatment refers to the 2,112 counties that ended eviction moratorium between May 2020 and June 2021. Control refers to the 252 counties that ended moratorium on or after June 2021.

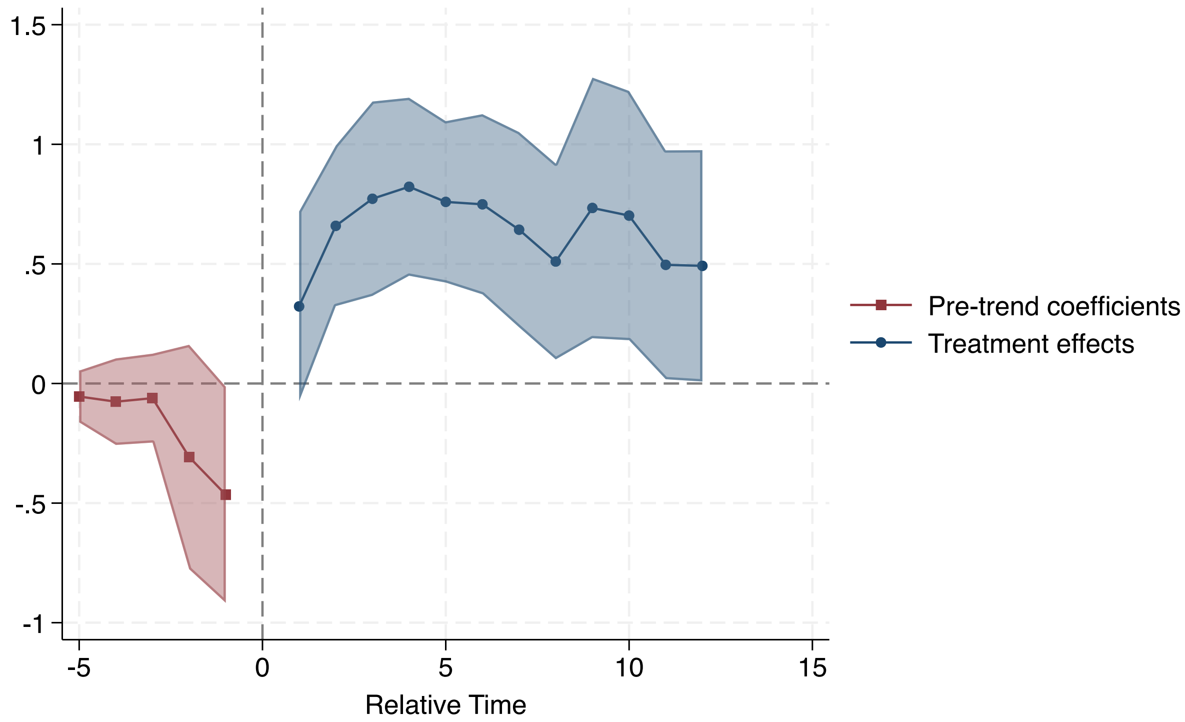

Figure 3: Monthly percent change in eviction filings following treatment. Dynamic estimates are of the form specified in equation 1 but using state-level observations rather than at the county level. Note that this is performed for all states, including the "manipulator states" referred to in section 5.3.

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short-term housing. This can cause job loss and lead to negative long-term outcomes like homelessness or depression, effects that are well-supported by previous literature (Collinson and Reed (2018), Collinson et al. (2024) and Desmond and Gershenson (2016)). I will refer to this as the "stressor" channel, in which evictions make it harder to work and increase the probability that an individual moves from employment to unemployment or exits the labor force entirely. Evidence for the "stressor" channel would include a rise in unemployed persons (due to people moving from employment to unemployment), a fall in labor force participation, and / or a rise in the unemployment rate (recall that Unemployment Rate $\equiv \frac{\text{Unemployed Persons}}{\text{Labor Force Participants}}$).

Alternatively, the long-term threats that evictions impose can be a motivator, especially for unemployed people to return to work. In this setting, it is obvious that eviction moratorium end had an immediate and significant effect on eviction filing counts. Using a two-way fixed effects difference-in-difference estimator and imputation estimator (described in section 5.2) with the state-level log of eviction filings as the outcome, I find that treatment is associated with between a 49% and 53% increase in eviction filings relative to untreated states. This is further illustrated by the dynamic estimates in figure 3. In this setting of high unemployment, it's plausible that many evicted individuals were unemployed but motivated to return to employment following eviction. In addition to actual evictions, the threat of being evicted can be equally motivating. Although this channel is unmeasurable in my setting, I explicitly include individuals who were not evicted but changed their behavior in response to treatment. I will refer to this channel, which includes motivation through both eviction and the threat of eviction, as the "motivator" channel of evictions. Evidence for the "motivator" channel would include some combination of a fall in unemployed persons (due to people moving from unemployment to employment), a rise in labor force participation, and / or a fall in the unemployment rate.

The motivator channel likely dominates the stressor channel in this setting. During the pandemic, states and the federal government instituted a variety of worker protections, including eviction moratoria. This included the Primary Pandemic Unemployment Programs, which provided federal unemployment insurance through July 25, 2020 (Gwyn (2022)). The additional protection of eviction moratoria made it even easier to remain unemployed. It's likely that during this period, there were many workers who were capable of returning to work but chose not to do so because of the relatively high benefit of remaining unemployed. Upon eviction moratorium end, the potential cost of remaining unemployed dramatically increased. Therefore, evictions were likely more of a motivator to the unemployed than a stressor.

5.2 Equation of Estimation

I begin by specifying static (equation 1) and dynamic (equation 2) difference-in-difference equations:

$$Y_{it} = \tau \mathbb{1}\{t > T_s\} + \lambda_t + X_i + \varepsilon_{it} \tag{1}$$

and

$$Y_{it} = \sum_{\tau=-6}^{\tau=12} \beta_\tau \mathbb{1}\{t - T_s = \tau\} + \lambda_t + X_i + \varepsilon_{it} \tag{2}$$

where Y_{it} is the outcome of interest (including the log of unemployment, labor force participants, added value and the unemployment rate) and λ_t and X_i are fixed effects in time and county, respectively. The binary variable $\mathbb{1}(\cdot)$ takes the value one if the unit is post-treatment (in equation 1) or τ periods post-treatment (in equation 2) and takes the value of zero for all periods of control county observations. Note that the outcome is at the county-month level. Equation 2 represents an event study design which estimates monthly average treatment effects (β_τ). Treatment for county c in state s begins when its state ends its eviction moratorium, and this month is labeled T_s. Equation 1 represents an aggregated version of equation 2, wherein τ represents the average treatment effect for county c in state s. This is a case of staggered adoption, in which different counties have different treatment times depending on T_s, the month in which state s determines treatment. Borusyak et al. (2024) demonstrate that difference-in-difference estimation is biased in the presence of heterogeneous treatment effects. This is because standard OLS estimation makes "forbidden comparisons" between already-treated observations across different relative time periods, leading to negative weights of treatment effects. The authors propose an alternate estimator for static treatment effects, which is consistent in the presence of heterogeneous treatment effects and proceeds in three steps. The first step is to estimate individuals and time fixed effects for all never treated or not yet treated units. Note that in this setting, estimates are only allowed for up to 12 months after treatment. Next, these estimates are extrapolated to all treated units to find the treatment effect for individual i at time t. Finally, treatment effects are calculated using a weighted average of treatment effect estimates, where weights are specified based on the research question. In this case, weights are specified by the simple ATT across all observations, or 1/(number of relevant observations). This formula can also be extended to dynamic treatment effects. I will henceforth refer to this estimator as the "imputation estimator" in accordance with the authors' nomenclature. Alongside standard difference-in-difference estimation, I use the imputation estimator to account for the possibility of heterogeneous treatment effects in my staggered adoption setting. Finally, to allow for possible effects of lockdown, I specify an additional equation of the form:

$$Y_{it} = \sum_{j=-6}^{j=12} \alpha_{0j} \mathbb{1}\{t - T_{0s} = j\} + \sum_{k=-6}^{k=12} \alpha_{1k} \mathbb{1}\{t - T_{1s} = k\} + \tau \mathbb{1}\{t > T_{0s}\} \mathbb{1}\{t > T_{1s}\} + \lambda_t + X_i + \varepsilon_{it} \quad (3)$$

Now, T_{0s} refers to eviction moratorium end in state s, and T_{1s} refers to lockdown end in state s. This allows for dynamic effects of lockdown captured by each α_{1k}, as well as potential interactions between evictions and lockdowns captured by τ.

5.3 Identification

The identifying assumption for difference in differences, regardless of specification, is parallel trends. Parallel trends means that the effect of time does not differ between the treated and control groups. As an illustrative example, the parallel trends assumption in a two period pre- and post-treatment model of equation 1 requires that

$$\lambda_{treat,2} - \lambda_{treat,1} = \lambda_{control,2} - \lambda_{control,1} \tag{4}$$

Put another way, in the absence of treatment, both the treatment and control groups *would have* changed in the same way over time. In this setting, parallel trends means that the change in observed outcomes (like unemployed persons or industrial added value) in counties under an eviction moratorium is what would have happened in eviction counties in the absence of treatment. One potential threat to identification is COVID-19 itself and statewide lockdowns. On average, states ended lockdowns prior to ending their eviction moratoria. Taken to the extreme, consider what happens when states only end their eviction moratorium after ending lockdown. According to the true data generating process:

$$Y_{it} = \tau_1 \mathbb{1}\{t > T_{evict}\} + \tau_2 \mathbb{1}\{t > T_{lockdownend}\} + \tau_3 \mathbb{1}\{t > T_{evict}\}\{t > T_{lockdownend}\} + \lambda_t + X_i + \varepsilon_{it} \tag{5}$$

where T_{evict} and $T_{lockdownend}$ represent the month evictions and lockdown end started, respectively. Since treatment counties must have experienced lockdown end, this must mean that at time t,

$$\mathbb{E}[Y_{it} | t > T_{evict}] = \tau_1 + \tau_2 + \tau_3 + \lambda_t + X_i \tag{6}$$

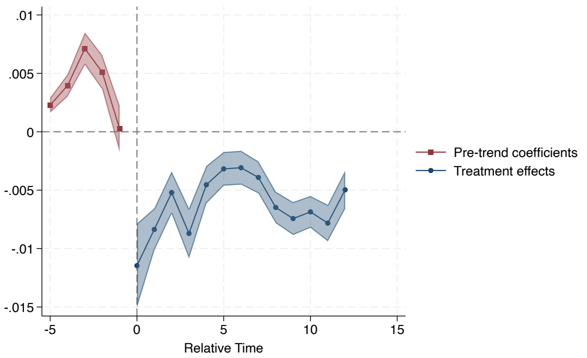
whereas

$$\mathbb{E}[Y_{it} | t \leq T_{evict}] = \lambda_t + X_i \tag{7}$$

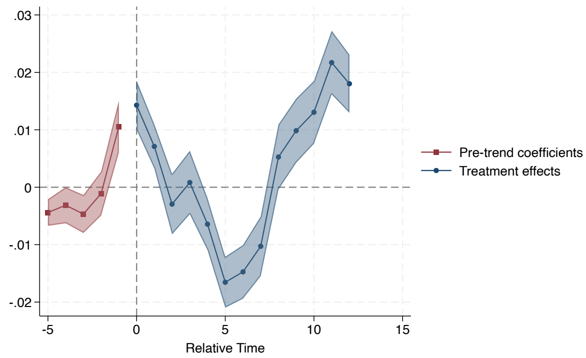
Naïve difference-in-difference estimation of equation 1 for the treatment effect of evictions alone would thus capture $\tau = \tau_1 + (\tau_2 + \tau_3)$, which would be biased by lockdown ending on the order of $(\tau_2 + \tau_3)$. To account for this potential bias, I include an indicator for being under lockdown at time t as a control.

The main threat to identification in this setting is the fact that states choose their own treatment. Although this is not directly chosen by counties, which are our unit of observation, it is still an obvious problem for two reasons. First, county-level economic outcomes are necessarily correlated with

Figure 4: Dynamic estimation of treatment effects using the imputation estimator. This is a full sample estimation of equation 3 with an additional third order population polynomial which includes manipulator states.



(a) Figure A: Percent Change in Unemployment Rate



(b) Figure B: Percent Change in Industrial Added Value

state-level outcomes because the statewide economy is the sum of its counties' economies. Second, statewide politicians theoretically represent county-level interests and factor them into policy, including eviction moratorium timing.

In a perfectly experimental setting, moratorium end would be randomly assigned, and parallel trends would necessarily hold. In this setting, however, moratorium end is obviously not randomly assigned, which is cause for major concern of potential reverse causality. It's possible that states, in response to improving economic conditions, chose to end their eviction moratoria. This is especially concerning given that worsening economic conditions caused by the pandemic were a justification for the introduction of eviction moratoria in different states (it is important to note, however, that start of eviction moratoria is not a problem in this setting because "treatment" is defined by moratorium *end*). If state governments began their moratoria in response to souring economic conditions, it would be perfectly reasonable to assume that governments also ended their moratoria in response to improving economic conditions. In the framework of equations 1 and 2, this would mean that within state s, T_s is endogenously determined by some function of $Y_{s,t}$. In other words, for some $k \in N$:

$$\frac{\partial \mathbb{P}(T_s = t)}{\partial \Delta LaborForce_{s,t-k}}, \frac{\partial \mathbb{P}(T_s = t)}{\partial \Delta AddedValue_{s,t-k}} > 0 \tag{8}$$

or

$$\frac{\partial \mathbb{P}(T_s = t)}{\partial \Delta Unemp_{s,t-k}} < 0 \tag{9}$$

where we should expect that the probability of treatment at time t rises with a previous rise in labor force participants or added value (equation 8) and falls with a previous rise in unemployment (equation 9). The evaluation of parallel trends in an initial estimation of the treatment effect on the unemployment rate and industrial added value, shown in Figure 4, illustrates this concern. Before treatment, unemployment in treatment counties falls relative to control counties and relative added value (an indicator of output) similarly rises.

To try to overcome this bias, I consider how each state determined moratorium end. Of the 43 states that imposed eviction moratoria, 16 were ended by a judge or judges, of which eight were appointed by the state governor or legislature. Levy and Karst (2000) explains that according to the US political tradition of judicial independence, judicial decisions should be insulated from political opinion and made based on "...what is right, not what is popular". Gur-Arie and Wheeler (2001) describe several measures employed in the US designed to protect judicial independence, such as secure tenure and compensation and self-administration of the courts. The extent to which judges are insulated from popular opinion is an active area of research, but I assume that judges do not respond to changes in economic conditions when deciding treatment. The remaining 27 were ended by expiration of a governor's moratorium orders, meaning governors in these states didn't entirely control eviction treatment. However, governors in 13 states changed the month or original moratorium expiry date, a sign of direct interference in treatment timing. In section 7.2, I test for and find evidence for endogeneity of treatment timing within these states, which I will henceforth refer to as "manipulator states". To eliminate this bias, I simply remove these states from my analysis. Doing so restricts my analysis to 37 states and 1,945 counties, as outlined in figure 5. All three groups of states did not directly manipulate their expiry date: seven states never instituted a moratorium, whereas the remaining 30 were determined by an expiry date or a judge's decision. Importantly, keeping only states which never changed their expiry date makes treatment plausibly random. The beginning of the COVID-19 pandemic was a period of notable uncertainty, and governors who issued an eviction moratorium with an expiry date could not reasonably estimate when economic conditions would improve. Of course, it's possible that governors in these states similarly selected *into* treatment only when their economies performed better. However, this concern can be directly evaluated with pre-trends.

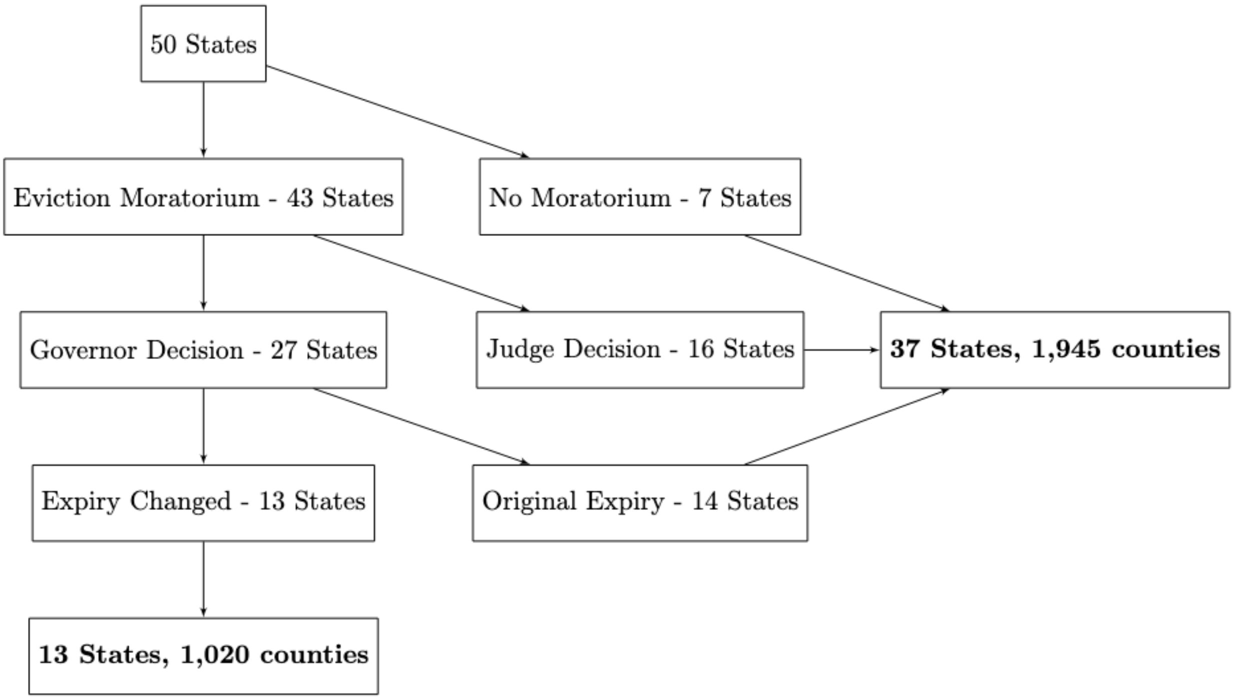


Figure 5: Breakdown of states by treatment decision type. Note that the box on the far right indicates all within-sample states. Manipulator states include the following: California, Florida, Illinois, Indiana, Kansas, Massachussetts, Michigan, Mississippi, North Carolina, Pennsylvania, Rhode Island, Texas, Washington, West Virginia.

6 Results

6.1 Main Results

Figure 6 shows results for difference in difference estimation of equation 3, allowing for static and dynamic effects of both treatment and being under lockdown. Notably, the lockdown treatment estimate shows that counties under lockdown had, on average, around a 1.3% higher unemployment rate and a 2.5% lower industrial added value relative to counties no longer under lockdown. My estimates are in the same direction as previous literature (Coibion et al. (2020), Deb et al. (2022)), with my labor results being particularly close to the 2.4% rise in the unemployment rate found in Coibion et al. (2020). However, it is important to note that my results are longer term than both articles: whereas my results are for the 12 months following treatment, both articles estimate the immediate effect of lockdowns within 30 days. On the other hand, my results indicate that eviction treatment was associated with a fall in the unemployment rate and a rise in added value. These estimates generally indicate that the unemployment rate fell by around 0.6 to 0.8% and added value rose around 0.2 to 0.6% following treatment. Although these results are of a lower magnitude than the lockdown results, it's important to

VARIABLES	(1) No Controls	(2) Dynamic Lockdown	(3) Static Lockdown, Evictions	(4) Dynamic Evictions
Panel A: Unemployment Rate				
Eviction Treatment	-0.0061*** (0.000)	-0.0077*** (0.000)	-0.0056*** (0.000)	
Lockdown x Eviction Treatment		0.0078*** (0.000)	0.0017*** (0.000)	0.0005 (0.001)
Lockdown Treatment			0.0129*** (0.000)	0.0138*** (0.000)
Panel B: Added Value				
Eviction Treatment	0.0056*** (0.001)	0.0053*** (0.001)	0.0016 (0.001)	
Lockdown x Eviction Treatment		0.0037* (0.002)	0.0109*** (0.002)	0.0119*** (0.002)
Lockdown Treatment			-0.0245*** (0.002)	-0.0254*** (0.002)
Observations	71,965	71,965	71,965	71,965
Number of Counties	1,945	1,945	1,945	1,945

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 6: Simple Difference in Difference Regression of Eviction and Lockdown Treatment on selected outcomes. Static Treatments refer to a binary dummy for treatment, whereas dynamic Treatments include a binary dummy for each month of treatment. Lockdown x Eviction Treatment refers to an interaction of the two binary dummies. The number of observations and counties is the same for both panels A and B.

VARIABLES	(1) State and Time Fixed Effects	(2) Population Controls	(3) Col. (2) with Full Controls
Unemployment Rate	-0.0078*** (0.001)	-0.0080*** (0.001)	-0.0092*** (0.001)
Unemployed Persons	-0.1007*** (0.019)	-0.1046*** (0.019)	-0.1282*** (0.020)
Labor Force Participants	-0.0044** (0.002)	-0.0050** (0.002)	-0.0050** (0.002)
Industrial Added Value	0.0011 (0.003)	-0.0009 (0.003)	0.0017 (0.003)
Observations	56,405	56,405	54,601

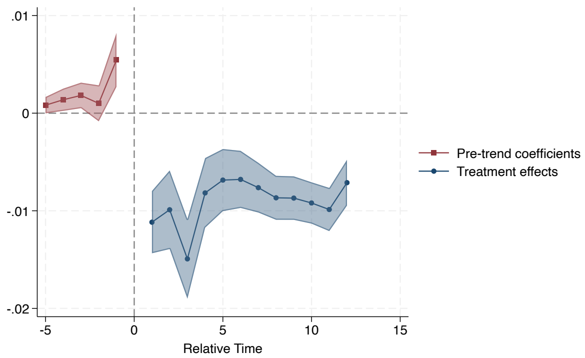
Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 7: Imputation estimates of static treatment effects with various controls. Columns 1 and 2 are estimates of equation 2, with column 2 including an additional 3rd order population polynomial. Column 3 uses the same controls as column 2 with additional estimates of dynamic Lockdown and Lockdown x Treatment interactions.

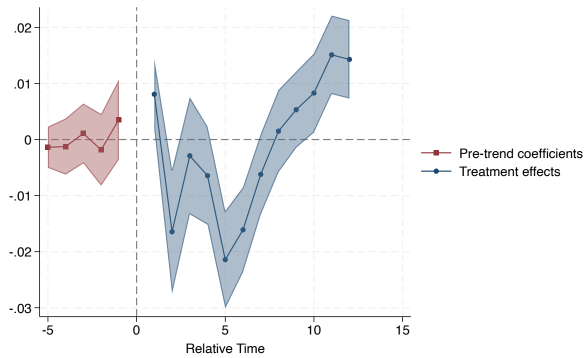
remember that lockdowns had massive structural effects on county economies, particularly in forcing the short-term closure of many businesses deemed "non-essential". The fact that my results indicate eviction treatment had around half the effect on unemployment and one fifth the effect on added value speaks to the relative impact of this policy. Including an interaction term between lockdown and treatment in figure 6 indicates that counties simultaneously under lockdown and allowing evictions experienced both a relatively higher unemployment rate and added value. This makes intuitive sense for the unemployment rate: treatment counties not under lockdown likely had more job openings than treatment counties under lockdown. Even if evictions are an extremely strong motivator to return to work, there's only so much a worker can do to find a job in a county under lockdown. The results for added value, on the other hand, move in the opposite direction as expected and should be investigated in future research.

As discussed in section 5, the simple-difference-in-difference estimation results in figure 6 will be biased in the presence of heterogeneous treatment effects because of the staggered rollout of moratorium end. To test for heterogeneous treatment effects and correct for any biases, I use the imputation estimator described in section 5.2. I estimate the eviction treatment effect on four different outcomes: the unemployment rate, and the percentage changes in labor force participation, industry added value

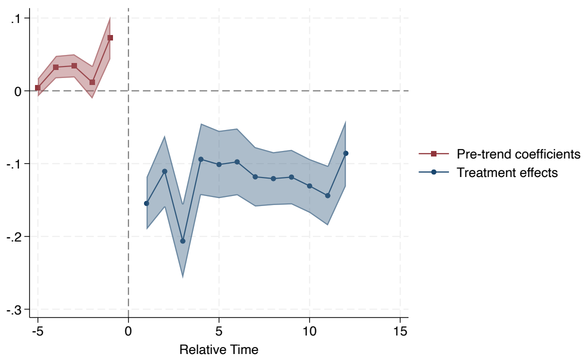
Figure 8: Dynamic estimation of treatment effects using the imputation estimator. Observations are restricted to the 37 non-manipulator states highlighted in figure 5. This is an estimation of equation 3 with an additional 3rd order population polynomial.



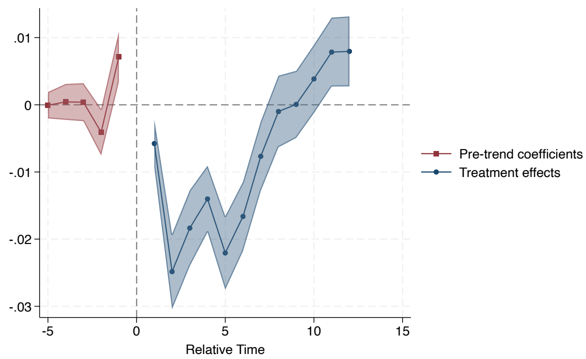
(a) Panel A: Percent Change in Unemployment Rate



(b) Panel B: Percent Change in Industrial Added Value



(c) Panel C: Percent Change in Unemployed Persons



(d) Panel D: Percent Change in Labor Force Participants and unemployed persons. I perform both static and dynamic estimates of the treatment effect, with static estimates presented in figure 7 and event study graphs of dynamic treatment effects presented in figure 8. Notably, in all four graphs, pre-trends largely indicate that treatment and control counties were moving in parallel prior to treatment, evidencing the parallel trends assumption. My results of the static treatment effect are robust in the presence of population controls and additional controls like lockdowns. These results largely agree with my two-way fixed effect difference-in-difference estimation of eviction treatment on the unemployment rate but find a relatively stronger effect around a 0.8% to a 0.9% drop in the unemployment rate. On the other hand, the imputation estimator finds a near-zero change in industrial added value for treatment counties. These results agree with the static estimates in figure 6 but generally find a much weaker change in added value.

To further investigate the cause of a fall in the unemployment rate, I include percent changes in the number of unemployed persons and labor force participants as outcomes. My results indicate that relative to control counties, treatment counties saw a fall in both unemployed persons and labor force participants post-treatment. My full-control results in column 3 indicate that this was around a 13% reduction in unemployed persons and a 5% reduction in labor force participants. Using the mean values for staggered treatment counties reported in figure 2, this would be roughly equivalent to a 226 person drop in unemployed persons and a 2,410 drop in labor force participants in the average county. Since Unemployment Rate $\equiv \frac{\text{Unemployed Persons}}{\text{Labor Force Participants}}$, the observed fall in the unemployment rate could be driven by a fall in unemployed persons, or a rise in labor force participants, or a combination of the two. If the number of labor force participants *fell* during this period, the observed fall in the unemployment rate must have been driven by the fall in unemployed persons. This is clearly illustrated by panels 8a and 8c: the percent change in the unemployment rate looks strikingly similar to a scaled version of the percent change in unemployed persons for the entire post-treatment period. More generally, my dynamic treatment estimation results in figure 8 indicate homogeneous treatment effects over time for unemployed persons and the unemployment rate. On the other hand, there appear to be heterogeneous treatment effects over time for industrial added value and labor force participants. Both graphs appear to indicate the same trend: a short-term reduction in the outcome for around 8-9 months post-treatment and a long-term increase in the outcome.

Generally, my results appear to support my hypothesis that the motivator channel dominates the stressor channel in section 5.1. There is clearly some evidence for the stressor channel, especially in the short term. Panel 8d shows a substantial reduction in labor force participants in the first six months following the end of the moratorium: especially in the short term, workers in treatment counties appear to be exiting the labor force, on average. The actual cause for exit is unobservable, but this is likely due in part to the stressors associated with eviction: it becomes significantly harder to work when one is homeless, in poor mental or general health, or living in a high-crime neighborhood. This reduction in labor force participants may also be driving the similar reduction in added value apparent in the first six months of panel 8b. However, my results largely point to evictions as a motivator for people to return to work. Panels 8a and 8c demonstrate clear evidence that unemployment and the unemployment rate fell immediately after moratorium end and remained depressed for the entirety of the observation period. These graphs both provide evidence for a short-run increase in employment. Furthermore, panel 8d demonstrates that within a year, the number of labor-force participants actually *increased* following moratorium end. Taken together with a long run reduction in unemployed persons in eviction states, average employment relative to control states necessarily rose within a year of moratorium end. Both pieces of evidence indicate that the motivator channel of evictions likely dominated the stressor channel in the high unemployment environment of COVID-19, leading on average to a reduction in unemployment and an increase in employment.

6.2 Heterogeneity Results

My results in figure 9 explore the underlying mechanism for the treatment effect of evictions in different counties. Evictions, or the threat of evictions, can only ever affect a certain subset of a county's population. This subset includes many renters that are either unemployed, not in the labor force, or employed but cannot or will not pay for rent. This population necessarily grows with the number of renters in a county, and likely also rises with the dollar amount of benefits, like unemployment insurance or rental assistance, that a county offers. Because this setting was generally a period of high benefits induced by COVID-19 lockdowns (including eviction moratoria), and because I lack county-level benefits data, I restrict my analysis to the population of renters. Ceteris parabus, counties with larger populations tend to have relatively larger renter proportions than smaller counties because they are more likely to contain cities with large proportions of young and economically flexible people. Figure 9 reports heterogeneity results restricted to population subgroups as well as the top 500 and 250 counties ranked by city population size. The results for the unemployment rate indicate that the

VARIABLES	(1) Population below mean	(2) Population above mean	(3) Bottom population decile	(4) Top population decile	(5) Top 500 cities by population	(6) Top 250 cities by population
Unemployment Rate	-0.0018	-0.0104***	-0.0045	-0.0092***	-0.0084***	-0.0095**
	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)	(0.004)
Industrial Added Value	-0.0116***	0.0069**	0.0299	0.0052	0.0055	0.0072
	(0.005)	(0.003)	(0.033)	(0.004)	(0.005)	(0.005)
Observations	27,778	27,558	2,726	5,479	4,577	2,676

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 9: Heterogeneity results for percent changes in Unemployment Rate and Industrial Added Value associated with treatment. After excluding "manipulator states", county population mean, bottom decile and top decile are as follows: 23812, 2500 and 176594.

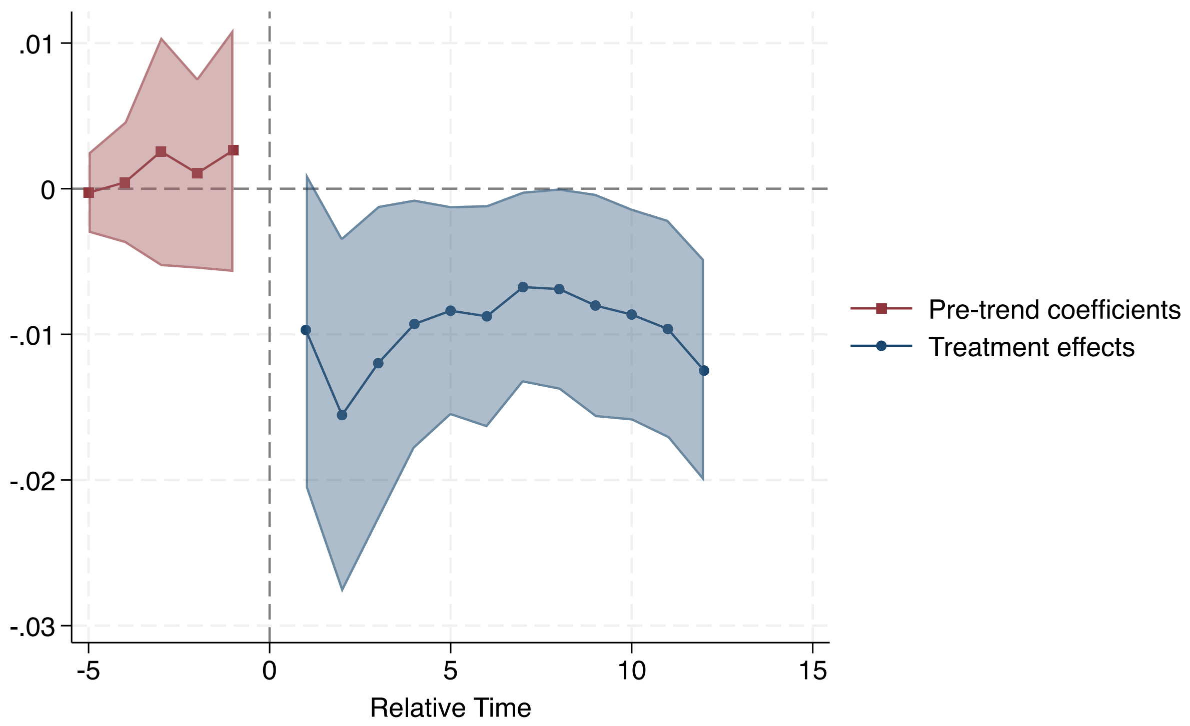


Figure 10: Percent change in the unemployment rate associated with treatment for the top 500 counties by city population size. Note that counties in "manipulator states", including California, are dropped. This means that counties like Los Angeles county, Cook county and Miami-Dade county are exlcuded. More information on "manipulator states" is provided in section 7.2.

treatment effect is clearly flowing through evictions: the treatment effect is stronger in high population counties relative to low population counties. Strikingly, estimation restricted to only counties with the largest cities nearly matches my full-sample results in figure 7. The significance of these results is demonstrated in figure 10: even with relatively few observations, I still find a statistically significant reduction in the unemployment rate in the 500 counties with the largest cities. My added value results similarly illustrate that industrial added value rose the most in counties with the most evictions. Interestingly, added value appears to *fall* in treatment counties below the mean. These results generally indicate that industrial added value rose by around 0.5% to 0.7% in treatment counties, estimates that largely agree with columns 1 and 2 of my estimates in figure 6. However, these estimates largely fit the trend of statistically insignificant estimates of added value found in figure 7.

7 Robustness

7.1 Synthetic Difference in Differences

To assess the bias caused by a potential violation of parallel trends in difference-in-difference estimation, I estimate eviction treatment effects using the synthetic difference-in-difference estimator created by Arkhangelsky et al. (2021). The synthetic difference-in-difference estimator is a standard difference-in-difference estimator with unit and time weights. Unit weights emphasize similarity of pre-trends between control and treatment observations, and are constructed such that the average treatment outcome is parallel to the average control outcome. According to the authors, time weights "are designed so that the average posttreatment outcome for each of the control units differs by a constant from the weighted average of the pretreatment outcomes for the same control units". Because synthetic difference-in-differences constructs parallel trends prior to treatment time, any bias from a violation of parallel trends in my standard and imputation DiD results will not plague my synthetic estimation. The most important caveat with use of the synthetic difference-in-differences estimator is that, like standard DiD, this estimator does not account for heterogeneous treatment effects. It's important to note, however, that my unemployment estimates do not seem to be plagued by heterogeneous treatment effects: both figures 6 and 7 largely agree on the treatment effect on the unemployment rate. Figure 11 demonstrates results of synthetic difference-in-difference regression of treatment time on the outcomes of interest. Note that the unemployment results are strikingly similar

VARIABLES	(1) No Controls	(2) Full Control
Unemployment Rate	-0.0090*** (0.001)	-0.0070*** (0.001)
Persons in Labor Force	0.0058*** (0.002)	0.0055*** (0.002)
Unemployed Persons	-0.1499*** (0.012)	-0.1246*** (0.012)
Industrial Added Value	0.0079*** (0.002)	0.0058*** (0.002)
Observations	109,705	109,705

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

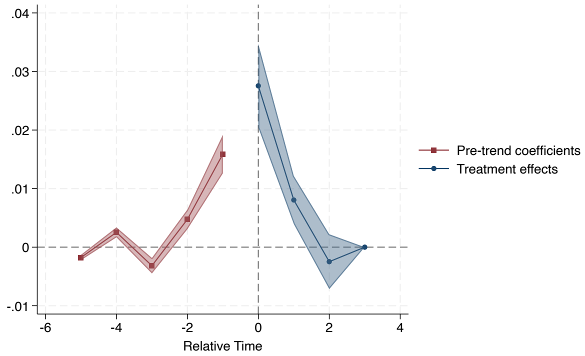
Figure 11: Synthetic Difference in difference estimation of static treatment effects using bootstrapping. Note that column 1 is an estimate of equation 2, whereas column 2 includes a 3rd order population polynomial and dynamic lockdown controls specified in equation 3.

to those of figure 7: both find nearly the same results for the unemployment rate, whereas imputation reports a slightly smaller drop in unemployed persons relative to synthetic difference-in-differences. On the other hand, the estimators clearly disagree on labor force participants and added value. This is most evident for labor force participants, where synthetic difference-in-differences reports a relative *rise* in treatment counties while imputation reports a *fall* of the same magnitude. These results, however, are further evidence in favor of the motivator channel: if the number of labor force participants *rose* in treatment states, the fall in unemployed persons could not have been caused by workers exiting the labor force. The added value estimates in figure 11 are also larger than both the imputation and heterogeneity estimates and are statistically significant, providing evidence for a positive output effect of evictions through a rise in employment.

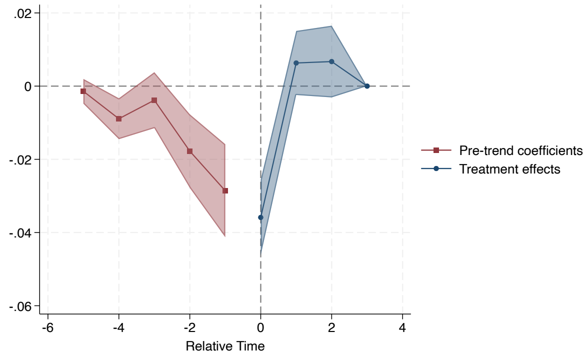
7.2 Effect of First Expiry

As described in section 5.3, 13 of the 43 eviction moratorium states changed the original expiry date of the moratorium. Since moratorium end is defined as treatment in this setting, this means that these states directly manipulated treatment - which is why they are referred to as "manipulator states". To check for potential endogeneity of treatment time described in section 5.3, I estimate dynamic treatment effects for counties within the manipulator states using the original expiry date as the new

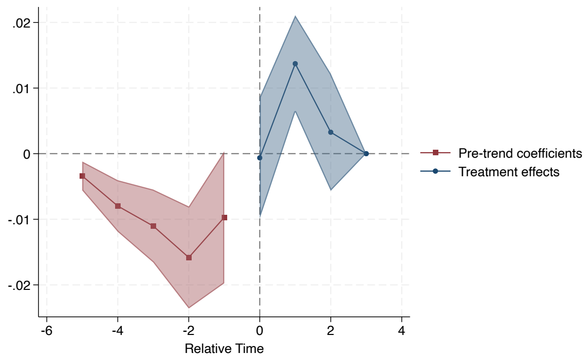
Figure 12: Imputation estimates of dynamic treatment effects using original moratorium expiry date as treatment. Estimates use the same full control specification as in figures 8 and 4 but are restricted to the 13 manipulator states.



(a) Panel A: Percent Change in Unemployment Rate



(b) Panel B: Percent Change in Industrial Added Value



(c) Panel C: Percent Change in Labor Force Participants

treatment time. The results in figure 12 confirm this suspicion: states only changed expiry date when economic trends were worsening. Relative to control counties, counties in states that changed expiry date had rising unemployment, falling labor force participants, and falling added value. In other words, these states were likely choosing treatment in response to economic trends, introducing the bias observable in full-sample regressions.

It is important to note, however, that my results are relatively similar when including the manipulator states. Appendix figure A.2 demonstrates imputation estimates of treatment using the full sample of states. These results are similar to my results in figure 7, but report a near-zero change in labor force participants. As with my synthetic difference in difference results, these results are further evidence that the fall in unemployment associated with eviction treatment was not being driven by workers exiting the labor force.

8 Conclusion

My results point to the importance of eviction moratorium policy in determining the economic outcomes of US states during the COVID-19 pandemic. Most significantly, I find robust results for a 0.8% fall in the unemployment rate associated with evictions treatment, which is around half of the change in unemployment associated with lifting lockdowns (about a 1.4% reduction). My results also indicate that this reduction in unemployment is driven by people returning to work rather than leaving the labor force. On the other hand, I find varying results for an increase in industrial added value associated with evictions. There is some evidence for a slight increase in added value, anywhere from 0.1% to 0.8%, but this evidence is inconclusive due to their general lack of significance. This lack of significance may be driven in part by heterogeneous effects over time, with added value appearing to decrease in the five months immediately after treatment and increasing in the subsequent months.

Because these are not general equilibrium estimates, I am unable to account for worker or firm responses through migration, interstate trade, or labor demand. However, if unemployed individuals respond to eviction treatment by moving states from treatment to control, this will only attenuate my results. Furthermore, if the effect of treatment on output is on the order of <1%, it's unlikely that state-level externalities introduce biases of any meaningful significance. Nonetheless, general equilibrium estimates of the effects of eviction on labor and output could verify my estimates and significantly improve the precision of my estimates of added value. An interesting area of future study could be exploring labor outcomes for those affected by the end of eviction moratoria. Because my results suggest unemployment fell immediately after treatment, affected renters likely rushed to find jobs and were not well matched based on their skills. Further research could investigate potential negative labor outcomes from treatment, such as lower wages or workers disproportionately entering low-skill or otherwise poorly matched jobs.

These results have valuable policy implications for states and countries with eviction protections, as well as in times of economic crisis. In a setting with a high social safety net and low eviction rates, evictions appear to be effective in restarting the economy by significantly lowering the unemployment rate and potentially increasing long-term economic output. Additionally, unemployed workers responded immediately to eviction by returning to work. These results indicate that eviction moratoria do not lead to permanent excess unemployment even after they end. Future research could expand upon this idea by investigating how many unemployed renters find a job after moratorium end and how long it takes them to find one. In areas with long eviction processes spanning months or even years, it may take time for evictions to become salient.

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A Appendix

	Mean	SD	Min	Max
Always Treated (601 counties)				
Population	60,384	136,164	917	1,323,807
State homelessness	4,754	2,684	427	7696
Unemployment	.039	.012	.013	.155
Added Value	3.534	11.434	.035	153
Unemployed persons	1,093	2,594	11	32,074
Labor force persons	29,446	70,240	494	715,904
Lockdown Start	14.799	.401	14	15
Lockdown End	15.463	.499	15	16
Treatment Time	13	0	13	13
Staggered Treatment (2,112 counties)				
Population	97,538	282,967	64	5,275,541
State homelessness	7,454	9,030	371	43,283
Unemployment	.04	.017	.007	.221
Added Value	5.787	20.256	.023	409
Unemployed persons	1,739	5,326	4	130,353
Labor force persons	48,191	142,504	199	2,695,116
Lockdown Start	14.385	.487	14	15
Lockdown End	15.895	.71	15	17
Treatment Time	17.181	2.392	15	24
Control (252 counties)				
Population	254,540	767,578	657	10,014,009
State homelessness	35,785	54,241	738	135,771
Unemployment	.047	.023	.011	.249
Added Value	15.232	50.027	.034	647
Unemployed persons	5,099	16,610	11	258,380
Labor force persons	126,824	391,447	253	5,296,665
Lockdown Start	14	0	14	14
Lockdown End	17.909	3.323	15	24
Treatment Time	29	0	29	29

Figure A.1: Summary Statistics broken down by treatment type. Observations are for the 13 months spanning January 2019 through January 2020 inclusive. Always Treated refers to the 601 counties that did not have eviction moratoria, Staggered Treatment refers to the 2,112 counties that ended eviction moratorium between May 2020 and June 2021. Control refers to the 252 counties that ended moratorium on or after June 2021. Notice that control counties are on average much larger than staggered and always treated in terms of population, added value and labor force participants. The large differences between control and treated counties likely contribute to the lack of evidence for parallel trends in the full dataset.

Controls:	(1) No Controls	(2) Month and Population	(3) State Fixed Effects	(4) Col. (3) with Full Controls
Unemployment Rate	-0.0067*** (0.001)	-0.0068*** (0.001)	-0.0067*** (0.001)	-0.0064*** (0.001)
Labor Force Participation	0.0016 (0.001)	0.0011 (0.001)	0.0016 (0.001)	0.0003 (0.001)
Industry Added Value	0.0054*** (0.002)	0.0022 (0.003)	0.0054*** (0.002)	0.0039** (0.002)
Unemployed Persons	-0.0707*** (0.011)	-0.0739*** (0.011)	-0.0707*** (0.011)	-0.0778*** (0.012)
Observations	85,985	85,985	85,985	83,989

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure A.2: Static imputation estimates of treatment effects using the full sample of counties (including those in manipulator states). Note that columns 1 and 3 are the same because county fixed effects are equivalent to state fixed effects.