Title
Essays in Urban and Public Economics

Author
Sayantani, S.

Publication Date
2023

Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA,
IRVINE

Essays in Urban and Public Economics

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

S. Sayantani

Dissertation Committee:
Professor Jan Brueckner, Chair
Associate Professor Matthew Freedman
Assistant Professor Jack Liebersohn

2023

TABLE OF CONTENTS

List of Figures

Page

List of Tables

ACKNOWLEDGMENTS

I would like to thank my advisor Jan Brueckner for his immense support, guidance and advise. Working with him has taught me lot more than just economics. His dedication and work ethic has inculcated in me a life-long desire to match up to the standards that he has set for me.

I would like to thank my committee members and peers for their suggestions and the conference attendees at the Southern Economic Association Coneference, the Western Economics International Conference and the Eastern Regional Science Conference for their comments on improving aspects of my research.

I would like to also thank my friends and family for their continued support and faith in me.

VITA

S. Sayantani

EDUCATION

Doctor of Philosophy in Economics **2023**
University of California, Irvine *Irvine, California*

Master of Arts in Economics **2019**
University of California, Irvine *Irvine, California*

Bachelor of Arts in Economics **2016**
Jadavpur University *India*

RESEARCH INTERESTS

Urban Economics, Public Economics, Applied Microeconomics, Causal Inference

TEACHING EXPERIENCE

Teaching Assistant **2018–2023**
University of California, Irvine *Irvine, California*

REFEREED JOURNAL PUBLICATIONS

Intercity Impacts of Work-from-Home with Both Remote and Non-Remote Workers **2022**
with Jan K. Brueckner

Journal of Housing Economics: COVID-19 and Housing Market Special Issue

CONFERENCES & PRESENTATIONS

Housing Sales and Construction Responses to COVID-19: Evidence from Shelter-in-Place and Eviction Moratoria in the U.S.
Department of Economics Seminar, CSU Long Beach *Oct 2022*

The Intercity Impacts of Work-from-Home in a Spatial Hedonic Model With Remote and Non-Remote Workers
92nd Southern Economic Association Annual Meeting, Fort Lauderdale *Nov 2022*
Eastern Regional Science Association (ERSA) 2022 Congress, Pecs & online *Aug 2022*
97th Western Economic Association International (WEAI) Conference, Portland *Jul 2022*

ABSTRACT OF THE DISSERTATION

Essays in Urban and Public Economics

By

S. Sayantani

Doctor of Philosophy in Economics

University of California, Irvine, 2023

Professor Jan Brueckner, Chair

My dissertation contains three chapters where I employ spatial hedonic modelling, reduced-form estimations and causal inference techniques to analyze urban demographics, migration and the housing market in the U.S. The first chapter employs generalized difference-in-difference to estimate the impact of COVID-19 regulations on the year-on-year of changes in housing sales and building permits approval. This chapter attempts to investigate if and how these interventions causally impacted the county-level housing sales and building permits approval in the U.S. The second chapter formulates a spatial hedonic equilibrium model to show inter-city impacts of the introduction of work-from-home, with multiple worker types. The third chapter explores how community characteristics at a city level correlate to fatal police encounters over the period 2000-2010.

The first chapter explores the effect of public policies like shelter-in-place and eviction moratoria on relative housing sales and building permits approved for construction. Shelter-in-place orders could potentially limit the ability of home-buyers and sellers to interact as well as they could pre-COVID, introducing frictions in the process of selling houses. Prolonged and over-lapping eviction moratoria could dampen the construction of multi-family units. This paper attempts to investigate if and how these interventions causally impacted the county-level housing sales and building permits approval in the U.S. The paper estimates the average treatment effect of these orders using a traditional generalized difference-in-difference estimator and a more recent variation of the estimator that is more suited multiple treatment groups with staggered treatment introductions and withdrawals. The results show that shelter-in-place is associated with significantly smaller

year-on-year changes in sales of single-family houses, condominiums and the collection of all residences. Selective moratoria on eviction hearings and judgements are also found to be associated with smaller year-on-year changes in multi-family building permit approvals. Apart from the baseline model, the chapter also employs an additional estimator to add robustness to the results. SIP is found to be associated with significantly smaller relative changes in sales of single-family houses, condominiums and the collection of all residences. Selective moratoria on eviction hearings and judgements are also found to be associated with smaller relative changes in multi-family building permit approvals.

The second chapter formulates a spatial hedonic equilibrium model that shows inter-city impacts of the introduction of work-from-home. Following Brueckner, Kahn and Lin (2021), the paper attempts to theoretically study how the introduction of work-from-home, which allows workers to relocate across cities while keeping their original jobs, impacts housing prices, population and employment levels. Extending Brueckner et al. (2021), the current paper divides the workforce into two types of workers, remote and non-remote, to allow for a more realistic work-from-home model. Additionally, the model uses explicit functional forms for production and utility, resulting in closed-form equilibrium solutions conditional on the extent of productivity advantages, amenity advantages, and degree of complementarity between worker types. The current paper aims to examine whether the main results in Brueckner et al. (2021) still hold in the modified model.

The third chapter aims to answer if and how community characteristics at a city level correlate to fatal police encounters over the period 2000-2010. Panel and cumulative datasets on police-involved deaths from FatalEncounters.org, along with a relevant set of socio-demographic controls, are used in a negative binomial regression model to find factors that might affect the count of officer-involved deaths that occurred in a city. For the longitudinal panel data, the Black population and the Hispanic population in a city along with police employment, the level of crime and median income constitute significant determinants of officer-involved deaths. With panel and cumulative datasets used in negative binomial regression models, it is found that, for the longitudinal panel data, the Black population and the Hispanic population in a city along with police employment, the level of crime and median income constitute significant determinants of officer-involved deaths.

Chapter 1

Housing Sales and Construction Responses to COVID-19 : Evidence from Shelter-in-Place and Eviction Moratoria in the U.S.

1.1 Introduction

The housing market in the U.S., through residential investment and consumption spending on housing, is one of the major drivers of the U.S. economy, averaging approximately 15-18% of the GDP.[1] With the onset of COVID-19, the increase in cases and the consequent responses to the spread of the disease caused substantial changes in the entire U.S. economy, including its housing market, with some of these changes prevailing long after the initial spike in cases. As

[1] Residential investment includes construction of new single-family and multifamily structures, residential remodeling, production of manufactured homes. Consumption spending on housing services includes rents and utilities (actual, for renters and imputed for home-owners). Statistics from the National Association of Home Builders.

the pandemic continued, the associated reductions in income and employment, and the lowering of mortgage rates, had spillover effects on housing demand[2] and rental markets (as discussed by Batalha et al. (2022).[3] Exploring more direct effects of COVID, some studies attribute shifts in consumer preferences and the introduction of work-from-home (discussed by Brueckner et al. (2021) and Brueckner and Sayantani (2022))[4] as key drivers of changing trends in housing prices, rents, valuations and urban flight.[5]

In addition, many studies are now attempting to isolate the effects of the non-pharmaceutical interventions (NPIs) related to COVID[6] from the overall pandemic-induced changes and to investigate how the housing market responded to the NPIs. These interventions, directed towards reducing the spread of the disease and mitigating the hardships it caused, ranged from closure of non-essential businesses and schools to prohibiting some or all parts of the rental eviction process. This paper focuses on two particular NPIs, shelter-in-place and eviction moratoria, and attempts to find their causal impacts on changes in housing sales and building permit approval in the U.S. With substantial variation in adoption both over time and across counties, the effects of shelter-in-place (SIP) and eviction moratoria can be reliably studied.

SIP orders, enacted at different points of time across different states, potentially caused frictions in the home-buying and selling process by severing the contact between buyers and sellers. These frictions, coupled with short-term disruptions in mobility (discussed by Elenev et al. (2022)) could have lead to a lower volume of housing sales. Hence, the first section of this paper attempts to estimate the impact of the enactment of SIP orders on the year-on-year changes in housing sales,

[2]Zhao (2020) finds an increase in housing demand since March 2020 in response to lowered mortgage rates.

[3]Analysing the impact of the negative shock on short-term rentals caused by COVID-19 in Lisbon, Batalha et al. (2022) compile findings suggesting that landlords relocated properties into the long-term rental markets.

[4]Brueckner et al. (2021) studies impacts of work-from-home in the housing prices and rent from both intercity and intracity perspectives in the presence of variation in productivities within and between cities. Brueckner and Sayantani (2022) study an extension, using a similar model that differentiates between remote and non-remote workers.

[5]Coven et al. (2022) find that urban residents fled to socially connected areas, consistent with the theory that individuals sheltered with friends and family, or in second homes. Populations that fled were disproportionately younger, whiter, and wealthier.

[6]These policy interventions do not include vaccinations or any other medical interventions to reduce the spread of the disease.

controlling for other COVID-driven factors, with the hypothesis being that SIP reduces change in housing sales.

The second section of the paper looks at moratoria on evictions, enacted federally or by individual states, aimed at reducing the hardship from COVID related income reductions and reducing the proliferation of COVID cases amongst evicted tenants. Eviction moratoria across the different states varied in terms of strength and time of adoption and lasted longer than SIP orders. Prolonged moratoria on eviction and the inability of landlords to evict tenants could disincentivize builders from constructing units for rent. Thus, a second focus of this paper is to analyze the response of year-on-year changes in approvals of building permits for multi-family units[7] to the adoption of eviction moratoria. The hypothesis here is that the presence of moratoria reduces the change in permits approved for building rental units.[8] Additionally, faced with the uncertainty regarding the continuation of the eviction moratoria, the landlords may decide that owning is infeasible and elect to sell for-rent apartments.[9] A third part of the paper hence attempts to quantify how the eviction moratoria changed year-on-year sales of multi-family buildings, with the hypothesis being that the moratoria would lead to bigger changes in multi-family sales.

For the housing sales and SIP section of the analysis, accounting for the heterogeneity in sales responses across housing types, the paper considers county-level, monthly sales of single-family houses, multi-family buildings, condos, townhouses and all-residences (the collection of all the preceding types) as outcome variables. The data spans 13 months and approximately 1300 counties. The primary treatment variable, SIP orders, were adopted around March 2020 and while these orders were adopted in a staggered fashion, they were not rescinded at the same time across counties. The data also contain a relatively low count of control counties, in which SIP was never enacted. The housing sales data are collected from a realty brokerage firm, Redfin, and the SIP data are collected from Keystone Strategy and Husch-Blackwell, firm-level resource centers on COVID

[7] According to a recent study, multifamily buildings make up about 61% of the country's available rental stock. The other 39% are single-family units, including detached houses, duplexes, townhouses, mobile homes, RVs, and other types of housing. Data are from www.jchs.harvard.edu/americas-rental-housing

[8] Multi-family refers to five-plus unit buildings. See the data section for more details.

[9] According to the National Rental Home Council (NRHC), 23% of single-family rental owners have already sold at least one property since the start of the COVID pandemic.

NPIs.

In the section of the analysis focusing on building permit and eviction moratoria, the paper uses only a singular outcome variable of interest: year-on-year changes in number of units approved for 5+ unit buildings.[10] Multiple treatment variables or moratoria indicators are used, which vary by blocking different stages of eviction process (hearings, judgements, executions) and by considering different causes for being evicted (hardship and/or non-payment). Notably, moratoria orders varied largely both in terms of length and continuity. These orders followed a staggered roll-out design, but did not end uniformly across states. In many cases, various degrees of the moratoria were instated, revoked and re-instated again through 2020 and 2021. This set-up makes for an interesting and challenging econometric analysis where treatments switch off and on multiple times. The building permits data are collected from the US Census Bureau Building Permits Survey and the eviction moratorium data is collected from Benfer et al. (2022).[11]

For both the NPIs considered, the paper uses three sequential specifications to identify how these treatments impact the outcomes of interest. The first and baseline specification involves employing a generalized difference-in-difference or two-way fixed effects (TWFE) estimation, controlling for county and month fixed effects. The addition of month-fixed effects captures shifts within the year that are associated with the progress of the pandemic, which occur in addition to the year-on-year changes in outcomes due to the NPIs. In order to account for the NPIs having potential anticipatory and lagged effects on sales and building permit approvals, the paper presents event-study graphs for selective housing types and eviction moratorium indicators. The graphs show approximately eight lag and eight lead indicators, relative to the first time the NPIs begin. As validity checks to these event-study graphs, the paper also presents (in the appendix) event-study graphs for the event of the NPIs ending.

Recently, many economists have discussed the challenges in interpreting the estimated co-

[10] The paper also presents valuation results for 5+ unit buildings and analyzes single, double and three-four unit buildings as well. However, assuming that most buildings up for rent are multi-family units, these type of buildings are not the primary focus of the paper.

[11] Researchers Emily Benfer and Robert Koehler employed longitudinal policy surveillance to comprehensively describe state involvement in the eviction moratorium resulting from the emergence of the COVID-19 pandemic and continuing through the end of substantive state intervention.

efficients from TWFE models when treatment effects are heterogeneous over time and/or across groups.[12] With the treatments in the current paper exhibiting considerable heterogeneity in their starting, progression and ending points, the paper attempts to add a third set of results to the baseline TWFE regressions by applying an estimator more suited to multiple treatment groups. This estimator is formulated by de Chaisemartin & D'Haultfoeuille (2020a) and is denoted by DID_M.[13] Two time-varying county-level covariates are controlled for: number of COVID cases and year-on-year employment change. However, owing to NPI-caused employment or COVID changes potentially confounding the treatment effects on the housing market, the paper presents results both with and without adding these controls, for all three specifications (baseline TWFE, event-studies and DID_M TWFE).

The baseline TWFE results from regressing housing sales on SIP show that enacting the order resulted in smaller year-on-year changes in sales for all types of houses, (although to a lesser degree for multi-family buildings), favoring the paper's hypothesis. From the event-study graphs, it appears that for the group of all residences, an anticipatory effect of the order starts depressing the year-on-year sales changes about three periods before its enactment. When no controls are added, the lags show continuation of smaller changes in sales after SIP is enacted, implying a lack of sales recovery.[14]

Using the DID_M estimator, significant results for change in sales are recorded only for single-family, condos and all-residences. Without controls, SIP is found to reduce the year-on-year change in county sales of single-family, condos and townhouses by 24, 15 and 29 units respectively. The magnitudes of these changes are around 5 times higher than the mean absolute sales changes of condos and single-family houses, and around half of the mean sales change of all-residences. Compared to pre-pandemic year-on-year changes, it appears that SIP reduced single-family, condominium, and all-residential year-on-year changes in housing sales by approximately 26%, 41% and 24%

[12] Comparing newly treated units relative to already treated units produces less robust average treatment effects owing to the presence of negative weights in the weighted sum of treatment effects in each treatment group.

[13] In terms of ensuring causality, DID_M relies on a variant of the standard common trends assumption that is tested by a new 'placebo effect' proposed by de Chaisemartin & D'Haultfoeuille (2020a).

[14] With the addition of controls however, the evolution of the lagged coefficients suggests a recovery with bigger sales changes changes, although they never reach as high as pre-pandemic levels.

respectively.

The baseline TWFE estimation shows that the overall eviction moratorium and moratoria on some eviction hearings and judgements result in smaller year-on-year change in multi-family units approved. Particularly, an overall eviction moratorium along with moratoria on non-payment based eviction hearings, on hardship based eviction judgements, and on either cause-based eviction hearings are associated with reductions of the year-on-year change in number of units approved. The event-studies do not show any significant anticipatory reduction no significant or lagged responses in building permit approval, to selective moratoria measures.

The DID_M results show that, upon adding controls, moratoria on non-payment based eviction hearings and on non-payment based eviction executions reduce the year-on-year change in units approved by approximately 44 units (around 6 times the mean) and 30 units (4.29 times the mean of approximately 7[15]) respectively.[16] Compared to pre-pandemic year-on-year change, moratoria on non-payment based eviction hearings and on non-payment based eviction executions reduce the year-on-year change in units approved by approximately 67% and 46% respectively.

An overall eviction moratorium along with moratoria on non-payment based eviction hearings and hardship-based eviction judgements are associated with higher year-on-year changes in multi-family sales from the baseline TWFE estimation, as predicted. However, this result is not robust to the application of the DID_M estimator, which when used produces negative regression coefficients. Negative coefficients translate to smaller year-on-year changes in multi-family sales as a response to moratoria, contrary to the paper's hypothesis.

This paper fits into the broader literature that discusses the effect of NPIs, like stay-at-home orders, shelter-in-place orders and eviction moratoria on urban outcomes. For instance, Elenev et al. (2021) explore U.S. county level direct and spillover effects of stay-at-home orders (SHO) by using contiguous county triplets and find that mobility in neighboring counties declined by a third

[15]The mean is obtained by averaging year-on-year sales changes over all the 36 months and across all the counties available in the data.

[16]On controlling for state-specific trends and the usual controls, an overall moratorium is associated with reduction of year-on-year change in units approved by approximately 12 units.

to a half as much as in the SHO adopting locations. Some recent studies explore how eviction moratoria could reduce the spread of COVID, reducing COVID deaths (Nande et al.(2021), Jowers et al. (2021)) and affect children's health and neighbourhood poverty (Leifheit et al. (2020)).

In the context of how COVID and NPIs have affected the housing market, this paper is most closely related to Yoruk (2022), who discusses the year-on-year drop in new home listings and pending home sales in response to multiple NPIs, particularly the closure of non-essential businesses in the U.S. The current paper analyses SIP and moratoria effects, NPIs not considered by Yoruk (2022), and improves further with the inclusion of longer term data. In terms of housing demand, Zhao (2020) uses non-parametric estimation to show that the growth rates of the median housing price increased faster with the onset of COVID-19 compared to any period preceding the 2007-09 financial crisis. Using microlevel data on U.S. residential property transactions, D'Lima et al. (2021) find that post-shutdown, pricing effects not only depend on population density but also on the size and structural density of properties. They also document a significant decrease in sales for markets under a shutdown, a conclusion supported by the sales change response to shelter-in-place orders, found in the current paper.

Discussing eviction mortatorium, Benfer et al. (2022) demonstrate how the framework they formulate, describing and analyzing eviction moratoria, can be used to assess the predictors of policy implementation and to evaluate the efficacy of these policies. Beyond that study, considerably less attention has been given to the construction and sales impacts of these moratoria in the U.S. Since such prolonged moratoria on eviction have never been adopted before, studying their effects can help recognize which stages of evictions being prohibited can have significant impacts on building permit approvals.

Liu and Su (2021) find that the diminished need for living close to telework-compatible jobs and the declining value of access to consumption amenities has caused housing demand to move away from dense locations in the U.S. Huang et al. (2022) reiterate similar conclusions using housing price and gradient data from 60 Chinese cities. Brueckner et al. (2021), Delventhal, Kwon & Parkhomenko (2020), Delventhal & Parkhomenko (2020) and Gokan, Kichko & Thisse (2021) use spatial models to explore how the introduction of work-from-home in the wake of the pandemic modified cities in the terms of housing prices, worker productivity and amenities. The current paper contributes to this literature by attempting to empirically segregate the public-intervention related effects of COVID, with special emphasis on a relatively lesser explored NPI: eviction moratoria.

1.2 Data

The empirical analyses and results use three major datasets constructed using four underlying databases: Housing Sales, Building Permits Survey, Shelter-in-Place and Eviction Moratoria. Details of the original databases are as follows:

Housing Sales: County-level monthly sales of residences are obtained from Redfin, a national real estate brokerage. Redfin computes sales, months of supply, sale-to-list price ratio and a host of other housing-related statistics daily as either a rolling 1, 4 or 12-week window. The local data are grouped by metropolitan area and by county.[17] Housing types available in the data include single-family houses, multi-family houses, condos, townhouses and the collection of all the preceding types of dwellings.

Building Permits Survey (BPS): At the county level, the monthly number and valuation of buildings approved to be constructed are obtained from the US Census Bureau Buildings Permits Survey. Building permits data are collected from individual permit offices, most of which are municipalities; the remainder are counties, townships, or New England and Middle Atlantic-type towns.[18] The 2014 universe of the BPS includes approximately 20,100 permit-issuing places and is used from January of 2014 forward. Data are shown for the number of buildings, number of housing units, and permit valuation within four sizes of residential buildings: (1) single family houses (attached and detached combined), (2) two-unit buildings, (3) three- and four-unit buildings,

[17] Data available here: https://www.redfin.com/news/data-center/

[18] The Building Permits Survey covers all “permit-issuing places”, defined as jurisdictions that issue building or zoning permits. Zoning permits are used only for areas that do not require building permits but require zoning permits. Areas for which no authorization is required to construct a new privately-owned housing unit are not included in the survey.

and (4) buildings with five or more units. When a report is not received, missing housing unit data are either obtained from the Survey of Construction (which is used to collect information on housing starts, sales, and completions) or imputed, based on the assumption that the ratio of authorizations for the current time period to the prior year total is the same for reporting and non-reporting jurisdictions in that Census region.

Shelter-in-Place (SIP): Shelter-in-Place orders are advisory notices enacted by the city, county or state Government that people should shelter at home except for essential reasons. The paper focuses on SIPs because apart from closure of non-essential businesses, they were the most widely adopted NPI, with substantial variation in adoption both over time and across counties. The dataset on the county-level implementation of SIPs merges county-specific and state-specific adoption dates from Keystone Strategy, a consulting firm, and Husch Blackwell, an industry-centric law firm that developed a COVID-19 resource center to monitor and update COVID-19 rules, restrictions, orders and guidance that affect businesses across the country. Figure 1.1 shows the variation in the prevalence of SIP orders across different U.S. states.

Eviction Moratoria: A comprehensive database of state-level eviction moratoria is obtained from "Eviction Moratoria & Housing Policy: Federal, State, Commonwealth, and Territory" from OpenICPSR.[19] Researchers Emily Benfer and Robert Koehler employed longitudinal policy surveillance to comprehensively describe state, court and legislature involvement in the eviction moratorium resulting from the emergence of the COVID-19 pandemic and continuing through the end of substantive state intervention.[20] Researchers developed a dynamic, novel dataset consisting of over 50 indicators that captured the temporal and substantive features of these moratoria and renter-supportive measures. The database documents dates at which states prohibited any part of the eviction process, dates at which states recognized the authority of CDC moratorium (or required that a landlord filing for eviction provide a certification/affirmation that the tenant had not

[19]https://www.openicpsr.org/openicpsr/project/157201/version/V1/view

[20]The study relied on an exhaustive collection of all emergency orders and legislation that controlled the eviction process, related to protections under federal moratoria, or provided support to tenants and that were issued by state governors, courts, and legislative bodies between March 13, 2020 and March 1, 2022. To confirm that the dataset was complete, researchers provided state governors and court officials with lists of collected orders from their states and incorporated any needed additions.

provided the landlord with a CDC Declaration), and dates at which the state required certification of CARES Act non-coverage for eviction-filings. Eviction moratoria indicators also capture the variation in degrees of strength of moratoria (prohibition on eviction notices, filling, court hearing, judgment and execution) and variation in causes for eviction (non-payment, hardship-related, or all causes). Figures 1.2, 1.3 and 1.4 show state-level variations in the longevity of the some eviction moratoria indicators that the paper focuses on.

As part of the main regressions, the paper also uses county-level monthly number of COVID cases and deaths, obtained from Covid Act Now API, and county-level monthly employment (sum of federally, locally, private and state government owned industries), obtained from the US Quarterly Census of Employment and Wages.

The paper uses multiple county-level balanced panel datasets constructed by merging some or all of the databases mentioned above, subject to the availability of the same counties in the different databases. The primary datasets used for the main regressions are as follows:

1. *Sales - SIP datasets*:

   This is a collection of 13-month panels containing housing sales, employment, COVID cases, COVID deaths and an indicator for shelter-in-place orders enacted in each county. The observed time period is September of 2019 to September of 2020, both months included. Shelter-in-place orders are enacted around March of 2020 in most counties, with some beginning around April 2020 and the orders end around May-June 2020. The SIP indicator is recorded as 1 for every month in which the shelter-in-place order is in effect and 0 when one is not in effect.[21] Housing sales and employment data are expressed as monthly year-on-year changes to account for seasonality of employment and sales. In other words, these variables are calculated as:

$$\Delta X_t^{cm} = X_t^{cm} - X_{t-1}^{cm} \ ,$$

[21] The order needs to be in effect for at least 2 full days in a month in order for the indicator to be set to 1 for that month. In other words, if SIP orders begin on the last day of a month, that month's SIP value is set to 0.

where X^{cm} is the employment or houses sold in month m of year t in county c.

To account for the heterogeneity in housing types, the paper estimates the effect of shelter-in-place on sales of five different types of housing. Owing to the lack of uniformity in the availability of sales data for the different types of houses, the number of counties observed vary for each type. The paper focuses on sales of three types of dwellings (corresponding counts of observed counties are in the brackets): single-family housing sales (804 counties), condominium sales (366 counties) and all residential sales (1269 counties). The last dataset contains all the four types of housing sales mentioned in the database.[22] Table 1.1 shows the summary statistics for these datasets.

2. *BP - Eviction Moratoria dataset*: This dataset is a 36-month panel containing data on building permits approved, their valuations, COVID cases, COVID deaths, employment data and indicators for eviction moratoria for 495 U.S. counties. The paper uses data and presents results applicable for single-unit, double-unit, three-four unit and five-plus unit buildings approved. However, the main focus is approval for buildings with five-plus units since buildings that are rented out predominantly consist of five-plus units. The observation period spans January 2019 to December 2021. The beginning period for the different indicators of overall eviction moratoria is around March of 2020 and the ending period spans all through 2020 and 2021 across various states. The building units approved, their valuation and employment are expressed as monthly year-on-year changes to account for seasonality (similar to the Sales-SIP datasets). The state-level eviction moratoria indicators are attributed to every county in a state and are recorded as 1 for every month in which a certain type of moratorium is in effect and 0 when it is not in effect.

   The paper focuses on the following strength indicators for the moratoria : prohibition on court hearings for eviction, prohibition on passing judgements on eviction cases, prohibition on execution of evictions. In terms of causes for evictions being

[22]The four types of housing include single-family houses, multi-family multi-family buildings, condos and townhouses.

prohibited, for each strength level, the paper distinguishes among evictions based on COVID related hardships, called "Hardship" moratorium and any evictions based on non-payment, called "Non-payment" moratorium.[23] The paper also constructs a third moratorium indicator for each strength level that records 1 for months for which either hardship or non-payment based evictions are prohibited, and 0 when neither are in effect. This third indicator is defined as "Either" cause moratorium. Finally, the moratoria that prohibit all evictions, both caused by non-payment and hardship are called "Both" cause moratoria.

In order to simplify referencing these different indicators for the moratoria, the paper henceforth defines eviction moratoria indicators based on strength (S) and cause (C) as "C/S" eviction moratoria, where C could be "Non-payment", "Hardship", "Either" or "Both" and S could be "Hearing", " Judgement" or "Execution". For instance, moratorium indicator Non-payment/Hearing would refer to moratorium on non-payment based eviction hearings.

An overall-moratorium indicator indicates periods when a state actor has prohibited some part of the eviction process. This prohibition may include suspending notice of eviction to tenants, suspending filing of eviction claims, suspending hearings on eviction, suspending the entry of judgment or issuance of writs of eviction, or suspending enforcement of eviction orders is also included. This overall indicator encompasses any moratoria, effective for any strength level, for any cause (including hardship or non-payment related evictions) in a given state.

A CDC-moratorium indicator exists in the dataset, and it captures periods when a state actor has recognized the authority of the CDC moratorium in the state or required that a landlord filing for eviction provide a certification/affirmation that the tenant had not provided the landlord with a CDC Declaration. A CDC Declaration necessitates

[23]The Hardship moratorium is more restrictive in terms of the tenants having to prove that their inconveniences have been caused specifically by COVID.

landlords to recognize the moratorium enacted only by CDC, if a moratorium has already not been enacted by the state.[24] States could hence have CDC-moratorium-recognition in conjunction with or independently of the overall moratorium. This implies that there could be months in which either, both or none were in effect across the different states.[25] Table 1.2 shows the details of the eviction moratoria indicators pertaining to the 495 counties in the dataset. Table 1.3 shows the summary statistics of the year-on-year change in building permit approval.

3. *Multi-family Housing Sales*:

   This dataset is a 36-month panel as well containing sales of multi-family residential buildings, COVID cases, COVID deaths, employment and indicators for eviction moratoria for 234 U.S. counties. Similar to the Building Permits - Eviction Moratoria dataset, the observation period spans January 2019 to December 2021. The multi-family sales and employment are expressed as monthly year-on-year changes to account for seasonality (similar to the Sales-SIP datasets). Similar to the Building Permits - Eviction Moratoria dataset, the regressions from this dataset focus on moratoria on eviction hearings, judgements, executiona and on hardship and/or non-payment related evictions and overall eviction moratorium. Table 1.4 shows the summary statistics of the eviction moratoria and change in multi-family housing sales pertaining to the 234 counties in the dataset.

Table 1.5 shows the demographic statistics of the counties in the various datasets used in this paper and compares them to the demographic statistics of all U.S. counties. This table shows that the sample of counties considered here are closely representative of the whole country.

[24]It's important to note that states that already had state-enacted eviction moratoria in effect did not necessarily require the federal moratorium declaration to prohibit the eviction process in that state, making the CARES-requirement and CDC-moratorium-recognition indicator equal to 0 in some cases even when some other moratoria were in effect.

[25]The paper focuses on the CDC and overall moratorium effects separately. See appendix for a figure on a union of overall and CDC moratorium indicators.

Robustness checks include running the original regressions on 6-month (January 2020 to June 2020) and 9-month (January 2020 to September 2020) versions of the Sales-SIP dataset[26] and 36-month (January 19 to December 21) versions of the Building Permits-Eviction Moratoria and Multi-family Housing Sales - Eviction Moratoria datasets. An additional robustness check involves adding time-invariant controls for the Sales-SIP dataset. These county-level controls include the 2020 population density levels (from the US Census Bureau), average temperature and precipitation levels (from National Centers for Environmental Information), the Wharton Residential Land-Use Regulatory Index (WRLURI), a measure of terrain ruggedness, work-from-home potential and a quality-of-life estimate. The data for the last four variables are sourced from Brueckner et al. (2021).

1.3 Methodology

The main goal of this paper is to estimate the average treatment effect of shelter-in-place orders and eviction moratoria on monthly changes in sales and building permits approval. The outcome variables are year-on-year changes in housing sales and building permits approved. The primary treatment variables are indicators for SIP and eviction moratoria. It is important to note the nature of the treatment variables under consideration.

The SIPs were enacted in a staggered fashion with most counties adopting them in March of 2020, while some others mandated the order in April 2020.[27] The orders, however, ended in different months across different counties, with most counties ending them in May 2020, while some maintained them through June 2020. This pattern implies that the treated counties do not stay treated throughout the observation period. Starting from July, the treatment switches off for every county. SIP orders being the primary treatment variable implies that the Sales-SIP panel includes approximately 6 months of pre-treatment and 7 months of post-treatment data,[28] with

[26]These datasets only record all-residential sales.

[27]Staggered treatment is defined as a treatment that is implemented at different points in time for different individuals. However, treated groups may or may not stay treated till the end of the observation period once treatment begins.

[28]Note that "pre-treatment" refers to time periods before the earliest treatment went into effect and "post-treatment" refers to every period following the first treated period.

the treatment switching off after around 2-3 months of being in effect.

For the state-level eviction moratoria, the treatment adoption is staggered, but the ending of the moratoria is not uniform across counties. Some of these moratoria were enacted, repealed and re-instated as COVID cases and vaccinations varied over time, implying that the treatment reverses (in some cases multiple times) for several states. With the different indicators for eviction moratoria being treatment variables, the BP-Eviction Moratoria panel includes approximately 14 months of pre-treatment and anywhere between 4 to 19 months of post-treatment data.

The next three subsections first start off with some event study graphs to ensure the lack of significant pre-trends before the treatments begin. The next two subsections discuss the baseline two-way fixed effects model and a more robust version of the same model.

1.3.1 Event-Study graphs

Goodman-Bacon and Marcus (2020) urge researchers to identify threats to the validity of difference-in-difference designs to estimate the causal effect of COVID measures. In the context of this paper, the issues of voluntary precautions and anticipation effects of the treatment may impact outcomes in some periods before the actual treatment goes into effect.

The traditional approach to identifying preemptive responses to treatments is analyzing a graphical illustration of an event study regression. Let $Event_c$ record the the month m in which the treatment is first adopted in a given county c. Consistent with the previous notation, the treatment D_{cm} is adopted in month m in county c, and the outcome of interest is Y_{cm}. The panel event-study specification can then be written as:

$$Y_{cm} = \alpha + \sum_{j=1}^{J} \beta_k (\text{Lead}\, j)_{cm} + \sum_{k=1}^{K} \mu_k (\text{Lag}\, k)_{cm} + \epsilon_{cm}. \tag{1.1}$$

In equation (1), lags and leads to the event of interest are defined as follows:

$$(\text{Lead}\, J)_{cm} = 1\,[m \leq Event_c - J], \tag{1.2}$$

$$(\text{Lead}\, j)_{cm} = 1\,[m = Event_c - j] \quad \forall j \in 1, ..., J-1, \tag{1.3}$$

$$(\text{Lag}\, k)_{cm} = 1\,[m = Event_c + k] \quad \forall k \in 1, ..., K-1, \tag{1.4}$$

$$(\text{Lag}\, K)_{cm} = 1\,[m \geq Event_c + K]. \tag{1.5}$$

Lags and leads are thus binary variables indicating that the given state was a given number of periods away from the event of interest in the respective time period. J and K leads and lags are included respectively, and, as indicated in equations (3) and (6), final lags and leads "accumulate" leads or lags beyond J and K periods. A single lag or lead variable is omitted to capture the baseline difference between areas where the event does and does not occur. In specification 1, as standard, this baseline omitted case is the first lead, where $j = 1$. The panel event-study is an extension of the standard two-way fixed effect model, where a single "Post Event" indicator is included for all periods posterior to the occurrence of the event in treated units.

Exploiting the assumption that any unmeasured determinants of the outcomes are either time invariant or group invariant, the two-way fixed effects or generalized difference-in-difference estimation (explained in further detail in the next section) includes a full set of county and month fixed effects, along with the treatment indicator. Also, the paper presents results both with and without controlling for employment and/or COVID cases.[29] Hence, adding controls, county and month fixed effects to the baseline event-study regression gives us:

$$Y_{cm} = \alpha + \sum_{j=1}^{J} \beta_k (\text{Lead}\, j)_{cm} + \sum_{k=1}^{K} \mu_k (\text{Lag}\, k)_{cm} + \gamma_c + \gamma_m + X_{cm} + \epsilon_{cm}. \tag{1.6}$$

In equation (6), γ_c and γ_m are the county and month fixed effects respectively, while X_{cm} are county-level monthly controls.

Noting that the treatment timing is staggered, Borusyak and Jaravel (2018) discuss that in

[29] Further discussion in the next section

the presence of unit and time fixed effects, the specification in equation (6) suffers from an under-identification problem.[30] Ideally, without the unit and time fixed effects, the outcome would capture the causal effect of anticipation of the treatment. However, with unit and time fixed effects, it is also possible that outcome captures the difference corresponding to the combination of cohort and time fixed effects. In other words, the formulation above is unable to segregate between outcome differentials that arise due to passage of time alone or due to the combination of unit fixed effects and time trends. In terms of event-studies, the effect of passing of absolute time and relative time are inseparable in the presence of unit fixed effects because the linear schedule of dynamic effects cannot correctly identify a non-linear trend.

Hence, in a staggered adoption design, without compromising the two-way fixed effect model, one needs to add an additional restriction on the pre-trends to correctly identify the model. Following the recommendation by Borusyak and Jaravel (2018), apart from setting the coefficient of the first lead to zero, the paper hence sets a second restriction on the 4th lead. Consequently, in specification (2), $k = 1$ and $k = 4$ are omitted for all the event studies shown in the paper.

In light of some treatments in the data switching off before the ending of the observed time period, the paper considers two sets of event-study graphs when illustrating the event-study of SIP and Δ Sales. One explores the effect of the treatment beginning and another studies the effect of the treatment ending. For the specification analyzing the effect of SIP beginning, 8 lags and 4 leads are considered with time=0 showing the first time a county is treated with SIP. Lags beyond $t = 4$ might interfere with the ending of SIP and are hence omitted from the event-study where the beginning of SIP is analyzed. For the specification analyzing the effect of the treatment ending, the paper focuses on 4 leads (all of which are assume to have had SIP in place) before it ends and 4 post-treatment periods once the SIP treatment starts switching off, starting around May 2020.[31] The results from the first specification are illustrated in the main text while those from the second specification are illustrated in the appendix.

[30]Sun & Abraham (2021) also discuss how the lead coefficients might be contaminated with effects of the treatment in periods before and after the concerned point of time.

[31]Under the second specification, time=0 shows the first time the observation stops being treated with SIP.

For the Building Permits-Eviction Moratoria event-study, owing to a large number of eviction moratoria indicators, the paper only presents three selected indicators, an overall moratorium indicator, an indicator for both cause-based eviction hearings and cause-based eviction judgements. For the specification in which the effect of the beginning of these three moratoriua indicators are analyzed, 8 lags and leads are considered with time=0 showing the first time a county is treated. The remaining periods are pooled in ≤ 9 and ≥ 9 bins. Given that an overall moratorium lasts for an average of approximately 10 months in the observation period, the 8 lags should ideally not be spuriously capturing the overall moratorium ending, making it safe to assume that once this treatment begins, it stays on for the next 8 periods. However, moratoria on eviction judgements end in less than 7 months on average after they begin, implying that the same biases discussed before might affect the event-study coefficients. Hence, similar to the Sales-SIP event-studies, a secondary specification is also considered where the treatment is the moratoria ending, with 4 lags and leads. Similar to the SIP-Sales, the results from the first specification are illustrated in the main text while those from the second specification are illustrated in the appendix.

In presenting the event-study graphs, specifications both including and excluding controls, COVID Cases and Δ Employment, are shown. These results should be read with caution owing to Δ Employment potentially being an outcome of Shelter-in-place and eviction moratorium (discussed in further detail in the next section). Also, a developing literature including papers by de Chaisemartin and D'Haultfoeuille (2020a), Callaway and Sant'Anna (2021) and Goodman-Bacon (2018) points to challenges in interpreting the estimated coefficients from two-way fixed effects models when treatment effects are heterogeneous (across either groups or time periods). Nevertheless, checking for the presence of significant lead coefficients or pre-trends using the event study graphs can gauge the reliability of results from the baseline two-way fixed effects estimation (discussed in the next section).

1.3.2 Baseline Two-way Fixed Effects

Exploiting the assumption that any unmeasured determinants of the outcomes are either time invariant or group invariant, the baseline regressions in the paper follow a generalized difference-in-difference (generalized DID) or a two-way fixed effects (TWFE) model. The model then estimates the average treatment effect on treated (ATT) or the expected effect of the treatment for individuals in the treatment group. To estimate the impact of SIP on sales, the TWFE model regresses the change in sales on SIP orders and a full set of county and month fixed effects. To estimate the impact of eviction moratoria on building permits approvals and multi-family home sales, the TWFE model regresses the county-level outcome on state-level eviction moratoria indicators and a full set of county and month fixed effects. By using year-on-year changes[32] as outcome and then using month fixed effects enables the treatment variables to capture the specific effect of the policies beyond the overall year-on-year pandemic effects.[33]

The baseline regressions run are hence specified as:

$$Y_{cm} = \gamma_c + \gamma_m + X_{cm} + \delta^{DD} D_{cm} + \epsilon_{cm} \tag{1.7}$$

where

Y_{cm} is the county-level outcome in county c and month m,

γ_c & γ_m are the county and month fixed effects respectively,

X_{cm} are time-variant county-level controls,

[32]While year-on-year log changes would help explain the coefficients relative to the baseline changes, the majority of the housing sales and units constructed data is small in magnitude. The log differences hence do not capture the variation in outcomes well enough to justify its usage.

[33]While the year-on-year changes account for seasonality and compare outcome evolution across COVID and non-COVID years, the addition of month fixed effects (with month counts measured from the beginning of the sample) capture changes within the year associated with the progress of the pandemic. Consequently, the analyses that follow, show the specific effect of SIP and eviction moratoria variables beyond the overall pandemic effects.

D_{cm} is the treatment.

In order to estimate how much of the outcome variation is captured by the treatments alone and how much is dependent on other time-varying county-level covariates like COVID cases and employment, the paper adds both COVID cases and year-on-year employment change as controls. However, the NPIs like shelter-in-place and eviction moratoria could cause changes in the covariates themselves, especially by reducing employment. The NPI-induced changes in employment could then affect the housing market outcomes independently, potentially confounding the reported effects of NPIs. Therefore, for each of the specifications considered henceforth, the paper presents results both with and without controlling for employment and/or COVID cases to account for the employment-change possibly being an inappropriate control.

Bertrand et al. (2004) show that the standard errors for generalized DID estimates are inconsistent if they do not account for the serial correlation of the outcome of interest. Because the outcomes studied usually vary at the group and time levels, it thus makes sense to correct for serial correlation. The authors show that using cluster–robust standard errors at the group level where treatment occurs provides correct coverage in the presence of serial correlation when the number of groups is not too small. Since the treatments are at county and state levels in our datasets, the groups are definitely not too small. The baseline results presented in the paper use the cluster-robust standard errors discussed above.

When analyzing the effect of SIP on changes in housing sales, the expected sign of the average treatment effect on the treated is negative following the idea that shelter-in-place reduces sales. When analyzing the effect of eviction moratoria on changes in building permits approved, the expected sign of the ATT is negative as well assuming that moratoria depress construction of multi-family houses. When analyzing the effect of eviction moratoria on change in multi-family housing sales, the moratoria increasing sales of multi-family houses, as hypothesized, should show up as a positive sign on the ATT.

1.3.3 Issue of Negative Weights

Applying the same notation as in (7), if $\hat{\beta}$ denotes the coefficient of D_{gm} and β denote its expectation, under the common trends assumption, de Chaisemartin and D'Haultfœuille (2020a) show that β is equal to a weighted sum of the treatment effect in each treated (g,m) cell (in a two-period binary treatment case):

$$\beta = E\left(\sum_{(g,m):D_{g,m}=1} W_{g,m}\delta_{g,m} \right) \tag{1.8}$$

$\delta_{g,m}$ is the average treatment effect (ATE) in group g and period m and the weights $W_{g,m}$ sum to one but may be negative. β compares the evolution of the outcome between consecutive time periods across pairs of groups that ideally compare treated units to only control units. However, what is considered the control group in some of these comparisons may be treated at both periods. Hence, while comparing newly treated units relative to "never treated" units and newly treated units relative to "not-yet treated" units involves no issues, comparing newly treated units relative to already treated units makes a causal interpretation unclear. The negative weights are especially an issue when the ATEs are heterogeneous across groups or periods. This problem has been discussed in detail by Borusyak and Jaravel (2018), Goodman-Bacon (2021), de Chaisemartin and D'Haultfoeuille (2020a), Sun and Abraham (2021), and Callaway and Sant'Anna (2021).

To overcome the problem with negative weights, de Chaisemartin and D'Haultfœuille (2020a) propose a new estimator, DID_M, that estimates the average treatment effect across all the (g,m) cells whose treatment changes from $m-1$ to m. This new estimator is a weighted average of two difference-in-differences, one for units that switch in to the treatment, and one for units that switch out of treatment. For the units switching in, the first DID compares the outcome evolution of groups going from untreated to treated, and of groups untreated at both dates across two consecutive time periods. Using the units that switch out, the second DID compares outcome evolution of groups treated at both dates, and of groups going from treated to untreated across consecutive time periods.

The weighting of these two DIDs makes the resulting estimator valid, even if the treatment effect is heterogeneous over time or across groups.[34]

DID_M applies to any TWFE regressions, not only to those where adoptions are staggered and treatments turn off at the same time. The estimator, hence, lends itself perfectly to the econometric design of the current paper with a binary treatment that is adopted in a staggered fashion and is withdrawn at different points in time. Following DID_M, this paper presents a secondary set of results for each dataset in order to supplement the baseline results with a set of more rigorous TWFE estimations. DID_M relies on common trends assumptions on both potential outcomes, and de Chaisemartin and D'Haultfœuille (2020a) propose a placebo estimator that can be used to test these assumptions. The placebos compare the outcome trends of switchers and non-switchers, before the switchers switch. For the parallel trends assumption to hold, the reported "placebo" estimator should not significantly differ from 0.

DID_M can be used in applications where, for each pair of consecutive dates, there are groups whose treatment does not change, a condition that holds in the data used in the current paper. The estimators with controls are similar to those without controls, except that the first-difference of the outcome is replaced by residuals from regressions of the first-difference of the outcome on the first-differences of the controls and time fixed effects. Those regressions are estimated in the sample of control (g, m)s: (g, m)s such that group g's treatment does not change from $m - 1$ to m. When "state_trends" is specified, the DID_M estimator calculates a weighted average of DIDs comparing switchers and non switchers (respectively first-time switchers and not yet switchers) within the same state. These estimator with are unbiased even if groups experience differential trends, provided all groups within the same state experience parallel trends.

[34] As discussed by de Chaisemartin and D'Haultfœuille (2022).

1.4 Results

1.4.1 SIP and Sales

Figure 1.5 illustrates the TWFE event-study graphs from regressing Δ Sales of various types of houses on SIP. t=0 refers to the month in which SIP was enacted for the first time. The leads correspond to months or periods leading up to the beginning of SIP i.e. -1, -2 and so on. The lags correspond to months or periods after the beginning of SIP i.e. 1, 2 and so on. The graphs also show 95% confidence intervals of the lag and lead coefficients, with the standard errors being clustered at the county level. The coefficients being above 0 imply bigger year-on-year change in sales while coefficients below 0 imply smaller year-on-year changes in sales. Sub-figures (a), (c), and (e) correspond to single-family, condos, and all-residential houses respectively, with the regressions not controlling for other time-varying factors. Sub-figures (b), (d), and (f) illustrate the counterparts with COVID cases and Δ Employment as controls.

None of the leads for single-family or all-residential houses are significant, with or without controls, indicating a lack of pre-trends. The lagged coefficients are significant and below zero, as expected, suggesting a reduced change in year-on-year sales post the adoption of SIP. Condos do show some significant leads (especially when controls are added in the regression); however, none is significantly close to the time of the event. The only significant coefficient corresponding to the 3rd lag is also positive showing some level of recovery in sales from the initial drop at the time of treatment. [35]

Table 1.6 shows the results of running the standard TWFE regression with year-on-year

[35] Figure A3 in the appendix illustrates the TWFE event-study graphs from regressing Δ Sales of various types of houses on the ending of SIP. t=0 refers to the month in which SIP was revoked for the first time, with the leads implying SIP still being in effect. Both with and without controls, all-residences and single-family houses register significant coefficients for lags, while condos show mostly insignificant lagged responses of the SIP ending. Hence, for condos, home buyers and sellers apparently continue to behave similarly for a few periods after the ending of SIP as they did before. For all residences and single-family, it appears that the ending of SIP led to bigger year-on-year sales changes, suggesting more traffic in the housing market once shelter-in-place is rescinded. This result follows the initial proposition that when SIP is in effect, year-on-year sales records smaller changes, with the opposite trend occurring when SIP terminates.

change in sales (or Δ Sales) of various types of housing as the outcome variable of interest and SIP as the primary treatment variable. The results should be read as follows: Corresponding to the first entry in the table, without controlling for other time-varying factors, when shelter-in-place is active, the year-on-year change of single-family housing sales is reduced by an average of 29 housing units compared to when shelter-in-place is inactive. Overall, without controls, comparing the magnitudes of the coefficients from the first row with the pre-pandemic means, it appears that SIP reduces single-family, condominium and all-residential year-on-year changes in housing sales by approximately 32%, 47% and 70% respectively.

Note that all the rows of the table show effect of SIP on Δ Sales, with the variation in coefficients appearing from adding COVID cases, Δ employment (year-on-year change) or both as controls. The signs on all the coefficients are negative, implying that enacting SIP resulted in smaller year-on-year change in sales for all types of houses, as hypothesized. The coefficients in column (5) being significantly higher in magnitude suggests that the collection of all types of residences together responded more to the SIP order than any one singular type of residence. This results from sales levels being higher for the set of all types of housing compared to individual types.[36] Adding the number of COVID cases and Δ employment seems to reduce the size of the baseline impacts in all types of houses, but the reduction is more pronounced for single-family and the collection of all residences. This pattern could potentially imply that some of the variation in year-on-year sales change comes from change in COVID cases and Δ Employment.

Table 1.7 shows the results from using the DID_M estimator to estimate the effect of SIP on change in sales of single-family, condos and all types of residential houses. The basic format of this table is similar to that of Table 1.6 with the exception of an additional 'state_trends' specification with each original regression.[37] N is the total number of panel observations used in the estimation of the coefficients reported in the table (equal to the number of first differences of the outcome

[36]The appendix includes the regressions on multi-family buildings and townhouses. Coefficients for single-family, townhouses and condos are significantly higher in magnitude compared to multi-family buildings, suggesting that SIP did not reduce the year-on-year sales change of multi-family buildings as much, compared to other types of houses.

[37]State_trends allow for state-specific trends. This estimator is calculated as a weighted average of DIDs comparing first-time switchers and not-yet switchers within the same state.

and of the treatment used in the estimation). $N\_switchers$ is number of first-time switchers the coefficient applies to. When state_trends is specified, $N\_switchers\_cont$ is the number of switchers or first-time switchers whose counterfactual trend at the time of their switch is estimated accounting for the controls. The results shown use 50 bootstrap replications in the computation of estimators' standard errors. These standard errors are clustered at the county level. † implies that DID_M's version of parallel trends assumption has been met.

The significant coefficients for change in sales are all negative, implying that the change in sales became smaller with SIP, matching what has been presented previously. Adding controls, makes the magnitude of the effect smaller as shown previously as well, with Δ Employment having a very pronounced effect for the reduction in all-residential houses. Without controls, and keeping all else constant, on an average, switching on of SIP reduces the year-on-year change in sales of single-family, condos and all-residences by approximately 24, 15 and 29 units respectively. These changes are substantial compared to the mean change in sales of approximately 5 (4.8 times increase), -4 (3.75 times increase), and 67 (0.43 times increase) units[38] for single-family houses, condos and all-residences respectively. Overall, without controls, comparing the magnitudes of the coefficients from the first row with the pre-pandemic means, it appears that SIP reduces single-family, condominium, and all-residential year-on-year changes in housing sales by approximately 26%, 41% and 24% respectively.

Notably, among the significant coefficients, while all the magnitudes are lower than in the baseline TWFE, the sizes of the all-residences coefficient is particularly smaller. Addition of state_trends overall seems to increase the size of the effect. While the parallel trends assumption holds across all specifications for single-family housing, it holds when no controls are added for condos and it holds when COVID cases and employment are individually controlled for, with Δ Sales for all-residences.

[38]Mean values are obtained from Table 1.1

1.4.2 Eviction Moratoria and Building Permits

Figure 1.6 illustrates the TWFE event-study graphs from regressing Δ multi-family units on the overall eviction moratorium indicator.[39]. Time=0 refers to the month in which the particular eviction moratorium was enacted for the first time. The graphs also show 95% confidence intervals of the lag and lead coefficients, with the standard errors being clustered at the county level.

None of the leads are significant for the overall indicator and most of the lag coefficients are negative suggesting a lack of anticipatory behaviour in response to the beginning of the overall moratorium. The lagged effect indicates a continued reduction in the year-on-year changes in building permits approved. [40]

Table 1.8 presents the results from running the baseline TWFE regression of the change in the total number of units and their corresponding valuation in 5+ unit buildings on various eviction moratorium indicators. Owing to a large number of eviction moratoria indicators present in the data, only the treatments that produce significant coefficients are presented in the results. For each moratorium, results show the regression coefficients with and without controlling for COVID cases and Δ Employment. All the coefficients are negative, implying that the eviction moratoria indicators listed are associated with smaller year-on-year change in units approved and their valuation. Adding controls increases the magnitude of each coefficient, and in the case of moratoria on hardship/judgements and either/hearings, the coefficients are only significant when controls are added.

The results show that the overall moratorium indicator reduces the year-on-year change in units approved for 5+ unit buildings. Multiple narrower moratorium indicators, for instance

[39] Owing to the presence of a large number of moratoria indicators in the data, event-studies associated with only the overall indicator are presented in the main text and two other indicators on all cause-based moratorium hearings and judgments are shown in the appendix

[40] Figure A4 in the appendix illustrates the TWFE event-study graphs from regressing Δ Units approved on the ending of the same moratoria indicators studied in Figure 4. t=0 refers to the month in which these eviction moratoria were repealed for the first time with the leads implying the moratoria still being in effect. The coefficients for the leads and lags are mostly insignificant, possibly implying that units approved neither respond preemptively before, nor record any lagged responses after the moratoria ends. This implies that the event studies in Figure 4 do not produce erroneous coefficients via the assumption that once the moratoria begin, they continue being treated beyond 4 to 6 periods, post-treatment.

overall-or-CDC, and non-payment/hearings, hardship/judgements and either/hearings, are also significant drivers of the reductions in units approved, possibly leading to the significant overall indicator. Moratoria on non-payment/hearings and the presence of overall or CDC moratorium have significant impacts on year-on-year change in valuation. This difference possibly arises from the valuation being correlated with multiple factors other than number of units approved, like market volatility and various general equilibrium features associated with the building construction market. Hence, the valuation coefficients may not be proportional to or even have the same sign as the unit coefficients. Consequently, the paper focuses on the results with respect to change in units approved more than results from analyzing the corresponding valuation.

To summarize the results, on an average, controlling for Δ Employment and COVID cases, the enactment of an overall moratorium is associated with reductions of the year-on-year change in number of multi-family units approved by approximately 13, which is substantially higher than the mean change in five-plus building units approved[41] of approximately 7 units. Compared to the pre-pandemic mean year-on-year change of 64.77 units[42], an overall moratorium reduces the changes in units approved by 20%. Moratoria on eviction executions, moratorium on evictions caused both by non-payment and COVID hardships and CDC moratorium alone do not seem to have any significant effect on the number of 5+ units approved.[43]

Table 1.9 shows the results from using the DID_M estimator to estimate the effect of different eviction moratoria on the change in multi-family (5+ Unit Buildings) units approved. The basic format of this table is similar to Table 1.7, with the treatments being moratoria instead of SIP and dependent variable being changes in units approved instead of changes in sales. The state_trends specification is analogous to that used in table 1.7, except that whenever the state_trends are

[41] Data obtained from Table 1.3

[42] Pre-pandemic refers to the average year-on-year change in number of multi-family units approved across counties in January of 2020

[43] With controls, the enactment of moratorium on non-payment/hearings evictions, moratorium on hardship/judgements evictions, moratorium on either/hearings evictions and presence of overall or CDC moratorium are associated with reductions of the year-on-year change in number of multi-family units approved by approximately 22, 18, 19 and 15 respectively. These magnitudes are substantially higher than the mean change of approximately 7 units. Compared to the pre-pandemic mean year-on-year change of 64.77 units, a moratorium on non-payment/hearings evictions, a moratorium on hardship/judgements evictions, a moratorium on either/hearings evictions and presence of an overall or CDC moratorium reduces the changes in units approved by 33%, 27%, 29% and 23% respectively.

specified, they are always being added after controlling for both COVID cases and Δ Employment.[44] N and $N\_switchers$ are defined the same way as in table 1.7 as well.[45] The results shown use 50 bootstrap replications in the computation of estimators' standard errors, which are also clustered at the county level. † implies that DID_M's version of parallel trends assumption has been met. Similar to table 1.8, owing to a large volume of indicators, only the moratoria indicators that produce significant changes in year-on-year units approved are presented in the table.

Among the many indicators considered, the DID_M estimator suggests that overall and non-payment based eviction moratoria significantly reduce the change in year-on-year number of multi-family units approved.[46] All the significant coefficients being negative suggests that eviction moratoria indeed led to smaller changes in units approved, as hypothesized. The parallel trends assumption seem to be satisfied for the regressions where an overall moratorium and moratoria on non-payment/executions are treatments. The regression with the moratorium on non-payment/hearings produces the highest magnitude of the coefficient.[47] Adding controls and state_trends renders most of the indicators insignificant except for overall moratorium.

To summarize the results, controlling for state-specific trends, COVID cases and Δ Employment, an overall moratorium is associated with a reduction of year-on-year change in units approved by approximately 12 units, which is a 19% reduction, compared to the pre-pandemic mean. Using DID_M, the CDC moratorium, moratoria on passing judgements on evictions, and moratoria on evictions caused only by COVID hardships do not seem to have any significant effect on the number of 5+ units approved.[48]

[44] State_trends allow for state-specific trends. This estimator is calculated as a weighted average of DIDs comparing first-time switchers and not-yet switchers within the same state.

[45] N is the total number of panel observations used in the estimation of the coefficients reported in the table. $N\_switchers$ is number of switchers or first-time switchers the coefficient applies to applies to.

[46] With controls, moratoria on non-payment/hearings and non-payment/executions reduce the year-on-year change in units approved by approximately 44 units and 30 units respectively. Compared to the pre-pandemic mean year-on-year change in units approved, moratoria on non-payment/hearings and non-payment/executions reduce the year-on-year change in units approved by approximately 68% and 46% respectively.

[47] The moratorium on both/judgements seems to produce a positive year-on-year changes in valuation, while satisfying the parallel trends assumption. However, as mentioned during the discussion of Table 1.8, it is difficult to conclusively interpret the coefficients on valuations and they should hence be read with caution.

[48] Table A1 in the appendix presents the results from running the baseline TWFE regression of change in number of Units and valuations of single, double and 3-4 Unit Building Permits on various eviction morato-

1.4.3 Eviction Moratoria and Multi-Family Sales

Table 1.10 presents the results from running the baseline TWFE regression of change in year-on-year sales of multi-family housing on various eviction moratorium indicators. Similar to Tables 6(a) and 6(b), moratoria indicators are referred to as Cause/Strength indicators, with cause being moratoria on evictions caused by non-payment, hardship, either or both, and strength levels being moratoria on eviction hearings, judgements or executions. For each moratorium, results show the regression coefficients with and without controlling for COVID cases and Δ Employment. Only moratoria that are associated with significant coefficients are presented in the Table. All the coefficients are positive, implying that the eviction moratoria indicators listed produce larger year-on-year change in multi-family housing sales, as hypothesized. Adding controls decreases the magnitude of each coefficient except for moratoria on either/hearings. In case of moratoria on hardship/hearings and either/hearings, the coefficients are only significant when controls are added.

The results show that an overall moratorium and some narrower moratorium indicators subsumed in the overall indicator[49] led to significantly larger year-on-year changes in multi-family sales. The moratorium on hardship/hearings had the highest magnitude of impact on year-on-year sales change. The signs are positive showing larger sales changes, but the magnitudes of the sales changes are considerably lower than when SIP is considered as treatment.

Without controls, on average the adoption of an overall moratorium is associated with an increase in year-on-year multi-family sales changes by approximately 4 units, which is approximately a 16% increase compared to the pre-pandemic mean. These results should, however, be read with caution[50] because the results from using the DID_M estimator show opposite signs for the coefficients

rium indicators. For each moratorium, results show the regression coefficients controlling for COVID cases and Δ Employment. For single units, moratoria on all types of evictoon executions (hardship/executions, non-payment/executions, either/executions, both/executions) produce significant negative coefficients. For double and three to four units, the results are mixed, with some coefficients being positive and some negative. The coefficients on valuations show mixed signs as well. It is hence difficult to identify how the various eviction moratoria drive changes in number of units approved for single, double and 3-4 unit buildings. Since the focus of the paper is multi-family and 5+ unit buildings, these results are of secondary importance.

[49]specifically moratoria on hardship/hearings, hardship/judgements and non-payment/hearings, either/hearings and either/judgements

[50]Table A2 in the appendix shows the results from using the DID_M estimator to estimate the effect of different eviction moratoria on change in multi-family housing sales. The DID_M estimator produces

and significant results corresponding to the CDC moratorium.[51]

1.5 Conclusion

This paper attempts to evaluate the housing sales and construction responses to two COVID-19 regulations, shelter-in-place and eviction moratoria. The primary outcomes of interest, year-on-year change in housing sales and the year-on-year change in building permits approved (a proxy for future construction) are hypothesized to be smaller in the presence of shelter-in-place and eviction moratoria, respectively. The paper also explores the multi-family housing sales response to eviction moratoria.

Whether COVID continues or ends shortly, the impacts of the pandemic could have longer run consequences for the economy. Current shifts in housing sales and, more importantly, building construction patterns could have sustained effects on the housing market long after the NPIs are rescinded. This paper hence contributes to the growing literature that studies lagged and/or continued effects of COVID regulations on the housing market, intended to inform future policies, both within and outside of the COVID context.

As individual behaviour continues to go through changes owing to the pandemic and the

---

contrasting results compared to the basic TWFE estimator, both in terms of the moratoria indicators that generate significant results as well as the sign of the coefficients found. The DID_M estimator suggests that several moratoria on eviction executions significantly reduce the year-on-year sales change in multi-family houses while the CDC moratorium is associated with larger sales change in multi-family housing. Indicators for moratoria on hardship/executions, either/executions and both/executions are associated with smaller changes in year-on-year sales, albeit the magnitude of these sales change responses are very low. The adoption of the CDC moratorium is associated with an increase in change in sales, favourable to the paper's hypothesis.

[51]Without controls, on an average, the adoption of moratorium on non-payment/hearings, hardship/judgements and either/judgements are associated with an increase in year-on-year multi-family sales changes by approximately 6 units, 6 units and 5 units respectively. With controls, moratoria on hardship/hearings and either/hearings are associated with increase in year-on-year sales changes by approximately 9 and 6 units respectively. On the other hand, the CDC moratorium, moratoria on eviction executions, and moratoria on evictions caused by both hardships and non-payment are found to be associated with no significant changes in sales. These results should however be read with caution because the results from using the DID_M estimator show opposite signs for the coefficients and significant results corresponding to the CDC moratorium.

associated NPIs, knowledge about the effects of shelter-in-place is valuable in informing policy-makers of the potential housing market impacts of future implementations of similar policies. The multitude of moratorium indicators considered here can also help policy-makers identify which stages of prohibition in the eviction process cause significant changes to housing construction and which do not. Given that historically, instances of eviction moratoria are rare, the novel empirical evidence found here can be taken into account if a prohibition on evictions is considered again in the future, even outside the purview of the pandemic.

1.6 Tables and Figures

1.6.1 Tables

Table 1.1: Summary Statistics of Sales-SIP Datasets

		Single-Family	Multi-Family	Townhouse	Condominium	All Residences
	Change in Sales	4.617089	-2.440318	3.278513	-3.749082	66.96
		(86.68514)	(13.36202)	(35.80716)	(52.40302)	(306.12)
	Change in Employment	-4181.765	-14034.31	-9987.91	-10840.2	-3565.485
		(22818.97)	(46031.27)	(38144.73)	(39334.2)	(21671.32)
	COVID Cases	33225.67	112839.3	90155.99	88603.46	29567.55
		(185261.5)	(371497.3)	(337050.5)	(317511.8)	(175066.8)
	COVID Deaths	1318.362	4914.171	3423.971	3701.487	1173.126
		(8698.874)	(17655.33)	(15220.19)	(14990.94)	(8216.052)
No. of Counties	Treatment (SIP=1)	789	261	290	376	1223
	Control (SIP=0)	15	4	3	6	46
	Overall	804	265	293	382	1269

[a] Mean values are presented with standard deviations in brackets.

[b] SIP stands for Shelter-in-Place order. SIP = 1 refers to counties in which SIP was enacted in at least 1 month out of the 13 month observation period. SIP = 0 refers to counties for which SIP was never enacted.

Table 1.2: Eviction Moratorium Summary Statistics

	Reason for Eviction:	**Overall**	**Hardship**			**Non-Payment**			**Either**			**Both**		
	Strength of Eviction:		Hear.	Judge.	Exec.	Hear.	Judge.	Exec.	Hear.	Judge.	Exec.	Hear.	Judge.	Exec.
Counties	Treatment	404	31	100	108	75	122	134	87	168	173	295	215	273
	Control	91	464	395	387	420	373	361	408	327	322	200	280	222
Months	Treatment	10.27	6.97	11.71	6.42	4.32	7.11	3.86	4.69	9.37	5.44	6.08	11.4	8.66
		(7.33)	(3.12)	(6.24)	(2.87)	(2.84)	(4.36)	(2.71)	(5.99)	(2.60)	(3.33)	(4.98)	(6.04)	(7.38)

(a) "Overall" denotes overall moratorium. 'Either' and 'Both' refer to moratoria on either non-payment or hardship based evictions and moratoria on both non-payment and hardship based evictions, respectively. "Hear.", "Judge." and "Exec." refer to moratoria on eviction hearings, judgements and executions respectively.

(b) Moratorium = 1 refers to counties in which moratorium was enacted in at least 1 month out of the 36 month observation period. Moratorium = 0 refers to counties for which moratorium was never enacted. Total number of counties = 495.

(c) First 2 rows shows no. of counties in the treatment and control groups. Months row show the average number of months (standard deviation in brackets) a certain eviction moratoria was in effect across the different counties.

Table 1.3: Building Permits Summary Statistics

	No. of Units	**Single Unit**				**Double Unit**				**Three-Four Units**				**Five + Units**			
		Mean	St. dev	Min	Max	Mean	St. dev	Min	Max	Mean	St. dev	Min	Max	Mean	St. dev	Min	Max
Δ Units	Overall	6.71	78.35	-2152	1578	2.61	31.67	-208	1892	3.66	45.49	-207	2599	6.87	172.18	-2950	9517
	Between Counties		24.96	-332.94	218.53		5.397353	-4.47	73.11		8.99	-3.14	111.72		34.93	-326.86	385.81
	Within Counties		74.27	-1962.82	1366.18		31.21	-207.67	1821.49		44.56	-203.2	2490.94		168.56	-2884.79	9138.06
Δ Valuation	Overall	48.8	2140	-49800	47200	3.63	137.61	-5520	6720	17.1	574.74	-28200	32800	102.22	2910	-75200	175000
	Between Counties		534	-6.920	3290		22.48	-81.38	202.71		79.28	-155.16	905.79		559.09	-3960	6720
	Within Counties		2070	-51600	45300		1357.58	-5590	6650		569.24	-28000	31900		2860	-72400	168000

[a] Valuation statistics are in 10000's of dollars.

[b] Overall mean, standard deviations, min and max are calculated across all 495 counties and 36 months with total observation count of 36*495 = 17820.

[c] "Over Counties" standard deviations, min and max are calculated at the county level over 495 counties.The observations used are county-specific month-level averages.

[d] "Within counties" standard deviations, min and max are calculated over months within each county. The observations used are obtained by subtracting the "Over counties" mean and adding the global mean to each original observation.

Table 1.4: Eviction Moratoria and Multi-family Housing Sales Summary Statistics

	Reason for Eviction:	**Overall**	**Hardship**			**Non-Payment**			**Both**			**Either**		
	Strength of Eviction:		Hear.	Judge.	Exec.	Hear.	Judge.	Exec.	Hear.	Judge.	Exec.	Hear.	Judge.	Exec.
Counties	Treatment	213	21	79	86	52	58	106	153	115	182	61	89	126
	Control	21	213	155	148	182	176	128	81	119	52	173	145	108
Δ Sales	Treatment	2.71	17.82	9.88	0.01	7.04	8.64	-3.81	0.49	5.34	0.49	9.70	8.52	0.10
		(23.55)	(34.04)	(28.55)	(25.57)	(20.05)	(24.27)	(22.26)	(23.7)	(36.16)	(24.01)	(30.14)	(27.65)	(23.42)
	Control	2.07	2.01	1.87	2.20	2.07	2.07	2.05	1.83	2.07	2.13	1.99	1.92	2.07
		(12.4)	(9.61)	(8.05)	(8.45)	(9.38)	(9.34)	(8.77)	(2.63)	(7.73)	(8.38)	(9.14)	(8.22)	(6.75)
Months	Treatment	12.9	8.38	12.71	6.20	4.38	10.98	3.52	7.24	14.61	9.57	4.72	12.46	5.27

(a) "Overall" denotes overall moratorium. 'Either' and 'Both' refer to moratoria on either non-payment or hardship based evictions and moratoria on both non-payment and hardship based evictions, respectively. "Hear.", "Judge." and "Exec." refer to moratoria on eviction hearings, judgements and executions respectively.

(b) Treatment refers to counties in which moratorium was enacted in at least 1 month out of the 36 month observation period. Control refers to counties for which moratorium was never enacted. Total number of counties = 495. Hence the first 2 rows shows no. of counties in the treatment and control groups.

(c) *Δ* Sales shows the average of county level means (and average of county-level standard deviations) of change in sales in the treated and control counties.

(d) Months row show the average number of months a certain eviction moratoria was in effect across the different counties.

Table 1.5: Demographic Summary of Counties

	Residential Sales Datasets					Building Permits	Multi-family Sales	Entire US
	Single	Multi	Townhouse	Condo	All Res			
Total male %	49.40	49.37	49.37	49.34	47.69	49.36	49.34	49.53
Total female %	50.60	50.63	50.63	50.66	50.54	50.64	50.66	50.47
White alone male %	36.91	36.34	35.99	36.45	37.56	35.99	36.23	37.85
White alone female %	37.19	36.63	36.22	36.78	37.83	36.24	36.54	38.05
Black alone male %	6.77	6.79	7.09	6.83	6.42	7.09	6.87	6.52
Black alone female %	7.49	7.45	7.80	7.52	6.96	7.79	7.55	7.02
Indian American alone Male %	0.56	0.56	0.53	0.52	0.57	0.55	0.56	0.66
Indian American alone Female %	0.54	0.54	0.54	0.51	0.56	0.53	0.54	0.65
Asian alone Male %	3.58	4.02	4.13	3.92	3.35	4.12	4.05	2.93
Asian alone Female %	3.86	4.33	4.45	4.21	3.61	4.43	4.35	3.16
Two or more Races Male %	1.45	1.52	1.49	1.49	1.43	1.49	1.50	1.44
Two or more Races Female %	1.49	1.55	1.52	1.52	1.46	1.52	1.54	1.46

[a] Average percentages are presented for a cumulative of all age groups dated July 2021. Data is sourced from US Census Bureau county level demographic data. Single and Multi denote Single-Family and Multi-Family houses respectively.

Table 1.6: TWFE results from regressing change in Sales on SIP

	Δ Sales		
	Single-Family	Condo	All Residences
	(1)	(2)	(3)
SIP	-29.02^{***}	-17.03^{***}	-79.52^{***}
	(4.93)	(3.86)	(10.49)
+ COVID Cases	-29.19^{***}	-17.09^{***}	-76.90^{***}
	(4.90)	(3.90)	(10.80)
+ Δ Employment	-24.38^{***}	-14.54^{***}	-52.32^{***}
	(4.06)	(3.29)	(8.13)
+ Both	-22.13^{***}	-15.57^{***}	-51.48^{***}
	(3.62)	(3.24)	(8.05)
Overall Mean	4.62	−3.75	66.96
Pre-pandemic mean	91.25	36.42	115.48
N(Treated)	789	376	1223
N(Control)	15	6	46
N	804	382	1269

Standard errors are clustered at county level and are presented in parentheses.
*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. N stands for total no. of counties. N(Treated) and N(Control) stand for number of treated and control counties, respectively.
Overall means show the average year-on-year changes for each housing type across the counties and months contained in the dataset.
Pre-pandemic means stand for average year-on-year changes for each housing type across the counties in January 2020 before the pandemic began.

Table 1.7: DID_M results from regressing change in Sales on SIP

	Δ **Sales**		
	Single-Family	Condo	All Residences
	(1)	(2)	(3)
SIP	$-23.28^{***\dagger}$	$-14.82^{***\dagger}$	-27.81^{***}
	(4.27)	(3.78)	(5.11)
State_trends	$-24.48^{***\dagger}$	$-15.05^{***\dagger}$	-28.59^{***}
	(3.59)	(3.61)	(5.42)
Adding Controls			
+ COVID Cases	$-21.22^{***\dagger}$	-13.85^{***}	$-24.03^{***\dagger}$
	(4.14)	(4.34)	(6.14)
State_trends	$-21.96^{***\dagger}$	-13.50^{***}	$-24.00^{***\dagger}$
	(4.11)	(3.82)	(5.30)
+ Δ Employment	$-17.70^{***\dagger}$	-6.90^{*}	$-8.56^{*\dagger}$
	(3.43)	(3.81)	(5.14)
State_trends	$-19.12^{***\dagger}$	-5.87	-8.54
	(3.03)	(3.84)	(5.04)
+ Both	$-17.43^{***\dagger}$	-7.1^{**}	-7.82
	(4.14)	(3.56)	(5.26)
State_trends	$-18.51^{***\dagger}$	-5.98	-7.07
	(4.18)	(3.89)	(5.04)
Overall Mean	4.62	-3.75	66.96
Pre-pandemic mean	91.25	36.42	115.48
N	3796	1126	3803
$N\_switchers$	2485	718	2429
$N\_switchers\_cont$	90	47	91

Standard errors are clustered at county level and are presented in parentheses.

*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively.

† implies that for significant DID_M estimators, the placebo effect is not significantly different from 0 at the 10% significance level.

Overall means show the average year-on-year changes for each housing type across the counties and months contained in the dataset.

Pre-pandemic means stand for average year-on-year changes for each housing type across the counties in January 2020 before the pandemic began.

Table 1.8: TWFE of regressing change in Building Permits on Eviction Moratorium

	5+ Units Building Permits			
	Δ Units	Δ Valuation	N(Treat)	N(Control)
Treatments	(1)	(2)	(3)	(4)
Overall Moratorium	-8.92^{*} (4.96)	-15.21 (78.81)	404	91
+ Controls	-13.00^{**} (5.92)	116.89 (91.01)	404	91
Non-Payment/Hearing	-18.34^{*} (10.23)	-283.11^{*} (147.81)	75	420
+ Controls	-22.27^{**} (10.42)	-375.85^{**} (166.22)	75	429
Hardship/Judgements	-7.04 (8.16)	21.04 (147.55)	100	395
+ Controls	-17.78^{**} (9.05)	-179.71 (185.93)	100	395
Either/Hearings	-12.33 (9.19)	187.64 (133.40)	87	408
+ Controls	-18.85^{**} (9.38)	-311.12^{**} (149.54)	87	408
Overall or CDC	-8.59^{**} (4.35)	-36.41 (65.70)	404	91
+ Controls	-15.46^{***} (5.19)	-164.84^{**} (76.69)	404	91

Standard errors are clustered at county level and are presented in parentheses.
*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. Controls include both COVID cases and Δ Employment.
Coefficients for ΔValuation are in 10000's of dollars.

Table 1.9: DID_M results from regressing change in Building Permits on Eviction Moratorium

	5+ Units Building Permits			
	Δ Units	Δ Valuation	$N\_switchers$	N
Treatments	(1)	(2)	(3)	(4)
Overall Moratorium	-11.29 (8.03)	-135.83 (200.25)	891	3920
+ Controls	$-12.67^{*\dagger}$ (7.12)	-169.50 (260.90)	816	3248
+ State_trends	$-12.32^{*\dagger}$ (6.51)	-165.28 (219.71)	816	3248
Non-Payment/Hearings	-43.83^{**} (22.10)	-334.94 (599.55)	95	2566
+ Controls	(-44.56) (35.23)	-515.56 (602.95)	77	2044
+ State_trends	-39.54 (33.15)	-528.49 (589.20)	77	2044
Non-Payment/Executions	$-29.61^{*\dagger}$ (16.81)	15.80 (295.94)	245	3583
+ Controls	-26.95 (18.40)	156.10 (294.22)	225	3165
+ State_trends	-26.96 (19.95)	169.10 (387.24)	225	3165
Both/Executions	-2.95 (11.33)	$330.10^{*\dagger}$ (184.89)	598	3445
+ Controls	1.45 (10.96)	360.06 (227.36)	549	2894
+ State_trends	1.44 (11.07)	359.93 (175.68)	549	2894

Standard errors are clustered at county level and are presented in parentheses. *, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. † implies that for significant DID_M estimators, the placebo effect is not significantly different from 0 at 10% significance level.
Controls include both COVID cases and Δ Employment.

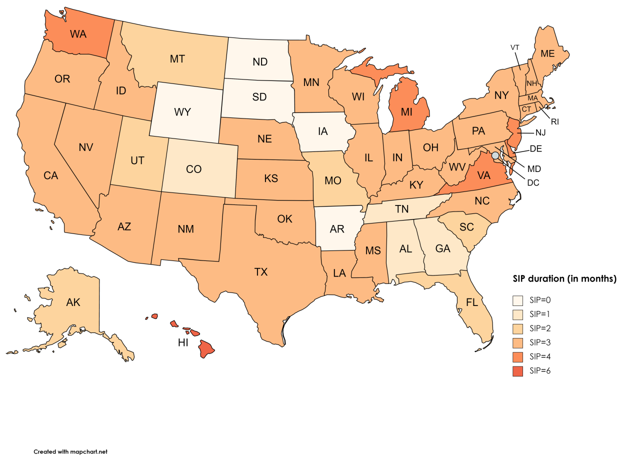
Table 1.10: TWFE of regressing change in Building Permits on Eviction Moratorium

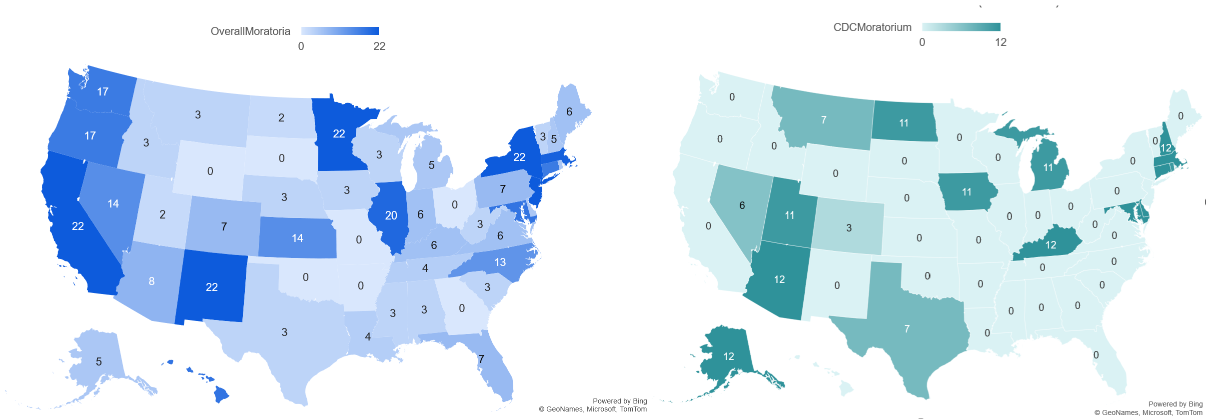
Treatments	Δ **Sales** (1)	N(Treat) (2)	N(Control) (3)
Overall Moratorium	4.24*** (1.21)	213	21
+ Controls	3.11*** (1.02)		
Hardship/Hearings	10.20 (1.50)	21	213
+ Controls	9.70* (5.33)		
Non-Payment/Hearings	5.62** (2.54)	52	182
+ Controls	6.17*** (2.00)		
Hardship/Judgements	6.07** (2.87)	79	155
+ Controls	2.87* (1.63)		
Either/Hearings	6.13 (4.10)	61	173
+ Controls	6.26* (3.20)		
Either/Judgements	5.16** (2.61)	89	145
+ Controls	2.56* (1.49)		

Standard errors are clustered at county level and are presented in parentheses. *, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. Controls include both COVID cases and Δ Employment.

1.6.2 Figures

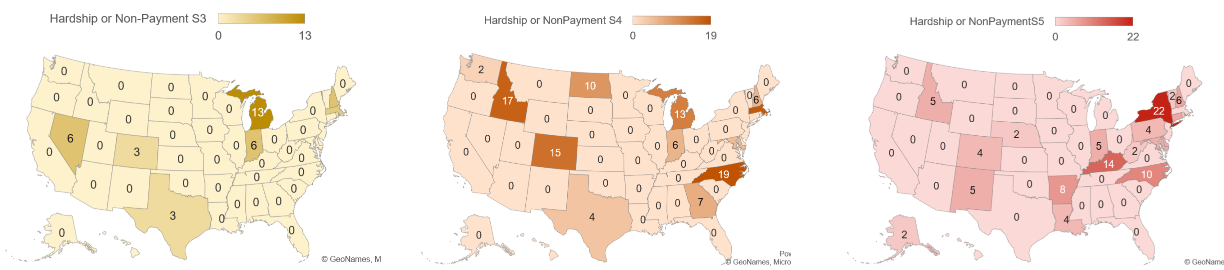
Figure 1.1: Shelter-in-Place orders in U.S. states





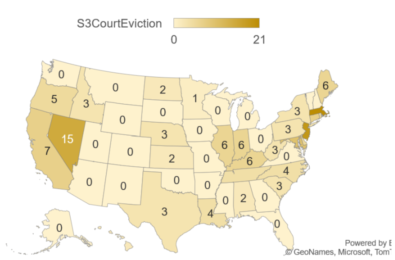
(a) Overall Moratoria (b) CDC Moratorium

Figure 1.2: Duration of overall and CDC Eviction Moratoria indicators (in months)

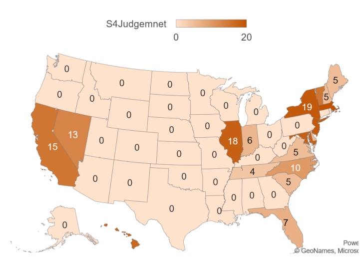


(a) Moratoria on hearings (b) Moratoria on judgements (c) Moratoria on executions

Figure 1.3: Duration (in months) of either-cause (hardship or non-payment) based eviction moratoria



(a) Moratoria on hearings



(b) Moratoria on judgements

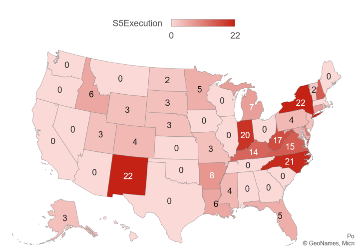
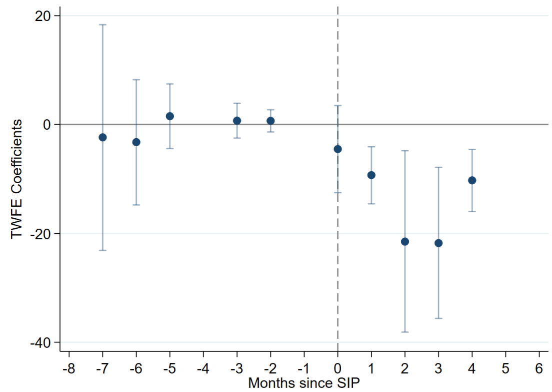
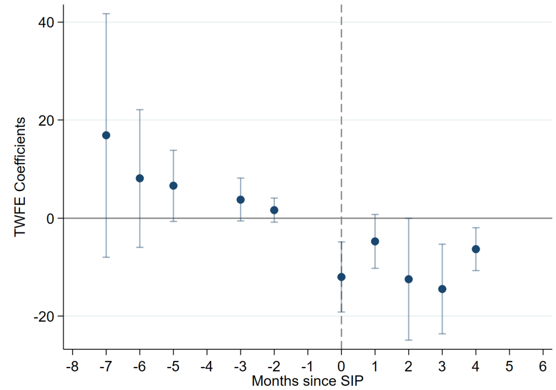


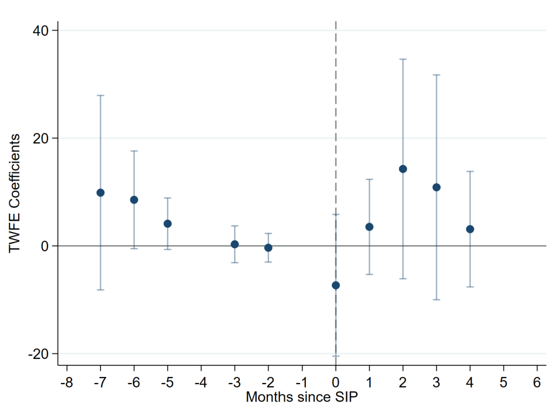
Figure 1.4: Duration (in months) of moratorium on both (non-payment and hardship caused) eviction



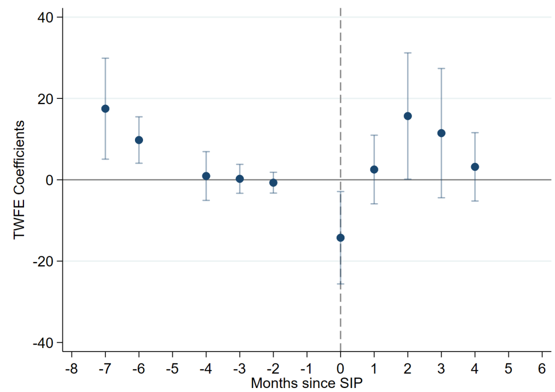
(a) Δ Sales for Single-Family houses without controls



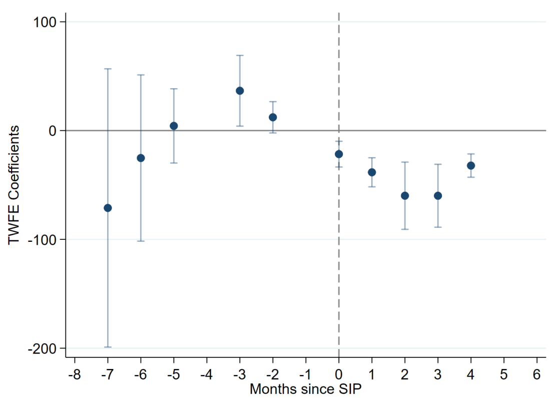
(b) Δ Sales for Single-Family Houses with controls



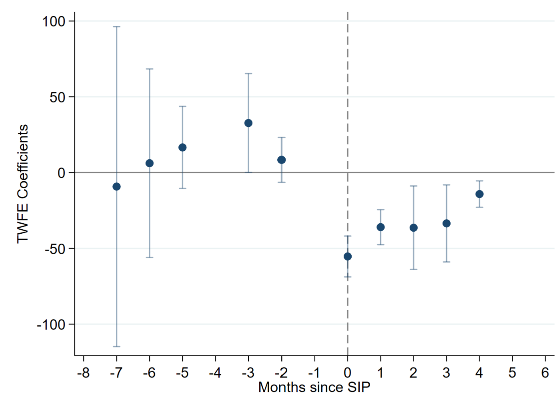
(c) Δ Sales for Condos without controls



(d) Δ Sales for Condos with controls

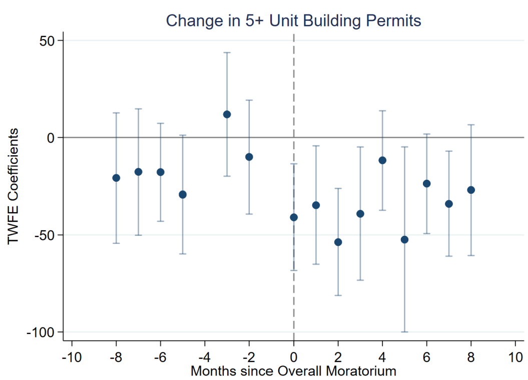


(e) Δ Sales for All Residential Houses without controls

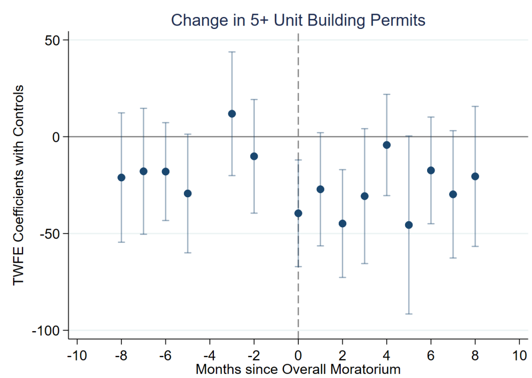


(f) Δ Sales for All Residential Houses with controls

Figure 1.5: TWFE event-study graphs with 95% CI of SIP beginning



(a) Overall Eviction Moratorium without controls



(b) Overall Eviction Moratorium with controls

Figure 1.6: TWFE Event-study graphs with 95% confidence intervals of Overall Eviction Moratotoria beginning

Chapter 2

The Intercity Impacts of Work-from-Home in a Spatial Hedonic Model with Remote and Non-Remote Workers

2.1 Introduction

Work-from-home (WFH) has increased since the onset of the coronavirus (COVID-19) pandemic. Before the pandemic, 17 percent of U.S. employees worked from home 5 days or more per week, a share that increased to 44 percent during the pandemic,[1] helping to spur expenditures to upgrade the home workspace.[2] Within a given city, WFH enables residents to work without physically trav-

[1]Statistics from https://www.statista.com/statistics/1122987/change-in-remote-work-trends-after-covid-in-usa/

[2]Interviews with major employers and monthly surveys from May 2020 to March 2021 of persons aged 20–64 who earned at least $20,000 in 2019 find that on average, workers invested 15 hours of time and $560 to upgrade their home work spaces. See https://www.weforum.org/agenda/2021/07/work-form-home-hybrid-working-covid-pandemic-us-office

eling to their offices, thus reducing commuting cost. Looking across cities, WFH enables employees to relocate to a different city while continuing to work remotely in their original city. Breaking the connection between residence and employment location offers an opportunity to rethink urban spatial models, which otherwise assume that people work in the city where they live.

Brueckner, Kahn & Lin (2021)[3] explore the intercity effects of the introduction of WFH and predict how WFH would change the spatial hedonic equilibrium. BKL analyze the patterns of WFH-induced changes in employment levels and populations as well as wages and housing prices across cities, exploring the effects of two city characteristics on the outcomes: productivity and amenities. BKL find that a shift to WFH causes some residents to relocate from high-productivity cities to low-productivity cities, which have cheaper housing, while maintaining their jobs in the original city. BKL also find that WFH leads some residents to relocate from low-amenity cities to high-amenity ones despite their expensive housing, while keeping their original jobs. In both cases, housing prices rise in the receiving city and fall in the one losing population.

While the model used by BKL attempts to capture the shift in the spatial equilibrium arising from the introduction of WFH, it unrealistically assumes that all workers are able to work remotely. The purpose of the current paper is to extend BKL's model to incorporate two kinds of workers, those who can work remotely and those who cannot. In the paper, only remote workers, whose jobs offer the option to telecommute, can now relocate to cities with better residential advantages while maintaining their jobs in other cities. Non-remote workers still have to live in the city where they work, implying that if they wish to relocate, they would need to find a job in the new city. The research question that the current paper attempts to answer is how the intercity impacts of WFH differ in this more realistic scenario while keeping the same general extension of the Rosen-Roback model that BKL employ.

Following BKL's two-city setup, San Francisco (city s) has better amenities and higher worker productivity than Detroit (city d). As in BKL, city land areas are fixed, implying that a higher population raises housing prices and reduces housing consumption. The total population (and hence,

[3]Referred to as BKL, from this point onward.

total employment) of each type of worker is fixed across the two cities. Remote and non-remote workers are imperfectly substitutable, being complements in the production function. Hence, the wage earned by each type of worker is dependent on the employment level of the other type. A resident's welfare is a function of the wage they earn, the amenities they enjoy, and net housing utility (utility from housing consumption minus costs). Residential relocation ensures that utilities are equalized across cities for each worker type with and without WFH. But since remote workers can choose their work locations independent of residence, they must be indifferent among them in equilibrium, implying equalization of wages across cities for these workers. While this conclusion follows BKL, wage-equalization for non-remote workers need not occur. Note that, as in BKL, the model ignores the internal spatial structure of cities and thus the effect of WFH on central versus suburban location choices, focusing only on inter-city choices.

Without WFH, city-level employment and population must be equal for both types of workers. Once WFH is introduced, however, the equality between population and employment need not hold for remote workers. In one city, for example, the remote population could exceed employment because some of the remote residents are living in the city while working remotely in the other city. Unlike the BKL model, however, the population-employment equality must still hold for non-remote workers. In contrast to BKL, the analysis uses explicit functional forms for production and utility functions, which is necessary to derive results given the greater complexity of the model.

BKL predict that if the only advantage of city s is better amenities, the introduction of WFH raises its population and housing price, while the opposite occurs in the lower-amenity city (city d). In addition, employment falls (rises) in city s (city d), changing in the opposite direction to population, and some continuing residents of city s now work remotely in city d, as do the city's new residents. On the other hand, if the only advantage of city s is higher productivity, BKL show that WFH reduces its population and housing price, while the opposite occurs in city d. Employment rises (falls) in city s (city d), again moving in the opposite direction to population. Some continuing residents of city d now work remotely in city s, as do the city's new residents.

The findings from the current analysis mostly match those of BKL, with the total and remote-worker population levels along with housing prices responding to WFH in the same manner as prices and populations in BKL (which consist entirely of remote workers). Mirroring the employment findings of BKL, remote, non-remote and total employment in the current model also move in opposite directions to population in both the amenity and productivity cases. The result that does not conform to BKL's conclusions, however, is the WFH response of non-remote populations. Unexpectedly, in the amenity case, the non-remote population falls in city s (rising in city d) under WFH, the reverse of remote and total populations. Similarly, in the productivity case, the non-remote population rises in city s and falls in city d, reversing the direction of change of remote and total populations. While some research (see below) predicts welfare effects from WFH, the current analysis yields zero welfare changes for both types of workers in both the amenity and productivity cases. Wage changes exactly offset the effects of housing price changes, leaving welfare unchanged. Welfare effects were ambiguous in BKL, although imposition of specific functional forms might have given a determinate answer in their model.

The overall analysis thus concludes that the main results of BKL are unaffected by the addition of a group of non-remote workers. While this welcome conclusion may not be remarkable, the only way to reach it is by going through the complicated analysis of the extended model. The contribution of the present paper is to carry out this analysis, providing a useful contribution to the WFH literature.

This literature has burgeoned since the onset of the COVID-19 pandemic. The current analysis resembles Delventhal, Kwon & Parkhomenko (2020), Delventhal & Parkhomenko (2020) and Gokan, Kichko and Thisse (2021) most closely. Gokan, Kichko & Thisse (2021) develop both single-city and two-city models (where one city is more productive than the other) to study how different intensities of telecommuting affect the spatial and social organization of cities. They find that the size of the more productive city shrinks and that of the less productive city increases with WFH, analogous to the findings from the productivity case in the current paper. Delventhal and Parkhomenko (2020) (DP) and Delventhal, Kwon & Parkhomenko (2020) (DKP) study quantitative spatial models of firm and worker location choices, in which some workers can substitute on-site workplace effort with WFH based on factors related to commuting time and cost. In terms of how the work-force is divided, both DP and DKP consider telecommutable and non-telecommutable workers, as in the current paper. Further, the general equilibrium model employed by DKP also makes the residence choice dependent on wages, housing cost and amenities, similar to the current model. However, unlike DKP, the current paper does not include idiosyncratic location preferences and commuting time as endogenous factors.

The current model is also related to recent work by Davis, Ghent & Gregory (2021) and Behrens, Kichko & Thisse (2021). Both of these papers develop stylized city models with office and remote work to study the implications of greater WFH on the demand for floorspace, productivity, income inequality, and city structure. They do so by dividing workers based on their skill-levels and then connecting skill-levels of workers to their ability to telecommute, as in Gokan, Kichko & Thisse (2021). The current paper draws upon the same remote versus non-remote distinction across workers, but in a simpler framework that does not include workers' skill levels. The current model is also related to the monocentric city models of WFH employed by Safirova (2002) and Davis, Ghent & Gregory (2021). Safirova (2002) considers a closed general equilibrium model where land is allocated to production, housing, and roads to study effects of WFH on the urban economy and land use pattern. Davis, Ghent & Gregory (2021) study the impact of WFH using an equilibrium model where people choose where to live, how to allocate their time between remote and non-remote work, and how much space to use in production. A key parameter in both their analyses is the elasticity of substitution in production between office-workers and telecommuters. Aligning with their approach, the current paper also incorporates imperfect substitution between worker types via complementarity in the production function.[4]

The rest of the paper proceeds as follows. Section 2.2 presents a general version of the model without imposing functional forms, culminating in the presentation of equilibrium conditions with and without WFH. Section 2.3 introduces explicit functional forms and derives closed-from equilibrium solutions. The analysis in section 2.4 then derives the changes in populations, employment levels, housing prices, wages and welfare levels in the two cities under WFH. Finally, section 2.5

[4]For empirical studies on WFH, see Althoff et, al. (2021), Bartik et al. (2020), Bloom et al. (2020), Bloom & Ramani (2021), Brueckner et al. (2021), Brynjolfsson et al. (2020), Gupta et al. (2021) and Stanton & Tiwari (2021).

presents comparative analysis, showing how the magnitudes of the changes under WFH depend on parameter values. Section 2.6 presents conclusions. Derivations of the key results are provided in the appendix.

2.2 Theoretical Framework

The model has two cities, denoted s (San Francisco) and d (Detroit), with equal fixed land areas, and two types of workers, denoted type 1 and type 2, which differ in their ability to work from home. Type 1 workers are remote workers who can live and work in different cities with the introduction of WFH, and type 2 workers are non-remote workers who always live in the same city where they work.

The employment of type i workers in city j is given by L_{ij}, while the population of type i workers in city j is given by N_{ij}. Assuming that all residents in both cities of both types are employed, the following relations give total population and employment levels by city and worker type:[5]

$$\overline{N_j} = N_{1j} + N_{2j}\,, \qquad j = s, d \tag{2.1}$$

$$\overline{L_j} = L_{1j} + L_{2j}\,, \qquad j = s, d \tag{2.2}$$

$$\overline{N_i} = N_{is} + N_{id}\,, \qquad i = 1, 2 \tag{2.3}$$

$$\overline{L_i} = L_{is} + L_{id}\,, \qquad i = 1, 2 \tag{2.4}$$

$$\overline{N_i} = \overline{L_i}\,, \qquad i = 1, 2 \tag{2.5}$$

$$2\overline{N} = 2\overline{L} = \sum_{i=1}^{2}\sum_{j=s,d} N_{ij} = \overline{N_1} + \overline{N_2} = \overline{N_s} + \overline{N_d} \tag{2.6}$$

Equations (1) and (2) give total city population and employment in terms of worker-type

[5]Equations (1)-(6) involve a slight abuse of notation, adopted for simplicity. In particular, $\overline{N}$ refers to both total population of a city and total population of a worker-type, with the distinction captured only by whether the subscript is s or d versus 1 or 2. The same point applies to $\overline{L}$.

employment and population levels in the city. Equations (3) and (4) give total population and employment by type of worker in terms of city-level employment and population for the type. Equation (5) says that the total population of each type of worker is equal to the type's total employment. Across cities and worker types, the aggregate population (given by $2\overline{N}$) equals the aggregate employment (given by $2\overline{L}$), as shown in equation (6).

Consumer utility in city j is given by the quasi-linear function $u(e_{ij}, q_j, Aj) = A_j + e_{ij} + v(q_j)$, where q_j is housing (land) consumption and e_{ij} is non-housing consumption. Note that amenities and non-housing consumption units are chosen such that their linear utility coefficients are same and equal to unity.

A worker's wage depends on type and on city of residence, both of which may influence productivity. The wage of a type-i worker in city j is given by $w_{ij}(L_{1j}, L_{2j})$, with the employment levels of both types affecting each type's wage. Denoting the unit housing price in city j by p_j, the budget constraint for a type-i worker in city j may be written as $e_{ij} + p_j q_j = w_{ij}(L_{1j}, L_{2j})$. Consumer utility can then be expressed as

$$A_j + w_{ij}(L_{1j}, L_{2j}) + v(q_j) - p_j q_j$$

City land areas are normalized at unity, which makes housing consumption the reciprocal of the total population of the city, yielding

$$q_j = \frac{1}{\overline{N_j}} = \frac{1}{N_{1j} + N_{2j}}$$

which implies that

$$v(q_j) = v\left(\frac{1}{N_{1j} + N_{2j}}\right)$$

The net housing utility is defined as $H \equiv v(q_j) - p_j q_j$, where $p_j = v'(q_j)$ from the housing first order condition. Hence, in terms of total city population, net housing utility can be written

$$H(N_{1j} + N_{2j}) = v\left(\frac{1}{N_{1j} + N_{2j}}\right) - v'\left(\frac{1}{N_{1j} + N_{2j}}\right)\left(\frac{1}{N_{1j} + N_{2j}}\right)$$

Consumer utility in city j may be rewritten in terms of net housing utility as follows:

$$utility_{ij} = A_j + w_{ij}(L_{1j}, L_{2j}) + H(N_{1j} + N_{2j}), \quad i = 1, 2, \ j = s, d \tag{2.7}$$

2.2.1 Non-WFH Equlibrium

In the equilibrium without WFH, the consumer utility expression in (7) is equalized between the two cities via migration of each type of worker. The non-WFH equilibrium condition for remote (type 1) workers is then

$$A_s + w_{1s}(L_{1s}, L_{2s}) + H(N_{1s} + N_{2s}) = A_d + w_{1d}(L_{1d}, L_{2d}) + H(N_{1d} + N_{2d}) \tag{2.8}$$

The non-WFH equilibrium condition for non-remote (type 2) workers is

$$A_s + w_{2s}(L_{1s}, L_{2s}) + H(N_{1s} + N_{2s}) = A_d + w_{2d}(L_{1d}, L_{2d}) + H(N_{1d} + N_{2d}) \tag{2.9}$$

In the absence of WFH, both remote and non-remote workers have to reside in the city they work in. Hence, employment equals the population of each type of worker in each city, yielding

$$L_{1s} = N_{1s}$$

$$L_{2s} = N_{2s}$$

$$L_{1d} = N_{1d}$$

$$L_{2d} = N_{2d}$$

Substituting for L_{ij} in the wage functions, they become $w_{ij}(N_{1j}, N_{2j}), i = 1, 2, j = s, d$. Modified city-$d$ wage functions, obtained by substituting $N_{1d} = \overline{N_1} - N_{1s}$ and $N_{2d} = \overline{N_2} - N_{2s}$ from relation (4) into the wage functions in equations (8) and (9), are written as:

$$\widehat{w_{1d}}\left(N_{1s}, N_{2s}\right) = w_{1d}\left(\overline{N_1} - N_{1s}, \overline{N_2} - L_{2s}\right)$$

$$\widehat{w_{2d}}\left(N_{1s}, N_{2s}\right) = w_{2d}\left(\overline{N_1} - N_{1s}, \overline{N_2} - N_{2s}\right)$$

Also, a modified H function, obtained by replacing $N_{1d} + N_{2d} = 2\overline{N} - (N_{1s} + N_{2s})$ from relation (5) into the H functions on the RHS of equations (8) and (9), is written as

$$\widehat{H}(N_{1s} + N_{2s}) = H(2\overline{N} - (N_{1s} + N_{2s}))$$

Using the modified wage and H functions (so as to remove city d's employment or populations from equations (8) and (9)), the non-WFH equilibrium conditions for remote and non-remote workers, respectively, are

$$A_s + w_{1s}(N_{1s}, N_{2s}) + H(N_{1s} + N_{2s}) = A_d + \widehat{w_{1d}}(N_{1s}, N_{2s}) + \widehat{H}(N_{1s} + N_{2s}) \tag{2.10}$$

$$A_s + w_{2s}(N_{1s}, N_{2s}) + H(N_{1s} + N_{2s}) = A_d + \widehat{w_{2d}}(N_{1s}, N_{2s}) + \widehat{H}(N_{1s} + N_{2s}) \tag{2.11}$$

Solving equations (10) and (11) simultaneously then yields $N^*_{1s}(\delta, A_s, A_d, \overline{N_1})$ and $N^*_{2s}(\delta, A_s, A_d, \overline{N_2})$ as the non-WFH equilibrium population/employment of remote and non-remote workers, respectively in city s. Using (3), the city d counterparts are $N^*_{1d}(\delta, A_s, A_d, \overline{N_1})$ and $N^*_{2d}(\delta, A_s, A_d, \overline{N_2})$. In these expressions, δ is a vector capturing productivity effects specific to types and cities.

2.2.2 WFH Equlibrium

In the equilibrium with WFH, since type-1 workers can work from home, they no longer need to reside in the city they are working in. Hence, while type-2 employment equals the population of type-2 workers in each city, s and d may now have unequal employment and population levels for type-1 workers, yielding

$$L_{1s} \neq N_{1s}$$

$$L_{2s} = N_{2s}$$

$$L_{1d} \neq N_{1d}$$

$$L_{2d} = N_{2d}$$

The WFH equal-utility condition for remote (type 1) workers is analogous to equation (10) from the non-WFH equilibrium model, with the same modified wage and H functions, but now without employment-population equalization for remote workers. It is written

$$A_s + w_{1s}(L_{1s}, N_{2s}) + H(N_{1s} + N_{2s}) = A_d + \widehat{w_{1d}}(L_{1d}, N_{2d}) + \widehat{H}(N_{1s} + N_{2s}) \tag{2.12}$$

where L_{1s} replaces N_{1s} and L_{1d} replaces N_{1d}.

Under WFH, remote workers can migrate across cities while also working at a job in the city they do not live in. In equilibrium, indifference between work locations must hold, implying equalization of wages for type-1 workers across cities, yielding

$$w_{1s}(L_{1s}, N_{2s}) = w_{1d}(L_{1d}, N_{2d})$$

Using the modified wage-function formulation as before, remote worker wage equalization across cities implies

$$w_{1s}(L_{1s}, N_{2s}) = \widehat{w_{1d}}(L_{1s}, N_{2s}) \tag{2.13}$$

Substituting this wage-equalization condition into (12), the equilibrium condition reduces to

$$A_s + H(N_{1s} + N_{2s}) = A_d + \widehat{H}(N_{1s} + N_{2s}), \tag{2.14}$$

with wages dropping out.

On the other hand, the WFH equilibrium condition for non-remote (type 2) workers is analogous to equation (11) from the non-WFH model with the same modified wage and H functions, but now without the employment-population equalization for remote workers. It is written

$$A_s + w_{2s}(L_{1s}, N_{2s}) + H(N_{1s} + N_{2s}) = A_{1d} + \widehat{w_{2d}}(L_{1s}, N_{2s}) + \widehat{H}(N_{1s} + N_{2s}) \tag{2.15}$$

Solving equations (13)-(15) simultaneously then yields $\widetilde{N_{1s}}(\delta, A_s, A_d, \overline{N_1})$ as the WFH equilibrium population of remote workers, $\widetilde{N_{2s}}\ (\delta, A_s, A_d, \overline{N_2})$ as the WFH equilibrium population/employment of non-remote workers, and $\widetilde{L_{1s}}\ (\delta, A_s, A_d, \overline{L_1})$ as the WFH equilibrium employment of remote workers in city s. Using (3) and (4), the city d counterparts are $\widetilde{N_{1d}}(\delta, A_s, A_d, \overline{N_1})$, $\widetilde{N_{2d}}(\delta, A_s, A_d, \overline{N_2})$ and $\widetilde{L_{1d}}\ (\delta, A_s, A_d, \overline{L_1})$.

Another more remarkable implication of the equilibrium conditions concerns type-2 wages. While type-1 wages are equalized across cities under WFH, the equilibrium conditions imply that the same conclusion applies to type-2 wages, which are also equalized. This conclusion follows because using (14), the non-wage terms in the equilibrium condition (15) then cancel, so that the condition yields wage equality. Thus, while the ability to work remotely implies indifference across worksites (and hence equal wages) for type-1 workers, this condition in conjunction with utility-equalization for type-2 workers (via (15)) requires that type-2 wages must also be equalized even though indifference across worksites is not an original equilibrium condition for these workers.

2.3 Explicit Functional Forms

In this section, explicit functional forms for wages and utility from housing consumption are introduced. In order to derive the wage function, the following production function is assumed:

$$F(L_{1j}, L_{2j}) = a_{1j}L_{1j} - L_{1j}^2 + a_2 L_{2j} - L_{2j}^2 + cL_{1j}L_{2j}, \qquad j = s, d$$

where c is a parameter capturing the degree of complementarity across worker types. The type-1 productivity parameters a_{1s} and a_{1d} satisfy $a_{1s} \geq a_{1d}$, indicating higher productivity in city s, while type-2 productivity (captured by a_2) is the same across cities.

Normalizing the output price to 1, the wage functions[6] are

$$w_{1j}(L_{1j}, L_{2j}) = \frac{\partial F}{\partial L_1} = a_{1j} - 2L_{1j} + cL_{2j}$$

$$w_{2j}(L_{1j}, L_{2j}) = \frac{\partial F}{\partial L_2} = a_2 - 2L_{2j} + cL_{1j}$$

Hence,

$$w_{1s}(L_{1s}, L_{2s}) = a_{1s} - 2L_{1s} + cL_{2s} \tag{2.16}$$

$$w_{2s}(L_{1s}, L_{2s}) = a_2 - 2L_{2s} + cL_{1s} \tag{2.17}$$

$$w_{1d}(L_{1d}, L_{2d}) = a_{1d} - 2L_{1d} + cL_{2d} = a_{1d} - 2(\overline{L_1} - L_{1s}) + c(\overline{L_2} - L_{2s}) = \widehat{w_{1d}}(L_{1s}, L_{2s}) \tag{2.18}$$

$$w_{2d}(L_{1d}, L_{2d}) = a_2 - 2L_{2d} + cL_{1d} = a_{2d} - 2(\overline{L_2} - L_{2s}) + c(\overline{L_1} - L_{1s}) = \widehat{w_{2d}}(L_{1s}, L_{2s}) \tag{2.19}$$

[6]Parametric restrictions required for positive wages, involving α, β, a_{ij}, $(\forall i, j)$ and c, are assumed to hold.

The responses of wages with respect to employment levels are as follows:

$$\frac{\partial w_{1j}(L_{1j},L_{2j})}{\partial L_{1j}} = \frac{\partial w_{2j}(L_{1j},L_{2j})}{\partial L_{2j}} = -2 < 0 \quad , \quad \frac{\partial w_{1j}(L_{1j},L_{2j})}{\partial L_{2j}} = \frac{\partial w_{2j}(L_{1j},L_{2j})}{\partial L_{1j}} = c > 0$$

Thus, wages decrease with own employment and rise with employment of the other type.

The utility from housing consumption is assumed to take the following functional form:

$$v(q_j) = \alpha - \frac{\beta}{q_j}, \qquad j = s, d,$$

where α, β are positive (note that β is otherwise unrestricted in magnitude). This assumption implies $p_j = v'(q_j) = \beta/q_j^2$ from the first order condition for housing consumption, yielding $p_j q_j = \beta/q_j$. Consequently, net housing utility is $v(q_j) - p_j q_j = \alpha - 2\beta/q_j$.

Since $q_j = \frac{1}{N_{1j}+N_{2j}}$ (due to city land areas being normalized to unity), it follows that

$$p_j = \frac{\beta}{q_j^2} = \beta(N_{1j}+N_{2j})^2 \equiv p(N_{1j}+N_{2j})$$

$$H(N_{1j}+N_{2j}) = \alpha - 2\beta(N_{1j}+N_{2j}),$$

noting that $p_j q_j = \beta(N_{1j}+N_{2j})$. Hence,

$$H(N_{1s}+N_{2s}) = \alpha - 2\beta(N_{1s}+N_{2s}) \tag{2.20}$$

$$H(N_{1d}+N_{2d}) = \alpha - 2\beta(N_{1d}+N_{2d}) = \alpha - 2\beta(2\overline{N} - (N_{1s}+N_{2s})) = \widehat{H}(N_{1s}+N_{2s}) \tag{2.21}$$

Also, $p(N_{1j}+N_{2j}) = \beta(N_{1j}+N_{2j})^2$ implies that $p'(N_{1j}+N_{2j}) = 2\beta(N_{1j}+N_{2j}) > 0, \;\; j = s, d$. Hence, under the specific functional forms used in this model, housing prices and the population of a city are positively related, a conclusion that will hold more generally.

2.3.1 Closed-Form Equilibrium Solutions

This section presents first the non-WFH and then the WFH equilibrium solutions for employment, population and wages of each type of worker in each city, based on the functional forms described in the previous section.

Non-WFH Equilibrium Outcomes

Without WFH, $L_{ij} = N_{ij}\ \forall i = 1, 2, j = s, d$. Using this employment-population equality, substituting the wage functions from equations (16) and (18) along with the net housing utility functions from equations (20)-(21) into the remote worker non-WFH equilibrium condition (10) yields

$$\begin{aligned} &A_s + a_{1s} - 2N_{1s} + cN_{2s} + \alpha - 2\beta(N_{1s} + N_{2s}) \\ &\qquad = A_d + a_{1d} - 2(\overline{N_1} - N_{1s}) + c(\overline{N_2} - N_{2s}) + \alpha - 2\beta(2\overline{N} - (N_{1s} + N_{2s})) \end{aligned} \tag{2.22}$$

For non-remote workers, using the same employment-population equality and substituting the wage functions from equations (17) and (19) and the net housing utility functions from equations (20)-(21) into the non-remote worker non-WFH equilibrium condition from equation (11) yields

$$\begin{aligned} &A_s + a_{2s} - 2N_{2s} + cN_{1s} + \alpha - 2\beta(N_{1s} + N_{2s}) \\ &\qquad = A_d + a_{2d} - 2(\overline{N_2} - N_{2s}) + c(\overline{N_2} - N_{1s}) + \alpha - 2\beta(2\overline{N} - (N_{1s} + N_{2s})) \end{aligned} \tag{2.23}$$

Solving (22) and (23) simultaneously yields (see the online appendix)

$$N_{1s}^* = \frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)(c+2)} + \frac{\overline{N_1}}{2} \tag{2.24}$$

$$N_{2s}^* = \frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(c - 2\beta)}{2(4\beta - c + 2)(c+2)} + \frac{\overline{N_2}}{2} \tag{2.25}$$

From equations (24) and (25), the total equilibrium population in city s without WFH is

$$N_{1s}^* + N_{2s}^* = \overline{N} + \frac{A_s - A_d}{4\beta - c + 2} + \frac{a_{1s} - a_{1d}}{2(4\beta - c + 2)} \tag{2.26}$$

Note that when city s has neither a type-1-productivity advantage ($a_{1s} = a_{1d}$) nor an amenity advantage ($A_s = A_d$), $N_{1s}^* = \frac{\overline{N_1}}{2}$ and $N_{2s}^* = \frac{\overline{N_2}}{2}$. Consequently, using (3), $N_{1d}^* = \frac{\overline{N_1}}{2}$ and $N_{2d}^* = \frac{\overline{N_2}}{2}$. Hence, as intuition suggests, in absence of an amenity or productivity advantage, remote and non-remote workers distribute themselves equally across the two cities.

At this point, a parameter restriction must be imposed to generate further results. In particular, the complementarity parameter, c, is assumed to be small, in which case $c < min\{2, 2\beta\}$ holds along with $4\beta - c + 2 > 0$. Without this assumption, few results comparing the WFH and non-WFH equilibria can be derived. With the denominator expression in equation (24) positive under this assumption, it follows that when city s has either a productivity advantage (when $a_{1s} \geq a_{1d}$) or an amenity advantage (when $A_s \geq A_d$), $N_{1s}^* > \frac{\overline{N_1}}{2}$ and consequently, $N_{1d}^* < \frac{\overline{N_1}}{2}$. Thus, in the absence of WFH, more than half the total remote workers live and work in city s when it offers either higher productivity or higher amenities.

Focusing on type-2 workers and using (25), when city s lacks a productivity advantage (when $a_{1s} = a_{1d}$) but has an amenity advantage (when $A_s \geq A_d$), $N_{2s}^* > \frac{\overline{N_2}}{2}$ and consequently, $N_{2d}^* < \frac{\overline{N_2}}{2}$. Thus, more than half of the type-2 population lives and works in city s. Conversely, when city s has a productivity advantage but no amenity advantage, $c - 2\beta < 0$ implies $N_{2s}^* < \frac{\overline{N_2}}{2}$ and consequently, $N_{2d}^* > \frac{\overline{N_2}}{2}$, so that more than half of non-remote workers lives and works in city d. Therefore, while an amenity advantage draws both workers to city s, a productivity advantage draws remote worker types to city s and non-remote workers to city d. The previous comparisons are summarized in the left panel of Table 2.1.

Finally, when city s offers either a productivity advantage or an amenity advantage, (26) implies $N^*_{1s} + N^*_{2s} > \overline{N}$, so that more than half of the total population lives and works in city s in the absence of WFH, as city s has some sort of advantage to provide. Note that this conclusion holds despite the opposing effects of a productivity advantage on type-1 and type-2 populations. Because of these population differences, housing prices are higher in city s than in city d, with $p^*_s > p^*_d$

The wages earned by the two types of workers in city s in the non-WFH equilibrium are given by

$$\begin{aligned} w_{1s}(N^*_{1s}, N^*_{2s}) &= a_{1s} - 2N^*_{1s} + cN^*_{2s} \\ &= \frac{c\overline{N_2}}{2} - \overline{N_1} - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(2\beta + 2 - c)a_{1d} + (6\beta + 2 - c)a_{1s}}{2(4\beta - c + 2)} \end{aligned} \tag{27}$$

$$w_{2s}(N^*_{1s}, N^*_{2s}) = a_{2s} - 2N^*_{2s} + cN^*_{1s} = \frac{c\overline{N_1}}{2} - \overline{N_2} - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{\beta(a_{1s} - a_{1d})}{(4\beta - c + 2)} \tag{28}$$

The corresponding wage expressions for city d are listed in the online appendix.[7]

WFH Equilibrium Outcomes

When the option of WFH exists for remote (type 1) workers, they no longer need to be employed in the same city they reside in, eliminating the employment-population equality for remote workers and yielding $L_{1j} \neq N_{1j},\ j = s, d$. Non-remote workers however, still must reside in the same city they work in, yielding $L_{2j} = N_{2j}\ \forall j = s, d$. Thus, using the employment-population equality

[7]To save space, wage comparisons across cities are not presented.

only for remote-workers and substituting the wage functions from equations (16) and (18) into the remote worker wage-equalization condition from equation (13), the condition becomes

$$a_{1s} - 2L_{1s} + cN_{2s} = a_{1d} - 2(\overline{L_1} - L_{1s}) + c(\overline{N_2} - N_{2s}) \tag{29}$$

Using the net housing utility functions from equations (20)-(21) in the remote-worker WFH equilibrium condition (14), that condition becomes

$$A_s + \alpha - 2\beta(N_{1s} + N_{2s}) = A_d + \alpha - 2\beta(2\overline{N} - (N_{1s} + N_{2s})) \tag{30}$$

For non-remote workers, substituting the wage functions from equations (17) and (19) and the net housing utility functions from equations (20)-(22) into the non-remote worker WFH equilibrium condition from equation (15) yields

$$\begin{aligned} &A_s + a_2 - 2N_{2s} + cL_{1s} + \alpha - 2\beta(N_{1s} + N_{2s}) \\ &\quad = A_d + a_2 - 2(\overline{N_2} - N_{2s}) + c(\overline{L_1} - L_{1s}) + \alpha - 2\beta(2\overline{N} - (N_{1s} + N_{2s})) \end{aligned} \tag{31}$$

Solving equations (29)-(31) simultaneously (see the online appendix) yields the WFH equilibrium population of remote workers in city s, which is given by

$$\widetilde{N_{1s}} = \frac{(A_s - A_d)}{4\beta} - \frac{c(a_{1s} - a_{1d})}{2(2-c)(2+c)} + \frac{\overline{N_1}}{2} \tag{32}$$

along with the WFH equilibrium employment of remote workers in city s,

$$\widetilde{L_{1s}} = \frac{a_{1s} - a_{1d}}{(2+c)(2-c)} + \frac{\overline{N_1}}{2} \tag{33}$$

and the WFH equilibrium employment/population of non-remote workers in city s,

$$\widetilde{N_{2s}} = \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2} \tag{34}$$

Note that since $c < min\{2, 2\beta\}$, the denominator expressions in (32)-(34) are positive.

Rearranging (30), the total population in city s under the WFH equilibrium is given by

$$\widetilde{N_{1s}} + \widetilde{N_{2s}} = \frac{A_s - A_d}{4\beta} + \overline{N} \tag{35}$$

Using (33) and (34), total employment in city s under WFH equals

$$\widetilde{L_{1s}} + \widetilde{N_{2s}} = \frac{a_{1s} - a_{1d}}{2(2-c)} + \overline{N} \tag{36}$$

When city s has neither a productivity advantage ($a_{1s} = a_{1d}$) nor amenity advantage ($A_s = A_d$), $\widetilde{N_{1s}} = \widetilde{L_{1s}} = \frac{\overline{N_1}}{2}$ and $\widetilde{N_{2s}} = \frac{\overline{N_2}}{2}$. Consequently, using (3), $\widetilde{N_{1d}} = \widetilde{L_{1d}} = \frac{\overline{N_1}}{2}$ and $\widetilde{N_{2d}} = \frac{\overline{N_2}}{2}$. Hence, as intuition suggests, in absence of amenity or productivity advantages, remote and non-remote workers distribute themselves equally across the two cities even when remote workers have the ability to work from home.

When city s has a productivity advantage ($a_{1s} \geq a_{1d}$) but lacks an amenity advantage ($A_s = A_d$), $\widetilde{N_{1s}} < \frac{\overline{N_1}}{2}$ from (32), and consequently, $\widetilde{N_{1d}} > \frac{\overline{N_1}}{2}$, so that less than half of remote workers lives in city s. However, when city s offers an amenity advantage, but lacks a productivity advantage, $\widetilde{N_{1s}} > \frac{\overline{N_1}}{2}$ and consequently, $\widetilde{N_{1d}} < \frac{\overline{N_1}}{2}$, so that more than half of remote workers lives in city s.

From (35), the opposing effects of a productivity advantage on type-1 and type-2 populations exactly cancel, so that the population of city s is independent of the advantage and equal to $\overline{N}$ (half the grand total). However, the population of city s exceeds $\overline{N}$ when it has amenity advantage, reflecting its larger share of the type-1 population. As a result, housing prices are higher in city s ($\widetilde{p_s} > \widetilde{p_d}$), although prices are equal in the productivity case. These results are summarized in the right panel of Table 2.1.

Turning to type-2 workers, when city s has a productivity advantage, but no amenity advantage, (34) yields $\widetilde{N_{2s}} > \frac{\overline{N_2}}{2}$ and consequently, $\widetilde{N_{2d}} < \frac{\overline{N_2}}{2}$, so that more than half of non-remote workers lives and works in city s, reversing the non-WFH pattern. However, when city s lacks a productivity advantage, but has an amenity advantage, (34) yields $\widetilde{N_{2s}} = \widetilde{N_{2d}} = \frac{\overline{N_2}}{2}$, so that half the type-2 population lives and works in city s. In other words, non-remote workers distribute themselves equally across the two cities when city s offers an amenity advantage.

Turning to type-1 employment levels, when city s offers a productivity advantage but no amenity advantage, (33) yields $\widetilde{L_{1s}} > \frac{\overline{N_1}}{2}$ and consequently, $\widetilde{L_{1d}} < \frac{\overline{N_1}}{2}$, so that more than half of remote workers works in city s. However, when city s offers an amenity advantage but no productivity advantage, (33) yields $\widetilde{L_{1s}} = \widetilde{L_{1d}} = \frac{\overline{N_1}}{2}$, so that remote worker employment is equal across the two cities.

When city s only has a productivity advantage, comparing the remote worker population (from (32)) and employment (from (33)) after the introduction of WFH yields $\widetilde{L_{1s}} > \widetilde{N_{1s}}$ and consequently, $\widetilde{L_{1d}} < \widetilde{N_{1d}}$. Thus, only a portion of the remote workers employed in city s also resides in city s, and since population equals employment for type-2 workers, total employment exceeds population, as seen in (35) and (36). This result is also consistent with BKL, who show that population falls short of employment in city s following the introduction of WFH in the productivity case. On the other hand, when city s only has an amenity advantage, comparing the remote worker population (from (32)) and employment (from (33)) yields $\widetilde{L_{1s}} < \widetilde{N_{1s}}$ and consequently, $\widetilde{L_{1d}} > \widetilde{N_{1d}}$. Thus, only a fraction of the remote workers living in city s also works in city s, implying that total employment falls short of population (as seen in (35)-(36)). This result is consistent with BKL, where employment falls short of population in city s following the introduction of WFH in the amenity case.

The wages earned by the original residents of city s and city d in the WFH equilibrium are given by

$$w_{1s}(\widetilde{L_{1s}},\widetilde{N_{2s}}) = w_{1d}(\widetilde{L_{1d}},\widetilde{N_{2d}}) = \frac{a_{1s}+a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} \tag{37}$$

$$w_{2s}(\widetilde{L_{1s}},\widetilde{N_{2s}}) = w_{2d}(\widetilde{L_{1d}},\widetilde{N_{2d}}) = a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} \tag{38}$$

Note that while wage equalization for type-1 workers is an equilibrium condition, (38) implies that wages also end up being equalized for type-2 workers, a striking conclusion, in both the amenity and productivity cases

2.4 Population, Employment, Rent and Welfare Changes under WFH

This section outlines the changes in population, employment levels, rent and welfare for each type of worker in each of the cities with the introduction of WFH. If city s is assumed to have a dual advantage, better amenities as well as higher productivity, many comparisons are ambiguous, as in BKL. However, assuming that city s offers only a single advantage, either in amenities or productivity, unambiguous comparisons can be made. The details of the comparisons are presented in the online appendix.

2.4.1 City s offers higher productivity to remote workers

This subsection assumes that $a_{1s} > a_{1d}$, but $A_s = A_d$. Comparing the remote-worker equilibrium populations before WFH existed (using (24)) and after WFH is introduced (using (32)) yields $\widetilde{N_{1s}} < N_{1s}^*$ and consequently, using equation (3), $\widetilde{N_{1d}} > N_{1d}^*$. Thus, remote workers relocate their residences from city s to city d with the introduction of WFH.

Comparing the non-remote worker equilibrium populations before the introduction of WFH (using (25)) and after WFH is introduced (using (34)) yields $\widetilde{N_{2s}} > N^*_{2s}$ and consequently, $\widetilde{N_{2d}} < N^*_{2d}$. Thus, some non-remote workers relocate from city d to city s when WFH becomes possible for remote workers, reversing the movement of type-1 workers.

Comparing the total populations before the introduction of WFH (using (26)) and after WFH is introduced (using (35)) yields

$$\widetilde{N_{1s}} + \widetilde{N_{2s}} < N^*_{1s} + N^*_{2s} \tag{39}$$

which implies $\widetilde{N_{1d}} + \widetilde{N_{2d}} > N^*_{1d} + N^*_{2d}$ via (5). Thus, when offering a productivity advantage for remote workers, city s records a reduction in total population after WFH is introduced, with the loss of type-1 population more than offsetting the gain in type-2 population. The condition in (39) is consistent with BKL, where city s is shown to contract when it has a productivity advantage, with residents relocating to city d while keeping their city s jobs.

Comparing remote worker employment before introduction of WFH (using (24)) and after WFH is introduced (using (33)) yields $\widetilde{L_{1s}} > N^*_{1s}$, implying $\widetilde{L_{1d}} < N^*_{1d}$. This result shows that the introduction of WFH leads more remote workers to work in city s when it provides a productivity advantage. Conversely, fewer remote workers are employed in city d after the introduction of WFH. The reduction in type-1 employment in city d implies that some of the original type-1 residents of city d work remotely in city s (along with the new arrivals) with the introduction of WFH.

Comparing total employment before introduction of WFH (using (26)) and after WFH is introduced (using (36)) yields $\widetilde{L_{1s}} + \widetilde{N_{2s}} > N^*_{1s} + N^*_{2s}$, implying $\widetilde{L_{1d}} + \widetilde{N_{2d}} < N^*_{1d} + N^*_{2d}$. Hence, as more remote and non-remote workers work in city s after the introduction of WFH, city s records an increase in total employment when it offers a productivity advantage. Conversely, fewer remote workers are employed in city d after the introduction of WFH. These results are also consistent with BKL, who show that total employment in city s (city d) rises (falls) following the introduction of WFH in the productivity case.

Recall that the rent and population in a city are positively related, as mentioned in section 2.4. Following the population comparison in (37), the positive relation between rent and population yields $p_s^* < \widetilde{p_s}$ and consequently $p_d^* > \widetilde{p_d}$. This conclusion shows that with the introduction of WFH, as people relocate from city s to city d, housing prices decrease in city s while they increase in city d. This result is consistent with BKL, who show that housing prices rise in city d and fall in city s following the migration of city s residents to city d.

Note that from the wage functions in (16) and (17), the wage of any type of worker is negatively related to the employment level of that type and positively related to the employment of the other type of worker. Comparing the wages of original remote workers of city s before WFH existed (from (27)) and after WFH is introduced (from (37)) yields,

$$w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) < w_{1s}(N_{1s}^*, N_{2s}^*) \tag{40}$$

Also, wage comparisons for city d yield $w_{1d}(\widetilde{L_{1d}}, \widetilde{N_{2d}}) > w_{1d}(N_{1d}^*, N_{2d}^*)$. The effects of more remote and non-remote workers simultaneously starting to work in city s after the introduction of WFH should drive the change of wage in different directions, but since c is small, it is the own-employment change that dominates in determining the direction of the wage change. Thus, since remote employment increases, the original remote workers of city s earn lower wages under WFH, while the original remote workers of city d earn higher wages under WFH. This conclusion is consistent with its counterpart in BKL, where the original residents of city s, all of whom are remote workers, earn lower wages while the original workers of city d earn higher wages with introduction of WFH.

Comparing the wages of original non-remote workers of city s before WFH existed (from (28)) and after WFH is introduced (from (38)) yields,

$$w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) < w_{2s}(N_{1s}^*, N_{2s}^*) \tag{41}$$

Also, for city d, a similar comparison yields, $w_{2d}(\widetilde{L_{1d}}, \widetilde{N_{2d}}) > w_{2d}(N_{1d}^*, N_{2d}^*)$. While the effects of more remote and non-remote workers simultaneously starting to work in city s after the introduction of WFH should drive the change of wage in different directions, the small c again means that the own-employment change dominates in determining the direction of the wage change. Hence, since non-remote employment rises, the original non-remote workers of city s earn lower wages under WFH, while the original non-remote workers of city d earn higher wages.

Note that from equation (7), the utility or welfare of worker i working in city j is given by the sum of amenities in city j, A_j, wages earned by worker type i, $w_{ij}(L_{1j}, L_{2j})$, and the net housing utility, $\alpha - 2\beta(N_{1s} + N_{2s})$ (from equation (20)). Computing and comparing the welfare of original remote residents of city s before and after WFH is introduced, using (26), (27), (35) and (37) yields

$$A_s + w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) + \alpha - 2\beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) = A_s + w_{1s}(N_{1s}^*, N_{2s}^*) + \alpha - 2\beta(N_{1s}^* + N_{2s}^*).$$

Thus, the welfare of the original remote workers of city s remains the same with the introduction of WFH when city s offers a productivity advantage. The decrease in remote worker wages exactly offsets the effects of the decrease in population on net housing utility, leaving the total welfare of remote workers unchanged in city s. The same conclusion holds in city d given that utilities are equalized.

Computing and comparing the welfare of original non-remote residents of city s before and after WFH is introduced, using (26), (28), (35) and (38), yields

$$A_s + w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) + \alpha - 2\beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) = A_s + w_{2s}(N_{1s}^*, N_{2s}^*) + \alpha - 2\beta(N_{1s}^* + N_{2s}^*).$$

Thus, the welfare of the original non-remote workers of city s too remains the same with the introduction of WFH when city s offers a productivity advantage. The same conclusion holds in city d given equalization of utilities.

Summarizing the changes in remote and non-remote worker populations, employment levels, wages, and welfare levels yields:[8]

Proposition 1: When city s only has a remote-worker productivity advantage, introduction of WFH has the following effects.

(i) *Total population falls in city s and rises in city d.*

(ii) *Underlying these population changes are a decrease (increase) in the remote population and an increase (decrease) in the non-remote population of city s (city d).*

(iii) *As a result of population changes, housing prices fall in city s and rise in city d.*

(iv) *Employment of both remote workers and non-remote workers rises in city s and falls in city d, with the remote employment change implying that some original type-1 residents of city d now work remotely in city s.*

(v) *Total employment rises in city s and falls in city d.*

(vi) *The employment of remote workers exceeds (falls short of) their population in city s (city d).*

(vii) *Both the original remote and non-remote workers of city s (city d) earn lower (higher) wages with the introduction of WFH.*

(viii) *The welfare of both remote and non-remote workers remains unchanged with the introduction of WFH.*

Overall, when city s has a remote-worker productivity advantage, both BKL and the current analysis show that the total population and housing prices fall (rise) in city s (city d) with the introduction of WFH. The current analysis also shows that WFH leads to an increase (decrease) in total employment in city s (city d), so that the population and employment within a city move in opposite directions, as in BKL's analysis. Employment of both type of workers moves in the same

[8] A second functional form for production involving higher substitutability between type-1 and type-2 workers may be introduced as follows $F(L_{1j}, L_{2j}) = a_j(L_{1j} + L_{2j}) - b(L_{1j} + L_{2j})^2 + cL_{1j}L_{2j}$, $j = s, d$ where c is still assumed to be small. Redoing the entire analysis assuming this functional form yields similar results to those presented below, with only minute changes compared to the previous functional form. Explanations of the results corresponding the new functional form are available on request.

direction as total employment, with remote employment exceeding (falling short of) the remote worker population in city s (city d). Thus, the results on total and remote employment match those in BKL. In addition, the wages of the original remote as well as non-remote workers fall (increase) in city s (city d) with the introduction of WFH. The same conclusion holds in BKL with respect to the wages of all (remote) workers. Also, the increase (decrease) in remote worker employment in city s (city d) implies that some original city d residents are now working remotely in city s, along with the new type-1 residents. Notably, even though the total and remote-worker population in the current analysis decreases (increases) in city s (city d), hence matching BKL's conclusion for all workers, non-remote workers conversely record an increase (decrease) in their population in city s (city d). Interestingly, the opposite population changes of remote and non-remote workers is one case where the spirit of BKL's conclusions does not translate to the current model. Another difference concerns welfare effects. Mainly because BKL did not impose specific functional forms, the welfare change of the single worker type in their analysis was ambiguous. The current finding of no welfare effects for either type of worker stands in contrast to BKL's conclusion.

2.4.2 City s offers better amenities to all residents

This subsection assumes that $a_{1s} = a_{1d}$, but $A_s > A_d$. Comparing the remote worker equilibrium populations before WFH existed (using (24)) and after WFH is introduced (using equation (32)) yields $\widetilde{N_{1s}} > N_{1s}^*$ and consequently, $\widetilde{N_{1d}} < N_{1d}^*$. Thus, when the advantage of city s comes from better amenities, remote workers relocate their residences from city d to city s with the introduction of WFH.

Comparing the non-remote worker equilibrium populations before WFH (using (25)) and after WFH is introduced (using (34)) yields $\widetilde{N_{2s}} < N_{2s}^*$ and consequently, $\widetilde{N_{2d}} > N_{2d}^*$. Thus, some non-remote workers relocate from city s to city d when WFH is introduced, reversing the movement of remote workers.

Comparing the total population before introduction of WFH (using (26)) and after WFH is introduced (using (35)) yields

$$\widetilde{N_{1s}} + \widetilde{N_{2s}} > N_{1s}^* + N_{2s}^*, \tag{42}$$

with the increase in type-1 population offsetting the decrease in type-2 population. Consequently, equation (5) yields $\widetilde{N_{1d}} + \widetilde{N_{2d}} < N_{1d}^* + N_{2d}^*$. These results show that city s records an increase in total population after WFH is introduced, a conclusion consistent with BKL, where city s is shown to grow under WFH when it has an amenity advantage, with residents relocating from city d. Intuitively, since the amenity advantage comes from residing in city s, more people in total choose to live in city s rather than in city d.

Comparing remote worker employment before WFH (using (24)) and after WFH is introduced (using (33)) yields $\widetilde{L_{1s}} < N_{1s}^*$, implying $\widetilde{L_{1d}} > N_{1d}^*$. Thus, fewer remote workers work in city s after the introduction of WFH when city s provides an amenity advantage. This conclusion follows because the better amenities of city s are enjoyed by workers residing, not necessarily working in city s. Conversely, more remote workers are employed in city d after the introduction of WFH.

Comparing total employment before WFH (using (26)) and after WFH is introduced (using (36)) yields, $\widetilde{L_{1s}} + \widetilde{N_{2s}} < N_{1s}^* + N_{2s}^*$ along with $\widetilde{L_{1d}} + \widetilde{N_{2d}} > N_{1d}^* + N_{2d}^*$. Thus, when offering an amenity advantage, city s records a reduction in total employment after WFH is introduced. The reduction in type-1 employment in city s implies that some original type-1 residents of city s work remotely in city d with the introduction of WFH (along with the new type-1 residents). Although the total population increases in city s due to its amenity advantage once WFH is introduced, some people live in city s to enjoy its amenities while working remotely in city d. This result is also consistent with BKL, who show that total employment in city s (city d) falls (rises) following the introduction of WFH.

Recalling that housing prices and population are positively related, the population comparison in equation (37) yields $\widetilde{p_s} > p_s^*$ and consequently $\widetilde{p_d} < p_d^*$. Thus, with the introduction of WFH, as more people relocate from city d to city s, housing prices increase in city s while they decrease in city d. This result is consistent with BKL, who show that housing prices rise in city s and fall in city d following the migration of city d residents to city s.

Comparing the wages of original remote workers of city s before WFH (from (27)) and after WFH is introduced (from (37)) yields

$$w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) > w_{1s}(N_{1s}^*, N_{2s}^*) \tag{43}$$

Also, for city d, wage comparisons yield, $w_{1d}(\widetilde{L_{1d}}, \widetilde{N_{2d}}) < w_{1d}(N_{1d}^*, N_{2d}^*)$. Given the properties of the wage function, the effects of fewer remote and non-remote workers simultaneously starting to work in city s after the introduction of WFH should drive the change of wage in different directions. However, since c is small, it is the own-employment change that dominates in determining the direction of the wage change. Hence, since the remote employment falls, the original remote workers of city s earn higher wages while the original remote workers of city d earn lower wages. This conclusion is consistent with the analogous result in BKL, who show that when city s has an amenity advantage, its original residents, all of whom are remote workers, earn higher wages while the original workers of city d earn lower wages with the introduction of WFH.

Comparing the wages of original non-remote workers of city s before (from (28)) and after WFH is introduced (from (38)) yields,

$$w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) > w_{2s}(N_{1s}^*, N_{2s}^*) \tag{44}$$

Also, for city d, the wage comparison yields $w_{1d}(\widetilde{L_{1d}}, \widetilde{N_{2d}}) < w_{1d}(N_{1d}^*, N_{2d}^*)$. As before, the own-employment change dominates in determining the direction of the wage change. Hence, since non-remote employment falls, the original non-remote workers of city s earn higher wages while the original non-remote workers of city d earn lower wages.

Computing and comparing the welfare of original remote residents of city s before and after

WFH is introduced using (26), (27), (35) and (37), yields

$$A_s + w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) + \alpha - 2\beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) = A_s + w_{1s}(N_{1s}^*, N_{2s}^*) + \alpha - 2\beta(N_{1s}^* + N_{2s}^*)$$

showing that the welfare of the original remote workers of city s remains the same with the introduction of WFH when city s offers an amenity advantage. The increase in remote worker wages exactly offsets the effects of increase in population on net housing utility, leaving the total welfare of remote workers unchanged in city s, and the same conclusion holds in city d given equalization of utilities.

Computing and comparing the welfare of original non-remote residents of city s before and after WFH is introduced, using equations (41) and (43), yields

$$A_s + w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) + \alpha - 2\beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) = A_s + w_{2s}(N_{1s}^*, N_{2s}^*) + \alpha - 2\beta(N_{1s}^* + N_{2s}^*).$$

Thus, the welfare of the original non-remote workers of city s too remains the same with the introduction of WFH when city s offers an amenity advantage.

Summarizing yields:

Proposition 2: When city s only has an amenity advantage, introduction of WFH has the following effects.

(i) Total population rises in city s and falls in city d.

(ii) Underlying these population changes are an increase (decrease) in the remote population and a decrease (increase) in non-remote population of city s (city d).

(iii) As a result of population changes, housing prices rise in city s and fall in city d.

(iv) Employment of both remote and non-remote workers falls in city s and rises in city d, with the remote employment change implying that some original type-1 residents of city s now work remotely in city d

(v) Total employment falls in city s and rises in city d.

(vi) *The employment of remote workers falls short of (exceeds) their population in city s (city d).*

(vii) *Both the original remote and non-remote workers of city s (city d) earn higher (lower) wages with the introduction of WFH.*

(viii) *The welfare of both remote and non-remote workers remains unchanged with the introduction of WFH.*

Overall, when city s has an amenity advantage, both BKL and the current analysis show that the total population and housing prices rise (fall) in city s (city d) with the introduction of WFH. The current analysis also shows that WFH leads to an decrease (increase) in total employment in city s (city d), so that the population and employment within a city move in opposite directions, as in BKL's analysis. Employment of both types of workers moves in the same direction as total employment, with remote employment falling short of (exceeding) the remote worker population in city s (city d). Thus, the results on total and remote employment match those in BKL. In addition, the wages of the original remote as well as non-remote workers increase (decrease) in city s (city d), with the introduction of WFH. The same conclusion holds in BKL with respect to wages of all (remote) workers. The decrease (increase) in remote worker employment in city s (city d) implies that some original city s residents are now working remotely in city d, along with the city's new type-1 residents. Notably, even though the total and remote-worker population in the current analysis increase (decrease) in city s (city d), hence matching BKL's conclusion for all workers, non-remote workers conversely record a decrease (increase) in their population in city s (city d). The opposite population changes of remote and non-remote workers is one outcome where the spirit of BKL's conclusions does not translate to the current model, as in the productivity case. The current finding of no welfare effects for either type of worker stands in contrast to BKL's ambiguous conclusion, as before.

2.5 Comparative Statics

While the previous results involved comparisons of the levels of the variables, with and without WFH, this section carries out comparative static analysis, showing how the magnitudes of the changes in the endogenous variables depend on the sizes of the parameters.

When city s provides only a remote worker productivity advantage:

Assuming $a_{1s} > a_{1d}$, but $A_s = A_d$ and computing the partial derivative of the difference between pre-WFH and post-WFH population levels in the cities with respect to the difference in productivities between the two cities, the following results emerge:

$$\frac{\partial(N_{1s}^* - \widetilde{N_{1s}})}{\partial(a_{1s} - a_{1d})} > 0\ ,\ \frac{\partial(\widetilde{N_{2s}} - N_{2s}^*)}{\partial(a_{1s} - a_{1d})} > 0\ ,\ \frac{\partial(\widetilde{N_{1d}} - N_{1d}^*)}{\partial(a_{1s} - a_{1d})} > 0\ ,\ \frac{\partial(N_{2d}^* - \widetilde{N_{2d}})}{\partial(a_{1s} - a_{1d})} > 0$$

Thus, the higher is the remote-worker productivity advantage in city s, the higher is the remote worker population difference in each city before and after WFH is introduced and the higher is the non-remote worker population difference as well.

Computing the partial derivative of the difference between pre-WFH and post-WFH remote worker employment levels in the cities with respect to the difference in productivities between the two cities, the following results emerge:

$$\frac{\partial(\widetilde{L_{1s}} - N_{1s}^*)}{\partial(a_{1s} - a_{1d})} > 0\ ,\ \frac{\partial(N_{1d}^* - \widetilde{L_{1d}})}{\partial(a_{1s} - a_{1d})} > 0$$

Thus, the higher is the remote worker productivity advantage in city s, the higher is the increase in type-1 worker employment in city s after WFH is introduced and the larger is the drop in type-1 worker employment in city d.

Analysing the comparative statics with respect to complementarity between worker types yields:

$$\frac{\partial(N^*_{1s} - \widetilde{N_{1s}})}{\partial c} > 0\ ,\ \frac{\partial(\widetilde{N_{2s}} - N^*_{2s})}{\partial c} > 0\ ,\ \frac{\partial(\widetilde{N_{1d}} - N^*_{1d})}{\partial c} > 0\ ,\ \frac{\partial(N^*_{2d} - \widetilde{N_{2d}})}{\partial c} > 0$$

Thus, the response of the remote as well as non-remote worker population difference to the degree of complementarity between worker types is positive in both cities. In other words, the higher the complementarity between worker types, the higher is the difference between pre-WFH and post-WFH population of remote workers and non-remote workers in city s and city d. Intuitively, when city s has a productivity advantage, both remote and non-remote workers respond more (in terms of cross-city migration) to the introduction of WFH as the complementarity between worker types increases.

Computing the partial derivative of the difference between pre-WFH and post-WFH remote worker employment levels in the cities with respect to the degree of complementarity between worker types, the following results emerge:

$$\frac{\partial(N^*_{1s} - \widetilde{L_{1s}})}{\partial c} > 0\ ,\ \frac{\partial(\widetilde{L_{1d}} - N^*_{1d})}{\partial c} > 0$$

Thus, the change in type-1 employment in both cities responds positively to an increase in complementarity between worker types.

When city s provides only an amenity advantage:

Assuming $a_{1s} = a_{1d}$, but $A_s > A_d$ and computing the partial derivative of the difference between pre-WFH and post-WFH population levels in the cities with respect to the difference in amenities between the two cities for each type of worker, the following results emerge:

$$\frac{\partial(\widetilde{N_{1s}} - N_{1s}^*)}{\partial(A_s - A_d)} > 0\ ,\ \frac{\partial(N_{2s}^* - \widetilde{N_{2s}})}{\partial(A_s - A_d)} > 0\ ,\ \frac{\partial(N_{1d}^* - \widetilde{N_{1d}})}{\partial(A_s - A_d)} > 0\ ,\ \frac{\partial(\widetilde{N_{2d}} - N_{2d}^*)}{\partial(A_s - A_d)} > 0$$

Thus, the higher is the amenity advantage in city s, the higher is the population change for both remote and non-remote workers in each city when WFH is introduced.

Computing the partial derivative of the difference between pre-WFH and post-WFH remote worker employment levels in the cities with respect to the difference in amenities between the two cities, the following results emerge:

$$\frac{\partial(N_{1s}^* - \widetilde{L_{1s}})}{\partial(A_s - A_d)} > 0\ ,\ \frac{\partial(\widetilde{L_{1d}} - N_{1d}^*)}{\partial(A_s - A_d)} > 0$$

Thus, the higher is the amenity advantage in city s, the higher is the drop in remote worker employment in city s and the higher is the rise in remote worker employment in city d after WFH is introduced.

Analysing the comparative statics with respect complementarity between worker types yields:

$$\frac{\partial(\widetilde{N_{1s}} - N_{1s}^*)}{\partial c} < 0\ ,\ \frac{\partial(N_{2s}^* - \widetilde{N_{2s}})}{\partial c} > 0\ ,\ \frac{\partial(N_{1d}^* - \widetilde{N_{1d}})}{\partial c} < 0\ ,\ \frac{\partial(\widetilde{N_{2d}} - N_{2d}^*)}{\partial c} > 0$$

The response of the remote worker population difference to the degree of complementarity between worker types is negative in both cities. In other words, the higher the complementarity between worker types, the lower is the difference between the pre-WFH and post-WFH populations of remote workers in city s and city d. The response of the non-remote worker populations, on the other hand, is positive in both cities. Intuitively, when city s has an amenity advantage, remote workers respond less while non-remote workers respond more (in terms of cross-city migration), to the introduction of WFH as the complementarity between worker types increases.

Computing the partial derivative of the difference between pre-WFH and post-WFH remote worker employment levels in the cities with respect to the degree of complementarity between worker types,the following results emerge:

$$\frac{\partial(N_{1s}^* - \widetilde{L_{1s}})}{\partial c} = \frac{\partial(\widetilde{L_{1d}} - N_{1d}^*)}{\partial c} = 0$$

Thus, when city s has a higher amenity advantage, the change in remote employment in either city does not respond to the complementarity between worker types.

2.6 Conclusion

This paper uses a theoretical framework to predict the impact of the introduction of WFH in a model with two different types of workers, one able to work remotely and one not. As intuition suggests, the current paper concludes that a productivity advantage attracts residents to work in the city while an amenity advantage attracts people to reside in the city. When one city provides a productivity advantage to remote workers, that city (the other city) records a rise (fall) in total employment, a fall (rise) in total population and housing prices and has remote employment that exceeds (falls short of) remote population. When a city provides an amenity advantage to all its residents, that city (the other city) records a fall (rise) in total employment, a rise (fall) in total population and housing prices, and has remote employment that falls short of (exceeds) remote population. Hence, WFH enables residents to relocate to the city with better amenities or lower productivity, while starting to work remotely in the city with worse amenities or higher productivity.

Notably, the direction of movement of total and remote population as well as total and remote employment summarized above matches the response of all (remote) workers in BKL. Recapturing BKL's conclusion, total and remote populations move in opposite directions to total and remote employment in the current paper whenever any relocation occurs. However, contrary to intuition and the previous results, the current paper shows that non-remote worker populations respond in a fashion opposite to that of remote workers. A productivity advantage raises the population of non-remote workers in a city, under WFH while an amenity advantage reduces their population.

The opposite direction of movement of non-remote workers is a major deviation in the findings of this paper from the spirit of BKL's conclusions. Another deviation in the results from BKL lies in the consumer welfare. The current paper uses explicit functional forms and concludes that the welfare levels of both remote and non-remote workers remain unchanged with the introduction of WFH. The analogous welfare analysis was ambiguous in BKL although use of explicit functional forms might have given a clear answer.

This paper has provided a contribution to the WFH literature by extending BKL's analysis of intercity effects. Intracity effects (suburban versus central city) are also important, but a more complex hybrid model is needed to analyze the two types of effects simultaneously. While some of the papers cited in the introduction provide such models, a sole focus of intercity effects is useful since these effects will be part of the economy's overall response to WFH.

2.7 Table

Table 2.1: Intercity comparisons for both the non-WFH and WFH cases

	Non-WFH		WFH	
City-s advantage:	Productivity	Amenity	Productivity	Amenity
City-s populations	$N_{1s}^* > \frac{\overline{N_1}}{2}$	$N_{1s}^* > \frac{\overline{N_1}}{2}$	$\widetilde{N_{1s}} < \frac{\overline{N_1}}{2}$	$\widetilde{N_{1s}} > \frac{\overline{N_1}}{2}$
	$N_{2s}^* < \frac{\overline{N_2}}{2}$	$N_{2s}^* > \frac{\overline{N_2}}{2}$	$\widetilde{N_{2s}} > \frac{\overline{N_2}}{2}$	$\widetilde{N_{2s}} = \frac{\overline{N_2}}{2}$
	$N_{1s}^* + N_{2s}^* > \overline{N}$	$N_{1s}^* + N_{2s}^* > \overline{N}$	$\widetilde{N_{1s}} + \widetilde{N_{2s}} = \overline{N}$	$\widetilde{N_{1s}} + \widetilde{N_{2s}} > \overline{N}$
Housing prices	$p_s^* > p_d^*$	$p_s^* > p_d^*$	$\widetilde{p_s} = \widetilde{p_d}$	$\widetilde{p_s} > \widetilde{p_d}$
Type-1 employment in city s	Same as population		$\widetilde{L_{1s}} > \frac{\overline{N_1}}{2} > \widetilde{N_{1s}}$	$\widetilde{L_{1s}} > \frac{\overline{N_1}}{2} = \widetilde{N_{1s}}$
Type-2 employment in city s	Same as population		Same as population	
Total employment in city s	Same as population		$\widetilde{L_{1s}} + \widetilde{N_{2s}} >$ city s population	$\widetilde{L_{1s}} + \widetilde{N_{2s}} <$ city s population

Chapter 3

Socio-Demographic Factors Correlating with Police-Involved Deaths: City-level Evidence from the US

3.1 Introduction

Officer-involved deaths of civilians have been a matter of public concern and are widely discussed to understand their social and economic roots. Apart from the negative societal connotations of unexpected, untimely and harrowing demises of civilians (innocent or otherwise) caused by police, officer-involved deaths have had an adverse impact on relationships between police and the communities they serve, raising questions about the role the police play in protecting civilians. In spite of multiple attempts by researchers to explore the factors associated factors with officer-involved deaths, more work is still needed. Hence, the goal of this paper is to attempt to provide further empirical evidence on these associated factors.

The paper performs an empirical analysis relating officer-involved killings to city characteristics. Although incidents of officer-involved fatalities are ideally considered as isolated, discrete events, the increasing frequency of these incidents engenders questions regarding their socio-demographic determinants. This research asks whether community characteristics measured at a city level correlate with officer-involved deaths. If the city level attributes do matter, which particular characteristics are important? Apart from analyzing how the racial makeup of a city relates to the number of officer-involved deaths, this paper asks whether crime rates, population, median income, police employment and other demographic features are correlated with the count of officer-involved fatalities. Following Mas (2006),[1] the paper also uses information on union contract clauses that might potentially impede the indictment of police involved in officer-involved deaths, asking whether these contract clauses might impact the count of deaths in a city.

The analyses rely on a longitudinal panel data set (for roughly 800 cites) used in regression analyses of the number of officer-involved deaths over the period 2000-2010, while also using a cross-sectional dataset (involving around 5400 cities) where the numbers of deaths are summed over the 11 years of the panel data set. The cross-section dataset allows the inclusion of a substantial number of additional cities, whose socio-demographic data is missing for the early 2000's. Although this dataset lacks the time dimension, it uses a larger quantity of data to explore whether the panel results hold at a larger scale. The data include cities that have had no fatal encounters recorded over the 11-year panel.[2] The paper's officer-involved deaths data were collected by FatalEncounters.org., while most of the demographic data are from the Census Bureau. The crime and police employment data are from the FBI's data release.

Multivariate regressions using the constructed datasets are run with the annual number of fatalities in a city as the dependent variable and a crime index, police employment, median income, the Black population, the Hispanic population, the non-minority population (defined as the the non-Black & non-Hispanic population), the city's sex ratio and the political party affiliation (county

[1]Mas (2006) finds evidence of a multitude of police behavior changes following a police union contract arbitration win or loss. These behavioral changes are captured by changes in clearance rates, crime rates and sentencing outcomes.

[2]The analysis later is run on data both including and excluding the group of cities that have recorded zero deaths throughout the period.

level for Presidential elections) as independent variables. Empirical specifications involve running negative binomial regressions on both the panel and the cross-section data sets. State and year fixed effects appear in the negative binomial regressions to account for time variation along with unobserved city-level factors that may have remained fixed across states.

Using the panel data, the key findings show that officer-involved deaths increase with police employment, the crime index and the sizes of the Black and the Hispanic populations while decreasing with median income. These results are confirmed in the cross-section regressions that include a larger number of cities, showing that the reported results are robust across the different formats of the datasets used. Most of these results confirm expected directions of correlation between the socio-demographic factors and the count of officer-involved deaths.

Also, using data on the police union contracts for a restricted set of large cities, additional regressions show that contract features that could potentially hinder accusation of police officers involved in officer-involved deaths are not correlated with the number of deaths from such incidents. This analysis is similar to that of Dharmapala et al. (2020), although in the current case, data from more large cities are analysed.

Officer-involved deaths have been discussed recently largely in the context of racial disparities in overall policing (especially in traffic stops) and racial differences in the risk of officer-involved deaths. Epp et al. (2017) claim that many of the controversial police shootings during 2014 and 2015 occurred during vehicle stops,[3] which involve disproportionate numbers of African-American drivers, a majority of whom were innocent of any violation. Similarly, Legewie (2016) use quasi-experimental data on pedestrian stops in New York City to conclude that racial bias in use-of-force increased after fatal shootings of two police officers by a black suspect. Researchers have proposed policy solutions to close the racial gap by exploring increased use of body-worn cameras by the police (Ariel et al. (2015)) and implicit bias training (Spencer et al. (2016)). However, Ray (2020) argues that while beneficial, these solutions do not address lack of accountability of the police. Also, more recently, Knox et al. (2020) use administrative records to claim that traditional

[3]They discuss how police stops are one of the most recognizable institutional sources of racial profiling by the police.

analyses underestimate levels of racial bias in policing. Smith (2004) examines homicides by police officers, testing organizational policies, racial threat[4] and community violence hypotheses and finds that while number of police killings were unrelated to the first of the two factors, they were related to racial threat and community violence. This literature on the racial disparities in policing lays out the groundwork for why the size of Black population in a city is an important explanatory variable when investigating factors correlated with the count of officer-involved deaths.

A majority of past studies that analyze how socio-demographic factors relate to officer-involved deaths focus on singular factors that affect or are related to officer-involved deaths, like the role of city-level police employment. Carmichael and Kent (2014) discuss how racial threat and economic inequality work both independently and jointly to produce substantial shifts in the size of police forces. Smith (2003) examines how the composition of the police force in large cities influences police-caused homicides. Kent and Jacobs (2005) use a fixed-effects panel design to detect nonlinear and interactive relationships between minority presence and the per capita number of police in large U.S. cities. These studies relating police employment to police-involved deaths prompts the inclusion of city-level police employment among explanatory factors in the present paper.[5] Moreover, while a majority of studies have ignored the Hispanic population either among victims or as a city-level factor, the focus of Holmes et al. (2018) and Carmichael and Kent (2014) on this factor motivates the use of city-level Hispanic population in the current study. Jacobs and O'Brien (1998) explore how police use of lethal force can be explained by political arrangements or by social divisions that are likely to have political consequences, hence encouraging the current study to incorporate city-level political party affiliation. The study on killing of felons[6] by

[4]The threat hypothesis, derived from the conflict theory of law, suggests that the use of force by police is not evenly distributed among the population and that the state using coercive force to control those segments of the population considered threatening to the existing social order. Inequality creates a sense of injustice and anger in which the state's use of violence is deemed necessary to control a racial and/or economic underclass who are the most seriously affected by economic and social injustices.

[5]Although a more nuanced police employment analysis (in similar lines to the studies mentioned above) is beyond the scope of this paper, the appendix presents a rudimentary regression showing how city-level factors correlate with the size of police-force in the current data set.

[6]In this situational analysis, the authors use Supplemental Homicide Reports (SHR) to analyze 4,419 "killings of felons" by police officers during 1976-1988. Whereas many of the previous studies on the use of deadly force by police officers included shootings and nonfatal woundings, the SHR data are limited to instances in which the use of deadly force by a police officer resulted in the death of a felon, a small subset of all deadly force episodes.

Sorensen at al. (1993)[7] suggests that economic inequality should be included in any macro-level explanation of police-caused homicide, motivating the use of median income level in the current study. Parker et al. (2005) examine relationships between police use of force and racial composition and social disorganization of 73 large U.S. cities.[8] The current study, given its inclusion of the Black population along with the data from smaller cities, is thus an improvement over Parker et al. (2005), especially given Willits and Nowacki (2014)'s view on the importance of including small city departments when studying the relationship between deadly force incidents and organizational variables. In past empirical crime-related research, Lim and Galster (2009)[9] note that consideration of certain neighborhood dynamics (like racial make-up) might be a more realistic approach to analyzing neighborhood responses to crime, inspiring the use of crime rates from a comparatively larger number of cities in conjunction with other socio-demographic controls in the current paper. Hence, the current paper employs factors from past studies and assembles them all in a cross-sectional and panel analysis that spans simultaneously a larger number of cities and years than previous studies.

In use-of-force related research, authors like Fridell and Lim (2016) and Nix et al. (2016) use logistic regressions owing to the categorical and dichotomous natures, respectively, of the dependent variables they considered. It is important to note that the present paper treats the dependent variable as a count, instead of a continuous variable, with a count-based regression design used instead of a linear model. Studies in this literature using Poisson regression models include Willits and Nowacki (2013), who consider the three-year sum of deadly force incidents for each city from 1999 to 2001 to examine the role of police organization on deadly force. They tabulate the sum of deadly force incidents as a count variable, not transforming these values into rates, and then proceed to use a Poisson regression model for the analysis. More recently, Goh (2020) claim that while on average, over 1,000 police killings take place every year, on a departmental basis they

[7]Sorensen et al. (1993) discuss that a relationship exists between violent crime rates and felon killing, but violent crime most often plays an intervening role between other social factors and the rate of felon killing.

[8]Parker et al. (2005) hypothesizes that macro-level patterns in police use of force are embedded in the racial and structural composition of cities and the organizational climate of local politics and police departments.

[9]Lim and Galster (2009) develop a microeconomic model of year-to-year changes in crime rates that incorporates endogenous relationships between the recruitment of criminals and deterrent effects spawned by responses of neighborhood residents and/or police in order to study the intertemporal dynamics of neighbourhood crime.

are rare, so that observed counts are small, making Poisson regression the most suitable method for analysing the data. Fagan and Campbell (2020) also treat the number of police killings as the dependent variable in a Poisson regression framework to assess effects of training curricula designed to reduce police use of deadly force.

In the current paper, because of over-dispersion of the count variable (explained further later), a negative binomial regression model is implemented. Nicholson-Crotty et al. (2017) argue that their data too showed over-dispersion and were characterized by a significant number of zeros. Examining fit statistics and assessing predicted versus actual probabilities, the authors argue that the negative binomial model is best suited to empirically test the assumption that more black police officers could reduce the number of police-involved killings of black citizens. Hemenway et al. (2019) also use a negative binomial regression model to assess the role of firearm availability in variations in rates of fatal police shootings. Other studies that use negative binomial regression are Holmes at al. (2019), Smith (2004), Smith (2005), Willits (2014) and Osgood and Chambers (2006).[10]

A common policy recommendation advanced by researchers in this area is the need for a National Police Use of Force Database. Although the FBI through its Uniform Crime Report has been keeping track of homicides, these incidents have been under-reported to a large extent, according to the data collected by the Guardian, Washington Post and the Wall Street Journal.[11] Fryer (2018) argues that more and better data is key to drawing definitive conclusions on racial bias. Edwards et al. (2019) also argue that the lack of basic data on the prevalence of police-involved deaths makes research on this topic particularly difficult. Hence, a crucial need is data that records and reports accurately the details of every officer-involved fatality. On this note, information collected by Fatal Encounters (FE), a journalist-led effort to document deaths involving police by relying on public records and news coverage, is recently being used in officer-related deaths

[10] Willits (2014) examines the relationship between police organizational structure and assaults on police officers in the USA and finds that organizational context and organizational complexity are important predictors of violence against police officers. Osgood and Chambers (2006) explore structural correlates of arrest rates for juvenile violence in 264 non-metropolitan counties of four states and use negative binomial regression owing to the small number of arrests in many counties being ill suited to least-squares regression.

[11] Analysis of data from 105 of the largest police agencies in the country shows that more than 550 police killings between 2007 and 2012 were missing from the FBI's records or, in a few dozen cases, not attributed to the police department whose officers were involved.

research, as in the present paper. As explained by Edwards et al. (2019), among the novel data collection efforts, FE has the broadest temporal scope and inclusion criteria for police-involved deaths. Despite certain complications with the data[12], Edwards et al. (2019) and Finch et al. (2019) conclude that the FE data is still arguably one of the most reliable sources for research in this area. For more papers that use FE, see Goh (2020), Jennings and Rubado (2017) and Edwards et al. (2018).[13]

The rest of the paper proceeds as follows. Section 3.2 discussed the data, summarizing the sources and details of the data used in the regressions that follow. Section 3.3 explains the empirical strategy used in the empirical analysis. Section 4.3 presents the regression results while section 5 presents conclusions.

3.2 Data

This paper uses 3 datasets : dataset (A), dataset (B) and dataset (C). All the datasets have the following variables: deaths, city crime index, number of full-time police employees, median household income, Black population, Hispanic population, non-minority population, sex ratio and political party affiliation. Details and differences between each of these datasets are explained below: [14]

Dataset (A): This dataset consists of longitudinal and cross sectional panel data. Time varies from 2000 to 2010, and the cross-section is at the city level. A total of 898 cities is analyzed, among which 574 cities had a positive number of officer-involved deaths in at least one of the sample years.

[12]In Edwards et al. (2019), the authors discuss the likeliness of negative bias in FE in the early 2000's (owing to dependence on media reports) and match the FE data with National Vital Statistics system (NVSS) data to do their analyses.

[13]Goh (2020) uses FE to evaluate consent decrees and their effect on levels of use of force. Jennings and Rubado (2017) also use FE data to collect number of officer-involved deaths for every 100,000 people in the community served by a police department to determine the relationship between police agency policies and rates of officer-involved gun deaths. So does Edwards et al. (2018) to estimate the risk of mortality from police homicide by race/ethnicity and place in the United States.

[14]The datasets generated during and/or analysed during the current study are available from the corresponding author on reasonable request.

The rest of the 324 cities recorded zero deaths over the sample period (not appearing in the officer-involved deaths database).[15] The regression analyses are done on both the large set of 898 cities as well as the sub-sample of 574 cities. Because of missing crime index and police employment data from 2000-2004, both datasets are somewhat restrictive as cities lacking these data are dropped.

Dataset (B): This is a cross section dataset with the socio-demographic controls pertaining to only the year 2010. The number of deaths corresponding to each city is added for all the years over the 2000-2010 period. Since more socio-demographic data are available for the year 2010 than for previous years, this dataset is much larger than dataset (A), with 2423 cities having a positive number of officer-involved deaths in at least one of the years between 2000 and 2010. Another set of 3216 cities recorded zero deaths between 2000 and 2010, again corresponding to cities not found in the FE database. The regression analyses are done separately for samples with and without the zero-death-cities.

Dataset (C): This is a cross-section and panel dataset and a proper subset of dataset (A) covering the period 2000 to 2010. Two additional variables are introduced in this dataset quantifying information on police union contracts for 36 of the largest cities in the country. The following discussion provides a list of variables, their sources, units used and limitations.

Officer-involved fatalities is the dependent variable for most of the analysis in this paper. The data are extracted from FatalEncounters.org.[16] As discussed by Edwards et al. (2019), FE has more comprehensive data on police-involved deaths than do official mortality files, has a broader scope than similar unofficial efforts to document deaths, and has been endorsed as a sound source of data by the Bureau of Justice Statistics. FE relies on photographs and victim obituaries to

[15] Edwards et al. (2019) posit that Fatal Encounters undercounts deaths due to unavailable online news records of officer-involved deaths. They add that the under-counting likely decreased over time until approximately 2007. This piece of evidence furnished through a transformed time series comparison of the Fatal Encounters deaths to the NVSS deaths, is according to the authors, not definitive. However, in case it is, it might not be accurate to assign zero deaths to cities that are not present in the Fatal Encounters data. Hence it is acknowledged that the count of cities with positive deaths may be under-counted in dataset A and B considering the concerns of Edwards et al. (2019).

[16] Fatal Encounters documents non-police deaths that occur when police are present or are precipitated by police action or presence. Officer vehicle-related deaths are included when they are caused by another officer. Homicides of officers by felons or deaths in the regular course of duties are not generally documented in the database.

classify the race-ethnicity of victims. FE does not currently collect data on variables that may be associated with variation in risk within racial/ethnic groups such as skin tone, multiracial identity or social class. The data include nuanced details of every fatality caused by or in the presence of the police, including exact locations, times, situational details and official dispositions, for over 10 years. The FE database involves observations where the official disposition ranges from 'vehicle/pursuit', 'accidental', 'suicide' to 'Officer indicted', to 'Officially unreported'. A limitation regarding using such an extensive range of situations is an overly broad definition of use-of-force instances. But, without delving deeply into the circumstantial details of every incident and devising a comprehensive filtering process to know what is unlawful, it is difficult to segregate these fatal occurrences. Hence, one should exercise caution in assuming that all the deaths counted here resulted from unlawful use of force.

The unit of measurement of this variable is number of deaths in a city (for the 574 cities in dataset(A) and 2423 cities in dataset (B)), in every year over the 2000-2010 period. This variable is used in all the three datasets (A), (B) and (C). However, in dataset (A), the numbers for each year are considered separately while in (B) and (C), the numbers of deaths are added over the years 2000-2010 for each city.

The crime index is an independent variable sourced from the Unified Crime Reporting Program (UCR) of the FBI. It accumulates data collected from the different police departments, measuring essentially the sum of violent and property crime, for every city for every year from 2000 to 2010 (used in dataset (A)). The numbers for the year 2010 are used in datasets (B) and (C). Violent crime includes murder and non-negligent manslaughter, forcible rape, robbery and aggravated assault. Property crimes include burglary, larceny-theft and motor vehicle theft. Some cities with missing reports are dropped, and for observations with incomplete arson figures, arson is dropped while calculating the total crime index. Since forcible rape figures in some cities were not in accordance with the national UCR guideline, these cities were dropped from the dataset.

Full time police employment is treated as an independent variable in the negative binomial regressions, and as the dependent variable in the regressions shown in the appendix. This variable is sourced from the Criminal Justice Information Service Division, FBI.[17] The count of full time police employees includes the total count of officers who ordinarily carry a firearm and a badge, have full arrest powers, and are paid from governmental funds set aside specifically for sworn law enforcement representatives.[18] Also counted are full-time agency personnel such as clerks, radio dispatchers, meter attendants, stenographers, jailers, correctional officers, and mechanics.

These data are available for every city for the years 2004 to 2010. The 2004 data are used for 2000-2003 to obtain a balanced panel for use in dataset (A). Numbers corresponding to only the year 2010 have been used in datasets (B) and (C). The FBI cautions that the data reflect existing staffing levels and should not be interpreted as preferred officer strengths recommended by the FBI. Given that the idea is to determine whether the extent of police presence in a locality impacts the number of officer-involved deaths, full-time police employment levels in the manner explained above should be appropriate.

Political Party Affiliation, another independent variable, is sourced from the County Presidential Election Returns 2000-2016, MIT Election Data, Harvard Dataverse.[19] County-level returns for Presidential Elections are recorded for the years 2000, 2004 and 2008. The political party affiliations of the candidates winning the highest votes in each county are tabulated. Democratic affiliation (denoted 1) and Republican affiliation (denoted 0) is assigned for the years 2000, 2001, 2002 and 2003 using on the highest vote-winning party in 2000. Similarly, the party obtaining the highest vote in 2004 is assigned for 2004, 2005, 2006 and 2007, while the winning party for 2008 is assigned to 2008, 2009 and 2010. While dataset (A) uses the affiliations corresponding to all the different years, as above, datasets (B) and (C) use only the affiliation for each city corresponding to the year 2010. The same affiliation is assigned to every city within a county. Even though this

[17] In the FBI's words, "Because of law enforcement's varied service requirements and functions, as well as the distinct demographic traits and characteristics of each jurisdiction, readers should use caution when drawing comparisons between agencies' staffing levels based on police employment data from the UCR Program". Consequently, cities in which data for all of its different police agencies (police departments, sheriff departments, highway patrol and so on) are not available have been dropped.

[18] The totals given for sworn officers for any particular agency reflect not only the patrol officers on the street, but also the officers assigned to various other duties such as those in administrative and investigative positions and those assigned to special teams.

[19] MIT Election Data and Science Lab, 2018, "County Presidential Election Returns 2000-2016", https://doi.org/10.7910/DVN/VOQCHQ, Harvard Dataverse, V6

county-level approach is somewhat imprecise, it is an appropriate method for recording political party affiliation for the analysis at hand.

Median income is an independent variable sourced from the U.S. Census Bureau, 2010 American Community Survey. This variable is the median income of households in the past 12 months (in 2010 inflation-adjusted dollars). Due to lack of available data for every year before 2010 in small cities and townships, only 2010 data is available for all the cities in datasets (A) and is assigned for all years to create a balanced panel. 2010 values are directly used in datasets (B) and (C).

Sex ratio is an independent variable sourced from the U.S. Census Bureau, 2010 American Community Survey. It counts the number of males for every 100 females in a city in the year 2010. 'Contacts between Police and Public' reports published by the Bureau of Justice Statistics show that use of force nearly doubled against men since 1999, but more than quadrupled against women,[20] making the sex ratio a potential determinant of the count of officer-involved deaths. Due to lack of data for every year before 2010 in small cities and townships, 2010 values are again used for all the prior years in dataset (A). 2010 values are directly used in datasets (B) and (C).

The Black population of a city is an independent variable extracted from the U.S. Census Bureau, 2010 Summary File 1. Due to lack of data for this variable, the '2010 percentage of Black population' values are used to generate population sizes for all years in dataset (A), while 2010 values are directly used in datasets (B) and (C). The total population variable, which varies across years, is multiplied by the percentage of Black population (2010 values, used for all the years before 2010) to obtain the number of Black residents in a city in each year.

Hispanic population is yet another independent variable sourced from the U.S. Census. Again, due to lack of data, the '2010 percentage of Hispanic population' values are used to generate populations for dataset (A) while 2010 values are directly used in datasets (B) and (C). The population variable, which varies by year, is again multiplied by the 2010 percentage of Hispanic population to obtain the number of Hispanic residents in a city in each year.

[20] https://www.prisonpolicy.org/blog/2019/05/14/policingwomen/

The non-Black & non-Hispanic population (or the "non-minority population") is calculated as the total population minus the sum of Black and Hispanic populations as computed above. Dataset (A) uses the non-minority population level in every city for every year between 2000 and 2010, while the numbers for the year 2010 have been used in datasets (B) and (C).

A police union contract variable called 'No. of clauses' is treated as an independent variable in dataset (C). This variable is extracted from Campaign Zero,[21] and it counts the number of police union contract clauses that impede rightful indictment of police employees involved in killings (varies between 1 and 6). Campaign Zero lists six 'problematic languages' used in police union contracts that might make it more difficult for police officers to be held accountable. Since this variable is only available for the 65 largest US cities, it is only used in dataset (C). The six relevant clauses in police union contracts are 'disqualifies complaints', 'restricts/delays interrogation', 'gives officers unfair access to information', 'limits oversight/discipline', 'requires city pay for misconduct' and 'erases misconduct reports'. All the 65 cities in dataset (C) have at least one of these clauses in their union contracts. Using dataset (C), regressions show whether variation in the number of these clauses in the police union contracts lead to any variation in the number of officer-involved deaths.

A second police union contract status variable called 'Revision of clauses' is also extracted from Campaign Zero. This variable is an indicator variable equal to 1 if a police union contract has been revised following the use of problematic clauses and 0 if not. The question is whether revision of the contracts affects the number of officer-involved deaths in a city.

The regressions also include state and year fixed effects. City-fixed effects cannot be used since they are perfectly collinear with the variables that are constant across years (median income and sex ratio). The analysis uses the natural logs of crime index, police employment, median income, Black population, Hispanic population, non-minority population as well as the unlogged version of these variables. More details on which version provides stronger results is explained in the next section of the paper. Table 3.1 provides summary statistics for the 3 datasets.

[21] campaignzero.org/contracts/

A final point is that an alternative empirical model would replace the officer-involved death count by a death rate, equal to deaths per capita, while using police employment per capita and Black and Hispanic population shares rather than levels. However, since using the actual death count along with negative binomial estimation seems conceptually superior, this approach is preferred.[22]

3.3 Empirical Strategy

The choice of a regression model appropriate for the data at hand is crucial in isolating socio-demographic factors correlated with the count of officer-involved deaths. It is important to note that the dependent variable is the counted number of occurrences of an event, which covers a small range of discrete values, is highly skewed, and includes many zero observations as seen in Figure 3.1.

Count regression models used in the literature include Poisson models, negative binomial models and zero-inflated models. All of these models are a strict improvement over OLS because they take into account the skewed nature of the dependent variable by assuming the data follows a distribution that presumes a large fraction of zero outcomes. However, zero-inflated models theoretically assume that a separate process (than the one generating the positive count data) is responsible for generating 'excess' zeros. Since the source data does not involve a counting process that would lead to excess zeros, it is not desirable to use zero-inflated models in this case. A Poisson model would also not be apt given the mean equals the variance (of the dependent variable) under a Poisson distribution, an assumption that does not hold in the current data. Hence, a negative binomial regression model is used. Figure **??** shows the average deviations between fitted and actual values from estimation of the three types of models on dataset (B), revealing that the negative binomial model has the best fit.

[22] The appendix presents a linear regression using normalized variables. Deaths and police employment are expressed on a per capita basis, and the minority-population variables are expressed as population shares.

3.4 Results

This section presents the results from running the negative binomial regressions. Before viewing the results it is important to note that these estimated coefficients show mere correlations and not causal relations owing to potential omitted variable bias. However, in explaining the magnitudes from the tables, quantitative interpretations do not acknowledge these biases. Although this potential bias is a limitation of the paper, the identification of factors significantly correlated with the count of officer-involved deaths can be a starting point in identifying actual causal factors.

The results using dataset (A) are shown in Table 3.2. Column (1) show the coefficients from the regression including zero-death cities without the addition of any state or year fixed effects.[23] Crime, median income, police employment, non-minority population, the Black population, Hispanic population and democratic affiliation have significant coefficients, with the effect of median income and democratic affiliation negative and other effects positive. Column (2) shows the effect of adding only state fixed effects, which renders the non-minority population and Democratic coefficients insignificant. Columns (3) show the effect of adding only year fixed effects without the addition of state fixed effects. The significance pattern is the same in columns (3) as in column (1).

Columns (4) and (5) show the coefficients from regressions including both year and state fixed effects (which is the preferred specification), including and excluding zero-death cities respectively. In column (4), where the sample includes zero-death cities, police employment, median income, Hispanic and Black population are significant determinants of deaths. The same conclusion applies in column (5), except that the crime effect is additionally present.

Intuitively, higher fatalities arise from a greater population, a higher crime index and a larger police force since all these factors cause greater police-citizen interactions in hostile settings. Cities with lower median incomes might also be more vulnerable to higher crime rates and more prone to violence, in turn leading to more violent actions by the police. The positive effects of higher

[23]In datatset (A), there is an average of around 20 cities (with a standard deviation of near 28) in each state when cities with zero deaths are included and an average of 13 cities (with a standard deviation of 18) in each state when cities with zero deaths are excluded.

Black and Hispanic populations in column (4), combined with the insignificant coefficient of the non-minority population, suggests a role for racial discrimination in lethal use of police force, particularly since the regression controls for income and overall crime.

It should be noted that when the unlogged specification is used, the police employment effect flips sign from positive to negative. A negative effect of police employment could arise if deaths come from an overworked police force (with relief coming from a larger force), while the positive effect in columns (1) through (5) would imply that more police-citizen interactions due to a larger force creates opportunities for more deaths. Given the better fit[24] of the logged model, the positive police employment effect from columns (1) and (2) appears more credible, but the contrary results from the unlogged specification suggest caution in adopting this conclusion.[25]

It is important to note that, by the innate structure of the negative binomial regression model, the interpretation of coefficients is in terms of logs of the dependent variable. As in a log-linear model, the coefficients here give the elasticities of the dependent variable with respect to the independent variable. Using the coefficients from column (4) of Table 3.2, a 1% increase in the Hispanic or Black population (or a unit increase in the logs of these populations) raises the log of count of deaths by 0.13 and 0.11 respectively, which corresponds to death increases of 0.13% and 0.11%, respectively. Using the mean value of annual city death counts (equal to 0.38 from Table 3.1), these percentage changes correspond to increases in the annual death counts of 0.04 and 0.05 respectively.

A 1% increase in median household income reduces deaths by 0.79%, which corresponds to a decrease of 0.3 deaths in a year. A 1% increase in police employment raises the count of deaths by 0.71%, increasing the mean death count by 0.3 deaths in a year. Also, a 1% increase in crime index raises death count by 0.08%, (using column (5)) increasing the mean death count by 0.03 deaths.

Following column (4) and using the mean annual death count of 0.38, a one standard deviation

[24]The deviations of predicted death values from actual values using logged and unlogged variables for dataset (A) is plotted in Fig. A2 (shown in the appendix). The deviation is found to be lower for the unlogged regression for observed death counts below 4 (approximately). For death counts above 4, the deviations are lower in the logged regression.

[25]These results can be furnished upon request.

increase in Black population and Hispanic population would increase the mean death count by 0.08 and 0.07 respectively. A one standard deviation increase in police employment and crime index increases the mean death count by 0.40 and 0.06 (corresponding to column (5)) respectively while a one standard deviation increase in median income would decrease the mean deaths by 0.11.

Table 3.3 lists the coefficients from running a negative binomial regression on dataset (B), where the death-count dependent variable is summed over all 11 years and the independent variables correspond to their 2010 values.[26]

Columns (1) and (2) of Table 3.3 show the results from including cities with zero deaths while columns (3) and (4) exclude cities with zero deaths. The results (from column (2)) for police employment, median income, Black and Hispanic population match those in Table 3.3 (column (4)), increasing confidence in the results from that table. Additionally, in column (2), crime and non-minority population also have significant coefficients. Also, when excluding cities with zero deaths, with the exception of the democratic affiliation losing significance, all other coefficients follow the same significance patterns as when zero-death cities are included.

In columns (1) through (4), the non-minority population coefficients are larger than both the Black and Hispanic population coefficients. Thus, an increment to the non-minority population leads to more deaths than in increment to the minority population, overturning the previous conclusion about racial bias in deaths. However since dataset (B) is dispreferred to dataset (A) because of its purely cross-sectional form, this outcome could be discounted.

Campaign Zero operates a website that presents police brutality data and recommends policies to reduce use of excessive force. Campaign Zero's count of police contract clauses that may reduce accountability is added to the negative binomial regressions in Table 3.4. The number of such contract clauses is thus included as an independent variable in the negative binomial regression. The variable 'revision of contract' (an indicator for whether the contracts were subsequently revised) is used as a control along with the number of clauses. In order to capture potential heterogeneity in

[26] In datatset (B), there is an average of around 105 cities (with a standard deviation of around 110) in each state when cities with zero deaths are included and an average of 42 cities (with a standard deviation of 52) in each state when cities with zero deaths are excluded.

the impact of the number of contract clauses on the count of deaths, the count of clauses has been divided into 3 bins: low for a clause count of 1 or 2, medium for 3 or 4 and high for 5 or 6. The bin can also be considered as the protection level faced by the police involved in officer-involved civilian deaths.

It is important to note that dataset (C) includes a limited set of 36 cities, which may be an obstacle to obtaining significant results. However, Table 3.4 shows that while the contract revision has no significant impact on deaths, an increase in the number of clauses has a negative impact on deaths when the high bin is considered. However, when all the bins are analyzed together, neither 'Number of clauses' nor 'Revision of contract' are significantly correlated with the count of fatalities. Notably, the negative impact of clauses in the high bin of clauses is highly significant, and its magnitude shows that increasing the number of clauses from 5 to 6 decreases the log of death count by 1.89, for a 1% decrease in deaths.

The negative direction of this impact warrants caution since a higher, not lower, count of union contract clauses protecting officers involved in civilian deaths was expected to be associated with higher count of deaths. Further research into the impact of police union contracts on the count of fatalities might be able to shed light on the size and magnitude of the coefficients seen here.

3.5 Conclusion

This paper investigates the effects of city-level variables on the number of officer-involved deaths. Robust findings from the results are evidence of positive correlations between officer-involved fatalities and crime, the Black population, and the Hispanic population as well as a negative correlation between fatalities and median income. The size of police force has positive effect in the preferred regression (with logged independent variables), suggesting that putting more police on the streets is associated with more officer-involved deaths. However, the opposite result emerges in dispreferred regressions (with the unlogged independent variables), suggesting a contrary conclusion. The significantly positive Black and Hispanic coefficients, combined with an insignificant non-minority effect, seems to confirm the presence of racial disparity in policing. However, a higher number of fatalities in a city with higher Black or Hispanic population does not necessarily prove that Black or Hispanic residents are targeted more than residents of other races in officer-involved deaths. Hence the conclusion on racial disparity, while suggestive, is not definitive.

This paper builds on the idea that city-level socio-demographic characteristics impact the incidence of officer-involved deaths. Coverage in terms of both the number of cities and the span of years is wider here compared to the majority of previous research.[27] Moreover a wider variety of independent variables is used to identify factors correlated with the number of killings in a city, compared to previous work. Since these variables are of a socio-demographic nature instead of being related to specific incidents, the approach differs from the circumstantial examination of death events done in some studies (see Holmes et al. (2018), Osgood and Chambers (2006), Epp et al. (2017)). Incorporating information on police contracts in over 60 cities that might restrict judiciary actions against police officers is also novel, although the results contradict expectations. Finally, the variety of demographic controls used here allows focusing on factors beyond racial bias explaining fatal police encounters.

A major limitation of this research is in the nature of the data used. Since it is possible that some of the fatalities counted in the FE data set were not caused by pure intent to kill, it might be inappropriate to characterize these officer-involved deaths as involving unfair use of excessive force despite the care taken by FatalEncounters.org in assembling the data. Situations that ultimately led to the fatalities might involve many incidental subtleties that require further investigation to decide if these officer-involved deaths are synonymous with use of force by the police. Also, as explained earlier, it is worthwhile to note that the database on officer-involved deaths used might have under-counted deaths before 2007, so that cities that have been counted as having zero deaths might have had positive counts. Also, owing to potential omitted variable bias, none of the correlations between socio-demographic factors and count of officer-involved deaths

[27]Klinger et al. (2016) and Nix et al. (2017) for instance, were limited to a single city or a single year. The research also improves on the latter of these papers by using crime indices for each city, rather than the less precise measures used by Nix et al. (2017)

may be considered causal in nature. Nevertheless, the present research has identified demographic and other factors correlated with officer-involved deaths, conditional on the potential measurement and research design limitations.

3.6 Tables and Figures

3.6.1 Tables

Table 3.1: Mean (St. dev.) of datasets (A) and (C) corresponding to cities in each year

	Dataset (A)			Dataset (C))
Variables	All cities	No-0-cities	0-cities	No-0-cities
Deaths	0.38 (1.45)	0.60 (1.78)	–	5.17 (6.06)
Crime Index	3089.82 (9606.74)	4274.54 (11811.54)	968.43 (854.87)	35941.06 (41905.80)
Police Employment	186.15 (626.69)	255.97 (772.86)	61.11 (46.04)	2454.53 (3154.70)
Median Income	52529.33 (22045.04)	51257.26 (18639.04)	54807.12 (26947.95)	48405.97 (13352.56)
Non-minority Population	41167.96 (87323.06)	54215.59 (106310.50)	17804.55 (13866.82)	337335.10 (312125.10)
Black Population	10563.61 (43903.59)	15198.86 (54138.66)	2263.61 (4859.71)	142257.80 (212944.90)
Hispanic population	17590.66 80274.38	24606.94 99320.46	5027.15 (8602.64)	231101.60 (347210.20)
Sex ratio	95.05 (9.22)	95.08 (6.82)	95.01 (12.41)	96.27 (3.46)
Democratic Affiliation	0.46 (0.50)	0.46 (0.50)	0.45 (0.50)	0.62 (0.49)
No. of clauses	–	–	–	3.61 (1.57)
Revision of clauses	–	–	–	0.19 (0.39)
Sample size	898	574	324	36

(1) *Source*: Constructed using data from FatalEncounters.org, US Census Bureau, FBI, Harvard Dataverse and Campaign Zero.

(2) 'All cities' implies inclusion of cities with 0 deaths throughout 2000-2010, no-0-cities implies exclusion of cities recording 0 deaths, and 0-cities implies cities without any deaths

Table 3.2: Negative binomial regression using logged independent variables from dataset (A)

Dep var: Deaths	(1)	(2)	(3)	(4)	(5)
Log of crime index	0.07^{***}	0.04^{**}	0.08^{**}	0.02	0.08^{**}
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Log of police employment	0.60^{***}	0.68^{***}	0.61^{***}	0.71^{***}	0.64^{***}
	(0.06)	(0.07)	(0.06)	(0.07)	(0.07)
Log of median income	-0.59^{***}	-0.79^{***}	-0.57^{***}	-0.79^{***}	-0.74^{***}
	(0.09)	(0.11)	(0.09)	(0.11)	(0.11)
Log of non-minority population	0.20^{***}	0.06	0.19^{***}	0.05	0.04
	(0.03)	(0.05)	(0.04)	(0.05)	(0.04)
Log of Black population	0.08^{**}	0.13^{***}	0.07^{**}	0.13^{***}	0.07^{**}
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Log of Hispanic population	0.10^{***}	0.11^{**}	0.10^{***}	0.11^{**}	0.06^{*}
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Sex ratio	0.004	0.001	0.004	0.001	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Democratic Affiliation	-0.08^{*}	-0.04	-0.09^{**}	-0.07	-0.07
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
0-cities	Y	Y	Y	Y	N
State fixed effects	N	Y	N	Y	Y
Year fixed effects	N	N	Y	Y	Y
Pseudo R^2	0.23	0.26	0.23	0.26	0.20
Log Likelihood	-5371	-5151	-5357	-5134	-4749
Observations	9823	9823	9823	9823	6303

Standard errors are in the brackets

Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.001$

Pseudo R^2 is McFadden's R^2 calculated as $1 - \frac{LL_f}{LL_n}$, where LL_f is the log-likelihood of the fitted model and LL_n is the log-likelihood of the incercept-only model

0-cities imply cities that have had no deaths throughout

No. of observations assumes the panel format of the data

Table 3.3: Negative binomial regression on datatset (B) using logged dependent variables

Dep var: Deaths	(1)	(2)	(3)	(4)
Log of crime index	0.21^{***}	0.18^{***}	0.15^{***}	0.17^{***}
	(0.03)	(0.04)	(0.06)	(0.03)
Log of police employment	0.25^{***}	0.19^{***}	0.12^{***}	0.09^{***}
	(0.03)	(0.02)	(0.02)	(0.02)
Log of median income	-0.52^{***}	-0.61^{***}	-0.58^{***}	-0.67^{***}
	(0.07)	(0.07)	(0.06)	(0.07)
Log of non-minority population	0.17^{***}	0.19^{***}	0.16^{***}	0.17^{***}
	(0.02)	(0.03)	(0.02)	(0.03)
Log of Black population	0.12^{***}	0.10^{***}	0.08^{***}	0.09^{***}
	(0.02)	(0.01)	(0.01)	(0.02)
Log of Hispanic population	0.09^{***}	0.10^{***}	0.09^{***}	0.10^{***}
	(0.02)	(0.02)	(0.01)	(0.02)
Sex ratio	-0.0003	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Democratic Affiliation	0.16^{***}	0.18^{***}	0.007	0.04
	(0.04)	(0.04)	(0.04)	(0.04)
0-cities	Y	Y	N	N
State fixed effects	N	Y	N	Y
Pseudo R^2	0.24	0.27	0.21	0.24
Log Likelihood	-6099	-4291	-6681	-4183
Observations	5370	5370	2158	2158

(1) Standard errors are in the brackets.

(2) Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.001$.

(3) Pseudo R^2 is McFadden's R^2 calculated as $1 - \frac{LL_f}{LL_n}$, where LL_f is the log-likelihood of the fitted model and LL_n is the log-likelihood of the incercept-only model.

(4) 0-cities imply cities that have had no deaths throughout.

Table 3.4: Negative binomial regression results including Police Union Contract related variables

Dep var: Deaths	(1)	(2)	(3)	(4)	(5)
Log of crime index	0.06	0.03	0.05	0.05	0.05
	(0.13)	(0.06)	(0.08)	(0.05)	(0.05)
Log of police employment	−0.86	−0.31	−0.57	0.28**	0.29**
	(1.25)	(0.98)	(0.67)	(0.14)	(0.14)
Log of median income	−0.29	0.89	9.94**	−1.68**	−1.65**
	(1.83)	(3.85)	(4.21)	(0.57)	(0.56)
Log of non-minority population	0.18	0.58	0.09	0.16	0.16
	(1.68)	(3.19)	(0.20	(0.17)	(0.17)
Log of Black population	0.62	1.11	1.79**	0.32**	0.31**
	(0.91)	(1.32)	(0.60)	(0.09)	(0.09)
Log of Hispanic population	1.07	−0.39	−1.82**	0.03	−0.03
	(1.06)	(0.73)	(0.53)	(0.12)	(0.12)
Sex ratio	−0.31	0.06	−0.66***	0.01	−0.001
	(0.45)	(0.06)	(0.18)	(0.02)	(0.02)
Democratic Affiliation	−0.08	0.05	0.53**	−0.08	−0.08
	(0.32)	(0.24)	(0.21)	(0.10)	(0.11)
No. of clauses	0.47	0.30	−1.89**	−0.02	–
	(1.14)	(0.45)	(0.03)	(0.78)	
Revision of contract	–	–	–	–	−0.08
					(0.13)
Police Protection Level	Low	Medium	High	All	All
State fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Pseudo R^2	0.34	0.20	0.28	0.23	0.24
Log Likelihood	−195	−291	−286	−824	−824
Observations	110	154	154	396	396

(1) Pseudo R^2 is McFadden's R^2 calculated as $1 - \frac{LL_f}{LL_n}$, where LL_f is the log-likelihood of the fitted model and LL_n is the log-likelihood of the incercept-only model.

(2) Standard errors are in brackets.

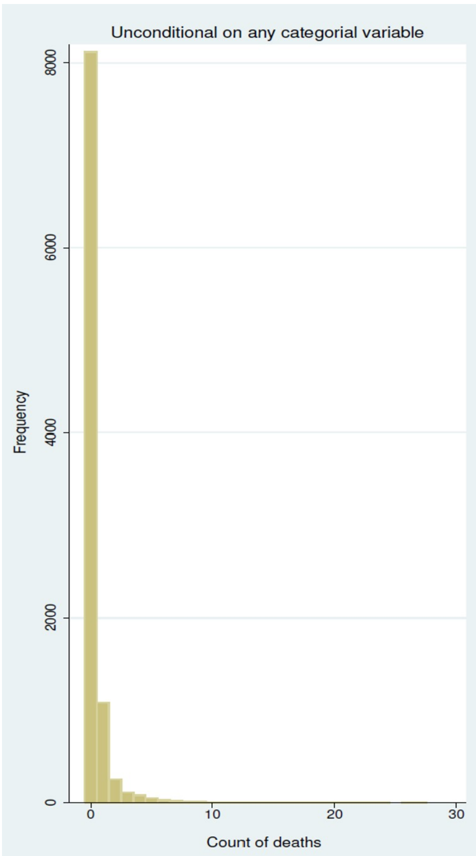
(3) Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.001$.

(4) Low bin corresponds to contracts with only 1 or 2 clauses, medium bin corresponds to 3 or 4 clauses and high bin corresponds to 5 or 6 clauses.

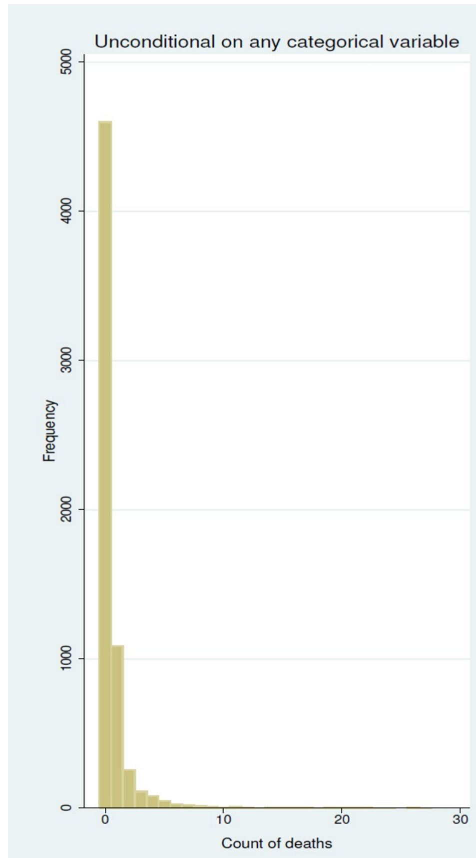
(5) No. of observations assumes the panel format of the data.

3.6.2 Figures

Figure 3.1: Overdispersion of death count

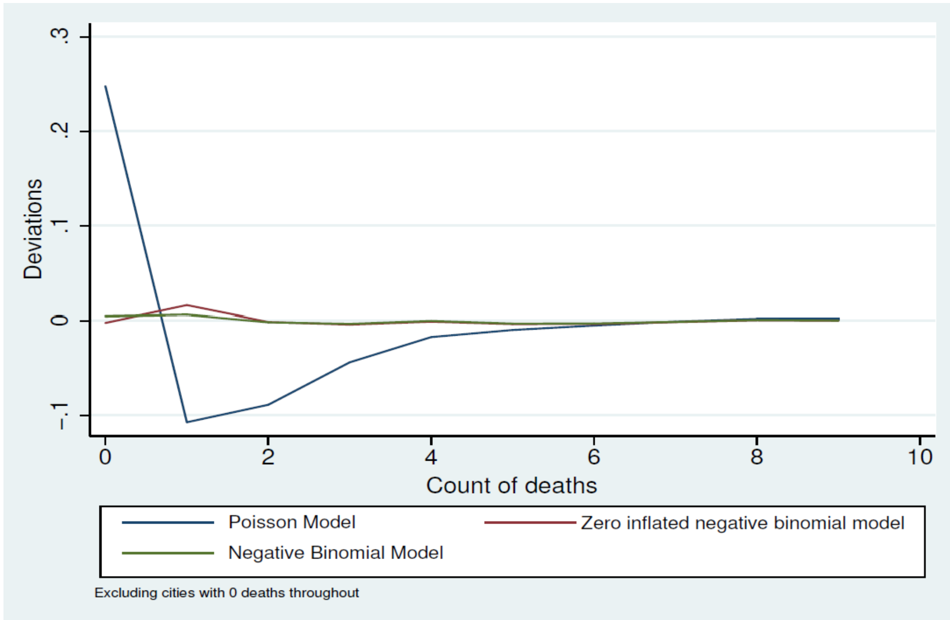


(a) Overdispersion including cities with 0 deaths throughout



(b) Overdispersion excluding cities with 0 deaths throughout

Figure 3.2: Deviations of predicted values of deaths using different models for dataset (B)



Bibliography

Althoff L., Eckert F., Ganapani S., Walsh, C. The Geography of Remote Work. 2021; *National Bureau of Economics Research* Working Paper 29181.

Ariel, B., Farrar, W.A. & Sutherland, A. The Effect of Police Body-Worn Cameras on Use of Force and Citizens' Complaints Against the Police: A Randomized Controlled Trial *Journal of Quantitative Criminology* 31, 509-535 (2015). https://doi.org/10.1007/s10940-014-9236-3

Bartik Alexander W., Cullen Zoe B., Glaeser Edward L., Luca M. , Stanton Christoper T. What Jobs are Being Done at Home During the Covid-19 Crisis? Evidence from Firm-Level Surveys."2020. *National Bureau of Economics Research* Working Paper 27422.

Batalha, M., Gonçalves, D., Peralta, S., Pereira, J.D.S. 2022. The virus that devastated tourism: The impact of covid-19 on the housing market. *Reg Sci Urban Econ.* 95:103774. doi: 10.1016/j.regsciurbeco.2022.103774. Epub 2022 Mar 3. PMID: 35945933; PMCID: PMC9353041.

Behrens K., Kichko S., Thisse J. Working From Home: Too much of a Good Thing. 2021; *CESifo* Working Paper 8831.

Benfer, E.A., Koehler, R., Mark, A., Nazzaro, V., Alexander, A.K., Hepburn, P., Keene D.E., Desmond, M. 2022. COVID-19 Housing Policy: State and Federal Eviction Moratoria and Supportive Measures in the United States During the Pandemic, Housing Policy Debate *Housing Policy Debate* DOI: 10.1080/10511482.2022.2076713

Bertrand, M., Duflo, E., Mullainathan, S. 2004. How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics* vol. 119 (1), 249–275, https://doi.org/10.1162/003355304772839588

Bloom N., Davis Stevens J., Zhestkova Y. COVID-19 Shifted Patent Applications Toward Technologies that Support Working from Home. 2020. *SSRN Scholarly Paper* 3695191.

Bloom N., Ramani A. The Donut Effect of Covid-19 on Cities. 2021; *Center for Economic Performance* Discussion Paper 1793

Borusyak, K., Jaravel, X. 2018 Revisiting Event Study Designs, with an Application to the Estimation of the Marginal Propensity to Consume

Brueckner, J. K., Kahn, E., Lin, G. C. 2021. A new spatial hedonic equilibrium in the emerging work-from-home economy? *American Economic Journal: Applied Economics* Forthcoming

Brueckner, J. K., Sayantani. S. 2022. Intercity Impacts of Work-from-Home with Both Remote and Non-Remote Workers. *Journal of Housing Economics:* 59B (2023) 101910, https://doi.org/10.1016/j.jhe.2022.101910.

Brynjolfsson E., Horton John J., Ozimek A., Rock D., Sharma G., TuYe H. COVID-10 and Remote Work: An Early Look at US Data. 2020. *National Bureau of Economics Research* Working Paper 27344.

Buehler, James W. Racial/Ethnic Disparities in the Use of Lethal Force by US Police, 2010-2014 *American Journal of Public Health* 107 (2017), 295_297, https://doi.org/10.2105/AJPH.2016.303575

Carmichael, Jason T. and Kent, Stephanie L. The Persistent Significance of Racial and Economic Inequality on the Size of Municipal Police Forces in the United States, 1980–2010. *Social Problems*, Volume 61, Issue 2, 1 May 2014, Pages 259–282, https://doi.org/10.1525/sp.2014.12213

Coven, J., Gupta, A., Yao, I. 2022. Urban Flight Seeded the COVID-19 Pandemic Across the United States. *Journal of Urban Economics* Aug:103489. DOI: 10.1016/j.jue.2022.103489. PMID: 35936355; PMCID: PMC9344811.

Davis Moris A., Ghent Andra C., Gregory J. The Work-at-Home Technology Boom and its Consequences. 2021. *National Bureau of Economics Research* Working Paper 28461.

de Chaisemartin, C., D'Haultfœuille, X. 2020. Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects. *American Economic Review* 110 (9): 2964-96.

de Chaisemartin, C., D'Haultfœuille, X. 2022. Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey. *National Bureau of Economic Research* Working Paper No. 29691.

Delventhal M., Parkhomenko A. Spatial Implications of Telecommuting. 2020. Unpublished Paper.

Delventhal, M., Kwon, E., Parkhomenko, A. 2022. How do cities change when we work from home? *Journal of Urban Economics* 127, article 10331

D'Lima, W., Lopez, L.A., Pradhan, A. 2021. COVID-19 and housing market effects: Evidence from U.S. shutdown orders *Real Estate Economics* vol 50 (2) : 303-339 https://doi.org/10.1111/1540-6229.12368

Delventhal, M., Parkhomenko, A. 2021. Spatial implications of telecommuting. *SSRN* Working Paper.

Dharmapala, D., McAdams, Richard H., Rappaport, J. Collective Bargaining Rights and Police Misconduct: Evidence from Florida. *University of Chicago Coase-Sandor Institute for Law & Economics Research* Paper No. 831, U of Chicago, Public Law Working Paper No. 655 (August 1, 2019), http://dx.doi.org/10.2139/ssrn.3095217

Edwards, F., Esposito, Michael H., Lee, H., 2018 Risk of Police-Involved Death by Race/Ethnicity and Place, United States, 2012-2018 *American Journal of Public Health* 108, 1241-1248, https://doi.org/10.2105/AJPH.2018.304559

Edwards, F., Lee, H., Esposito, Michael H. Risk of being killed by police use of force in the United States by age, race-ethnicity, and sex. *PNAS* (August 20, 2019) 116 (34) 16793-16798

Elenev, V., Quintero, L.E., Rebucci, A., Simeonova, E. 2021. Direct and Spillover Effects from Staggered Adoption of Health Policies: Evidence from COVID-19 Stay-at-Home Orders. *National Bureau of Economic Research* Working Paper No. 29088.

Epp, C.R., Maynard-Moody, S., Haider-Markel, D. Beyond Profiling: The Institutional Sources of Racial Disparities in Policing. *Public Administration Review*, 77 (2017): 168-178. doi:10.1111/puar.12702

Epp, Charles R., Maynard-Moody, S., Haider-Markel, Donald P. Pulled Over How Police Stops Define Race and Citizenship. *The University of Chicago Press Books*, 2014

Fagan, Jeffrey A., Campbell, Alexis D. Race and Reasonableness in Police Killings *Boston University Law Review*, Vol. 100, P. 951, (2020); Columbia Public Law Research Paper No. 14-655.

Finch, B.K., Beck, A., Burghart, D.B., Johnson, R., Klinger, D. and Thomas,K. Using Crowd-Sourced Data to Explore Police-Related-Deaths in the United States (2000-2017): The Case of Fatal Encounter. *Open Health Data* (2019) 6(1), p.1. DOI: http://doi.org/10.5334/ohd.30

Fridell, L., Lim, H. Assessing the racial aspects of police force using the implicit- and counter-bias perspectives *Journal of Criminal Justice* Volume 44, (March 2016), Pages 36-48 [13]

Fryer Jr., Roland G. Reconciling Results on Racial Differences in Police Shootings *American Economic Review (Papers and Proceedings)* (2018). Forthcoming

Goh, Li S. Going Local: Do Consent Decrees and Other Forms of Federal Intervention in Municipal Police Departments Reduce Police Killings? *Justice Quarterly*, (2020)37:5, 900-929, DOI: 10.1080/07418825.2020.1733637

Gokan T., Kichko S., Thisse J. On the Impact of Telecommuting on Cities. 2021. Unpublished Paper.

Goodman-Bacon, A. 2018 Difference-in-Differences with Variation in Treatment Timing *National Bureau of Economic Research* Working Paper No. 25018.

Goodman-Bacon, A., Marcus, J. 2020. Using Difference-in-Differences to Identify Causal Effects of COVID-19 Policies. *Survey Research Methods* 14(2), 153-158. https://doi.org/10.18148/srm/2020.v14i2.7723

Gupta A., Mittal V., Peters J., Nieuwerburgh Stijin V. Flattening the Curve: Pandemic Induced Revaluation of Urban Real Estate. 2021. Unpublished Paper.

Gurmu, S. and P.K. Trivedi Excess Zeros in Count Models for Recreational Trips. *Journal of Business and Economic Statistics*, 14 (1996) 469-477.

Hemenway, D., Azrael, D., Conner, A. Variation in Rates of Fatal Police Shootings across US States: the Role of Firearm Availability. *Journal of Urban Health* 96 (2019), 63-73 (2019). https://doi.org/10.1007/s11524-018-0313-z

Holmes, Malcom D., Painter II, Matthew A., Smith, Brad W. (2019) Race, Place, and Police-Caused Homicide in U.S. Municipalities. *Justice Quarterly*, 36:5, 751-786, DOI: 10.1080/07418825.2018.1427782

Huang, N., Pang, J., Yang, Y. 2022. COVID-19 and Household Preference for Urban Density in China. *Journal of Urban Economics* 103487. doi: 10.1016/j.jue.2022.103487. Epub ahead of print. PMID: 35873868; PMCID: PMC9295400.

Jacobs, D. and O'Brien, Robert M. The Determinants of Deadly Force: A Structural Analysis of Police Violence (1998) *American Journal of Sociology*, Vol. 103, No. 4, pp. 837-862

Jennings, Jay T., Rubado, Meghan E. Preventing the Use of Deadly Force: The Relationship between Police Agency Policies and Rates of Officer-Involved Gun Deaths. *Public Adminstration Review*, 77 (2017): 217-226

Jowers, K., Timmins,C., Bhavsar, N., Hu, Q., Marshall, J. 2021.Housing Precarity & the COVID-19 Pandemic: Impacts of Utility Disconnection and Eviction Moratoria on Infections and Deaths Across US Counties, *National Bureau of Economic Research* Working Paper No. 28394.

Kent, S. L., & Jacobs, D. (2005). Minority threat and police strength from 1980 to 2000: A fixed-effects analysis of nonlinear and interactive effects in large US cities. *Criminology*, 43(3), 731–760.

Klinger, D., Rosenfeld, R., Isom, D., Deckard, M. Race, Crime, and the Micro-Ecology of Deadly Force. *Criminology & Public Policy*, 15 (2016) : 193-222.

Knox, D., Lowe, W., Mummolo, J. (2020) Administrative Records Mask Racially Biased Policing. *American Political Science Review*, 114(3), 619-637. doi:10.1017/S0003055420000039

Legewie, J. Racial Profiling and Use of Force in Police Stops: How Local Events Trigger Periods of Increased Discrimination. *American Journal of Sociology* (2016) 122:2, 379-424

Leifheit, K.,M., Schwartz, G.,L., Pollack, C.,E., Black, M.,M., Edin, K.,J., Althoff, K.,N., Jennings, J.,M. 2020. Eviction in early childhood and neighborhood poverty, food security, and obesity in later childhood and adolescence: Evidence from a longitudinal birth cohort. *SSM Popul Health.* Apr 4;11:100575. doi: 10.1016/j.ssmph.2020.100575. PMID: 32322657; PMCID: PMC7171520.

Lim, U., Galster, G. (2009) The dynamics of neighborhood property crime rates. *Annals of Regional Science*, 43(4), 925-945. https://doi.org/10.1007/s00168-008-0226-y

Liska, A. E., Yu, J. (1992) Specifying and testing the threat hypothesis: Police use of deadly force. *Social Threat and Social Control*, 53–68.

Liu, S., Su, Y. 2021. The Impact of the COVID-19 Pandemic on the Demand for Density: Evidence from the U.S. Housing Market. *SSRN* https://ssrn.com/abstract=3661052 or http://dx.doi.org/10.2139/ssrn.3661052

Mas, Alexandre. Pay, Reference Points, and Police Performance. *The Quarterly Journal of Economics* 121, no. 3 (2006): 783-821. Accessed December 12, 2020. http://www.jstor.org/stable/25098809.

Nande, A., Sheen, J., Walters, E.L. et al. 2021. The effect of eviction moratoria on the transmission of SARS-CoV-2. *Nat Commun* 12, 2274 https://doi.org/10.1038/s41467-021-22521-5

Nicholson-Crotty, S., Nicholson-Crotty, J., Fernandez, S. Will More Black Cops Matter? Officer Race and Police-Involved Homicides of Black Citizens. *Public Adminstration Review*, 77 (2017): 206-216. doi:10.1111/puar.12734

Nix, Justin, Campbell, Bradley A., Byers, Edward H., Alpert, Geoffrey P. A Bird's Eye View of Civilians Killed by Police in 2015 *Criminology and Public Policy*, 16 (2017) : 309-340.

Osgood, D. W., Chambers, J. M. (2000). Social disorganization outside the metropolis: Analysis of rural youth violence. *Criminology*, 38(1), 81–116. doi:10.1111/j.1745-9125.2000.tb00884.

Parker, K., MacDonald, J., Jennings, W., Alpert, G. (2005). Racial Threat, Urban Conditions and Police Use of Force: Assessing the Direct and Indirect Linkages Across Multiple Urban Areas. *Justice Research and Policy*, 7(1), 53-79.

Ray, R. (2020) Restructuring Civilian Payouts for Police Misconduct". *Sociol Forum*, 35: 806-812. doi:10.1111/socf.12618

Safirova E. Telecommuting, Traffic Congestion and Agglomertaion: A General Equilibrium Model. *Journal of Urban Economics* 2002. 52(1): 26-52.

Sant'Anna, P., Callaway, B. 2021. Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics* vol. 225 (2), p. 200-230, 2021.

Smith, B. W. (2003) The impact of police officer diversity on police-caused homicides. *Policy Studies Journal*, 31(2), 147–162. doi:10.1111/1541-0072.t01-1-00009

Smith, Brad W. 2004 Structural and organizational predictors of homicide by police. *Policing: An International Journal of Police Strategies and Management*, 27: 539–557.

Sorenson, Jonathan R., James W. Marquart, Deon E. Brock. 1993. Factors related to killings of felons by police officers: A test of the community violence and conflict hypotheses. *Justice Quarterly*, 10: 417–440.

Spencer, K. B., Charbonneau, A. K., Glaser, J. (2016) Implicit Bias and Policing. *Social and Personality Psychology Compass*, 10: 50-63. doi: 10.1111/spc3.12210.

Stanton Christopher T., Tiwari P. Housing Consumption and the Cost of Remote Work. 2021; *National Bureau of Economics Research* Working Paper 28461.

Sun, L., Abraham, S. 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects *Journal of Econometrics* vol. 225 (2), p. 175-199, 2021.

Willits D. W. The Organisational Structure of Police Departments and Assaults on Police Officers. *International Journal of Police Science & Management.* 2014;16(2):140-154. doi:10.1350/ijps.2014.16.2.334

Willits, Dale W., Nowacki Jeffrey S. (2014) Police organisation and deadly force: an examination of variation across large and small cities. *Policing and Society*, 24:1, 63-80, DOI: 10.1080/10439463.2013.784314

Yoruk, B.K. 2022. Early effects of COVID-19 pandemic-related state policies on housing market activity in the United States. *Journal of Housing Economics* vol. 57, 101857, ISSN 1051-1377, https://doi.org/10.1016/j.jhe.2022.101857.

Zhao Y. 2020. US Housing Market during COVID-19: Aggregate and Distributional Evidence. *IMF Working Paper* No. 2020/212, 9781513557816/1018-5941

Appendix A

Chapter 1

Figure A1 shows the variation in the duration of overall or CDC moratorium across states. This indicator is 1 if overall moratorium, CDC moratorium or both were in effect in a given state.

Figure A2 shows the event-study graphs of the impact of ending SIP on year-on-year sales change for condos, townhouses, single-family houses, multi-family houses and all residences.

Figure A3 shows the event-study graphs of the impact of ending overall moratorium, either cause-based eviction execution moratorium and hardship-based eviction execution moratorium indicators on the change in 5+ Unit Building Permits.

Table A1 shows the TWFE results with change in number of units and their valuations (for which on building permits are approved) as dependent variables, for single-unit, double-unit and three-four unit buildings. Different indicators of eviction moratorium are the treatment variables.

Table A2 shows the results from using the DID_M estimator to estimate the effect of different eviction moratoria on change in multi-family housing sales. The basic format of this table is similar to Table 5(b) and 6(b) with the treatments being moratoria and dependent variable being change in sales. The state_trends specification is analogous to that used in table 6(b). N and $N\_switchers$

are defined the same way as in table 5(b) and 6(b) as well.[1] The results shown use 50 bootstrap replications in the computation of estimators' standard errors. These standard errors are clustered at the county level. † implies that DID_M's version of parallel trends assumption has been met. Similar to tables 6(a), 6(b), and 7(a) owing to a large volume of indicators, only the moratoria indicators that are associated with significant changes in year-on-year sales changes are presented in the table.

The parallel trends assumption seem to be satisfied for the regressions where moratoria on eviction judgements are used as treatments and when CDC moratorium is used as treatment without controls and state_trends. The regression with the CDC moratorium as treatment produces the highest magnitude of the coefficient, although the parallel trends assumption isn't satisfied when controls and state_trends are added, unlike the other indicators listed. Adding controls and state_trends raises the magnitudes of the coefficients associated with CDC moratorium and moratorium on both/executions. Controls and state_trends reduce the magnitudes of the impact on year-on-year sales change associated with moratorium on hardship/executions and either/executions. To summarize, controlling for COVID cases, Δ Employment and state_trends, the adoption of moratoria on multiple indicators of eviction executions are associated with an average increase in year-on-year multi-family sales by approximately 2 units. Controlling for the same factors, the adoption of CDC moratorium is associated with an average increase in year-on-year multi-family sales by approximately 4 units.

Table A3 and A4 show the summary statistics and regression results, respectively, using a 9-month (January 2020 to September 2021) unbalanced county-level panel dataset that includes multiple covariates sourced from Brueckner et al. (2021). Table A4 shows the OLS results with year-on-year change in sales of all residential buildings as dependent variable and a shelter-in-place indicator as the primary treatment variable. Time-invariant controls like a ruggedness index, a quality of life index, a WFH potential indicator, and the Wharton Residential Land Use Regulation Index are added in all the regressions. Columns (2) and (3) display results from including only the

[1] N is the total number of panel observations used in the estimation of the coefficients reported in the table. $N\_switchers$ is number of switchers or first-time switchers the coefficient applies to applies to.

county level number of COVID cases and year-on-year employment change as controls, respectively. Column (1) includes neither and column (4) includes both.

Tables A5 and A6 show results from running the baseline TWFE regressions of sales change on SIP, using a 9-month panel (January 2020 to September 2020) and 6-month panel (January 2020 to June 2020) respectively. With slight changes in magnitudes, the coefficients in both the tables follow similar patterns to those in Table 5(a) in the main text.

Tables A7 and A8 show results from running the regressions of sales change on SIP with the DID_M estimator, using a 9-month panel (January 2020 to September 2020) and 6-month panel (January 2020 to June 2020) respectively. With slight changes in magnitudes, the coefficients and placebo effects in both the tables follow similar patterns to those in Table 5(b) in the main text.

Table A9 show results from running the baseline TWFE regressions of change in 5+ unit building permit approvals on eviction moratoria indicators, using a 24-month panel (Jan 2019 to Jan 2021). Treatments that produce significant effects and their corresponding coefficients in this table follows similar patterns to those in Table 6(a) in the main text.

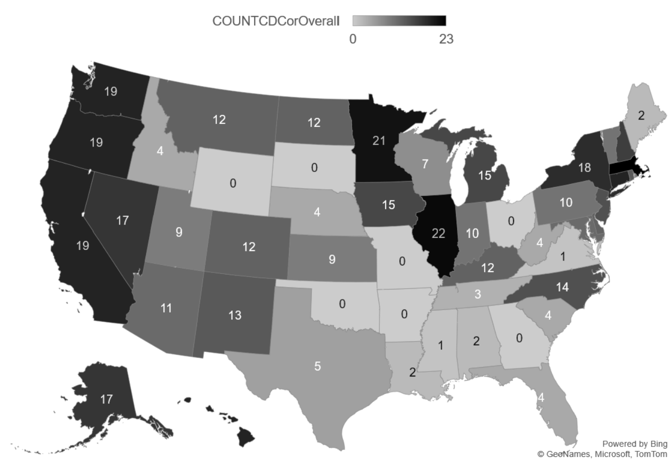
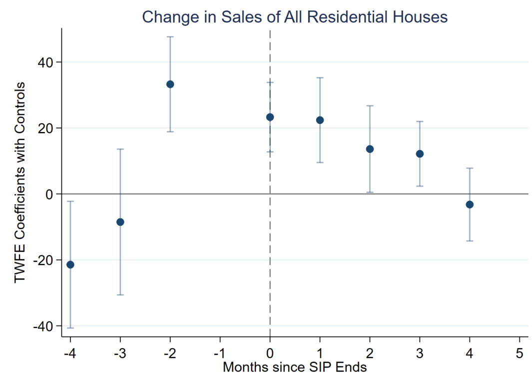
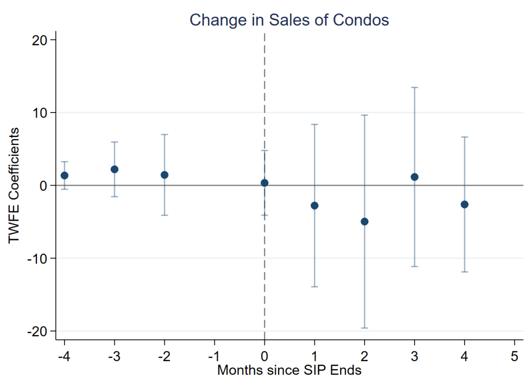


Fig A1: Overall or CDC Moratoria

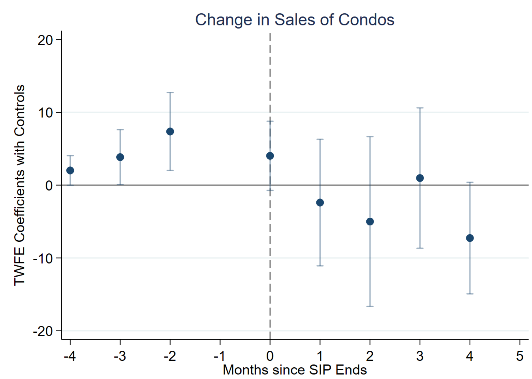
(a) Δ Sales for All Residential Houses without controls



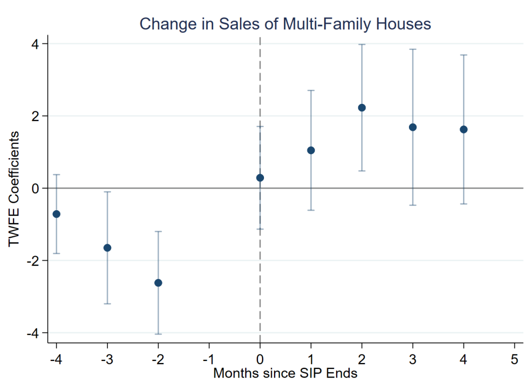
(b) Δ Sales for All Residential Houses with controls



(c) Δ Sales for Condos without controls



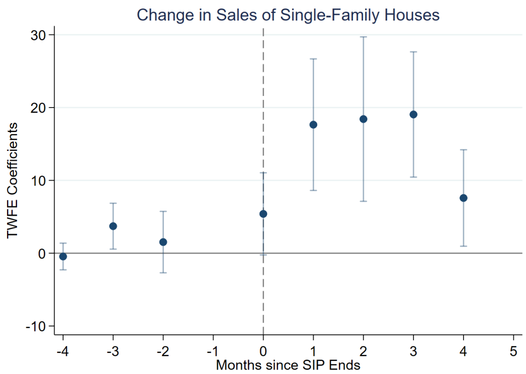
(d) Δ Sales for Condos with controls



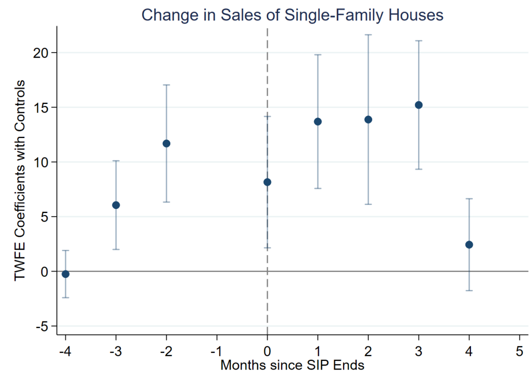
(e) Δ Sales for Multi-Family houses without controls

(f) Δ Sales for Multi-Family Houses with controls

Fig A2: TWFE Event-study graphs with 95% CI of SIP Ending



(g) Δ Sales for Single-Family houses without controls

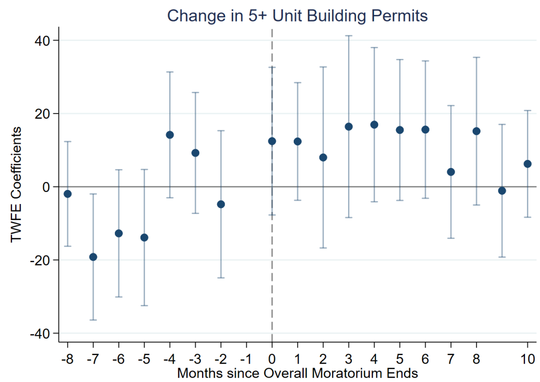


(h) Δ Sales for Single-Family Houses with controls

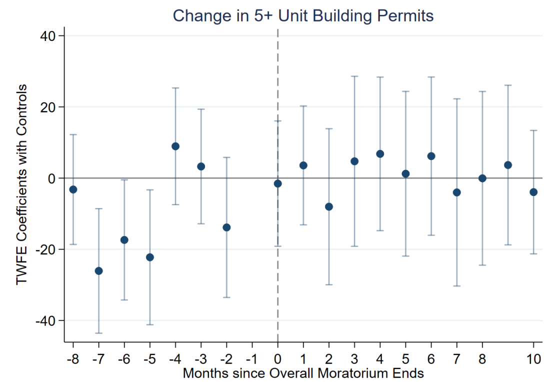
(i) Δ Sales for Townhouses without controls

(j) Δ Sales Townhouses with controls

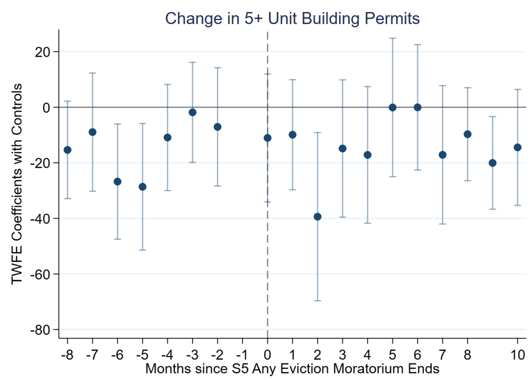
Fig A3: TWFE Event-study graphs with 95% CI of SIP Ending (continued)



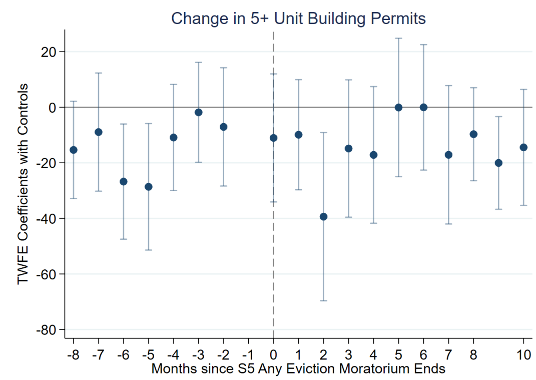
(a) Overall Eviction Moratorium without controls



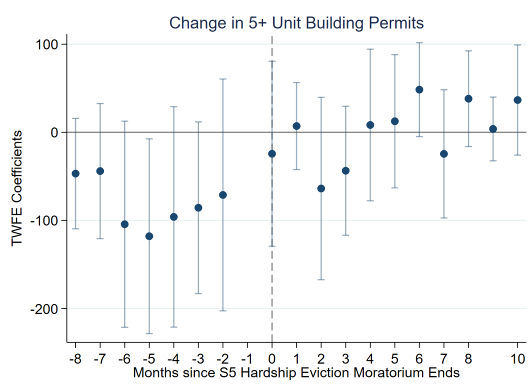
(b) Overall Eviction Moratorium with controls



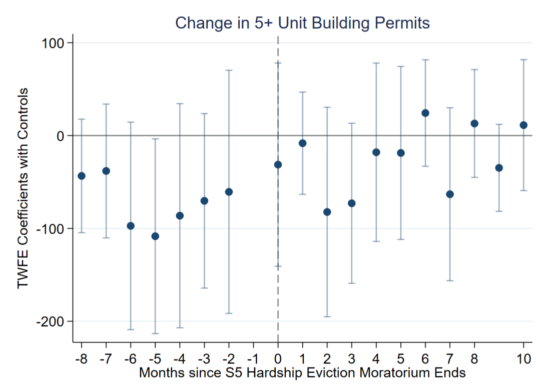
(c) Both/Execution Moratorium without controls



(d) Both/Exection Moratorium with controls

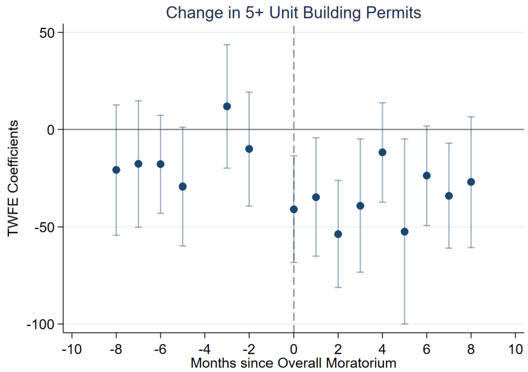


(e) Hardship/Execution Moratorium without controls

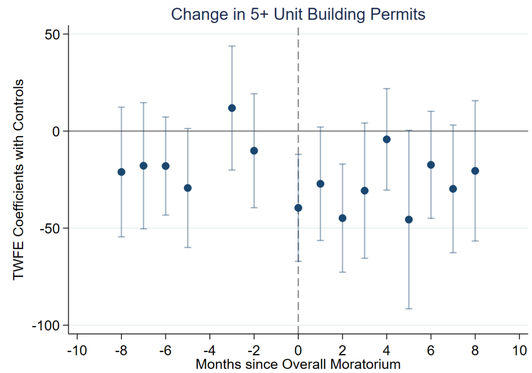


(f) Hardship/Execution Moratorium with controls

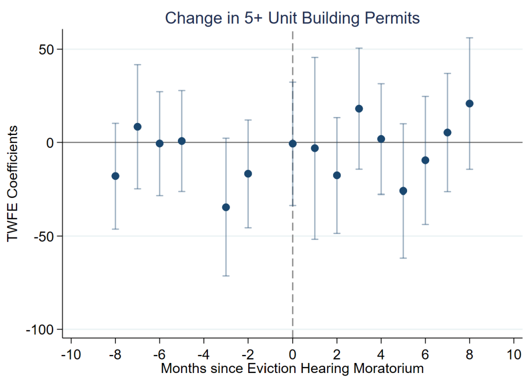
Fig A4: TWFE Event-study graphs with 95% confidence intervals of Moratoria ending



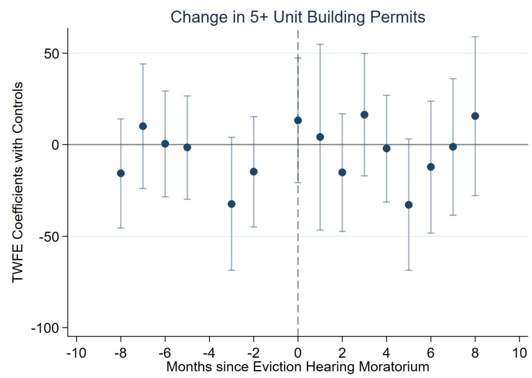
(a) Overall Eviction Moratorium without controls



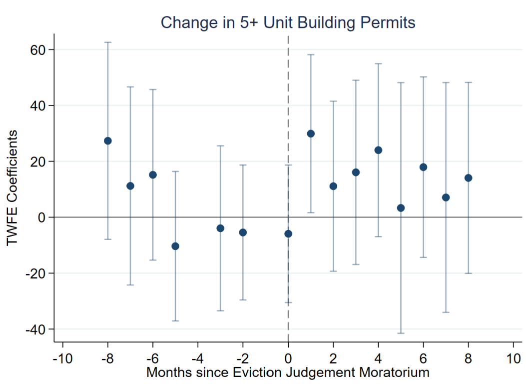
(b) Overall Eviction Moratorium with controls



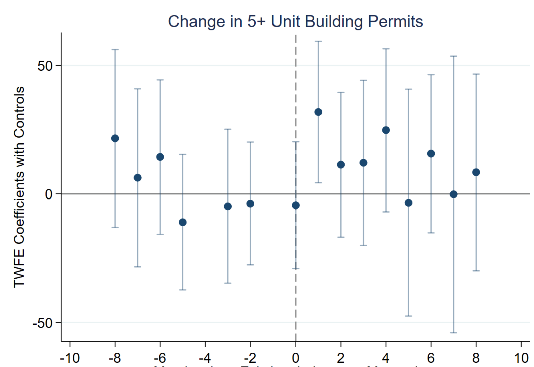
(c) ./Hearing Moratorium without controls



(d) ./Hearing Moratorium with controls



(e) ./Judgement Moratorium without controls



(f) ./Judgement Moratorium with controls

Fig A5: TWFE Event-study graphs with 95% confidence intervals of Eviction Moratotoria beginning

Table A1: TWFE results from regressing change in Building Permits on Eviction Moratorium

	Δ **Single Units**		Δ **Double Units**		Δ **Three - Four Units**	
	Δ Units	Δ Valuation	Δ Units	Δ Valuation	Δ Units	Δ Valuation
Treatments	(1)	(2)	(3)	(4)	(5)	(6)
Overall Moratorium	6.68* (3.44)	−108.00 (92.36)	1.34*** (0.50)	−2.48 (4.17)	2.69 (1.70)	2.20 (25.88)
Hardship/Hearing	−5.79 (4.85)	−178.08 (140.78)	2.54*** (0.87)	5.44 (5.99)	2.02 (1.30)	−9.59 (29.09)
Non-Payment/Hearing	7.95 (5.10)	280.54* (146.32)	−0.31 (0.87)	−25.25*** (8.60)	−3.23** (1.66)	−61.69** (28.04)
Both/Hearing	−8.90* (4.62)	−172.26* (96.35)	1.07* (0.56)	2.92 (4.53)	0.43 (2.62)	−42.55 (51.98)
Hardship/Judgement	−2.75 (3.94)	−158.22 (120.71)	2.15** (0.93)	−2.93 (6.41)	2.84* (1.50)	15.62 (20.31)
Non-Payment/Judgement	0.40 (5.47)	176.58 (182.31)	−1.39* (0.83)	−0.46 (7.04)	−3.25** (1.50)	−26.59 (18.76)
Both/Judgement	−4.04 (3.62)	−65.31 (94.10)	1.68** (0.67)	0.80 (4.51)	0.22 (1.25)	−48.34** (22.03)
Hardship/Execution	−8.71* (4.82)	−266.63** (105.43)	−0.45 (1.49)	−8.89 (12.29)	7.60** (3.41)	144.85** (69.89)
Non-Payment/Execution	−9.49** (4.59)	−408.82*** (115.53)	0.89 (0.97)	−11.26 (13.07)	12.09*** (4.30)	173.66** (75.51)
Both/Execution	−13.58*** (4.27)	−365.73*** (90.02)	1.55*** (0.57)	−0.62 (5.00)	7.75*** (2.54)	59.91 47.03
Either/Hearings	6.56 (4.52)	225.08* (128.97)	0.06 (0.78)	−20.59*** (7.61)	2.49* (1.45)	−52.46** (24.29)
Either/Executions	−7.06** (3.47)	−254.39*** (81.27)	0.16 (1.02)	−8.70 (8.57)	8.39*** (2.76)	128.25** (50.74)
CDC Moratorium	3.29 (9.65)	281.92 (223.35)	−.88 (1.25)	11.52* (6.93)	−2.51 (1.55)	2.62 (22.67)
CDC or Overall Moratorium	−1.31 (3.91)	−13.74 (93.28)	0.90* (0.48)	2.81 (4.19)	1.54 (1.45)	1.89 (22.51)

Standard errors are clustered at county level and are presented in parentheses.

*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. All results include COVID cases COVID cases and Δ Employment. Δ Valuations are in 10000's of dollars

Table A2: DID_M results from regressing change in Multi-Family Sales on Eviction Moratorium

	Δ Sales	*N_switchers*	*N*
Both/Execution	$-2.04^{**\dagger}$	380	1746
	(0.95)		
+ Controls	$-2.04^{***\dagger}$	380	1746
	(0.77)		
+ State_trends	$-2.13^{**\dagger}$	380	1746
	(0.90)		
Hardship/Execution	$-1.92^{**\dagger}$	225	2583
	(0.98)		
+ Controls	$-1.81^{*\dagger}$	225	2583
	(1.05)		
+ State_trends	$-1.80^{*\dagger}$		
	(0.96)	225	2583
Either/Execution	-1.21	305	2628
	(0.82)		
+ Controls	$-1.21^{*\dagger}$		
	(0.70)	305	2628
+ State_trends	$-1.20^{*\dagger}$		
	(0.79)	305	2628
CDC Moratorium	$3.66^{**\dagger}$		
	(1.86)	77	801
+ Controls	3.62^{*}	77	801
	(1.91)		
+ State_trends	3.64^{**}	77	801
	(1.90)		

Standard errors are clustered at county level and are presented in parentheses. *, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. † implies that for significant DID_M estimators, the placebo effect is not significantly different from 0 at 10% significance level. Controls include both COVID cases and Δ Employment.

Table A3: Summary Statistics of All Residential Sales datasets in 9-month panel with Brueckner et al. (2021) covariates

	Mean	St. Dev	Min	Max
Change in Sales	-4.8	1984.92	-3406	1907
Change in Employment	-11933.88	37304.8	-707976	73083
COVID cases	88698.87	306690.4	0	7694092
COVID deaths	3644.70	14489.65	0	225295
Average Temperature	60.10	7.20	46.39	78.69
Average Precipitation	3.90	1.50	0.33	7.61
Ruggedness	0.014	0.013	0.00	0.09
WFH Potential	0.30	0.06	0.19	0.69
Quality of Life Index	-0.01	0.04	-0.1	0.18
WRLURI	0.024	0.70	-1.76	4.31
2020 Population Density	393.69	1512.26	1.02	27470.61

Data collected for 584 counties

WRLURI stands for Wharton Residential Land Use Regulation Index

Table A4:OLS with month-fixed effects results from regressing change in All-residential Sales on SIP with Brueckner et al. (2021) covariates

	Δ All Residential Sales			
	(1)	(2)	(3)	(4)
SIP	-66.02^{***} (10.40)	-66.17^{***} (10.41)	-59.01^{***} (9.39)	-58.83^{***} (8.73)
COVID Cases		0.00 (0.00)		0.00^{***} (0.00)
ΔEmployment			0.00^{***} (0.00)	0.00^{***} (0.00)
Average Precipitation	2.97 (1.96)	3.20 (1.98)	-2.84 (1.79)	0.46 (1.67)
Average Temperature	-1.10^{***} (0.40)	-1.18^{***} (0.41)	0.13 (0.37)	-1.10^{***} (0.34)
Ruggedness	-115.18 (234.84)	-102.46 (235.07)	-317.40 (212.15)	-114.22 (197.18)
Quality of Life Index	5.20 (76.59)	2.42 (76.62)	118.62^{*} (69.21)	113.14^{*} (64.31)
WFH Potential	-144.15^{***} (41.80)	-152.65^{***} (42.40)	199.76^{***} (39.04)	181.47^{***} (36.28)
WRLURI	-8.40^{**} (4.21)	-8.73^{**} (4.22)	-4.21 (3.80)	-10.28^{***} (3.54)
2020 Pop. Density	-0.01^{***} (0.00)	-0.01^{***} (0.00)	0.01^{***} (0.00)	0.01^{***} (0.00)
Month Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.21	0.21	0.35	0.44

Standard errors are clustered at county level and are presented in parentheses. *, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively.

Table A5: TWFE results from regressing change in Sales on SIP using 9-month panel

	Δ **Sales**				
	Single-Family	Townhouse	Condo	Multi-Family	All Residences
	(1)	(2)	(3)	(4)	(5)
SIP	-28.58^{***}	-15.59^{***}	-16.97^{***}	-2.31^{***}	-44.46^{***}
	(5.02)	(4.39)	(4.09)	(0.88)	(6.83)
+ COVID Cases	-27.94^{***}	-15.77^{***}	-17.41^{***}	-2.06^{**}	-43.74^{***}
	(4.93)	(4.41)	(4.18)	(0.89)	(6.73)
+ Δ Employment	-25.70^{***}	-15.87^{***}	-16.28^{***}	-2.08^{**}	-40.022^{***}
	(4.27)	(4.21)	(3.61)	(0.89)	(5.70)
+ Both	-22.82^{***}	-16.44^{***}	-17.40^{***}	-2.19^{**}	-36.37^{***}
	(3.70)	(4.20)	(3.64)	(0.90)	(4.94)
N(Treated)	1278	311	375	261	1280
N(Control)	50	4	7	4	50
N	1328	315	382	265	1330

Standard errors are clustered at county level and are presented in parentheses.

*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. N stands for total no. of counties. N(Treated) and N(Control) stand for number of treated and control counties, respectively.

Table A6: TWFE results from regressing change in Sales on SIP using 6-month panel

	Δ Sales				
	Single-Family	Townhouse	Condo	Multi-Family	All Residences
	(1)	(2)	(3)	(4)	(5)
SIP	-22.75^{***}	-12.64^{***}	-17.55^{***}	-2.54^{***}	-35.94^{***}
	(4.97)	(3.60)	(4.55)	(0.90)	(6.71)
+ COVID Cases	-18.89^{***}	-11.93^{***}	-13.27^{***}	-1.00	-28.98^{***}
	(4.67)	(3.31)	(4.31)	(0.82)	(6.11)
+ Δ Employment	-15.93^{***}	-12.57^{***}	-15.17^{***}	-2.15^{**}	-25.37^{***}
	(3.57)	(3.19)	(3.85)	(0.91)	(4.55)
+ Both	-16.94^{***}	-12.59^{***}	-14.66^{***}	-1.24	-26.15^{***}
	(3.61)	(3.10)	(3.87)	(0.88)	(4.62)
N(Treated)	1278	311	375	261	1280
N(Control)	50	4	7	4	50
N	804	1382	382	265	1330

Standard errors are clustered at county level and are presented in parentheses.

*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. N stands for total no. of counties. N(Treated) and N(Control) stand for number of treated and control counties, respectively.

Table A7: DID_M results from regressing change in Sales on SIP using 9-month panel

			Δ Sales		
	Single-Family	Townhouse	Condo	Multi-Family	All Residences
	(1)	(2)	(3)	(4)	(5)
SIP	$-23.29^{***\dagger}$	-2.65	$-14.83^{***\dagger}$	-0.54	-27.81^{***}
	(2.94)	(4.56)	(3.03)	(0.82)	(5.27)
State_trends	$-24.48^{***\dagger}$	-1.76	$-15.05^{***\dagger}$	-0.42	-28.59^{***}
	(4.17)	(5.69)	(3.58)	(1.15)	(6.13)
Adding Controls					
+ COVID Cases	$21.22-^{***\dagger}$	-2.57	-13.85^{***}	-0.53	$-24.03^{***\dagger}$
	(3.46)	(4.94)	(3.89)	(1.29)	(6.46)
State_trends	$-21.96^{***\dagger}$	-1.62	-13.50^{***}	-0.40	$-24.00^{***\dagger}$
	(3.40)	(5.65)	(3.33)	(0.89)	(5.67)
+ Δ Employment	$-8.12^{*\dagger}$	5.39	-5.21	1.21	-7.70
	(4.22)	(4.79)	(4.29)	(1.00)	(5.08)
State_trends	$-9.36^{***\dagger}$	7.58	-3.75	1.61	-8.19
	(3.23)	(5.99)	(4.59)	(1.52)	(6.35)
+ Both	$-8.25^{**\dagger}$	5.04	-5.50	1.09	-7.12
	(3.80)	(4.02)	(4.80)	(1.48)	(5.13)
State_trends	$-9.16^{**\dagger}$	7.05	-3.98	1.46	-6.92
	(4.44)	(6.32)	(3.97)	(1.34)	(5.63)
N	3796	945	1126	779	3803
$N\_switchers$	2485	586	718	498	2489
$N\_switchers\_cont$	90	54	47	34	91

Standard errors are clustered at county level and are presented in parentheses.
*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. † implies that for significant DID_M estimators, the placebo effect is not significantly different from 0 at the 10% significance level.

Table A8: DID_M results from regressing change in Sales on SIP using 6-month panel

	Δ **Sales**				
	Single-Family	Townhouse	Condo	Multi-Family	All Residences
	(1)	(2)	(3)	(4)	(5)
SIP	$-23.29^{***\dagger}$	-2.64	$-14.83^{***\dagger}$	-0.54	-27.81^{***}
	(4.38)	(4.61)	(3.26)	(1.08)	(5.55)
State_trends	$-24.48^{***\dagger}$	-1.76	$-15.05^{***\dagger}$	-0.42	-28.59^{***}
	(4.21)	(5.74)	(3.48)	(0.97)	(5.65)
Adding Controls					
+ COVID Cases	$-24.55^{***\dagger}$	-3.42	-14.56^{***}	-0.60	$-27.94^{***\dagger}$
	(3.37)	(4.70)	(3.65)	(1.11)	(5.87)
State_trends	$-25.24^{***\dagger}$	-2.68	-14.26^{***}	-0.47	$-27.87^{***\dagger}$
	(3.90)	(6.86)	(4.45)	(1.47)	(7.23)
+ Δ Employment	-1.82	4.35	-7.77^{**}	0.53	-1.97
	(6.18)	(5.07)	(3.15)	(1.45)	(8.19)
State_trends	-2.69	8.11	-7.82^{**}	0.46	-2.44
	(6.67)	(7.66)	(3.47)	(1.27)	(7.12)
+ Both	$-14.68^{***\dagger}$	-1.02	-10.00^{***}	0.40	$-16.09^{***\dagger}$
	(4.43)	(5.42)	(3.68)	(1.69)	(5.23)
State_trends	$-14.98^{***\dagger}$	2.11	-9.64^{***}	0.17	$-15.75^{***\dagger}$
	(4.13)	(5.38)	(3.26)	(1.29)	(5.93)
N	3796	954	1126	779	3803
$N\_switchers$	2485	586	718	498	2489
$N\_switchers\_cont$	90	54	47	34	91

Standard errors are clustered at county level and are presented in parentheses.
*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. † implies that for significant DID_M estimators, the placebo effect is not significantly different from 0 at the 10% significance level.

Table A9: TWFE of regressing change in Building Permits on Eviction Moratorium using 24-moth panel

	5+ Units Building Permits			
	Δ Units	Δ Valuation	N(Treat)	N(Control)
Treatments	(1)	(2)	(3)	(4)
Non-Payment/Hearing	-25.94^{**}	-384.47^{**}	75	422
	(11.45)	(166.29)		
+ Controls	-32.39^{***}	-508.75^{***}	75	422
	(11.72)	(185.13)		
Both/Judgements	8.72	205.34^{**}	216	281
	(6.27)	(106.27)		
+ Controls	−3.56	−25.10	216	281
	(6.89)	(125.10)		
Non-Payment/Judgement	2.04	160.99	123	374
	(9.03)	(167.82)		
+ Controls	-18.31^{**}	−140.26	123	374
	(9.27)	(186.67)		
Either/Hearing	−16.78	-260.26^{*}	87	410
	(10.79)	(149.93)		
+ Controls	-27.86^{***}	-428.89^{***}	87	410
	(10.46)	(164.55)		
Overall or CDC Moratorium	−2.91	21.82	406	91
	(5.39)	(77.86)		
+ Controls	-11.47^{*}	−105.80	406	91
	(6.12)	(89.10)		

Standard errors are clustered at county level and are presented in parentheses.
*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. Controls include both COVID cases and Δ Employment.
Coefficients for ΔValuation are in 10000's of dollars.

Table A10:TWFE results from regressing change in Sales on SIP

	Δ Sales				
	Single-Family	Townhouse	Condo	Multi-Family	All Residences
	(1)	(2)	(3)	(4)	(5)
SIP	-29.02^{***}	-15.77^{***}	-17.03^{***}	-3.20^{***}	-79.52^{***}
	(4.93)	(4.65)	(3.86)	(0.97)	(10.49)
+ COVID Cases	-29.19^{***}	-15.89^{***}	-17.09^{***}	-2.66^{***}	-76.90^{***}
	(4.90)	(4.66)	(3.90)	(0.94)	(10.80)
+ Δ Employment	-24.38^{***}	-15.51^{***}	-14.54^{***}	-2.80^{***}	-52.32^{***}
	(4.06)	(4.44)	(3.29)	(0.92)	(8.13)
+ Both	-22.13^{***}	-15.97^{***}	-15.57^{***}	-2.76^{***}	-51.48^{***}
	(3.62)	(4.42)	(3.24)	(0.94)	(8.05)
Overall Mean	4.62	3.28	−3.75	−2.44	66.96
Pre-pandemic mean	91.25	28.94	36.42	9.22	115.48
N(Treated)	789	290	376	261	1223
N(Control)	15	3	6	4	46
N	804	293	382	265	1269

Standard errors are clustered at county level and are presented in parentheses.

*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. N stands for total no. of counties. N(Treated) and N(Control) stand for number of treated and control counties, respectively.

Overall means show the average year-on-year changes for each housing type across the counties and months contained in the dataset.

Pre-pandemic means stand for average year-on-year changes for each housing type across the counties in January 2020 before the pandemic began.

Table A11: DID_M results from regressing change in Sales on SIP

	Δ **Sales**				
	Single-Family	Townhouse	Condo	Multi-Family	All Residences
	(1)	(2)	(3)	(4)	(5)
SIP	$-23.28^{***\dagger}$	-2.64	$-14.82^{***\dagger}$	-0.54	-27.81^{***}
	(4.27)	(4.91)	(3.78)	(1.08)	(5.11)
State_trends	$-24.48^{***\dagger}$	-1.83	$-15.05^{***\dagger}$	-0.42	-28.59^{***}
	(3.59)	(5.05)	(3.61)	(0.90)	(5.42)
Adding Controls					
+ COVID Cases	$-21.22^{***\dagger}$	-2.57	-13.85^{***}	-0.53	$-24.03^{***\dagger}$
	(4.14)	(5.28)	(4.34)	(1.10)	(6.14)
State_trends	$-21.96^{***\dagger}$	-1.73	-13.50^{***}	-0.40	$-24.00^{***\dagger}$
	(4.11)	(6.23)	(3.82)	(1.02)	(5.30)
+ Δ Employment	$-17.70^{***\dagger}$	5.11	-6.90^{*}	1.13	$-8.56^{*\dagger}$
	(3.43)	(3.74)	(3.81)	(1.19)	(5.14)
State_trends	$-19.12^{***\dagger}$	7.82	-5.87	1.44	-8.54
	(3.03)	(4.91)	(3.84)	(1.35)	(5.04)
+ Both	$-17.43^{***\dagger}$	4.81	-7.1^{**}	1.01	-7.82
	(4.14)	(3.56)	(3.56)	(1.07)	(5.26)
State_trends	$-18.51^{***\dagger}$	7.31	-5.98	1.30	-7.07
	(4.18)	(7.01)	(3.89)	(1.33)	(5.04)
Overall Mean	4.62	3.28	-3.75	-2.44	66.96
Pre-pandemic mean	91.25	28.94	36.42	9.22	115.48
N	3796	945	1126	779	3803
$N\_switchers$	2485	586	718	498	2429
$N\_switchers\_cont$	90	52	47	34	91

Standard errors are clustered at county level and are presented in parentheses.
*, **, and *** imply that coefficients are significant at 10%, 5% and 1% levels, respectively. † implies that for significant DID_M estimators, the placebo effect is not significantly different from 0 at the 10% significance level.
Overall means show the average year-on-year changes for each housing type across the counties and months contained in the dataset.
Pre-pandemic means stand for average year-on-year changes for each housing type across the counties in January 2020 before the pandemic began.

Appendix B

Chapter 2

B.1 Non-WFH Equilibrium and Wage Computation

The non-WFH equilibrium condition for type-1 workers from (22) may be rewritten as

$$
\begin{aligned}
&A_s + a_{1s} - 2N_{1s} + cN_{2s} + \alpha - 2\beta(N_{1s} + N_{2s}) \\
&= A_d + a_{1d} - 2(\overline{N_1} - N_{1s}) + c(\overline{N_2} - N_{2s}) + \alpha - 2\beta(2\overline{N} - (N_{1s} + N_{2s})) \\
&\implies A_s - A_d + a_{1s} - a_{1d} - 2N_{1s}(1+\beta) + N_{2s}(c - 2\beta) \\
&\qquad = c\overline{N_2} - 2\overline{N_1} - 2\beta\overline{N_1} - 2\beta\overline{N_2} + 2N_{1s}(1+\beta) - N_{2s}(c-2\beta)
\end{aligned}
$$

$$
\implies A_s - A_d + a_{1s} - a_{1d} = (4N_{1s} + 2\overline{N_1})(1+\beta) - (2N_{2s} - \overline{N_2})(c - 2\beta) \tag{A1}
$$

The non-WFH equilibrium condition for type-2 workers from (23) may be rewritten as

$$
\begin{aligned}
&A_s + a_{2s} - 2N_{2s} + cN_{1s} + \alpha - 2\beta(N_{1s} + N_{2s}) \\
&= A_d + a_{2d} - 2(\overline{N_2} - N_{2s}) + c(\overline{N_1} - N_{1s}) + \alpha - 2\beta(2\overline{N} - (N_{1s} + N_{2s})) \\
&\implies A_s - A_d + a_{2s} - a_{2d} - 2N_{2s}(1+\beta) + N_{1s}(c - 2\beta) \\
&\qquad = c\overline{N_1} - 2\overline{N_2} - 2\beta\overline{N_1} - 2\beta\overline{N_2} + 2N_{2s}(1+\beta) - N_{1s}(c-2\beta)
\end{aligned}
$$

$$\implies A_s - A_d = (4N_{2s} + 2\overline{N_2})(1+\beta) - (2N_{1s} - \overline{N_1})(c - 2\beta)$$

$$\implies N_{2s} = \frac{A_s - A_d}{4(1+\beta)} + \frac{(2N_{1s} - \overline{N_1})(c-2\beta)}{4(1+\beta)} - \frac{\overline{N_2}}{2} \tag{A2}$$

Substituting the expression for N_{2s} from (A2) in (A1) in order to solve (A1) and (A2) simultaneously yields

$$A_s - A_d + a_{1s} - a_{1d} = (4N_{1s} + 2\overline{N_1})(1+\beta) - (2\left(\frac{A_s - A_d}{4(1+\beta)} + \frac{(2N_{1s} - \overline{N_1})(c-2\beta)}{4(1+\beta)} - \frac{\overline{N_2}}{2}\right) - \overline{N_2})(c - 2\beta) \tag{A3}$$

Solving the above equation for N_{1s}^* yields

$$
\begin{aligned}
&(4N_{1s} + 2\overline{N_1})(1+\beta) - 2\left(\frac{A_s - A_d}{4(1+\beta)} + \frac{(2N_{1s} - \overline{N_1})(c-2\beta)}{4(1+\beta)} + \frac{\overline{N_2}}{2}\right)(c - 2\beta) + \overline{N_2}(c-2\beta) \\
&= A_s - A_d + a_{1s} - a_{1d} \\
&\implies 4N_{1s}(1-\beta) - 2\overline{N_1}(1-\beta) - \frac{N_{1s}(c-2\beta)^2}{1+\beta} + \frac{\overline{N_1}(c-2\beta)^2}{2(1+\beta)} \\
&= A_s - A_d + a_{1s} - a_{1d} + \frac{(A_s - A_d)(c-2\beta)}{2(1+\beta)} \\
&\implies N_{1s}(4(1-\beta)^2 - (c-2\beta)^2) \\
&\qquad = 2\overline{N_1}(1-\beta^2) + \frac{\overline{N_1}(c-2\beta)^2}{2(1+\beta)} + \frac{(A_s - A_d)(c+2)}{2} + (a_{1s} - a_{1d})(1+\beta)
\end{aligned}
$$

$$\implies N_{1s} = \frac{\overline{N_1}}{2} + \frac{(A_s - A_d)(c+2)}{8(1-\beta)^2 - 2(c-2\beta)^2} + \frac{(a_{1s} - a_{1d})(1+\beta)}{4(1-\beta)^2 - (c-2\beta)^2} \tag{A4}$$

Simplifying the above expression yields the following expression for N^*_{1s} (same as that in (24) from the text):

$$N^*_{1s} = \frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)(c+2)} + \frac{\overline{N_1}}{2} \tag{A5}$$

Substituting the expression for N^*_{1s} in (A2) and solving for N^*_{2s} yields

$$\begin{aligned} &N_{2s} \\ &= \frac{A_s - A_d}{4(1+\beta)} + \frac{(A_s - A_d)(c - 2\beta)}{4(4\beta - c + 2)(1+\beta)} + \frac{\overline{N_1}(c-2\beta)}{4(1+\beta)} - \frac{\overline{N_1}(c-2\beta)}{4(1+\beta)} + \frac{(a_{1s} - a_{1d})(c-2\beta)(1+\beta)}{2(1+\beta)(4\beta - c + 2)(c+2)} \end{aligned} \tag{A6}$$

The above expression simplifies to the expression for N^*_{2s} in (25) from the text:

$$N^*_{2s} = \frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(c - 2\beta)}{2(4\beta - c + 2)(c+2)} + \frac{\overline{N_2}}{2} \tag{A7}$$

The total employment or population in city s under the non-WFH equilibrium is calculated as

$$\begin{aligned} &N^*_{1s} + N^*_{2s} \\ &= \left(\frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)(c+2)} + \frac{\overline{N_1}}{2} \right) \\ &\qquad + \left(\frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(c - 2\beta)}{2(4\beta - c + 2)(c+2)} + \frac{\overline{N_2}}{2} \right) \end{aligned} \tag{A8}$$

The above expression simplifies to the expression for total population in (26) from the text:

$$N^*_{1s} + N^*_{2s} = \frac{\overline{N}}{2} + \frac{A_s - A_d}{4\beta - c + 2} + \frac{a_{1s} - a_{1d}}{2(4\beta - c + 2)} \tag{A9}$$

Recalling that $\overline{N_i} = N_{is} + N_{id}$ and $\overline{N} = \overline{N_s} + \overline{N_d}$ yields the following expressions for the type-1, type-2 and total population in city d under non-WFH equilibrium

$$N_{1d}^* = \frac{\overline{N_1}}{2} - \frac{A_s - A_d}{2(4\beta - c + 2)} - \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)(c+2)}$$
$$N_{2d}^* = \frac{\overline{N_2}}{2} - \frac{A_s - A_d}{2(4\beta - c + 2)} - \frac{(a_{1s} - a_{1d})(c - 2\beta)}{2(4\beta - c + 2)(c+2)}$$

$$N_{1d}^* + N_{2d}^* = \frac{\overline{N}}{2} - \frac{A_s - A_d}{4\beta - c + 2} - \frac{a_{1s} - a_{1d}}{2(4\beta - c + 2)} \tag{A10}$$

The wage for the original type-1 workers of city s is calculated as $w_{1s}(N_{1s}^*, N_{2s}^*)$, which equals

$$\begin{aligned}
&a_{1s} - 2N_{1s}^* + cN_{2s}^* \\
&= a_{1s} - \frac{A_s - A_d}{4\beta - c + 2} - \frac{(a_{1s} - a_{1d})2(1+\beta)}{(c+2)(4\beta - c + 2)} - \overline{N_1} + \frac{c(A_s - A_d)}{2(4\beta - c + 2)} + \frac{c(a_{1s} - a_{1d})(c - 2\beta)}{(c+2)(4\beta - c + 2)} + \frac{c\overline{N_2}}{2} \\
&= a_{1s} + \frac{(A_s - A_d)(c-2)}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(c^2 - 2\beta c - 4 - 4\beta)}{2(c+2)(4\beta - c + 2)} + \frac{c\overline{N_2}}{2} - \overline{N_1}
\end{aligned}$$

$$= \frac{(A_s - A_d)(c-2)}{2(4\beta - c + 2)} + \frac{a_{1s}(2(4\beta - c + 2) - (2\beta - c + 2)) + a_{1d}(2\beta - c + 2)}{2\beta - c + 2} + \frac{c\overline{N_2}}{2} - \overline{N_1} \tag{A11}$$

The above expression simplifies to the following expression for the type-1 wage in city s:

$$\begin{aligned}
w_{1s}(N_{1s}^*, N_{2s}^*) &= a_{1s} - 2N_{1s}^* + cN_{2s}^* \\
&= \frac{c\overline{N_2}}{2} - \overline{N_1} - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(2\beta + 2 - c)a_{1d} + (6\beta + 2 - c)a_{1s}}{2(4\beta - c + 2)}
\end{aligned} \tag{A12}$$

The wage for the original type-1 workers of city d under the non-WFH equilibrium is calculated as $w_{1d}(N_{1d}^*, N_{2d}^*)$, which equals

$$\begin{aligned}
&a_{1d} - 2N_{1d}^* + cN_{2d}^* \\
&= a_{1d} - 2(\overline{N_1} - N_{1s^*}) + c(\overline{N_2} - N_{2s}^*) \\
&= a_{1d} + a_{1s} - (a_{1s} - 2N_{1s}^* + cN_{2s}^*) - 2\overline{N_1} + c\overline{N_2} \\
&= a_{1s} + a_{1d} - 2\overline{N_1} + c\overline{N_2} - \left(\frac{c\overline{N_2}}{2} - \overline{N_1} - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(2\beta + 2 - c)a_{1d} + (6\beta + 2 - c)a_{1s}}{2(4\beta - c + 2)} \right)
\end{aligned}$$

$$= \frac{c\overline{N_2}}{2} - \overline{N_1} + \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(8\beta - 2c + 4)(a_{1s} + a_{1d}) - (6\beta + 2 - c)a_{1s} - (2\beta + 2 - c)a_{1d}}{24\beta - c + 2} \tag{A13}$$

Simplifying, the wage for the original type-1 workers of city d under the non-WFH equilibrium is given by

$$w_{1d}(N^*_{1d}, N^*_{2d}) = \frac{c\overline{N_2}}{2} - \overline{N_1} + \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(6\beta + 2 - c)a_{1d} + (2\beta + 2 - c)a_{1s}}{2(4\beta - c + 2)} \tag{A14}$$

The wage for the original type-2 workers of city s is calculated as $w_{2s}(N^*_{1s}, N^*_{2s})$, which equals

$$\begin{aligned}
&a_2 - 2N^*_{2s} + cN^*_{1s} \\
&= a_2 - 2\left(\frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(c - 2\beta)}{2(4\beta - c + 2)(c+2)} + \frac{\overline{N_2}}{2}\right) \\
&+ c\left(\frac{A_s - A_d}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)(c+2)} + \frac{\overline{N_1}}{2}\right)
\end{aligned}$$

$$= a_2 - \frac{(A_s - A_d)}{4\beta - c + 2}\left(1 - \frac{c}{2}\right) + \frac{(a_{1s} - a_{1d})}{(c+2)(4\beta - c + 2)}(c + c\beta - c + 2\beta) + \frac{c\overline{N_1}}{2} - \overline{N_2} \tag{A15}$$

The above expression simplifies to the following expression for type-2 worker wage in city s:

$$w_{2s}(N^*_{1s}, N^*_{2s}) = a_2 - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})\beta}{4\beta - c + 2} + \frac{c\overline{N_1}}{2} - \overline{N_2} \tag{A16}$$

The wage for the original type-2 workers of city d under the non-WFH equilibrium is calculated as $w_{2d}(N_{1d}, N^*_{2d})$, which equals

$$\begin{aligned}
&a_2 - 2N^*_{2d} + cN^*_{1d} \\
&= a_2 - 2(\overline{N_2} - N_{2s^*}) + c(\overline{N_1} - N^*_{1s}) \\
&= a_2 + a_{2s} - (a_{2s} - 2N^*_{2s} + cN^*_{1s}) - 2\overline{N_2} + c\overline{N_1}
\end{aligned}$$

$$= a_2 + a_2 - 2\overline{N_2} + c\overline{N_1} - \left(a_2 - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{(a_{1s} - a_{1d})\beta}{4\beta - c + 2} + \frac{c\overline{N_1}}{2} - \overline{N_2}\right) \tag{A17}$$

Simplifying, the wage for the original type-2 workers of city d under non-WFH equilibrium is given by

$$w_{2d}(N_{1d}^*, N_{2d}^*) = a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} + \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} - \frac{(a_{1s} + a_{1d})\beta}{4\beta - c + 2} \tag{A18}$$

B.2 WFH Equilibrium and Wage Computation

Recall that the type-1 employment, type-1 population and the type-2 population in the WFH equilibrium are found by simultaneously solving equations (16),(18) and (13). Condition (29) from the text may be re-written as:

$$a_{1s} - 2L_{1s} + cN_{2s} = a_{1d} - 2(\overline{L_1} - L_{1s}) + c(\overline{N_2} - N_{2s})$$

$$\implies 2L_{1s} + c(\overline{N_2} - n_{2s}) - a_{1s} + a_{1d} = cN_{2s} + 2(\overline{L_1} - L_{1s})$$

$$\implies 4L_{1s} - 2cN_{2s} - a_{1s} + a_{1d} = 2\overline{L_1} - c\overline{N_2} \tag{A19}$$

Recalling that $\overline{L_1} = \overline{N_1}$, L_{1s} may be re-written in terms of N_{2s} as

$$L_{1s} = \frac{a_{1s} - a_{1d}}{4} + \frac{cN_{2s}}{2} + \frac{\overline{N_1}}{2} - \frac{c\overline{N_2}}{4} \tag{A20}$$

Condition (30) from the text may be re-written as:

$$A_s + \alpha - 2\beta(N_{1s} + N_{2s}) = A_d + \alpha - 2\beta(N_{1d} + N_{2d})$$

$$\implies A_s - A_d = 4\beta(N_{1s} + N_{2s} - \frac{\overline{N}}{2})$$

Hence, the total population in the WFH equilibrium in city s is

$$N_{1s} + N_{2s} = \frac{A_s - A_d}{4\beta} + \frac{\overline{N}}{2} \tag{A21}$$

Condition (31) may be re-written as:

$$A_s + a_2 - 2N_{2s} + cL_{1s} + \alpha - 2\beta(N_{1s} + N_{2s}) = A_d + a_2 - 2N_{2d} + cL_{1d} + \alpha - 2\beta(N_{1d} + N_{2d})$$
$$\implies A_s - 2N_{2s}(1+\beta) + cL_{1s} - 2\beta N_{1s} = A_d - 2N_{2d}(1+\beta) + cL_{1d} - 2\beta N_{1d}$$
$$\implies A_s - 2(1+\beta)(N_{2s} - \overline{N_2} + n_{2s}) + c(L_{1s} - \overline{N_1} + L_{1s}) - 2\beta(N_{1s} - \overline{N_1} + N_{1s}) = A_d$$

$$\implies 2cL_{1s} - 4\beta(N_{1s} + N_{2s}) - 4N_{2s} = (c - 2\beta)\overline{N_1} - 2(1+\beta)\overline{N_2} + A_d - A_s \tag{A22}$$

Substituting the expression for L_{1s} from (A5) into the above equation yields

$$2c\left(\frac{2c(a_{1s} - a_{1d})}{4} + \frac{cN_{2s}}{2} + \frac{\overline{N_1}}{2} - \frac{c\overline{N_2}}{4}\right)$$
$$- 4\beta(N_{1s} + N_{2s}) - 4N_{2s} = (c - 2\beta)\overline{N_1} - 2(1+\beta)\overline{N_2} + A_d - A_s$$
$$\implies \frac{2c(a_{1s} - a_{1d})}{4} + c^2N_{2s} + c\overline{N_1} - \frac{c^2\overline{N_2}}{2} - \frac{(A_s - A_d)4\beta}{4\beta} - \frac{4\beta(\overline{N_1} + \overline{N_2})}{2} - 4N_{2s}$$
$$= (c - 2\beta)\overline{N_1} - 2(1+\beta)\overline{N_2} + A_d - A_s$$
$$\implies \frac{c(a_{1s} - a_{1d})}{2} + c\overline{N_1} - (c - 2\beta)\overline{N_1} - \frac{c^2\overline{N_2}}{2} + 2(1+\beta)\overline{N_2} - 2\beta(\overline{N_1} + \overline{N_2}) = (4 - c^2)N_{2s}$$

$$\implies (4 - c^2)N_{2s} = \frac{c(a_{1s} - a_{1d})}{2} + \frac{(4 - c^2)\overline{N_2}}{2} \tag{A23}$$

Hence, as shown in (34) in the text, the type-2 worker employment/population in city s at the WFH equilibrium is given by

$$\widetilde{N_{2s}} = \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2} \tag{A24}$$

Substituting the expression for $\widetilde{N_{2s}}$ from (A24) into (A21) and solving for $\widetilde{N_{1s}}$ yields

$$\widetilde{N_{1s}} + \widetilde{N_{2s}} = \frac{A_s - A_d}{4\beta} + \frac{\overline{N}}{2}$$
$$\implies \widetilde{N_{1s}} = \frac{A_s - A_d}{4\beta} + \frac{\overline{N_1} + \overline{N_2}}{2} - \widetilde{N_{2s}}$$

$$\implies \widetilde{N_{1s}} = \frac{A_s - A_d}{4\beta} + \frac{\overline{N_1}}{2} + \frac{\overline{N_2}}{2} - \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2} \tag{A25}$$

Hence, as shown in (32) in the text, the type-1 worker population in city s in the WFH equilibrium is given by

$$\widetilde{N_{1s}} = \frac{A_s - A_d}{4\beta} - \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_1}}{2} \tag{A26}$$

Substituting the expression for $\widetilde{N_{2s}}$ from (A24) into (A20) and solving for $\widetilde{L_{1s}}$ yields

$$\begin{aligned}
\widetilde{L_{1s}} &= \frac{a_{1s} - a_{1d}}{4} + \frac{cN_{2s}}{2} + \frac{\overline{N_1}}{2} - \frac{c\overline{N_2}}{4} \\
&= \frac{a_{1s} - a_{1d}}{4} + \frac{c}{2}\left(\frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2}\right) + \frac{\overline{N_1}}{2} - \frac{c\overline{N_2}}{4}
\end{aligned}$$

$$= \frac{a_{1s} - a_1 d}{4}\left(1 + \frac{c^2}{(2+c)(2-c)}\right) + \frac{\overline{N_1}}{2} \tag{A27}$$

$$= \frac{a_{1s} - a_1 d}{(2+c)(2-c)} + \frac{\overline{N_1}}{2} \tag{A28}$$

which equals the expression in (33) in text. Using (A26) and (A27), total employment in city s under the WFH equilibrium is given by

$$\begin{aligned}
\widetilde{L_{1s}} + \widetilde{N_{2s}} &= \left(\frac{a_{1s} - a_{1d}}{(2+c)(2-c)} + \frac{\overline{N_1}}{2}\right) + \left(\frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2}\right) \\
&= \frac{a_{1s} - a_{1d}}{(2+c)(2-c)}\left(1 + \frac{c}{2}\right) + \frac{\overline{N}}{2}
\end{aligned}$$

$$= \frac{a_{1s} - a_{1d}}{2(2-c)} + \frac{\overline{N}}{2}, \tag{A29}$$

as in (36) in the text.

Recalling that $\overline{N_i} = N_{is} + N_{id}$ yields the following expressions for the type-1 and type-2 population and employment levels in city d under the WFH equilibrium:

$$\begin{aligned}\widetilde{N_{1d}} &= \overline{N_1} - \widetilde{N_{1s}} \\ &= \overline{N_1} - \left(\frac{A_s - A_d}{4\beta} - \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_1}}{2}\right)\end{aligned}$$

$$= \frac{\overline{N_1}}{2} - \frac{A_s - A_d}{4\beta} + \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} \tag{A30}$$

$$\begin{aligned}\widetilde{N_{2d}} &= \overline{N_2} - \widetilde{N_{2s}} \\ &= \overline{N_2} - \left(\frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2}\right)\end{aligned}$$

$$= \frac{\overline{N_2}}{2} - \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} \tag{A31}$$

$$\begin{aligned}\widetilde{L_{1d}} &= \overline{N_1} - \widetilde{L_{1s}} \\ &= \overline{N_1} - \left(\frac{a_{1s} - a_{1d}}{(2+c)(2-c)} + \frac{\overline{N_1}}{2}\right)\end{aligned}$$

$$= \frac{\overline{N_1}}{2} - \frac{a_{1s} - a_{1d}}{(2+c)(2-c)} \tag{A32}$$

Using (A30)-(A32), the total population and total employment in city d in the WFH equilibrium are

$$\begin{aligned}\widetilde{N_{1d}} + \widetilde{N_{2d}} &= 2\overline{N} - \left(\frac{A_s - A_d}{4\beta} + \overline{N}\right) \\ &= \overline{N} - \frac{A_s - A_d}{4\beta} \\ \widetilde{L_{1d}} + \widetilde{N_{2d}} &= \left(\frac{\overline{N_1}}{2} - \frac{a_{1s} - a_{1d}}{(2+c)(2-c)}\right) + \left(\frac{\overline{N_2}}{2} + \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)}\right) \\ &= \overline{N} + \frac{a_{1s} - a_{1d}}{(2+c)(2-c)}\left(\frac{c}{2} - 1\right)\end{aligned}$$

$$= \overline{N} - \frac{a_{1s} - a_{1d}}{2(2+c)} \tag{A33}$$

The wage for the original type-1 workers of city s in the WFH equilibrium is calculated as:

$$\begin{aligned}
w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) &= a_{1s} - 2\widetilde{L_{1s}} + c\widetilde{N_{2s}} \\
&= a_{1s} - 2\left(\frac{a_{1s} - a_{1d}}{(2+c)(2-c)} + \frac{\overline{N_1}}{2}\right) + c\left(\frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2}\right) \\
&= a_{1s} + \frac{a_{1s} - a_{1d}}{(2+c)(2-c)}\left(\frac{c^2}{2} - 2\right) + \frac{c\overline{N_2}}{2} - \overline{N_1} \\
&= a_{1s} - \frac{a_{1s} - a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1}
\end{aligned}$$

$$= \frac{a_{1s} + a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1}. \tag{A34}$$

The wage for the original type-2 workers of city s at the WFH equilibrium is calculated as:

$$\begin{aligned}
w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) &= a_2 - 2\widetilde{N_{2s}} + c\widetilde{L_{1s}} \\
&= a_2 - 2\left(\frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} + \frac{\overline{N_2}}{2}\right) + c\left(\frac{a_{1s} - a_{1d}}{(2+c)(2-c)} + \frac{\overline{N_1}}{2}\right) \\
&= a_2 - \frac{c(a_{1s} - a_{1d})}{(2+c)(2-c)} + \frac{c(a_{1s} - a_{1d})}{(2+c)(2-c)} - \overline{N_2} + \frac{c\overline{N_1}}{2}
\end{aligned}$$

$$= a_2 - \overline{N_2} + \frac{c\overline{N_1}}{2} \tag{A35}$$

From (13) in the text, type-1 wages are equalized in city s and city d. Since the same conclusion was shown to hold for type-2 wages, the city-d wages are given by

$$w_{1d}(\widetilde{L_{1d}}, \widetilde{N_{2d}}) = w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) = \frac{a_{1s} + a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} \tag{A36}$$

$$w_{2d}(\widetilde{L_{1d}}, \widetilde{N_{2d}}) = w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) = a_2 - \overline{N_2} + \frac{c\overline{N_1}}{2} \tag{A37}$$

B.3 Non-WFH and WFH comparisons

B.3.1 When city s only has a productivity advantage

Note that all the expressions used in this section assume $A_s = A_d \equiv A$ but $a_{1s} > a_{1d}$. To compare N^*_{1s} and $\widetilde{N_{1s}}$, note that using (A5) and (A26),

$$
\begin{aligned}
&2(2-c)(1+\beta) + c(4\beta - c + 2) > 0 \\
&\implies 2(2-c)(1+\beta) > -c(4\beta - c + 2) \\
&\implies \frac{1+\beta}{4\beta+2-c} > \frac{-c}{2(2-c)} \\
&\implies \frac{(a_{1s}-a_{1d})(1+\beta)}{(4\beta+2-c)(c+2)} > \frac{-c(a_{1s}-a_{1d})}{2(c+2)(2-c)}
\end{aligned}
$$

$$
\implies N^*_{1s} = \frac{(a_{1s}-a_{1d})(1+\beta)}{(4\beta+2-c)(c+2)} + \frac{\overline{N_1}}{2} > \frac{-c(a_{1s}-a_{1d})}{2(c+2)(2-c)} + \frac{\overline{N_1}}{2} = \widetilde{N_{1s}} \tag{A38}
$$

To compare N^*_{2s} and $\widetilde{N_{2s}}$, note that using (A7) and (A24),

$$
\begin{aligned}
&c > -2 \\
&\implies 2c > -4 \\
&\implies 4\beta c - c^2 + 2c > 2c - 4\beta - c^2 + 2\beta \\
&\implies c(4\beta + 2 - c) > (c - 2\beta)(2 - c)
\end{aligned}
$$

$$
\implies \widetilde{N_{2s}} = \frac{c(a_{1s}-a_{1d})}{2(2+c)(2-c)} > \frac{(c-2\beta)(a_{1s}-a_{1d})}{2(c+2)(4\beta+2-c)} = N^*_{2s} \tag{A39}
$$

To compare $N^*_{1s} + N^*_{2s}$ and $\widetilde{N_{1s}} + \widetilde{N_{2s}}$, note that using (A9) and (A25),

$$
\begin{aligned}
&2(4\beta - c + 2) > 0 \\
&\implies \frac{a_{1s}-a_{1d}}{2(4\beta - c + 2)} > 0
\end{aligned}
$$

$$\implies N_{1s}^* + N_{2s}^* = \frac{a_{1s} - a_{1d}}{2(4\beta + 2 - c)} + \frac{\overline{N}}{2} > \frac{\overline{N}}{2} = \widetilde{N_{1s}} + \widetilde{N_{2s}} \tag{A40}$$

To compare $\widetilde{N_{1s}}$ and $\widetilde{L_{1s}}$, note that using (A26) and (A28),

$$-\frac{c}{2} < 1$$

$$\implies \widetilde{N_{1s}} = \frac{c(a_{1s} - a_{1d})}{2(c+2)(2-c)} + \frac{\overline{N_1}}{2} < \frac{a_{1s} - a_{1d}}{(2+c)(2-c)} + \frac{\overline{N_1}}{2} = \widetilde{L_{1s}} \tag{A41}$$

To compare N_{1s}^* and $\widetilde{L_{1s}}$, note that using (A5) and (A28),

$$\begin{aligned}
& 2\beta + \beta c > 0 \\
\implies & 4\beta - c + 2 > 2 - c + 2\beta - \beta c \\
\implies & 4\beta - c + 2 > (1+\beta)(2-c) \\
\implies & \frac{a_{1s} - a_{1d}}{2-c} > \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)}
\end{aligned}$$

$$\implies \widetilde{L_{1s}} = \frac{a_{1s} - a_{1d}}{2-c} + \frac{\overline{N_1}}{2} > \frac{(a_{1s} - a_{1d})(1+\beta)}{(4\beta - c + 2)} + \frac{\overline{N_1}}{2} = N_{1s}^* \tag{A42}$$

To compare $N_{1s}^* + N_{2s}^*$ and $\widetilde{L_{1s}} + \widetilde{N_{2s}}$, note that using (A9) and (A29),

$$\begin{aligned}
& 4\beta + 2 - c > 2 - c \\
\implies & \frac{a_{1s} - a_{1d}}{2-c} > \frac{a_{1s} - a_{1d}}{4\beta - c + 2}
\end{aligned}$$

$$\implies \widetilde{L_{1s}} + \widetilde{N_{2s}} = \frac{a_{1s} - a_{1d}}{2(2-c)} + \frac{\overline{N}}{2} > \frac{a_{1s} - a_{1d}}{2(4\beta - c + 2)} + \frac{\overline{N}}{2} = N_{1s}^* + N_{2s}^* \tag{A43}$$

To compare $w_{1s}^* = w_{1s}(N_{1s}^*, N_{2s}^*)$ and $\widetilde{w_{1s}} = w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}})$, note that using (A12) and (A34),

$$\begin{aligned}
&a_{1s} > a_{1d}\\
&\implies 2\beta a_{1d} < 2\beta a_{1s}\\
&\implies a_{1d}\left(1 - \frac{2\beta - c + 2}{4\beta - c + 2}\right) < a_{1s}\left(\frac{6\beta - c + 2}{4\beta - c + 2} - 1\right)\\
&\implies \frac{a_{1s} + a_{1d}}{2} < \frac{a_{1s}(6\beta - c + 2)}{2(4\beta - c + 2)} + \frac{a_{1d}(2\beta - c + 2)}{2(4\beta - c + 2)}
\end{aligned}$$

$$\implies \widetilde{w_{1s}} = \frac{a_{1s} + a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} < \frac{a_{1s}(6\beta - c + 2)}{2(4\beta - c + 2)} + \frac{a_{1d}(2\beta - c + 2)}{2(4\beta - c + 2)} + \frac{c\overline{N_2}}{2} - \overline{N_1} = w_{1s}^* \quad \text{(A44)}$$

To compare $w_{1d}^* = w_{1d}(N_{1d}^*, N_{2d}^*)$ and $\widetilde{w_{1d}} = w_{1d}(\widetilde{L_{1d}}, \widetilde{N_{2d}})$, note that using (A14) and (A36),

$$\begin{aligned}
&a_{1s} > a_{1d}\\
&\implies 2\beta a_{1d} < 2\beta a_{1s}\\
&\implies a_{1s}\left(1 - \frac{2\beta + 2 - c}{4\beta - c + 2}\right) > a_{1d}\left(\frac{6\beta + 2 - c}{4\beta + 2 - c} - 1\right)\\
&\implies \frac{a_{1s} + a_{1d}}{2} > \frac{a_{1d}(6\beta + 2 - c)}{2(4\beta + 2 - c)} + \frac{a_{1s}(2\beta + 2 - c)}{2(4\beta + 2 - c)}
\end{aligned}$$

$$\implies \widetilde{w_{1d}} = \frac{a_{1s} + a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} > \frac{a_{1d}(6\beta + 2 - c)}{2(4\beta + 2 - c)} + \frac{a_{1s}(2\beta + 2 - c)}{2(4\beta + 2 - c)} + \frac{c\overline{N_2}}{2} - \overline{N_1} = w_{1d}^* \quad \text{(A45)}$$

To compare $w_{2s}^* = w_{2s}(N_{1s}^*, N_{2s}^*)$ and $\widetilde{w_{2s}} = w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}})$, note that using (A14) and (A35),

$$\frac{(a_{1s} - a_{1d})\beta}{4\beta - c + 2} > 0$$

$$\implies w_{2s}^* = a_2 - \frac{(a_{1s} - a_{1d})\beta}{4\beta - c + 2} + \frac{c\overline{N_1}}{2} - \overline{N_2} > a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} = \widetilde{w_{2s}} \quad \text{(A46)}$$

To compare $w_{2d}^* = w_{2d}(N_{1d}^*, N_{2d}^*)$ and $\widetilde{w_{2d}} = w_{2d}(\widetilde{L_{1d}}, \widetilde{N_{2d}})$, note that using (A14) and (A37),

$$-\frac{(a_{1s} - a_{1d})\beta}{4\beta - c + 2} < 0$$

$$\implies w_{2d}^* = a_2 - \frac{(a_{1s} - a_{1d})\beta}{4\beta - c + 2} + \frac{c\overline{N_1}}{2} - \overline{N_2} < a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} = \widetilde{w_{2d}} \tag{A47}$$

Using (A5), (A7) and (A8), utility of original type-1 workers in city s in the non-WFH equilibrium (which equals the utility in city d) is given by

$$\begin{aligned}
&A + w_{1s}^* + \alpha - 2\beta(N_{1s}^* + N_{2s}^*) \\
&= A + a_{1s} - 2N_{1s}^* + cN_{2s}^* + \alpha - 2\beta\left(\frac{a_{1s} + a_{1d}}{2(4\beta - c + 2)} + \frac{\overline{N}}{2}\right) \\
&= A + a_{1s} - \frac{a_{1s} - a_{1d}}{4\beta - c + 2}\left(\frac{2(1+\beta)}{c+2} - \frac{c(c - 2\beta)}{2(2+c)} + \beta\right) - \overline{N_1} + \frac{c\overline{N_2}}{2} + \alpha - \beta\overline{N}
\end{aligned}$$

$$= A + \frac{a_{1s} + a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} + \alpha - \beta\overline{N} \tag{A48}$$

Using (A34) and A(25), welfare of type-1 workers in city s under WFH is given by

$$A + w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) + \alpha - \beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) \quad = A + \frac{a_{1s} + a_{1d}}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} + \alpha - \beta\overline{N} \tag{A49}$$

Hence, as stated in the text, the computations above yield the same utility for type-1 workers with and without WFH.

Using (A5), (A7), and (A8), the utility of original type-2 workers in city s in the non-WFH equilibrium is given by

$$\begin{aligned}
&A + w_{2s}(N_{1s}^*, N_{2s}^*) + \alpha - 2\beta(N_{1s}^* + N_{2s}^*) \\
&= A + a_2 - 2N_{2s}^* + cN_{1s}^* + \alpha - \frac{\beta(a_{1s} - a_{1d})}{4\beta - c + 2} - 2\beta\overline{N} \\
&= A + a_2 + \frac{\beta(a_{1s} - a_{1d})}{4\beta - c + 2} - \overline{N_2} - \frac{\beta(a_{1s} - a_{1d})}{4\beta - c + 2} + \frac{c\overline{N_1}}{2} + \alpha - 2\beta\overline{N}
\end{aligned}$$

$$= A + a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} + \alpha - \beta\overline{N} \tag{A50}$$

Using (A35) and (A25), the utility of original type-2 workers in city s under WFH is given by

$$A + w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}}) + \alpha - \beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) \qquad = A + a_2 - \overline{N_2} + \frac{c\overline{N_1}}{2} + \alpha - \beta\overline{N} \tag{A51}$$

Hence, as stated in the text, welfare is the same for type-2 workers with and without WFH.

B.3.2 When city s only has an amenity advantage

Note that all the expressions used in this section assumes $A_s > A_d$ but $a_{1s} = a_{1d} \equiv a_1$. To compare N^*_{1s} and $\widetilde{N_{1s}}$, note that using (A5) and (A26),

$$4\beta + 4 > 2c$$

$$\implies 4\beta < 4\beta + 4 - 2c$$

$$\implies 4\beta < 2(4\beta + 2 - c)$$

$$\implies \frac{A_s - A_d}{4\beta} > \frac{A_s - A_d}{2(4\beta + 2 - c)}$$

$$\implies \widetilde{N_{1s}} = \frac{A_s - A_d}{4\beta} + \frac{\overline{N_1}}{2} > \frac{A_s - A_d}{2(4\beta + 2 - c)} + \frac{\overline{N_1}}{2} = N^*_{1s} \tag{A52}$$

To compare N^*_{2s} and $\widetilde{N_{2s}}$, note that using (A7) and A(24),

$$0 < \frac{A_s - A_d}{2(4\beta - c + 2)}$$

$$\implies \widetilde{N_{2s}} = \frac{\overline{N_2}}{2} < \frac{A_s - A_d}{2(4\beta + 2 - c)} + \frac{\overline{N_2}}{2} = N^*_{2s} \tag{A53}$$

To compare $N_{1s}^* + N_{2s}^*$ and $\widetilde{N_{1s}} + \widetilde{N_{2s}}$, note that using (A9) and (A25),

$$\begin{aligned} & 4\beta < 4\beta - c + 2 \\ \implies & \frac{A_s - A_d}{4\beta} > \frac{A_s - A_d}{4\beta + 2 - c} \end{aligned}$$

$$\implies \widetilde{N_{1s}} + \widetilde{N_{2s}} = \frac{A_s - A_d}{4\beta} + \frac{\overline{N}}{2} > \frac{A_s - A_d}{4\beta + 2 - c} + \frac{\overline{N}}{2} = N_{1s}^* + N_{2s}^* \tag{A54}$$

To compare $\widetilde{N_{1s}}$ and $\widetilde{L_{1s}}$, note that using (A26) and (A28),

$$0 < \frac{A_s - A_d}{2(4\beta + 2 - c)}$$

$$\implies \widetilde{L_{1s}} = \frac{\overline{N_2}}{2} < \frac{A_s - A_d}{2(4\beta + 2 - c)} + \frac{\overline{N_2}}{2} = \widetilde{N_{1s}} \tag{A55}$$

To compare N_{1s}^* and $\widetilde{L_{1s}}$, note that using (A5) and (A28),

$$0 < \frac{A_s - A_d}{2(4\beta - c + 2)}$$

$$\implies \widetilde{L_{1s}} = \frac{\overline{N_1}}{2} < \frac{A_s - A_d}{2(4\beta + 2 - c)} + \frac{\overline{N_1}}{2} = N_{1s}^* \tag{A56}$$

To compare $N_{1s}^* + N_{2s}^*$ and $\widetilde{L_{1s}} + \widetilde{N_{2s}}$, note that using (A9) and (A29),

$$0 < \frac{A_s - A_d}{2(4\beta - c + 2)}$$

$$\implies \widetilde{L_{1s}} + \widetilde{N_{2s}} = \frac{\overline{N}}{2} < \frac{A_s - A_d}{2(4\beta + 2 - c)} + \frac{\overline{N}}{2} = N_{1s}^* + N_{2s}^* \tag{A57}$$

To compare $w_{1s}^* = w_{1s}(N_{1s}^*, N_{2s}^*)$ and $\widetilde{w_{1s}} = w_{1s}(\widetilde{L_{1s}}, \widetilde{N_{2s}})$, note that using (A12) and (A34),

$$\begin{aligned} & \frac{c-2}{4\beta - c + 2} < 0 \\ \implies & -\frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} < 0 \end{aligned}$$

$$\implies w_{1s}^* = a_1 - \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} + \frac{c\overline{N_2}}{2} - \overline{N_1} < a_1 + \frac{c\overline{N_2}}{2} - \overline{N_1} = \widetilde{w_{1s}} \tag{A58}$$

To compare $w_{1d}^* = w_{1d}(N_{1d}^*, N_{2d}^*)$ and $\widetilde{w_{1d}} = w_{1d}(\widetilde{L_{1d}}, \widetilde{N_{2d}})$, note that using (A14) and (A36),

$$\begin{aligned} & \frac{2-c}{4\beta - c + 2} > 0 \\ \implies & \frac{(A_s - A_d)(2-c)}{2(4\beta - c + 2)} > 0 \end{aligned}$$

$$\implies w_{1d}^* = a_1 + \frac{(A_s - A_d)(2-c)}{2(4\beta + 2 - c)} + \frac{c\overline{N_2}}{2} - \overline{N_1} > a_1 + \frac{c\overline{N_2}}{2} - \overline{N_1} = \widetilde{w_{1d}} \tag{A59}$$

To compare $w_{2s}^* = w_{2s}(N_{1s}^*, N_{2s}^*)$ and $\widetilde{w_{2s}} = w_{2s}(\widetilde{L_{1s}}, \widetilde{N_{2s}})$, note that using (A14) and (A35),

$$\begin{aligned} & \frac{c-2}{4\beta + 2 - c} < 0 \\ \implies & -\frac{(A_s - A_d)(2-c)}{2(4\beta + 2 - c)} < 0 \end{aligned}$$

$$\implies w_{2s}^* = a_2 - \frac{(A_s - A_d)(2-c)}{2(4\beta + 2 - c)} + \frac{c\overline{N_1}}{2} - \overline{N_2} < a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} = \widetilde{w_{2s}} \tag{A60}$$

To compare $w_{2d}^* = w_{2d}(N_{1d}^*, N_{2d}^*)$ and $\widetilde{w_{2d}} = w_{2d}(\widetilde{L_{1d}}, \widetilde{N_{2d}})$, note that using (A14) and (A37),

$$\begin{aligned} & \frac{2-c}{4\beta + 2 - c} > 0 \\ \implies & \frac{(A_s - A_d)(2-c)}{2(4\beta + 2 - c)} > 0 \end{aligned}$$

$$\implies w_{1d}^* = a_2 + \frac{(A_s - A_d)(2-c)}{2(4\beta + 2 - c)} + \frac{c\overline{N_1}}{2} - \overline{N_2} > a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} = \widetilde{w_{2d}} \tag{A61}$$

Using (A5), (A7) and (A8), the utility of original type-1 workers in city s in the non-WFH equilibrium (which equals the utility in city d) is given by

$$\begin{aligned}
&A_s + w_{1s}^* + \alpha - 2\beta(N_{1s}^* + N_{2s}^*) \\
&= A_s + a_1 - 2N_{1s}^* + cN_{2s}^* + \alpha - 2\beta\left(\frac{A_s - A_d}{4\beta + 2 - c} + \frac{\overline{N}}{2}\right) \\
&= A_s + a_1 - \frac{A_s - A_d}{4\beta + 2 - c} - \overline{N_1} + \frac{c(A_s - A_d)}{2(4\beta + 2 - c)} + \frac{c\overline{N_2}}{2} - \frac{2\beta(A_s - A_d)}{4\beta + 2 - c} - \beta\overline{N} + \alpha \\
&= A_s + a_1 - \overline{N_1} + \frac{c\overline{N_2}}{2} - \frac{A_s - A_d}{2} - \beta\overline{N} + \alpha
\end{aligned}$$

$$= \frac{A_s + A_d}{2} + a_1 + \frac{c\overline{N_2}}{2} - \overline{N_1} + \alpha - \beta\overline{N} \tag{A62}$$

Using (A34) and (A25), the utility of type-1 workers in city s under WFH is given by

$$\begin{aligned}
&A_s + \widetilde{w_{1s}} + \alpha - 2\beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) \\
&= A_s + \frac{a_1 + a_1}{2} + \frac{c\overline{N_2}}{2} - \overline{N_1} - 2\beta\left(\frac{A_s - A_d}{4\beta} + \frac{\overline{N_1} + \overline{N_2}}{2}\right) + \alpha
\end{aligned}$$

$$= \frac{A_s + A_d}{2} + a_1 + \frac{c\overline{N_2}}{2} - \overline{N_1} + \alpha - \beta\overline{N} \tag{A63}$$

Hence, as stated in the text, the computations above yield the same utility for the type-1 workers with and without WFH.

Using (A5), (A7) and (A8), the utility of original type-2 workers in city s in the non-WFH equilibrium is given by

$$
\begin{aligned}
& A_s + w_{2s}^* + \alpha - 2\beta(N_{1s}^* + N_{2s}^*) \\
&= A_s + a_2 - 2N_{2s}^* + cN_{1s}^* + \alpha - 2\beta\left(\frac{A_s - A_d}{4\beta - c + 2} + \frac{\overline{N}}{2}\right) \\
&= A_s + a_2 - \frac{A_s - A_d}{4\beta + 2 - c} + \frac{c(A_s - A_d)}{2(4\beta + 2 - c)} - \frac{2\beta(A_s - A_d)}{4\beta - c + 2} - \beta\overline{N} - \overline{N_2} + \frac{c\overline{N_1}}{2} + \alpha
\end{aligned}
$$

$$
= \frac{A_s + A_d}{2} + a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} + \alpha - \beta\overline{N} \tag{A64}
$$

Using (A35) and (A25), the utility of original type-2 workers in city s is given by

$$
\begin{aligned}
& A_s + \widetilde{w_{2s}} + \alpha - 2\beta(\widetilde{N_{1s}} + \widetilde{N_{2s}}) \\
&= A_s + a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} - 2\beta\left(\frac{A_s - A_d}{4\beta} + \frac{\overline{N}}{2}\right) + \alpha
\end{aligned}
$$

$$
= \frac{A_s + A_d}{2} + a_2 + \frac{c\overline{N_1}}{2} - \overline{N_2} + \alpha - \beta\overline{N} \tag{A65}
$$

Hence, as stated in the text, utility is the same for type-2 workers with and without WFH.

B.4 Comparative-Static Results

B.4.1 When city s only has a productivity advantage

When WFH is introduced, the change in type-1 worker population in city s is given by

$$
N_{1s}^* - \widetilde{N_{1s}} = \frac{(a_{1s} - a_{1d})(1 + \beta)}{4(4\beta + 2 - c)(c + 2)} - \frac{-c(a_{1s} - a_{1d})}{2(2 + c)(2 - c)}
$$

$$= (a_{1s} - a_{1d}) \left(\frac{1+\beta}{(4\beta + 2 - c)(c+2)} + \frac{c}{2(2+c)(2-c)} \right) \tag{A66}$$

The change in type-1 worker population in city d is also given by

$$\widetilde{N_{1d}} - N_{1d}^* = (a_{1s} - a_{1d}) \left(\frac{1+\beta}{(4\beta - c + 2)(c+2)} + \frac{c}{2(2+c)(2-c)} \right)$$

Partially differentiating $(N_{1s}^* - \widetilde{N_{1s}})$ and $(\widetilde{N_{1d}} - N_{1d}^*)$ with respect to the difference of productivities between the two cities yields

$$\frac{\partial(N_{1s}^* - \widetilde{N_{1s}})}{\partial(a_{1s} - a_{1d})} = \frac{\partial(\widetilde{N_{1d}} - N_{1d}^*)}{\partial(a_{1s} - a_{1d})} = \frac{1+\beta}{(4\beta + 2 - c)(c+2)} + \frac{c}{2(2+c)(2-c)} > 0$$

Also, partially differentiating $(N_{1s}^* - \widetilde{N_{1s}})$ and $(\widetilde{N_{1d}} - N_{1d}^*)$ with respect to the degree of complementarity between worker types yields

$$\begin{aligned} &\frac{\partial(N_{1s}^* - \widetilde{N_{1s}})}{\partial c} = \frac{\partial(\widetilde{N_{1d}} - N_{1d}^*)}{\partial c} \\ &= (a_{1s} - a_{1d}) \frac{\partial}{\partial c} \left(\frac{1+\beta}{4\beta c + 8\beta - c^2 + 4} \right) + \frac{\partial}{\partial c} \left(\frac{c}{2(4-c^2)} \right) \\ &= (a_{1s} - a_{1d}) \left(-\frac{(1+\beta)(4\beta - 2c)}{(4\beta c + 8\beta - c^2 + 4)^2} \right) + \left(\frac{8 + 2c^2}{4(4-c^2)^2} \right) \end{aligned}$$

$$= (a_{1s} - a_{1d}) \left(\frac{2c + 2\beta c - 4\beta - 4\beta^2}{(4\beta c + 8\beta - c^2 + 4)^2} + \frac{8 + 2c^2}{4(4-c^2)^2} \right) \tag{A67}$$

Given that $c \approx 0$, assuming $c = 0$ for computational simplicity reduces the above equation to

$$\frac{\partial(N_{1s}^* - \widetilde{N_{1s}})}{\partial c} = \frac{\partial(\widetilde{N_{1d}} - N_{1d}^*)}{\partial c} = \frac{-4\beta^2 - 4\beta}{(8\beta+4)^2} + \frac{1}{8} = \frac{-32\beta^2 - 32\beta + (8\beta+4)^2}{8(8\beta+4)^2} = \frac{4\beta^2 + 4\beta + 2}{(8\beta+4)^2} > 0 \tag{A68}$$

The changes in type-2 worker population in city s and city d are given by

$$\widetilde{N_{2s}} - N_{2s}^* = N_{2d}^* - \widetilde{N_{2d}} = \frac{c(a_{1s} - a_{1d})}{2(2+c)(2-c)} - \frac{(c-2\beta)(a_{1s} - a_{1d})}{2(4\beta - c + 2)(c+2)} \tag{A69}$$

Assuming $c = 0$ for computational simplicity and partially differentiating with respect to the difference of productivities between the two cities yields

$$\frac{\partial(\widetilde{N_{2s}} - N_{2s}^*)}{\partial(a_{1s} - a_{1d})} = \frac{\partial N_{2d}^* - \widetilde{N_{2d}}}{\partial(a_{1s} - a_{1d})} = \frac{2\beta}{16\beta + 8} > 0 \tag{A70}$$

Also, partially differentiating $(\widetilde{N_{2s}} - N_{2s}^*)$ and $(N_{2d}^* - \widetilde{N_{2d}})$ with respect to the degree of complementarity between the two worker types yields

$$\frac{\partial(\widetilde{N_{2s}} - N_{2s}^*)}{\partial c} = \frac{\partial(N_{2d}^* - \widetilde{N_{2d}})}{\partial c}$$
$$= \frac{(a_{1s} - a_{1d})}{2}\left(\frac{4+c^2}{(4-c^2)^2}\right) - \frac{a_{1s} - a_{1d}}{2}\left(\frac{8\beta + 4 + 8\beta^2 + c^2 - 4\beta c}{(4\beta c + 8\beta - c^2 + 4)^2}\right)$$

$$= \frac{(a_{1s} - a_{1d})}{2}\left(\frac{4+c^2}{(4-c^2)^2} - \frac{8\beta + 4 + 8\beta^2 + c^2 - 4\beta c}{(4\beta c + 8\beta - c^2 + 4)^2}\right) \tag{A71}$$

Assuming $c = 0$ for computational simplicity reduces the above equation to

$$\frac{\partial(\widetilde{N_{2s}} - N_{2s}^*)}{\partial c} = \frac{\partial(N_{2d}^* - \widetilde{N_{2d}})}{\partial c}$$
$$= \frac{(a_{1s} - a_{1d})}{2}\left(\frac{1}{4} - \frac{8\beta + 4 + 8\beta^2}{(8\beta + 4)^2}\right) = 2(a_{1s} - a_{1d})\left(\frac{32\beta^2 + 32\beta}{16(8\beta+4)^2}\right) > 0 \tag{A72}$$

The changes in type-1 employment in city s and city d are given by

$$\widetilde{L_{1s}} - N_{1s}^* = N_{1d}^* - \widetilde{L_{1d}} = \frac{a_{1s} - a_{1d}}{4 - c^2} - \frac{(1+\beta)(a_{1s} - a_{1d})}{4\beta c - 8\beta - c^2 + 4} \tag{A73}$$

Assuming $c = 0$ for computational simplicity and partially differentiating with respect to the difference of productivities between the two cities yields

$$\frac{\partial(\widetilde{L_{1s}} - N_{1s}^*)}{\partial(a_{1s} - a_{1d})} = \frac{\partial N_{1d}^* - \widetilde{L_{1d}}}{\partial(a_{1s} - a_{1d})} = \frac{4\beta c + 8\beta - c^2 + 4 - (1+\beta)(4-c^2)}{(4-c^2)(4\beta c + 8\beta - c^2 + 4)} = \frac{4\beta}{4(8\beta+4)} > 0 \tag{A74}$$

Also, partially differentiating $(\widetilde{L_{1s}} - N_{1s}^*)$ and $(N_{1d}^* - \widetilde{L_{2d}})$ with respect to the degree of complementarity between the the worker types yields

$$\frac{\partial(\widetilde{L_{1s}} - N_{1s}^*)}{\partial c} = \frac{\partial(N_{1d}^* - \widetilde{L_{1d}})}{\partial c} = \frac{\partial \widetilde{L_{1s}}}{\partial c} - \frac{\partial N_{1s}^*}{\partial c} = \frac{(a_{1s} - a_{1d})2c}{(4-c^2)^2} + \frac{(a_{1s} - a_{1d})(1+\beta)(4\beta - 2c)}{4\beta c + 8\beta - c^2 + 4}$$

Assuming $c = 0$, the expression above simplifies to

$$\frac{\partial(\widetilde{L_{1s}} - N_{1s}^*)}{\partial c} = \frac{(a_{1s} - a_{1d})(1+\beta)\beta}{2\beta + 1} > 0 \tag{A75}$$

B.4.2 When city s only has an amenity advantage

When WFH is introduced, the changes in type-1 worker population in city s is given by

$$\begin{aligned}
\widetilde{N_{1s}} - N_{1s}^* &= \frac{A_s - A_d}{4\beta} - \frac{A_s - A_d}{2(4\beta + 2 - c)} \\
&= (A_s - A_d)\left(\frac{1}{4\beta} - \frac{1}{2(4\beta + 2 - c)}\right) \\
&= (A_s - A_d)\left(\frac{4\beta - c + 2 - 2}{4\beta(4\beta + 2 - c)}\right)
\end{aligned}$$

$$= \frac{(A_s - A_d)(4\beta - c)}{4\beta(4\beta + 2 - c)} \tag{A76}$$

The changes in type-1 worker population in city d is also given by

$$N_{1d}^* - \widetilde{N_{1d}} = \frac{(A_s - A_d)(4\beta - c)}{4\beta(4\beta - c + 2)} \tag{A77}$$

Partially differentiating with respect to the difference of amenities between the two cities yields

$$\frac{\partial(\widetilde{N_{1s}} - N_{1s}^*)}{\partial(A_s - A_d)} = \frac{\partial(N_{1d}^* - \widetilde{N_{1d}})}{\partial(A_s - A_d)} = \frac{4\beta - c}{4\beta(4\beta - c + 2)} > 0 \tag{A78}$$

Also, partially differentiating with respect to the degree of complementarity between worker types yields

$$\frac{\partial(\widetilde{N_{1s}} - N_{1s}^*)}{\partial c} = \frac{\partial(N_{1d}^* - \widetilde{N_{1d}})}{\partial c}$$
$$= \frac{A_s - A_d}{4\beta}\left(\frac{(2-c+4\beta)(-1)+(c-4\beta)(-1)}{(4\beta-c+2)^2}\right) = \frac{-2(A_s - A_d)}{4\beta(4\beta-c+2)^2} < 0 \quad \text{(A79)}$$

The changes in type-2 worker population in cities s and d are given by

$$N_{2s}^* - \widetilde{N_{2s}} = \widetilde{N_{2d}} - N_{2d}^* = \frac{A_s - A_d}{2(4\beta - c + 2)} \quad \text{(A80)}$$

Partially differentiating with respect to the difference of amenities between the two cities yields

$$\frac{\partial(N_{2s}^* - \widetilde{N_{2s}})}{\partial(A_s - A_d)} = \frac{\partial(\widetilde{N_{2d}} - N_{2d}^*)}{\partial(A_s - A_d)} = \frac{1}{2(4\beta - c + 2)} > 0 \quad \text{(A81)}$$

Also, partially differentiating with respect to the degree of complementarity between worker types yields

$$\frac{\partial(N_{2s}^* - \widetilde{N_{2s}})}{\partial c} = \frac{\partial(\widetilde{N_{2d}} - N_{2d}^*)}{\partial c} = -\frac{A_s - A_d}{4(4\beta - c + 2)^2}(-1) = \frac{A_s - A_d}{4(4\beta - c + 2)^2} > 0 \quad \text{(A82)}$$

The changes in type-1 employment in cities s and d are given by

$$N_{1s}^* - \widetilde{L_{1s}} = \widetilde{L_{1d}} - N_{1d}^* = \frac{A_s - A_d}{4\beta} \quad \text{(A83)}$$

Partially differentiating with respect to the difference of amenities between the two cities yields

$$\frac{\partial(N_{1s}^* - \widetilde{L_{1s}})}{\partial(A_s - A_d)} = \frac{\partial(\widetilde{L_{1d}} - N_{1d}^*)}{\partial(A_s - A_d)} = \frac{1}{4\beta} > 0 \quad \text{(A84)}$$

Also, partially differentiating with respect to the degree of complementarity between worker types yields

$$\frac{\partial(N_{1s}^* - \widetilde{L_{1s}})}{\partial c} = \frac{\partial(\widetilde{L_{1d}} - N_{1d}^*)}{\partial c} = 0 \quad \text{(A85)}$$

Appendix C

Chapter 3

C.1 Alternate Specifications

To further justify the use of a negative binomial regression model instead of a Poisson model, Fig. A1 shows the deviation of Poisson predicted death values and negative binomial predicted values from the observed values for dataset (A) when zero-death cities are excluded. As is visible from Fig. A6, the negative binomial model seems to predict the observed values better than the Poisson model, further justifying the use of that model.

The deviations of predicted death values from actual values using logged and unlogged variables for dataset (A) is plotted in Fig. A7. As can be seen, the deviations are much lower for the unlogged regression for observed death counts below 4 (approximately). For death counts above 4, the deviations are lower in the logged regression. In addition, Fig. A8 shows that deviations of predicted values from the observed values of deaths in dataset (B) are mostly lower when logged rather than unlogged independent variables are used in the negative binomial model.

C.2 Results from linear regression analysis of officer-involved deaths

This section offers a robustness check on the main results. Instead of using a negative binomial model, Table A1 shows the results from using a linear model on the same cross-sectional and longitudinal panel. Following Buehler (2017) and Edwards et al. (2018), the deaths statistic has been standardized by dividing it by the total population in millions. The independent variables, crime index, police employment, Black population and Hispanic/Latino population, are also converted into rates by dividing them by city population for each year. Since population-standardized measures are already employed, the linear regression does not include non-minority population.

Columns (1) and (2) of Table A12 correspond to inclusion and exclusion of cities with zero-deaths, respectively. The results show that median income, the crime rate and the police employment rate are significantly correlated with the officer-involved death rate. The signs of these coefficients as well as their significance reiterates findings from the negative binomial model. However, the coefficients of the Black and Hispanic/Latino population shares are not found to be significant. As discussed by Fryer (2018), differences in current research findings on racial bias in officer-involved shootings stems from differences in research design. Hence, the contrary conclusions on racial disparity from different regression designs may not be surprising. Also, the non-randomness of the residuals with respect to the fitted values of the residual-versus-fitted plot (available on request) suggests that the assumptions required for Ordinary Least Squares are not met. This fact, coupled with the very low R^2 values associated with these regressions, suggests that the results should be treated with caution.

C.3 Results from Analysis on Police Employment Levels

Table A13 displays results from running an OLS regression on dataset (A) with police employment as the dependent variable and the other controls (except the crime index, owing to a potential simultaneity bias) as unlogged independent variables. Non-minority population, Black population, Hispanic/Latino population and median household income are positive and significant factors correlated with the size of police force in a city. Quantitatively, while an increase of a million non-minority population correlates to an increase of around 2500 police, more than double that increment (about 5900) is associated with the same increase in Black population. While it makes intuitive sense that police forces are large in more populous cities, the difference in coefficients might suggest racial disparity in the choice of police employment, especially given that the regression controls for median income. While these are intuitive findings, neither the sex ratio or a city's democratic affiliation has a significant effect on the size of police force.

Fig. A6: Deviations of predicted values of deaths corresponding to different models using dataset (A)

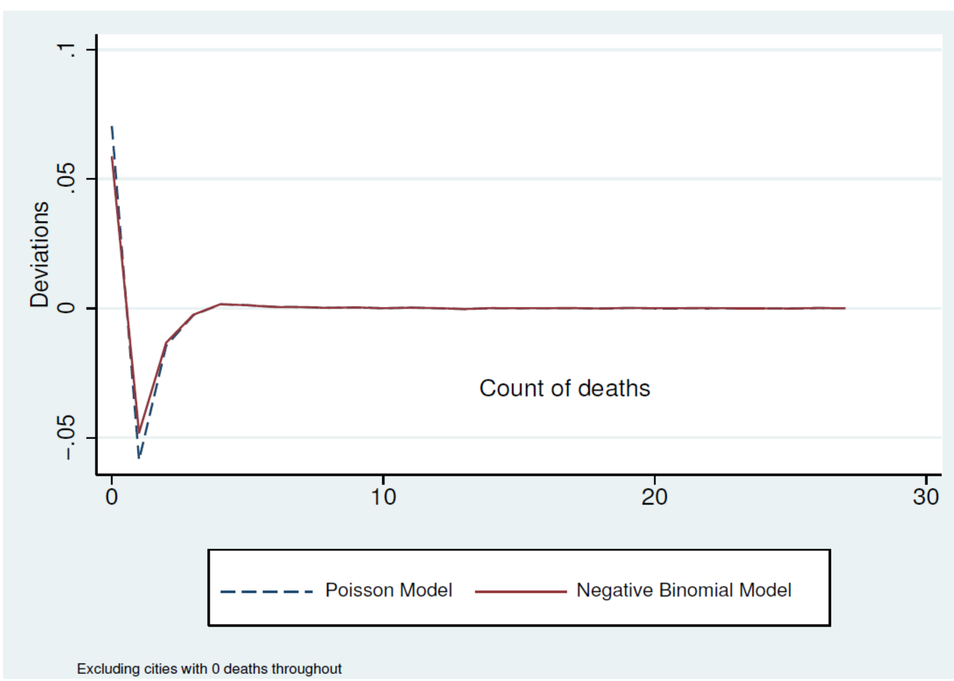


Fig. A7: Deviations of predicted values of deaths from dataset (A)

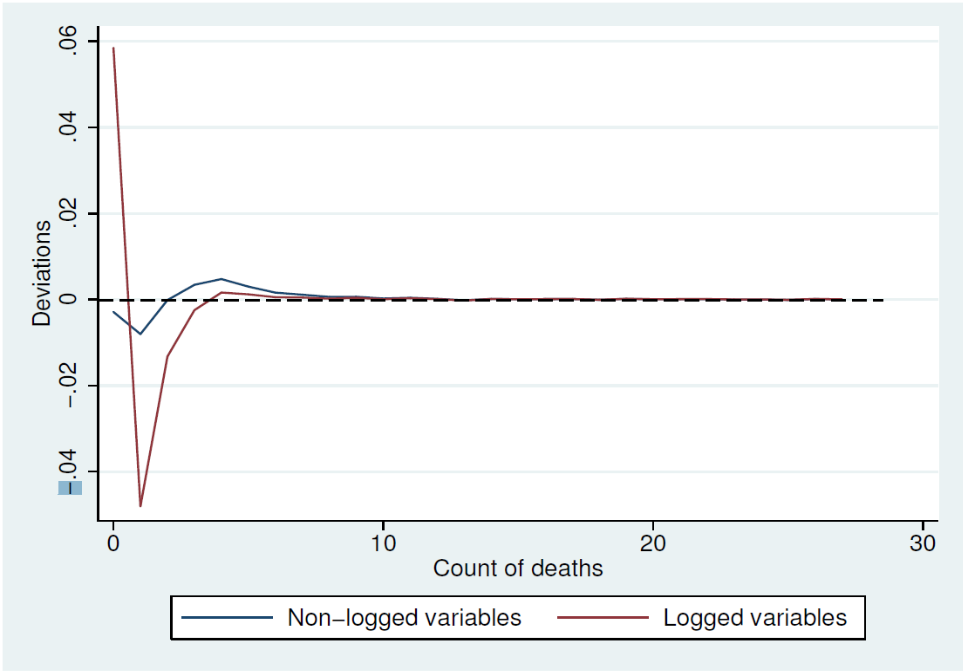


Fig. A8: Deviations of predicted values of deaths from dataset (B)

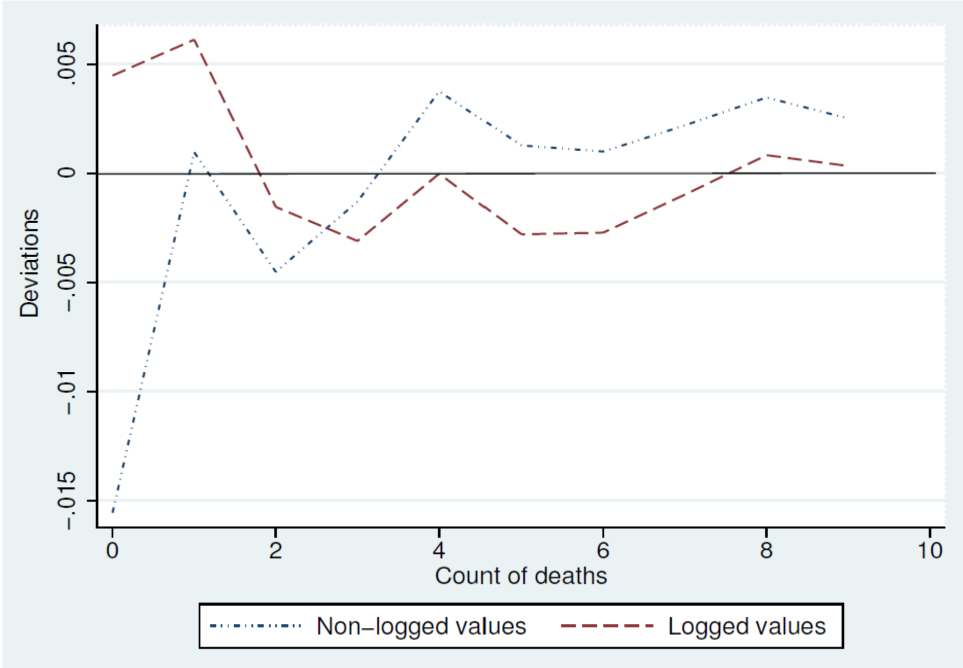


Table A12: OLS regression on datatset (A)

Dep var: Death rate per 1,000,000	(1)	(2)
Crime index	44.50^{***}	43.49^{***}
	(2.85)	(3.62)
Police employment	1562.08^{***}	2106.58^{***}
	(503.27)	(723.52)
Median income	-62.75^{**}	-147.59^{***}
	(26.01)	(48.12)
% Black	-0.13	-9.62
	(4.05)	(6.07)
%Hispanic	-3.29	-8.53^{*}
	(3.05)	(5.16)
Sex ratio	0.04	0.09
	(0.05)	(0.11)
Democratic Affiliation	0.23	-0.44
	(1.12)	(1.76)
0-cities	Y	N
State fixed effects	Y	Y
Year fixed effects	Y	Y
Adjusted R^2	0.04	0.04
Observations	5262	2138

(1) Median income is in millions of dollars and death rate per million is calculated as $\frac{Deathcount}{\frac{Population}{1000000}}$

(2) Standard errors are in the brackets

(3) Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.001$

(4) 0-cities imply cities that have had no deaths throughout

Table A13: OLS Regression from datatset (A) using only unlogged variables

Dep var: Police Employment	(1)	(2)
Median income	219.43^{***}	197.17^{*}
	(56.84)	(102.36)
Non-minority population	2502.50^{***}	2550.90^{***}
	(25.39)	(32.04)
Black population	5955.00^{***}	5952.39^{***}
	(32.6)	(40.42)
Hispanic/Latino population	2927.50^{***}	2915.31^{***}
	(25.44)	(31.66)
Sex ratio	-0.06	-0.04
	(0.13)	(0.27)
Democratic Affiliation	-1.77	3.85
	(2.74)	(4.18)
Year fixed effects	Y	Y
State fixed effects	Y	Y
0-cities	Y	N
Adjusted R^2	0.96	0.97
Observations	9823	6303

(1) Non-minority population, Black population and Hispanic/Latino population are in millions while median income is in millions of dollars

(2) Standard errors are in the brackets

(3) Significance levels are denoted by $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.001$

(4) 0-cities imply cities that have had no deaths throughout