Minimum Wage as a Place-based Policy: Evidence from US Housing Rental Markets*

Diego Gentile Passaro Santiago Hermo Gabriele Borg †

February 6, 2022

Abstract

Recently many state and substate minimum wage (MW) policies have been instituted in the US, resulting in significant dispersion of MW levels within metropolitan areas. In this paper we study the effect of MW changes on local housing rental markets exploiting the placed-based nature of MW policies. For each location we define both the minimum wage where the average resident works (the "workplace MW") and the minimum wage in the location itself (the "residence MW"). We write down a partial-equilibrium model of a housing market in which minimum wage levels in each location affect housing demand by changing the income of commuters and the prices of non-tradable consumption. We show that, in the model, the workplace MW has a positive effect on rents whereas the residence MW has a negative effect. We take our model to the data by constructing a ZIP code monthly panel using rents data from Zillow. We use a difference-in-differences design to estimate the effect of residence and workplace MW changes on median housing rents. We find that a ZIP code experiencing a 10 percent increase in workplace MW and no change in residence MW will have an increase in rents of between 0.65 and 1.2 percent. If statutory MW also increases by 10 percent within that same ZIP code, then the increase in rents will only be lower—between 0.35 and 0.9 percent—. We use our results to study the consequences of a counterfactual increase in the federal MW from \$7.25 to \$9. We estimate that, in ZIP codes where the residence MW increases, landlords pocket between 5 and 9 cents on the extra dollar. In ZIP codes where the residence MW does not change, landlords pocket between 9 and 16 cents.

*We are thankful to Jesse M. Shapiro, John N. Friedman, Matthew Turner, Kenneth Chay, Neil Thakral, Matthew Pecenco, Peter Hull, Jonathan Roth, and Jesse Bruhn for numerous comments and valuable feedback during the development of this project. We also thank seminar participants of the Applied Micro Lunch at Brown University, and the Montevideo Graduate Workshop at Universidad Católica del Uruguay. All errors are our own.

†Gentile Passaro: Department of Economics, Brown University (email: diego_gentile_passaro@brown.edu); Hermo: Department of Economics, Brown University (email: santiago_hermo@brown.edu); Borg: Amazon Web Services.

1 Introduction

In recent years, many US jurisdictions have introduced minimum wages above the federal level of \$7.25, which resulted in minimum wage levels that vary substantially within metropolitan areas. Minimum wage policies (hereafter MW) are *place-based* in that they are tied to a location, and workers may work and live in locations under different MW levels, determining the effects of changes in these policies across space. While most research on the effects of the MW has focused on employment and wages irrespective of location (e.g., Card and Krueger 1994; Cengiz et al. 2019), a full account of the welfare effects of the MW requires an understanding of how it affects different markets and how its effects spill over across neighborhoods. In fact, while the MW appears to lower income inequality through the labor market (Lee 1999; Autor, Manning, and Smith 2016), its effect on disposable incomes may be lower as one considers changes in prices of consumption and housing.

In this paper we take a step in this direction by studying the short-run effect of MW policies across local rental housing markets. Consider a new MW policy in some locations within a metropolitan area. Because low-wage workers tend to reside in specific neighbordhoods with access to those (now better-paying) low-wage jobs, one would expect an increase in the income of these workers leading to a higher demand for housing and thus a rise rental prices, harming (at least partially) the group the policy is intended to benefit. Similarly, the MW hike will translate into higher prices of non-tradable goods that employ low-wage workers intensively, also affecting demand for housing and, as a result, rental prices. Commuting patterns thus become an essential ingredient to understand the heterogeneous effect of local MW policies on the housing market, even more so when there is a divergence in the workplace and residence locations of workers, as is the case in US data (see Figure 1). These local effects on rental prices will in turn influence the distributional impact of the policy.

There is little research attempting to estimate the causal effect of minimum wage policies on the housing market and none accounting for spatial spillovers. To the best of our knowledge, the only papers that estimate the causal effect of minimum wages on rents in the same location are Tidemann (2018) and Yamagishi (2019; 2021).[1] Estimating empirically the effects of MW policies on rents is challenging. First, as opposed to assessing effects on the labor market where jobs and wages are tied to the workplace, when evaluating the housing market it is crucial to account for the fact that people may reside and work under different MW levels.[2] This is challenging because accounting for changes in the MW where residents of a location work requires data on commuting patterns at the local level. Second, estimation at the local level requires spatially dissagregated data on rents. Using large geographies might result in null or even negative effects on average, even if no one commutes outside of this region and the actual effect (of workplace MW) on some local housing markets is positive.[3] Even if these aggregate results are correct, they will miss the fact that some people are in fact paying higher rents due to the policy. In addition, as MW changes are unlikely

[1] Yamagishi (2019) explores this question using data from both the US and Japan. In the published version of the paper, Yamagishi (2021) excludes the analysis of the US case.

[2] However, several papers have highlighted the importance that studies on the effect of the MW on employment account for potential spillovers that "contaminate" the control group (Kuehn 2016; Huang 2020).

[3] Rents in neighborhoods where low-wage workers live are likely to increase, whereas elsewhere they are likely not to change or even decrease, as those residents "pay" for the higher MW through higher prices and lower profits. The sign of the resulting effect in the larger geography is ambiguous.

to be set considering the dynamics of local rental markets, when using small geographic units the exogeneity assumptions required for identification appear more plausible.

We introduce several innovations to tackle these challenges. First, we theoretically recognize that minimum wage policies will spill over across housing markets through commuting. We device a new model-based estimation approach where rents in each local housing market are affected by two MW-based measures, one summarizing the effect of residence MW and a second one the effect of workplace MW. Second, we use a novel panel dataset on rents at the USPS ZIP code level and with a monthly frequency from Zillow, the largest online rental marketplace in the US. We couple those data with an original dataset of binding minimum wages at the ZIP code level, and commuting origin-destination matrices from US Census Bureau (2020b). As a result, we are able to estimate the effect of MW policies on rents using variation of hundreds of policy changes staggered across small jurisdictions and months that generate plausibly exogenous variation of workplace and residence MW levels.

We use our estimated model to evaluate the short-run impact of a federal MW increase from $7.25 to $9 on rents. Coupling our estimates with IRS data, we approximate the ZIP code-specific share on each dollar of income change that accrues to landlords. We discuss the implications of our results for assessing the distributional impact of MW policies.

We start by laying out a partial equilibrium model of a ZIP code's rental market, which is embedded in a larger geography. We allow residents of this ZIP code to commute to other ZIP codes to work, potentially under a different MW policy. In the model workers demand square feet of housing as a function of local prices and income, which in turn depend on the MW levels workers face at residence and workplace locations, respectively. This short-run model imposes fixed commuting patterns and fully flexible prices.[4] Motivated by the evidence of the effect of MW policies on income (Dube 2019b; Cengiz et al. 2019) and prices (Allegretto and Reich 2018; Leung 2021), we assume that MW hikes in the workplace increase income and MW hikes in the residence increase local prices. The model illustrates that, if housing is a normal good and it is complementary with non-tradable consumption, then the effect of a change in MW legislation would be heterogeneous across ZIP codes depending on whether it mostly changes the workplace or residence MW of its residents. In particular, we show that a MW increase in some workplace will cause rents to go up, whereas an increase in the residence will (conditional on workplace MW) lower rents. We also show that, under some homogeneity assumptions on the effect of MWs through income, the effect of changes in MW at workplaces on log rents can be summarized in a single measure, which we call a ZIP code's *workplace MW*. This measure is defined as the weighted average of log minimum wage levels across a ZIP code's workplaces, using commuting shares as weights. We use this result to motivate our empirical model.

We construct a panel at the USPS ZIP code and monthly levels with rental prices and binding MW levels. Our main rent variable comes from Zillow, the largest online real estate platform in the

[4]This assumption is motivated by our dataset, which varies at the monthly level. Thus, we expect the first order effects of MW changes to not affect where agents live and work. We also believe that this assumption is consistent with the recent MW literature finding small effects of MW changes on employment over longer time horizons (see Dube 2019a, for a review). Relatedly, Pérez Pérez (2021) finds small elasticities of commuting to MW policies in a time horizon of several years.

US (PDX 2020; Investopedia 2020), and corresponds to the median rent price per square foot across Zillow listings in the given ZIP code-month cell of the category Single Family, Condominiums and Cooperative Houses (SFCC). This is the most popular housing category in the US (Fernald 2020), and also the most populated series in the Zillow data. We collect data on MW changes from Vaghul and Zipperer (2016) for the period 2010–2016, which we update until January 2020 using data from UC Berkeley Labor Center (2020) and validating with offical sources. We assign a binding MW to each ZIP code by taking the maximum across all the MWs that affect that ZIP code (city, state, and federal levels).[5] We use our MW data coupled with commuting origin-destination matrices obtained from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES; US Census Bureau 2020b) database to construct the workplace MW for each ZIP code and month.

We also collect data on regional economic trends from the Quarterly Census of Employment and Wages US Bureau of Labor Statistics (QCEW; 2020); wage and business income from at the ZIP code-year levels from Internal Revenue System (2019); and measures of characteristics of a ZIP codes' residents from US Census Bureau (2020a) and IPUMS (2019).

Guided by the theoretical model, we pose an empirical model where log rents in a location depend linearly on (1) residence MW—the log of the same location statutory MW—, (2) workplace MW—the weighted average of log statutory MW in other ZIP codes, where weights are commuting shares—, (3) ZIP code and time period fixed effects, and (4) time-varying controls. As shocks to rents are expected to be serially correlated over time within ZIP codes, we estimate the model in first-differences. As we discuss in the body of the paper, this model recovers the true causal effect of the minimum wage assuming that, within a ZIP code, changes in each of our MW variables are *strictly exogenous* with respect to changes in the error term, conditional on the other MW measure. To mitigate concerns of changes in the composition of our sample of ZIP codes while keeping as many of them as possible, in our baseline analysis we use a partially balanced panel.[6] Using an argument akin to the recent difference-in-differences literature (e.g., Callaway and Sant'Anna 2021; Callaway, Goodman-Bacon, and Sant'Anna 2021), in an appendix we lay down the assumptions that yield identification of the parameters under a MW policy that increases the MW of a set of ZIP codes only.

Our preferred specification implies that a 10 percent increase in the workplace MW only increases rents by 0.58 percent (SE=0.28). A 10 percent increase in the residence MW only decrease rents by 0.24 percent (SE=0.18) As a result, if both measures increase simultaneously by 10 percent then rents would increase by 0.34 percent instead (SE=0.15). These results are clear evidence that, holding fixed the commuting shares, MW changes spill over spatially through commuting, affecting local housing markets in places beyond the boundary of the jurisdiction that instituted the policy. We find that a naive model estimated only on the same-location MW would yield a similar coefficient to the sum of our workplace and residence coefficients. However, this model would predict changes

[5] To do this we perform two matching procedures between geographical units. First, we match USPS ZIP codes to census ZIP code tabulation areas (ZCTA) using the crosswalk from UDS Mapper (2020). Second, we match ZCTAs to city and states using crosswalk from Missouri Census Data Center (2014).

[6] We use all ZIP codes with valid rents data as of July 2015. In February 2010, when our Zillow data starts, there are only 9 ZIP codes.

in rents only at residence locations and would not account for MW spillovers, which are central to characterizing the rich pattern that these policy changes generate and to understanding their distributional consequences.

Heterogeneity analyses show that ...

We conduct several robustness checks to test the validity of our results. First, we test our identifying assumption using leads and lags of each MW variable. Reassuringly, our models show no effects of future MW changes on current rents. We also show the robustness of our results by estimating our model with different sets of controls that should account for a variety of confounders, such as the state of the local economy or local heterogeneity in rental dynamics. Second, in an appendix we show that our results are similar in a "stacked regression" model that compares ZIP codes within metropolitan areas where some but not all experienced a change in the statutory MW. Third, as rental listings may stay on Zillow for more than a month, one may worry about structural auto-correlation in the dependent variable which, if not accounted for, may bias our estimates. In an appendix, we present an alternative model that includes the lagged first difference of rents as a control and estimate it via instrumental variables following Arellano and Bond (1991) and Meer and West (2016). Both alternative estimation procedures yield results that are very similar to our baseline. Finally, we estimate variations of our model under a fixed composition of ZIP codes; using an unbalanced panel with full set of ZIP codes and "cohort-by-time" fixed effects; and re-weightingn the data to match key moments of the distribution of US urban ZIP codes. Our results are robust to these exercises.

In the final part of the paper, we develop a simple extension to our baseline model to estimate the ZIP code-specific share on each dollar that accrues to landlords following a MW increase. This parameter depends on the change in the total wage bill of a ZIP code, and the share of a ZIP code's total income spent in housing. We posit a model for the wage bill similar to our baseline, and estimate an elasticity of wages to the minimum wage that is in line with the literature (e.g., Cengiz et al. 2019). Due to data constraints, we assume a range of values for the share of housing expenditure at the ZIP code level. We focus on studying the consequences of a counterfactual increase in the federal MW from $7.25 to $9 on January 2020. We find large variation in the estimated resulting rent changes across ZIP codes. We estimate that, in ZIP codes where both the residence and workplace MWs increase due to the policy, landlords pocket between 5 and 8 cents on the dollar. However, in ZIP codes where the residence MW does not change, the share pocketed by landlords is higher. These results imply that a share of the income that accrues to low-wage workers due to the policy is actually captured by landlords due to a finite elasticity of housing supply.[7] Ignoring this fact will lead to an overstatemnt of the gains of low-wage workers following a MW increase.

This paper is related to several strands of literature. First, our paper relates to the large literature estimating the effects of minimum wage policies on labor market outcomes. Starting with Card and Krueger's (1994) classical study, many papers have explored the effect of these policies on employment (some recent examples include Meer and West 2016; Cengiz et al. 2019).[8] Similarly,

[7]This result is consistent with the mechanism proposed by Kline and Moretti (2014), whereby place-based policies generate welfare losses due to inneficiencies in the housing market. However, in our model we do not allow for migration responses that may mitigate this coefficient in the medium run.

[8]See Neumark and Wascher (2006) for an earlier review of this literature, and Dube (2019a) and Neumark and

several papers study the consequences of minimum wage policies on income inequality (Lee 1999; Autor, Manning, and Smith 2016). There is also a growing literature studying the effects of local minimum wage changes (Dube and Lindner 2021). We contribute to this literature by focusing on a relatively less studied channel through which minimum wage policies at subnational jurisdictional levels may affect welfare: the housing market.

Second, this paper is related to the literature studying the effects of MW policies on housing markets. We already mentioned the scant literature estimating the effects of MW policies on rental housing prices (Tidemann 2018; Yamagishi 2021). We innovate in several ways relative to these papers. First, while these papers estimate the effect of same-location MW on rents, we differentiate between residence and workplace MW levels, fully incorporating spillovers across regions. Second, we use data at a more detailed geography and higher frequency.[9] Both of these facts enrich our understanding of the estimated effects and make the required identification assumptions more plausible. Our paper also relates to Hughes (2020) who uses a triple difference strategy to study the effect of MW policies on rent-to-income ratios. Like us, the author explicitly mentions disentangling general equilibrium effects from effects on rental markets as a motivation for his approach. [10] Our work is also related to work studying the effects of MW policies on commuting and migration (Cadena 2014; Monras 2019; Pérez Pérez 2021), and prices of consumption goods (Allegretto and Reich 2018; Leung 2021)

Third, we also contribute to the literature on place-based policies. (Kline and Moretti 2014) presents a review of place-based policies, and argues that these policies result in inefficiencies due to finite housing supply elasticites in different locations. Relatedly, Hsieh and Moretti (2019) quantify the aggregate cost of housing constraints. In line with this insight, we show in our counterfactual analysis that landlords may benefit from a MW increase, eroding some of the rise in low-wage workers' income generated by the policy.

Finally, our paper relates to the literature on the econometric issues arising from the presence of spillover effects across units, both in the context of minimum wage policies (Kuehn 2016; Huang 2020), and more generally of any policy that spills over spatially (Delgado and Florax 2015; Butts 2021). In our setting we exploit knowledge of commuting patterns to specify the exposure of each unit to treatment in other units. Under this functional form assumption we are able to account for spatial spillovers of MW policies on rents, allowing us to estimate rich effect patterns on the rent gradient.

The rest of the paper is organized as follows. In Section 2 we introduce a motivating model of the rental market. In Section 3 we present our data. In Section 4 we discuss our empirical strategy and we discuss our identification assumptions. In Section 5 we present our results. Section 6 discusses a counterfactual minimum wage policy, and Section 7 concludes.

Shirley (2021) for more recent reviews.

[9] Both Tidemann (2018) and Yamagishi (2019) for the US exploit Fair Markets Rents data from the US Department of Housing and Urban Development (HUD), which is available at the yearly level and aggregated at the geographical level of counties.

[10] Another related paper is Agarwal, Ambrose, and Diop (2019) who show that MW increases lower the probability of rental default.

2 A Partial-Equilibrium Model

In this section we layout a simple demand and supply model of local rental markets. We use the model to illustrate why we expect a different impact of MW changes on rents at workplace and residence locations. Because we study the consequences of MW changes in the very-short run, our model is static and we assume an exogenous distribution of workers across residence and workplace locations. We discuss the addition of the time dimension in Appendix A. We think of a spatial model with worker mobility across ZIP codes as an avenue for future work.

We emphasize that the model is designed to highlight a possible mechanism through which one may expect residence and workplace MWs to have a different impact on the housing market. Our empirical results do not hinge on any of the assumptions made in this section; however, they reject a model in which workplace and residence locations have the same effect.

2.1 Setup

We consider the rental market of some ZIP code i embedded in a larger geography composed of a finite number of ZIP codes $\mathcal{Z}$. Workers with residence i work in some other ZIP code $z \in \mathcal{Z}(i)$, where $\mathcal{Z}(i) \subseteq \mathcal{Z}$. More precisely, we let L_{iz} denote the measure of i's residents who work in z; and $L_i = \sum_{z\in\mathcal{Z}(i)} L_{iz}$ and $L_z = \sum_{i\in\mathcal{Z}(i)} L_{iz}$ the number of residents in i and workers in z, respectively. We assume that the distribution of residence-workplace pairs is fixed.[11] Each ZIP code has a binding minimum wage, which we denote by $\{\underline{W}_z\}_{z\in\mathcal{Z}(i)}$.

Housing demand

Each group (i, z) consume square feet of living space h_{iz}, a non-tradable good produced in their residence c_{iz}^{NT}, and a tradable good c_{iz}^{T}. A representative (i, z) worker chooses between these alternatives by maximizing a quasi-concave utility function $u_{iz} = u\left(h_{iz}, c_{iz}^{\mathrm{NT}}, c_{iz}^{\mathrm{T}}\right)$ subject to a budget constraint

$$R_i h_{iz} + P_i(\underline{W}_i) c_{iz}^{\mathrm{NT}} + c_{iz}^{\mathrm{T}} \leq y_{iz}(\underline{W}_z),$$

where R_i gives the rental price of housing per square feet, $P_i(\underline{W}_i)$ gives the price of local consumption, the price of tradable consumption is normalized to one, and $y_{iz}(\underline{W}_z)$ is an income function. We summarize the effect of MWs below.

Assumption 1 (Effect of Minimum Wages)**.** *We assume that (i) the prices of non-tradable goods are increasing in i's MW,* $\frac{dP_i}{d\underline{W}_i} > 0$*, and (ii) income is weakly increasing in z's MW, and stricly increasing for at least one* $z \in \mathcal{Z}(i)$*.*

We think that the structure of the problem and Assumption 1 are supported by the literature. First, recent evidence by Miyauchi, Nakajima, and Redding (2021) shows that individuals tend to consume close to home. As a result, we expect them to be sensible to prices of local consumption

[11] To simplify we assume that all of i' residents work, so that the number of residents equals the number of workers.

in their same neighborhood, justifying the inclusion of c_{iz}^{NT} in the utility function.[12] Second, MWs hikes have been shown to increase prices of local consumption (e.g., Allegretto and Reich 2018; Leung 2021), and also to increase wage income even for wages above the MW level (e.g., Cengiz et al. 2019),.[13]

The solution to the worker's problem for each z yields a set of continuously differentiable housing demand functions $\{h_{iz}(R_i, P_i, Y_z)\}_{z\in\mathcal{Z}(i)}$. Standard arguments imply that this function is decreasing in its own price R_i. We assume conditions that ensure that housing demand is decreasing in local prices P_i.[14] Finally, we assume that housing is a normal good, so that housing demand is increasing in income Y_z.

Note that, given our assumptions, an increase in a group's (i, z) workplace MW will tend to increase housing demand in i, and an increase in residence MW will have a negative effect (conditional on its effect via the workplace MW of the group i, i).

Housing supply

We assume that the supply of square feet in i is given by a function $D_i(R_i)$, and we assume that this function is weakly increasing in R_i. Note that this formulation allows for an upper limit on the number of houses at which point the supply becomes perfectly inelastic.

2.2 Equilibrium and Comparative Statics

Total demand of housing in ZIP code i is given by the sum of the demands of each group. Thus, we can write the equilibrium condition in this market as

$$\sum_{z\in\mathcal{Z}(i)} L_{iz} h_{iz}\left(R_i, P_i(\underline{W}_i), Y_z(\underline{W}_z)\right) = D_i(R_i). \tag{1}$$

Given that housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.[15] Equilibrium rents are a function of the entire set of minimum wages, formally, $R_i^* = f(\{\underline{W}_i\}_{i\in\mathcal{Z}(i)})$.

We are interested in two questions. What is the effect of a change in the vector of MWs $(\{d\ln \underline{W}_i\}_{i\in\mathcal{Z}(i)})'$ on equilibrium rents? Under what conditions can we reduce the dimensional-

[12]An extension of the model would allow workers to consume in any ZIP code in the metropolitan area. While theoretically straightforward, this extension would require data on consumption trips, which we lack. We think of our model as an approximation.

[13]An extension would allow separate wage income and business income in the budget constraint. If firm owners tend to live where they work, and MW increases damage profits, then business income would depend negatively on the local MW.

[14]To formalize the required condition, let h_{iz} and c_{iz} denote Marshallian demands resulting from the choice problem, and $\tilde{h}_{iz}$ denote the Hicksian housing demand. The Slutsky equation implies that

$$\frac{\partial h_{iz}}{\partial P_i} = \frac{\partial \tilde{h}_{iz}}{\partial P_i} - \frac{\partial h_{iz}}{\partial y_{iz}} c_{iz}.$$

To obtain $\frac{\partial h_{iz}}{\partial P_i} < 0$, we require that $\frac{\partial \tilde{h}_{iz}}{\partial P_i} < \frac{\partial h_{iz}}{\partial y_{iz}} c_{iz}$, i.e., the income effect of an increase in non-tradable prices is larger than the corresponding substitution effect.

[15]To see this, assume that $D_i(0) - \sum_{z\in\mathcal{Z}(i)} L_{iz} h_{iz}(0, P_i, Y_z) < 0$ and apply the intermediate value theorem.

ity of the rents function and represent the effects of MW changes on equilibrium rents in a simpler way? We start with the first question.

Proposition 1 (Comparative Statics)**.** *Consider residence ZIP code i and a change in MW policy such that for $z \in \mathcal{Z}_0 \subseteq \mathcal{Z}(i)$ binding MWs increase, and for $z' \in \mathcal{Z}(i) \setminus \mathcal{Z}_0$ binding MWs do not change, where $\mathcal{Z}_0$ is non-empty. Under the assumptions of unchanging $\{L_i z\}_{z\in\mathcal{Z}(i)}$ and Assumption 1, we have that (1) for $z \in \mathcal{Z}_0 \setminus \{i\}$ for which $\frac{dY_z}{d\underline{W}_z} > 0$, the policy has a positive partial effect on rents; (2) for ZIP code i the partial effect of the policy is ambiguous; and (3) as a result, the overall effect on rents is ambiguous if $i \in \mathcal{Z}_0$ and weakly positive if $i \notin \mathcal{Z}_0$.*

Proof. Fully differentiate the market clearing condition with respect to $\ln R_i$ and $\ln \underline{W}_i$ for all $i \in \mathcal{Z}(i)$. Dividing by 1 and each of the variables appropriately, one can show that

$$\left(\eta_i - \sum_z \pi_{iz}\xi^r_{iz}\right) d\ln R_i = \sum_z \pi_{iz}\left(\xi^p_{iz}\epsilon^p_i d\ln \underline{W}_i + \xi^y_{iz}\epsilon^y_z d\ln \underline{W}_z\right), \tag{2}$$

where $\pi_{iz} = \frac{L_{iz}}{L_i}$ represents the share of i's residents working in z; $\xi^x_{iz} = \frac{dh_{iz}}{dx_i}\frac{x_i}{\sum_z \pi_{iz}h_{iz}}$ for $x \in \{R, P, Y\}$ is the elasticity of housing demand at the average demand of ZIP code i; $\epsilon^p_i = \frac{dP_i}{d\underline{W}_i}\frac{\underline{W}_i}{P_i}$ and $\epsilon^y_z = \frac{dY_z}{d\underline{W}_z}\frac{\underline{W}_z}{Y_z}$ are elasticities of prices and income to minimum wages; and $\eta_i = \frac{dD_i}{dR_i}\frac{R_i}{D_i}$ is the elasticity of housing supply in ZIP code i.

For each $z \in \mathcal{Z}_0 \setminus \{i\}$ the partial effect on rents of the policy is given by

$$\left(\eta_i - \sum_z \pi_{iz}\xi^r_{iz}\right)^{-1} \pi_{iz}\xi^y_{iz}\epsilon^y_z d\ln \underline{W}_z.$$

Because $\eta_i > 0$ and $\xi^r_{iz} \leq 0 \ \forall z \in \mathcal{Z}(i)$, the first factor is positive. Thus, if $\epsilon^y_z > 0$ then this effect is positive, as desired.

For ZIP code i the partial effect is given by

$$\left(\eta_i - \sum_z \pi_{iz}\xi^r_{iz}\right)^{-1} \left(\epsilon^p_i \sum \pi_{iz}\xi^p_{iz} + \pi_{ii}\xi^y_{ii}\epsilon^y_i\right) d\ln \underline{W}_i.$$

Because $\epsilon^p_i > 0$, $\xi^p_{iz} < 0 \ \forall z \in \mathcal{Z}(i)$, and $\epsilon^y_i \geq 0$, then the sign of this partial effect is ambiguous. The statement under (iii) follows directly. $\square$

Proposition 1 (1) shows that, if at least some low-wage worker (for whom $\frac{dY_z}{d\underline{W}_z} > 0$) commutes to a ZIP code where the MW increased, then the MW hike will tend to increase rents. Proposition 1 (2) establishes that a decreasing effect on rents may follow if the minimum wage also increases in ZIP code i. As a result, the sign of the overall effect of the policy is not determined a priori.

The following proposition establishes conditions under which the dimensionality of equation 2 can be reduced.

Proposition 2 (Representation)**.** *Under the assumptions of homogeneous elasticity of housing demand to incomes Y_z and homogeneous elasticity of income to minimum wages $\underline{W}_z$, we can write the* *change in log rents as a function of the change in two MW-based measures: ZIP code i's **workplace MW** and **residence MW**. Furthermore, the workplace MW has a positive effect on rents, whereas the residence MW has a negative effect.*

Proof. We assume that, for all $z \in \mathcal{Z}(i)$, we have $\xi_{iz}^{y} = \xi_{i}^{y}$ and $\epsilon_{z}^{y} = \epsilon^{y}$. Then, we can manipulate (2) to write

$$dr_i = \beta_i d\underline{w}_i^{\text{exp}} + \gamma_i d\underline{w}_i^{\text{res}} \tag{3}$$

where $r_i = \ln R_i$ reprensents the log of rents, $\underline{w}_i^{\text{wkp}} = \sum_{z\in\mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_z$ and $\underline{w}_i^{\text{res}} = \ln \underline{W}_i$ are defined as in i's *workplace* and *residence* MW levels; and $\beta_i = \frac{\sum_{z\in\mathcal{Z}(i)} \pi_{iz}\xi_{iz}^{p}\epsilon_{i}^{p}}{\eta_i - \sum_z \pi_{iz}\xi_{iz}} > 0$ and $\gamma_i = \frac{\sum_z \xi_i^y \epsilon^y}{\eta_i - \sum_z \pi_{iz}\xi_{iz}} < 0$ are parameters. □

Proposition 2 shows that, under an homogeneity assumption,[16] the change in rents following a small changes in the profile of MWs can be expressed as a function of two MW-based measures: one summarizing the effect of MW changes in workplaces, and another one summarizing the effect of the MW in the same ZIP code i. This motivates our empirical strategy, where we regress log rents on the empirical counterparts of these measures.

3 Data

In this section, we describe the construction of our data set. First, we explain in detail what our sources of data are and the steps we take to put them together in a ZIP code by month panel data set. We focus on describing data on rents coming from Zillow, and our construction of the actual and experienced MW—a new measure of the MW that accounts for the fact that residence and workplace may differ—. Later, we explore how the sample of ZIP codes available in Zillow, our source for rents data, compares to the U.S. sample of ZIP codes. We conduct our main analysis on a balanced panel of ZIP codes which construction we describe as well.

3.1 Rents Data from Zillow

One of the main challenges to estimate the effects of any policy on the housing market is to obtain reliable data. Housing rent data has been particularly scant in the literature. Recent papers have used Small Area Fair Market Rents (SAFMRs) series from US HUD (2020a), available at the ZIP code and year level (Tidemann 2018; Yamagishi 2019). We, on the other hand, leverage newly available data from Zillow at the ZIP code and month level. The high frequency of the Zillow data is an advantage since it allows us to explore the effects of MW changes on rents exploiting the precise timing of their enactment.

Zillow is the leader online real estate and rental platform in the U.S., hosting more than 110 million homes and 170 million unique monthly users in 2019 (Zillow 2020a). Zillow provides the median rental and sale price (both total and per square foot) among homes listed on the platform

[16] We acknoweldge that this simpliflying assumption will not hold exactly in practice. For our empirical estimates, we need the weaker assumption that heterogeneity in the effect of workplace MWs is not correlated to shocks in the housing market.

in a given period. Time series are provided for different house types and at several geographic and time aggregation levels (Zillow 2020b).[17] We collect the USPS ZIP code level monthly time series. The time span of the data varies at the ZIP code level, and geographical units with a small number of listings are omitted.[18] As explained below, we construct a balanced panel to address the changing composition of the sample.

Clearly, even within a single ZIP code, there could be a great deal of heterogeneity in terms of house sizes and types, threatening the validity of our estimations. To minimize price variation arising from housing units' characteristics, we focus our primary analysis on a single housing category: *single-family* houses, *condominium, and cooperative* units (SFCC). This is by far the series with the largest number of non-missing ZIP codes, as it covers the most common U.S. rental house types. In fact, roughly a third of the nation's 47.2 million rental units in 2018 fit the category of single-family homes, with the remaining 43 percent made up from buildings with five or more units (**fernald2020americas**). Because we want to condition our comparisons on house size we focus on *per square foot* rents. As a result, our main outcome variable represents the median rental price per square foot in the SFCC category among units listed in the platform for a given ZIP code and month.

Zillow data has several limitations. The first one is that we do not observe the underlying number of units listed for rent in a given month. Therefore, changes in the inventory introduce additional variation in the reported median rental price that we are unable to control for. We do observe the number of houses listed *for sale*, which we use as a proxy for this variable in robustness analyses.[19] A second limitation is that Zillow's market penetration dictates the sample of ZIP codes available. As a result, we observe a selected sample of typically urban ZIP codes. We describe our sample in more detail later in this section.

To ensure that our data correctly captures the price evolution of the U.S. rental market, we compare Zillow's median rental price with 5 SAFMRs series for houses with a different number of bedrooms (0, 1, 2, 3, and 4 or more). SAFMRs are calculated for ZIP codes within metropolitan areas at a yearly level, and generally correspond to the 40th percentile of the rent distribution for that ZIP code.[20] The correlation between Zillow's SFCC and SAMFR's ZIP-code-level time series is consistently above 90 percent. Appendix **??** compares the time series variation of the Zillow SFCC series and a weighted average of the SAFMR series for different number of bedrooms.[21] The Zillow rent data is always higher in levels. Part of this difference is intuitively related to the fact that Zillow reports median rent prices while SAFMRs are based on the 40th percentile of the rent distribution. However, the two series show similar trends, confirming that Zillow does a decent job in capturing

[17] The availability of different time series changed over time, so not all series used for the analysis might be still available to download. See Zillow (2020b) for more details on the data shared by Zillow.

[18] Two related notes are the following: (i) once a ZIP code enters our panel, it shows a complete time-series; (ii) we do not know the threshold used by Zillow to censor the data.

[19] We are not aware of a ZIP code-month dataset that provides counts of houses for rent.

[20] For more information on how SAFMRs are calculated, see US HUD (2017, page 41641).

[21] To compute the weighted SAMFR series, we proceed as follows. First, we compute the national yearly average for both the Zillow SFCC and the 5 SAFMR series. Then, for each of the latter, we compute the U.S. share of single family, condo, and cooperative houses with that number of bedrooms using the *American Housing Survey* (AHS). To ensure comparability, we only use the estimated count for rental houses in this step. (Additionally, AHS data is available only for years 2011, 2013, 2015, 2017, and 2019. We therefore fill missing years with the previous year's share.) Finally, we weight SAFMR series using the shares mentioned above.

the overall dynamics of the U.S. rental market in metropolitan areas.

3.2 The Statutory and Experienced Minimum Wage

Our main explanatory variable is the minimum wage. We collect data on federal, state, county, and city-level MWs from Vaghul and Zipperer (2016). We complement their data, which runs up to mid-2016, with MW data for the years 2016 to 2019 from UC Berkeley Labor Center (2020). Because we are interested in studying rental dynamics at the ZIP code level using Zillow, we assign MW levels to ZIP codes by taking the following steps. First, we map each ZIP code to a metropolitan statistical area or rural town using HUD crosswalks (US HUD 2020b). Given that ZIP codes can cross different administrative borders, we use the number of housing units from the 2010 census and geographic codes to map each ZIP code to a unique county by assigning it to the one with the highest share of houses from that ZIP code. We also map each ZIP code to a county and state analogously. After this process, we are able to assign a unique state and local level MW to each ZIP code. We define the *statutory* MW variable as the maximum between the ones required by the federal, state, county, and city levels.[22] As a result, we only use MW changes that are binding, meaning that they actually impact that maximum.

When restricting to the sample of ZIP codes available in Zillow, our data reports 18,689 MW changes at the ZIP code-month level. These, in turn, arise from 151 state-level and 182 county- and city-level changes. **??** shows the distribution of positive increases in our statutory MW variable among all ZIP codes available in the Zillow data.[23] Panel (a) shows the distribution of intensity of our MW changes. The average percent change among Zillow ZIP codes is 5.5%. However, we observe a decent amount of large increases. Our estimation strategy will exploit the intensity of MW changes. On the other hand, panel (b) shows the timing of those changes between 2010 and 2019. Most changes occur in either January or July, and the majority of them take place later in the panel. This could be problematic since the timing of entry of ZIP codes into the panel is also concentrated in these months. We construct a balanced sample of ZIP codes to tackle this issue.

We construct an alternative measure to capture the effects of MW policies: the *experienced* MW. This measure aims to account for the fact that workplace location often differs from the residence one. The MW that matters for a given local rental market is the one experienced by the people living in it, and so by tracking where people in each ZIP code work we can get a better sense of the relevant MW there. To construct this measure we need to know, for each ZIP code, where workers residing in that ZIP code work. We obtain this information from the 2017 Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES). In particular, we use the origin-destination matrix mapping jobs from residence to workplace locations. The data come at the block group level. We aggregate that to construct a ZIP code residence-workplace matrix where we observe the number of workers for each residence-workplace pair.

[22]Some states and cities issue different MW levels for small businesses (usually identified by having less than 25 employees). In these cases, we select the general MW level as the prevalent one. In addition, there may be different (lower) MW levels for tipped employees. We do not account for them because employers are typically required to make up for the difference between tipped MW plus tips and actual MW.

[23]There are a few cases of decrease in the MW arising from judicial decisions overthrowing local MW ordinances. For expository reasons, they are not shown in the figure. However, they are used in estimations throughout the paper.

We then use the ZIP code residence-workplace matrix to build exposure weights. Denote ZIP codes by i and monthly dates by t. Let $\mathbb{Z}_i$ be the set of ZIP codes in which i's residents work (including i). We construct the set of weights $\{\omega_{iz}\}_{z\in\mathbb{Z}_i}$ as

$$\omega_{iz} = \frac{N_{iz}}{N_i},$$

where N_{iz} is the number of workers who reside in ZIP code i and work in z, and N_i is the total working-age population of ZIP code i.[24] Given that origins present a large number of destinations with extremely low percentages of workers, we trim the number of destination ZIP codes to those making up to 90 percent of the workforce.[25] Letting $\underline{w}_{it}$ denote the statutory MW in ZIP code i and month t, we define the experienced minimum wage measure as

$$\underline{w}_{it}^{\text{exp}} = \sum_{z\in\mathbb{Z}_i} \omega_{iz}\underline{w}_{zt} \ . \tag{4}$$

The experienced MW of a ZIP code is based on the MWs binding in other ZIP codes where its residents work. An increase in a city, for example, may not have an impact on the local rental market if most residents are not MW workers. It will, however, affect neighboring ZIP codes where MW workers reside. We will use this insight in our analysis. See **??** for some discussion on how we use this measure, and **??** for further details and estimation results.

3.3 Other Data Sources

We collect socio-demographic information from the 2010 Census and the 5-years 2008-2012 American Community Survey (ACS). The data is initially obtained at the Census tract level and mapped into USPS ZIP codes using HUD crosswalks (US HUD 2020b). We assign the following characteristics to each ZIP code: population, number of housing units, median income, African-American population, number of unemployed, and number of college students. We use this information to classify ZIP codes into, for example, high or low median income to perform heterogeneity analysis.

To proxy for local economic activity we collect data from the Quarterly Census of Employment and Wages (QCEW) at the county-quarter and county-month levels for every main industrial division.[26] For each county-quarter-industry cell, we observe the number of establishments and the average weekly wage. For each county-month-industry cell, we additionally observe the number of employed people. We merge this data onto our ZIP code-month panel by county and quarterly date.

Finally, we use the LODES data to proxy for MW workers' residence and workplace location. Beyond the origin-destination matrices, the LODES data provides block-level information on jobs by residence area (RAC) and workplace area (WAC) characteristics. These include jobs for various

[24] The LODES data additionally reports origin-destination matrices for number of workers 29 years old and younger and number of workers earning less than $1,251 per month. We compute weights based on both these sub-groups as well. However, the resulting experienced MW measures with any set of weights are highly correlated among each other ($\rho > 0.99$ for every pair). Thus, we use working population weights throughout the paper.

[25] Results based on the full distribution are identical to those presented in the paper.

[26] The QCEW covers the following industrial aggregates: "Agriculture, Forestry, and Fishing", "Mining", "Construction", "Manufacturing", "Transportation and Public Utilities", "Wholesale Trade", "Retail Trade", "Financial activities" (including insurance and real state), "Services", and "Public Administration".

types of workers.[27] We use RAC and WAC datasets to "locate" workers likely to earn MW by looking at the state-level distribution of such type of workers. We build, for each ZIP code in the sample, the share (out of the state total) of workers under 30 years old earning less than $1,251 per month that either *live* or *work* there. We take these data as time-invariant characteristics of our ZIP codes.

3.4 The Resulting Panel

Using the data described above, we put together a panel dataset at the ZIP code and monthly date levels from February 2010 to December 2019. Given that ZIP codes enter the Zillow data progressively over time affecting the composition of the sample, we construct our baseline *estimating panel* by keeping in the sample those ZIP codes with valid rents data as of July 2015.[28] This panel contains 5,302 MW increases, which arise from 124 state changes and 99 county and local level changes. 4,224 of those changes take place after ZIP codes already entered the panel, and thus are used in estimation.

We stress the fact that our data does not cover the full sample of ZIP codes, but rather a selected one. Appendix **??** maps the full set of available ZIP codes in the Zillow data, together with population density. The Zillow sample seems fairly distributed across urban areas, although some important areas have limited coverage.

?? further compares the Zillow sample to the population of ZIP codes along several critical demographic dimensions. Columns 1 and 2 report data for the whole universe of U.S. ZIP codes and for the top 100 U.S. metropolitan areas, respectively. In column 3 we show the complete set of ZIP codes in the Zillow data. Finally, column 4 shows our baseline estimating sample. Focusing on our preferred variable—median rent per square foot in the SFCC category—, we collect rent data from Zillow for 3,315 unique ZIP codes, which amount to 8.5 percent of the 38,893 total for the entire U.S. and 46.7 percent of the 2010 U.S. population.

The average median household annual income for those ZIP codes is $65,475, almost 25 percent higher than the same figure for the average U.S. ZIP code and 5 percent higher than the top 100 metropolitan areas. ZIP codes in the baseline sample are even richer, with an average household income of $66,920. Furthermore, both Zillow ZIP codes and those in our estimating panel have a higher share of urban population, college students, African-American and Hispanic population, and houses for rent than the average urban ZIP code. In an attempt to capture the treatment effect for the average urban ZIP code we conduct an estimation re-weighting our sample to match characteristics of the top 100 CBSA sample of ZIP codes. Because our ZIP codes are richer than the average (i.e., arguably less influenced by MW changes), we expect to find a larger effect in this

[27] LODES RAC and WAC datasets provide workers' breakdown for the following characteristics: age (less than 29, 30 to 54, more than 55); workers' earnings (less than $1,251/mo., $1,251/mo. to $3,333/mo., more than $3,333/mo.); NAICS(11, 21, 22, 23, 31-33, 42, 44-45, 48-49, 51, 52, 52, 54, 55, 56, 61, 62, 71, 72, 81, 92); race (White alone, Black or African-American alone, American-Indian or Alaskan Native alone, Asian alone, Native Hawaiian alone, two or more race groups, not Hispanic or Latino, Hispanic or Latino); educational attainment (less than high school, high school or equivalent, some college or associate, bachelor's degree or advanced degree); sex (male, female).

[28] We note that the resulting panel is still unbalanced, in the sense that the time series for some ZIP codes starts before July 2015. However, from July 2015 onward our data contains no missing values in the main rent variable used in the analysis.

exercise.

Finally, **??** shows some basic sample statistics of our baseline estimating panel. As suggested in the table, the statutory and experienced MW are quite similar. We compare these measures in more detail in **??**. We also show summary statistics of median rents in the SFCC category. The average of monthly median rents is \$1,651 in absolute values and \$1.27 per square foot, although these variables show a great deal of variation. Finally, for illustration, we show average weekly wage, employment and establishment count for the "Financial activities" sector from the QCEW. Appendix **??** additionally shows summary statistics for the experienced MW computed using alternative weights, rents in different categories of the Zillow data, and the full set of QCEW industries we use as controls in our regressions.

4 Empirical Strategy

4.1 First-differences model

Consider the following two-way fixed effects model relating rents and the minimum wage:

$$r_{it} = \alpha_i + \hat{\delta}_t + \gamma \underline{w}_{it}^{\text{res}} + \beta \underline{w}_{it}^{\text{wkp}} + \mathbf{X}_{it}^{'} \eta + \varepsilon_{it}$$

where i and t index ZIP codes and time periods (months), r_{it} represents the log of rents per square foot, $\underline{w}_{it}^{\text{res}}$ is the ZIP code's residence MW, defined as $\ln \underline{W}_{it}$, $\underline{w}_{it}^{\text{wkp}}$ is the ZIP code's workplace MW, defined as $\sum_{z \in \mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_{zt}$, α_i and $\hat{\delta}_t$ are fixed effects, and $\mathbf{X}_{it}$ is a vector of county-level time-varying controls. Time runs from February 2010 ($\underline{T}$) to December 2019 ($\overline{T}$). The parameters of interest are γ and β, which we interpret as the elasticity of rents to the residence and workplace MW, respectively.

By taking first differences on the previous equation we obtain

$$\Delta r_{it} = \delta_t + \gamma \Delta \underline{w}_{it}^{\text{res}} + \beta \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta \varepsilon_{it}, \tag{5}$$

where $\delta_t = \hat{\delta}_t - \hat{\delta}_{t-1}$. We spell out the model in first differences because it is reasonable to expect unobserved shocks to rental prices to be persistent over time. Appendix Table XX shows evidence of AR(1) auto-correlation in the error term. The main results of the paper are obtained under the model in 5. However, to compare to previous results we also estimate versions of the model that exclude some of the MW measures.

4.2 Identification and Causality

We start by noting that, in order to separately identify the effect of residence and workplace MW changes, we need these variables to have independent variation. While this requirement is standard, it is not obvious that it holds in our application. For instance, if there were a single national minimum wage level or if everybody lived and worked in the same location, then we would have $\Delta \underline{w}_{it}^{\text{res}} = \Delta \underline{w}_{it}^{\text{wkp}}$ for all (i, t). In the next section we show that there is substantial independent variation in the MW measures.

Being able to compute γ and β does not mean that they can be given a causal interpretation. For this, we require a *strict exogeneity* of both MW variables. Formally,

$$E\left[\begin{pmatrix}\Delta \underline{w}_{is}^{\text{res}} \\ \Delta \underline{w}_{is}^{\text{wkp}}\end{pmatrix} \Delta\varepsilon_{it} \middle| \delta_t, \Delta \mathbf{X}_{it}\right] = \begin{pmatrix}0 \\ 0\end{pmatrix} \tag{6}$$

for all $s \in \{\underline{T}, ..., \overline{T}\}$. That is, we require the unobserved shocks to rents to be uncorrelated with past and present values of changes in our MW measures conditional on time-period fixed effects and controls.

This assumption has two important implications. First, it implies no pre-trends in rents leading up to minimum wage changes (conditional on controls). We will test this implication more formally by including leads of the MW variables. Second, it rules out feedback effects from current values of rents on our MW variables, i.e., MW changes are assumed not to be influenced by past values of rents. While we think this is a reasonable assumption—MW levels are usually not set by considering their effects on the housing market—, we allow this type of feedback effects in a specification described later. Finally, we note that our identifying assumption allows for arbitrary correlation between ZIP code effects α_i and both MW variables (e.g., our empirical strategy is robust to the fact that richer districts tend to vote for MW policies).

We worry that unobserved shocks, such as local business cycles, may systematically affect both rents and minimum wage changes, which is why we include period-fixed effects and time-varying controls. The period fixed effects should capture common trends in the housing market. In some specifications we allow this trends to vary by CBSA. To control for variation arising from unobserved trends in local markets we include economic controls from the QCEW.[29] Specifically, we use average weekly wages, employment and establishment counts for the sectors "Professional and business services," "Information," and "Financial activities." We assume that these sectors are not affected by the minimum wage.[30] We also try models where we control for ZIP code-specific linear and quadratic trends, which should account for time-varying heterogeneity not captured by our economic controls that follows this pattern.

We can test assumption 6 using models that include leads and lags of the MW variables:

$$\Delta r_{it} = \delta_t + \sum_{r=-s}^{s} \gamma_s \Delta \underline{w}_{is}^{\text{res}} + \sum_{r=-s}^{s} \beta_s \Delta \underline{w}_{is}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta\varepsilon_{it}. \tag{7}$$

In this equation s is the number of months of a symmetric window around the MW change. We use $s = 6$ as baseline but our results are very similar for windows of 3 or 9 months. Because the MW measures are strongly correlated, adding leads and lags of both leads to a decline in precision. Thus, we try models with leads and lags of only one of the MW measures as well.

Under the assumption that there are no anticipatory effects in the housing market, we interpret

[29] These data are aggregated at the county level, and represent a second best given the unavailability of controls at the ZIP code level.

[30] In fact, according to U.S. Bureau of Labor Statistics (2020, table 5), in 2019 such industries accounted for 3.5, 1, and 1.2 percent of the total number of MW workers, respectively.

the absence of pre-trends as evidence against the presence of unobserved economic shocks driving our results. We think that, given the high frequency of our data and that we focus on short windows around MW changes, the assumption of no anticipatory effects is reasonable.[31] We further present evidence in favor of this assumption by showing that our MW measures do not predict the number of listings of houses for sale in Zillow.[32]

As written, the model in 5 assumes a balanced panel of ZIP codes. However, as explained in Section XX we use as our baseline a partially balanced panel. This estimation is valid under a stronger version of 6, where we also condition on sample selection. If MW changes and rents change together in a systematic way upon entry of a ZIP code to the data, then our results would be invalid. Because of this we show that our results are similar when we estimate our model on a fully balanced panel. Our results are also similar under an unbalanced panel where we include for time-of-entry fixed effects.

4.3 Identification beyond the MW measures

As stated earlier, to be able to compute γ and β we require the residence and workplace MWs to vary independently within each period. Furthermore, we require the strict exogeneity assumption to assure that our estimates will be unbiased. However, it is not totally clear what these assumptions mean for the underlying commuting shares and the dynamics of unobserved heterogeneity in trends for the most common type of treatment in our data: an increase in the minimum wage of a city or state that affects a subset of the ZIP codes in a metropolitan area.

Following such a policy there will be ZIP codes where the residence MW goes up and ZIP codes where it does not. We call ZIP codes in the first group "directly treated." Appendix B shows that, for this policy, β and γ can be computed under the following assumptions: 1) parallel trends between ZIP codes that are directly treated and ZIP codes that are not; 2) the existence of two groups of ZIP codes that are not treated directly and have differential exposue to the policy via the commuting shares; and 3) parallel trends between those two groups.

4.4 Heterogeneity and Representativeness of the Results

If the mechanism proposed in Section 2 is correct, then we expect the effect of the residence MW to be stronger where there are many MW workers. The reason is that the production of non-tradable goods presumably uses more low-wage work, and thus the increase in the MW would affect prices more. Similarly, we expect the effect of the workplace MW to be stronger in locations with lots of MW residents, as income would increase more strongly there. We use different proxies for the shares

[31] We can also interpret the absence of pre-trends as a test for anticipatory effects if we are willing to assume that the controls embedded in $\mathbf{X}_{ct}$ capture all relevant unobserved heterogeneity arising from local business cycles. While we find the interpretation given in the text more palatable, the data is consistent with both.

[32] Ideally, we would run this regression on the number of rental units. Unfortunately this information is not available in the data. Specifically, we track the number of houses listed for sale in a sample of ZIP codes during the period 2013-2019 for our preferred house type (SFCC).

of MW workers and residents, and estimate the following model:

$$\Delta r_{it} = \delta_t + \sum_{q\in\{1,2\}} \gamma_q \Delta I_q^{\text{wkp}} \underline{w}_{it}^{\text{res}} + \sum_{q\in\{1,2\}} \beta_q \Delta I_q^{\text{res}} \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta\varepsilon_{it}, \tag{8}$$

where I_1^{wkp} and I_2^{wkp} are indicators for being below and above median in the share of low-wage workers, and I_1^{res} and I_2^{res} are analogous indicators for the share of low-wage residents. We conduct similar heterogeneity exercises with socioeconomic variables, such as the share of non-white workers in a ZIP code.

Because our ZIP codes come from a selected sample, they may not represent the causal effect for the average urban US ZIP code. To obtain effects that are more representative we follow Hainmueller (2012) and estimate our main models re-weighting observations to match key moments of the distribution of characteristics of urban ZIP codes.

4.5 Alternative models

Recent literature on difference-in-differences methods has shown that usual estimators do not correspond to the average treatment effect when the treatment roll-out is staggered and there is treatment-effect heterogeneity (Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022). The solution to this issue usually relies on carefully defining the control group of the treatment. While our setting does not correspond exactly to the models discussed in this literature, we worry about the validity of our estimator. In an appendix, we take two steps to ease those concerns: 1) we estimate equation 5 allowing the time fixed effects to vary by metropolitan area; and 2) we constructe a "stacked" implementation of our model in which we take 6 months of data around MW changes for ZIP codes in CBSAs where some ZIP codes did not received a direct MW change, and then estimate equation 5 on this sample including event-by-time fixed effects. These strategies limit the comparisons that identify the coefficients of interest to ZIP codes within the same metropolitan area.

Regardless of the estimation strategy, our results still rely on the strict exogeneity assumption. In an appendix we show that we obtain similar results under a model that includes the lagged dependent variable as control. In such a model, β and γ have a causal interpretation under a weaker *sequential exogeneity* assumption (Arellano and Bond 1991; Arellano and Honoré 2001). This alternative assumption allows for feedback of rents onto MW changes.

5 Results

In this section we...

5.1 Baseline Results

Alternative specifications Discuss results using either of the measures in more detailed. Discuss results using counties, pointing to appendix.

5.2 Robustness checks

Alternative rents measure Discuss results with SAFMR

Sample Selection and External Validity Discuss unbalanced, reweighted, fully balanced, and so on.

5.3 The Heterogeneous Impact of MW Changes on Rents

Construct measure of MW workers residing in each zip code using ACS 2015?

6 Counterfactual Analysis

Discuss pass-through estimates. Stress that they depend on geography of prevailing MWs across the commmuting zone.

Discuss welfare briefly. Maybe conjecture on long-run effects (low-wage workers relocating to areas with low MW and commuting to areas with high MW).

Discuss policy implications.

7 Conclusions

In this paper, we ask whether minimum wage changes affect housing rental prices. To answer this question we use rental listings from Zillow and MW changes collected from Vaghul and Zipperer (2016) and UC Berkeley Labor Center (2020), to construct a panel at the ZIP code-month level. The high frequency and resolution of our data allows us to analyze state, county, and city-level changes in the MW to identify the causal impact of raising the MW on the local rental housing market.

To do that, we leverage on a panel difference-in-differences approach that exploits the staggered implementation and the intensity of hundreds of MW increases across thousands of ZIP codes. Our results indicate that minimum wage increases have a positive impact on rents, which we find to be robust to different specifications. Our models suggest that a 10 percent increase in MW causes rents to increase approximately by 0.26 percent in the same month, and 0.5 to 0.6 percent in the long run. We go beyond the average MW effect and we look at the heterogeneity of effects across ZIP codes. We show that rents disproportionately increase in ZIP codes where: (i) it is more likely to find MW workers as residents; (ii) there is a lower share of college graduates; (iii) a larger share of younger residents (15-24 yers old); and (iv) a larger share of African-American residents.

We then leverage LODES data to create a measure of experienced MW that accounts for the difference between residence and workplace location of MW earners. We are able in this way to better track the changes in MW that residents of different ZIP codes experience, which allows us to (i) estimate an arguably more relevant treatment effect when using the experienced MW instead of the statutory one, and (ii) identify suggestive evidence of income transfers across ZIP codes to locations where MW workers leave.

To assess the magnitude of the effect of MW on housing rents, we perform benchmarking exercises to recover the income-to-rent pass-through associated with the causal estimates. We use both QCEW county-quarter wage data, and results from Cengiz et al. (2019) to first recover the average wage elasticity to the MW used to obtain compute the pass-through. All exercises consistently show that a share approximately between 19 and 28 percent of the additional income generated by MW policies end up captured by landlords via rents.

Our results highlights that place-based policies aimed at the labor market can also have significant impacts on other related markets. In particular, MW provisions are usually thought as a way to guarantee economic means to low income workers. However, they may also be benefiting landlords in ways that are unintended. In this sense, studying how place-based policies affect the housing market becomes an important step to better understand income inequality across U.S. neighborhoods.

References

Agarwal, Sumit, Brent W Ambrose, and Moussa Diop (2019). "Do minimum wage increases benefit intended households? Evidence from the performance of residential leases". In: *Evidence from the Performance of Residential Leases (February 15, 2019)*.

Allegretto, Sylvia and Michael Reich (2018). "Are local minimum wages absorbed by price increases? Estimates from internet-based restaurant menus". In: *ILR Review* 71.1, pp. 35–63.

Arellano, Manuel and Stephen Bond (1991). "Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations". In: *The review of economic studies* 58.2, pp. 277–297.

Arellano, Manuel and Bo Honoré (2001). "Panel data models: some recent developments". In: *Handbook of econometrics*. Vol. 5. Elsevier, pp. 3229–3296.

Autor, David, Alan Manning, and Christopher L Smith (2016). "The contribution of the minimum wage to US wage inequality over three decades: a reassessment". In: *American Economic Journal: Applied Economics* 8.1, pp. 58–99.

Butts, Kyle (2021). "Difference-in-Differences Estimation with Spatial Spillovers". In: *arXiv preprint arXiv:2105.03737*.

Cadena, Brian C (2014). "Recent immigrants as labor market arbitrageurs: Evidence from the minimum wage". In: *Journal of urban Economics* 80, pp. 1–12.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant'Anna (2021). "Difference-in-differences with a continuous treatment". In: *arXiv preprint arXiv:2107.02637*.

Callaway, Brantly and Pedro HC Sant'Anna (2021). "Difference-in-differences with multiple time periods". In: *Journal of Econometrics* 225.2, pp. 200–230.

Card, David and Alan B Krueger (1994). "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania". In: *American Economic Review* 84.4, pp. 772–93.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer (2019). "The effect of minimum wages on low-wage jobs". In: *The Quarterly Journal of Economics* 134.3, pp. 1405–1454.

Chaisemartin, Clément de and Xavier D'Haultfoeuille (2022). *Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey*. Tech. rep. National Bureau of Economic Research.

Delgado, Michael S and Raymond JGM Florax (2015). "Difference-in-differences techniques for spatial data: Local autocorrelation and spatial interaction". In: *Economics Letters* 137, pp. 123–126.

Dube, Arindrajit (2019a). "Impacts of minimum wages: review of the international evidence". In: *University of Massachusetts, Amherst. https://www. gov. uk/government/publications/review-of-theinternational-evidence-on-the-impacts-of-minimum-wages/review-of-the-international-evidenceon-the-impacts-of-minimum-wages-terms-of-reference*.

— (2019b). "Minimum wages and the distribution of family incomes". In: *American Economic Journal: Applied Economics* 11.4, pp. 268–304.

Dube, Arindrajit and Attila Lindner (2021). "City Limits: What do local-area minimum wages do?" In: *Journal of Economic Perspectives* 35.1, pp. 27–50.

Fernald, M (2020). "Americas rental housing 2020". In: *Joint Center for Housing Studies of Harvard University, Cambridge, MA*.

Hainmueller, Jens (2012). "Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies". In: *Political analysis*, pp. 25–46.

Hsieh, Chang-Tai and Enrico Moretti (2019). "Housing constraints and spatial misallocation". In: *American Economic Journal: Macroeconomics* 11.2, pp. 1–39.

Huang, Chen (2020). "Cross-Border Spillover Effects of State Increases in Minimum Wages". In: *Available at SSRN 3888842*.

Hughes, Samuel (2020). "Housing Demand and Affordability for Low-Wage Households: Evidence from Minimum Wage Changes". In: *Available at SSRN 3541813*.

Internal Revenue System (2019). *SOI Tax Stats - Individual Income Tax Statistics - ZIP Code Data (SOI).* `https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi`. Accessed: 2020-05-10.

Investopedia (2020). *The 7 Best Real Estate Websites of 2020.* Accessed: 2020-11-01. URL: `https://www.investopedia.com/best-real-estate-websites-5069964#citation-1`.

IPUMS (2019). *US Census data tables and mapping files.* Integrated Public Use Microdata Series. `https://www.nhgis.org/`. Accessed: 2020-01-06.

Kline, Patrick and Enrico Moretti (2014). "People, places, and public policy: Some simple welfare economics of local economic development programs". In.

Kuehn, Daniel (2016). "Spillover bias in cross-border minimum wage studies: Evidence from a gravity model". In: *Journal of Labor Research* 37.4, pp. 441–459.

Lee, David S (1999). "Wage inequality in the United States during the 1980s: Rising dispersion or falling minimum wage?" In: *The Quarterly Journal of Economics* 114.3, pp. 977–1023.

Leung, Justin H (2021). "Minimum wage and real wage inequality: Evidence from pass-through to retail prices". In: *Review of Economics and Statistics* 103.4, pp. 754–769.

Meer, Jonathan and Jeremy West (2016). "Effects of the minimum wage on employment dynamics". In: *Journal of Human Resources* 51.2, pp. 500–522.

Missouri Census Data Center (2014). *Geographic Correspondence Engine.* `https://mcdc.missouri.edu/applications/geocorr2014.html`. Accessed: 2020-02-15.

Miyauchi, Yuhei, Kentaro Nakajima, and Stephen J Redding (2021). *Consumption Access and Agglomeration: Evidence from Smartphone Data.* Tech. rep. National Bureau of Economic Research.

Monras, Joan (2019). "Minimum wages and spatial equilibrium: Theory and evidence". In: *Journal of Labor Economics* 37.3, pp. 853–904.

Neumark, David and Peter Shirley (2021). *Myth or measurement: What does the new minimum wage research say about minimum wages and job loss in the United States?* Tech. rep. National Bureau of Economic Research.

Neumark, David and William Wascher (2006). *Minimum wages and employment: A review of evidence from the new minimum wage research.* Tech. rep. National Bureau of Economic Research.

PDX, Real Estate Agent (2020). *Top 10 Real Estate Websites by Traffic – 2020 Update*. Accessed: 2020-11-01. URL: https://realestateagentpdx.com/top-10-real-estate-websites-by-traffic-2020-update/16956.

Pérez Pérez, Jorge (2021). "City Minimum Wages". In: *Job Market Paper, Brown University*. URL: http://jorgeperezperez.com/files/Jorge_Perez_JMP.pdf.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe (2022). "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature". In: *arXiv preprint arXiv:2201.01194*.

Tidemann, Krieg (2018). "Minimum Wages, Spatial Equilibrium, and Housing Rents". In: *Job Market Paper*.

UC Berkeley Labor Center (2020). *Inventory of U.S. City and County Minimum Wage Ordinances*. https://laborcenter.berkeley.edu/inventory-of-us-city-and-county-minimum-wage-ordinances/. Accessed: 2020-04.

UDS Mapper (2020). *ZIP Code to ZCTA Crosswalk*. https://udsmapper.org/zip-code-to-zcta-crosswalk/. Accessed: 2020-02-15.

U.S. Bureau of Labor Statistics (2020). *Characteristics of minimum wage workers, 2019*. https://www.bls.gov/opub/reports/minimum-wage/2019/home.htm. Accessed: 2020-11.

US Bureau of Labor Statistics (2020). *Quarterly Census of Employment and Wages*. https://www.bls.gov/cew/downloadable-data-files.htm. Accessed: 2020-05-10.

US Census Bureau (2020a). *American Community Survey 5-year Data*. https://www.census.gov/data/developers/data-sets/acs-5year.html. Accessed: 2020-10-03.

— (2020b). *LEHD Origin-Destination Employment Statistics Data (2009-2018) [version 7]*. U.S. Census Bureau, Longitudinal-Employer Household Dynamics Program. https://lehd.ces.census.gov/data/. Accessed: 2021-10-03.

US HUD (2017). *Federal Register*. US Department of Housing and Urban Development. https://www.huduser.gov/portal/datasets/fmr/fmr2018/FY2018-FMR-Preamble.pdf.

— (2020a). *Fair Market Rents*. US Department of Housing and Urban Development. https://www.huduser.gov/portal/datasets/fmr.html. Accessed: 2020-03-10.

— (2020b). *USPS crosswalks*. US Department of Housing and Urban Development. https://www.huduser.gov/portal/datasets/usps_crosswalk.html. Accessed: 2020-03.

Vaghul, Kavya and Ben Zipperer (2016). "Historical state and sub-state minimum wage data". In: *Washington Center for Equitable Growth Working Paper* 90716.

Yamagishi, Atsushi (2019). *Minimum Wages and Housing Rents: Theory and Evidence from Two Countries*. Tech. rep. Accessed at https://mpra.ub.uni-muenchen.de/94238/1/MPRA_paper_94238.pdf.

— (2021). "Minimum wages and housing rents: Theory and evidence". In: *Regional Science and Urban Economics* 87, p. 103649.

Zillow (2020a). *Zillow Facts and Figures*. https://www.zillowgroup.com/facts-figures/. Accessed: 2020-10-23.

— (2020b). *Zillow Research Data*. https://www.zillow.com/research/data/. Accessed: 2020-02-15.

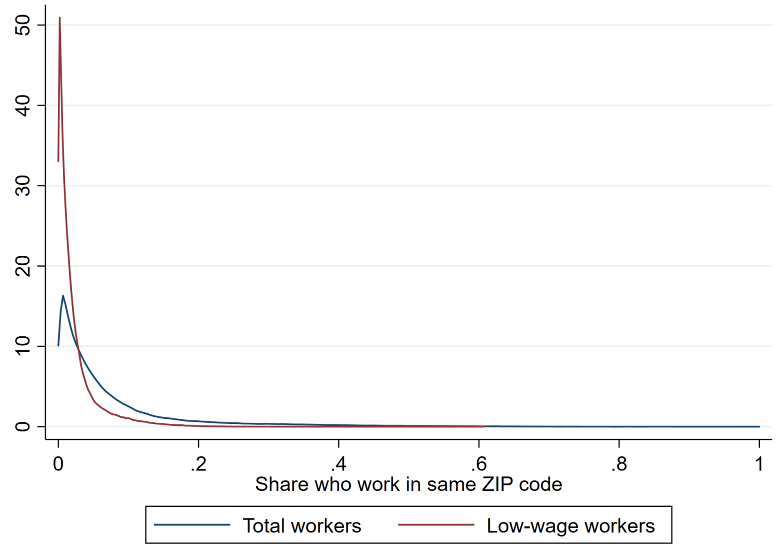
Table 1: Static model

	Change wrk. MW	Change log rents		
	(1)	(2)	(3)	(4)
Change residence minimum wage	0.8681	0.0262		-0.0241
	(0.0295)	(0.0136)		(0.0180)
Change workplace minimum wage			0.0321	0.0579
			(0.0150)	(0.0284)
Sum of coefficients				0.0338
				(0.0152)
County-quarter economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0764
R-squared	0.9443	0.0209	0.0209	0.0209
Observations	131,196	131,196	131,196	131,196

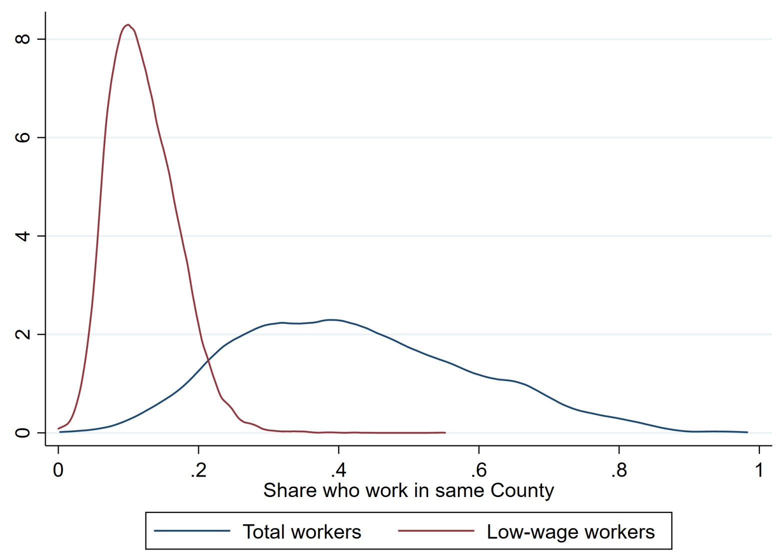
Notes:

Figure 1: Distribution of share of workers who work where they live, 2017

(a) ZIP code



(b) County

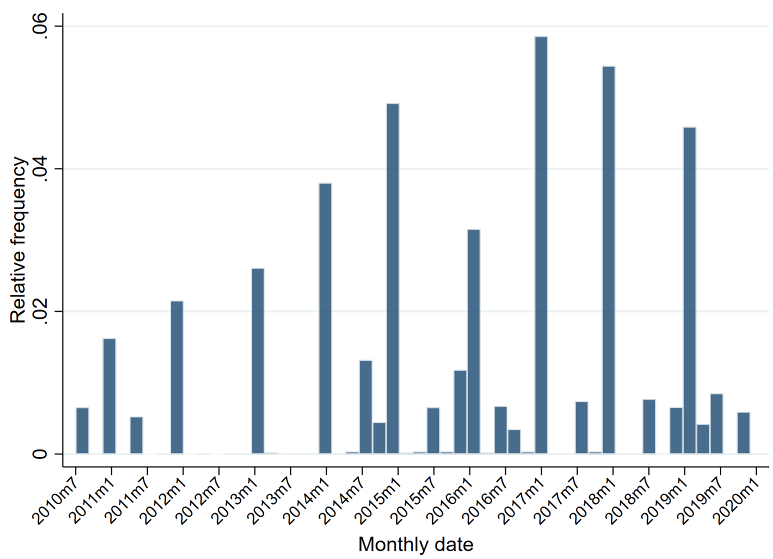


Notes: Data are from LODES origin-destination matrices (US Census Bureau 2020b), aggregated at the ZIP code and county levels. The figure shows the estimated distribution of the share of workers who reside and work in the same geographical unit, at the ZIP code (panel a) and County (panel b) levels.

Figure 2: Distribution of Minimum Wage Changes

(a) Intensity

(b) Timing



Notes: The histograms show the distribution of positive MW changes in the full sample of ZIP codes available in the Zillow data. Panel (a) reports the intensity of the changes in percentage terms. Panel (b) plots the distribution across time of such changes.

Appendix

A A dynamic supply and demand model

The geography is represented by a set of ZIP codes $\mathcal{Z}$. There is an exogenously given distribution of people with differing residence i and workplace z locations across these ZIP codes which, as in the main body of the paper, we denote by $\{L_{iz}\}_{i,z\in\mathcal{Z}\times\mathcal{Z}}$.

Let H_{it} be the stock of square feet rented in period t, where t is characterized by a month m and year y. This stock is composed of contracts starting at different calendar months. We assume that all contracts last for one year. We impose that that $H_{it} \leq D_i$ for all t, where D_i denotes the total number of available square feet in i.

We further decompose H_{it} as follows. Let $h_{izt} = h_{iz}\left(R_{it}, \underline{W}_{it}, \underline{W}_{zt}\right)$ be the per-person demand of housing of group (i, z) in period t, which depends on the prevailing MW at the time of contract sign-up. We assume that this demand function is decreasing in residence MW and decreasing in workplace MW, just as in Section 2. For simplicity we omitted the mediation channels of prices and income. Let λ_{it} denote the share of i's residents who started their contracts in period t.[33] Then, we can write the stock of contracted square feet during period t as

$$H_{it} = \sum_{\tau=t-11}^{t} \lambda_{i\tau} \sum_{z\in\mathcal{Z}} L_{iz} h_{iz\tau}(r_{i\tau}, \underline{W}_{i\tau}, \underline{W}_{z\tau})$$

where $r_{i\tau}$ represents rents *per square foot* in period τ. It is convenient to define the stock of contracted square feet excluding the ones that were signed 12 months ago. We denote them by

$$\tilde{H}_{it} = \sum_{\tau=t-10}^{t} \lambda_{i\tau} \sum_{z\in\mathcal{Z}} L_{iz} h_{iz\tau}(r_{i\tau}, \underline{W}_{i\tau}, \underline{W}_{z\tau}).$$

We assume that all square feet are homogeneous and so they have the same price in the market.

Within-period equilibrium

Within this simple model, we assume the following timing: (1) At the beginning of period t, a share λ_{it} of people's contracts expire (the ones that started on $t-12$); (2) The square feet from expiring contracts are added to the pool of available rental space for new renters; (3) Renters in t and a flow supply of rental space in t determine equilibrium rents R_{it}. Let's go by these steps more formally.

As of the start of every period t, $\lambda_{i,t-12}\sum_z L_{iz} h_{iz,t-12}$ square feet become available for rent from each group of workers (i, z). The square feet available to rent in period t (vacant) are then

$$\lambda_{i,t-12} \sum_{z} L_{iz} h_{iz,t-12} + (D_i - H_{i,t-1}) = D_i - \tilde{H}_{i,t-1}.$$

[33] We assume that these shares do not vary by workplace.

Note that $(D_i - H_{i,t-1})$ are the non-rented square feet as of $t-1$.

We denote by $V_{it}(R_{it}, \lambda_t)$ the supply of housing, increasing in R_{it}. A feasilibity constraint is that

$$V_{it}(R_{it}, \lambda_t) \leq D_i - \tilde{H}_{i,t-1}. \tag{9}$$

The flow demand for new rentals in t by those whose contract expired is given by

$$\lambda_{it} \sum_z L_{iz} h_{izt} \left(R_{it}, \underline{W}_{it}, \underline{W}_{zt}\right).$$

This demand arises because a share of the ZIP code's contracts expired. Those people go to the market and may desire to rent more square feet given changes in their income.

The market in period t cleans if

$$\lambda_t \sum_z L_{iz} h_{iz} \left(R_{it}, \underline{W}_{it}, \underline{W}_{zt}\right) = V_{it}(R_{it}, \lambda_t). \tag{10}$$

Given minimum wages in t, $\{\underline{W}_{it}\}_{i \in \mathcal{Z}}$, the share of workers looking to rent in period t, λ_t, and a number of vacancies that satisfies (9), equation (10) determines equilibrium rents in period t. Because the properties of housing demand and housing supply are the same as in the model in Section 2, the equilibrium condition (10) implies an analogue of Propositions 1 and 2.

B Identification of first-differenced model with spillovers

Consider the causal model for rents given by

$$r_{it} = f_{it}(\{\underline{w}_{zt}\}_{z\in\mathcal{Z}})$$

where $\underline{w}_{zt}$ represents the "dose" of treatment received by unit i from ZIP code z in period t. We say that a ZIP code i is treated "directly" at time t if $\underline{w}_{it} > 0$. For this appendix, we think of $\mathcal{Z}$ as the ZIP codes in a closed metropolitan area. Following Section 4 we assume the following functional form for the causal model in first differences:

$$\Delta r_{it} = \gamma \Delta \underline{w}_{it} + \beta \sum_{z\in\mathcal{Z}} \pi_{iz} \Delta \underline{w}_{zt} + \delta_t + \Delta\varepsilon_{it} \tag{11}$$

where δ_t is a time effect, $\Delta\varepsilon_{it}$ stands for other factors that determine the evolution of rents in ZIP code i, and other objects are defined as in the paper. We show that the parameters β and γ can be recovered from data on rent changes and minimum wage changes under suitable parallel-trends assumptions.

Proposition 3 (Identification)**.** *Consider a policy such that, for some (directly treated) ZIP codes $z \in \mathcal{Z}_0 \subset \mathcal{Z}$ for non-empty $\mathcal{Z}_0$, $\Delta\underline{w}_{zt} = \Delta\underline{w} > 0$ if $t = \bar{t}$ and $\Delta\underline{w}_{zt} = 0$ if $t \neq \bar{t}$, and for (not directly treated) ZIP codes $z \notin \mathcal{Z}_0$, $\Delta\underline{w}_{zt} = 0$ for all t. Assume (1) there exist at least two not directly treated ZIP codes with differential exposure to the policy, (2) parallel trends among two non-empty subgroups of not directly treated ZIP codes (specified below), (3) parallel trends across directly treated ZIP codes and not directly treated ZIP codes. Then, the parameters β and γ in the structural model 11 are identified.*

Proof. The expected evolution of rents in ZIP codes not treated directly is

$$E[\Delta r_{it}|i \notin \mathcal{Z}_0] = \begin{cases} \delta_t + E[\Delta\varepsilon_{it}|z \notin \mathcal{Z}_0] & \text{if } t \neq \bar{t} \\ \beta \sum_{z\in\mathcal{Z}_0} \pi_{iz}\underline{w}_{zt} + \delta_t + E[\Delta\varepsilon_{it}|z \notin \mathcal{Z}_0] & \text{if } t = \bar{t}, \end{cases} \tag{12}$$

Now, rank theese ZIP codes according to their exposure to the policy $\Pi_i = \sum_{z\in Z_0} \pi_{iz}$. Consider a partition of ZIP codes in $\mathcal{Z}\setminus\mathcal{Z}_0$ in two non-empty subsets such that ZIP codes with $\Pi_i > \bar{\Pi}$ belong to a "high exposure" group $\mathcal{Z}_h$, and the rest to a "low exposure" group $\mathcal{Z}_l$, where $\bar{\Pi} \in (\min \Pi_i, \max \Pi_i)$. Using 12 we can compute, for $t \neq \bar{t}$,

$$E[\Delta r_{it}|i \in \mathcal{Z}_h] - E[\Delta r_{it}|i \in \mathcal{Z}_l] = E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_h] - E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_l],$$

and for $t = \bar{t}$,

$$E[\Delta r_{it}|i \in \mathcal{Z}_h] - E[\Delta r_{it}|i \in \mathcal{Z}_l] = \beta \left(E\left[\sum_{z\in\mathcal{Z}_0} \pi_{iz} \middle| i \in \mathcal{Z}_h\right] - E\left[\sum_{z\in\mathcal{Z}_0} \pi_{iz} \middle| i \in \mathcal{Z}_l\right]\right) \Delta\underline{w} \\ + E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_h] - E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_l].$$

Under assumption (2), namely that $E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_h] - E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_l] = 0$, we can re-arrange the previous equation to obtain

$$\beta = \frac{E[\Delta r_{it}|i \in \mathcal{Z}_h] - E[\Delta r_{it}|i \in \mathcal{Z}_l]}{\left(E[\sum_{z\in\mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_h] - E[\sum_{z\in\mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_l]\right)\Delta\underline{w}}. \tag{13}$$

Assumption (1) guarantees that $\left(E[\sum_{z\in\mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_h] - E[\sum_{z\in\mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_l]\right) \neq 0$, thus β is identified.

For those ZIP codes that are treated directly, the expected evolution of rents is

$$E[\Delta r_{it}|i \in \mathcal{Z}_0] = \begin{cases} \delta_t + E[\Delta\varepsilon_{it}|z \notin \mathcal{Z}_0] & \text{if } t \neq \bar{t} \\ \gamma\Delta\underline{w} + \beta\sum_{z\in\mathcal{Z}_0} \pi_{iz}\Delta\underline{w} + \delta_t + E[\Delta\varepsilon_{it}|z \notin \mathcal{Z}_0] & \text{if } t = \bar{t}. \end{cases}$$

Differencing with respect to 12 obtains, for $t = \bar{t}$,

$$\begin{aligned} E[\Delta r_{it}|i \in \mathcal{Z}_0] - E[\Delta r_{it}|i \notin \mathcal{Z}_0] = \gamma\Delta\underline{w} & \\ + \beta\left(E\left[\sum_{z\in\mathcal{Z}_0} \pi_{iz}\middle| i \in \mathcal{Z}_0\right] - E\left[\sum_{z\in\mathcal{Z}_0} \pi_{iz}\middle| i \notin \mathcal{Z}_0\right]\right)\Delta\underline{w} & \\ + E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_0] - E[\Delta\varepsilon_{it}|i \notin \mathcal{Z}_0]. & \end{aligned}$$

Assumption (3) means that $E[\Delta\varepsilon_{it}|i \in \mathcal{Z}_0] - E[\Delta\varepsilon_{it}|i \notin \mathcal{Z}_0] = 0$. Substituting in the previous equation yields

$$\gamma = \frac{E[\Delta r_{it}|i \in \mathcal{Z}_0] - E[\Delta r_{it}|i \notin \mathcal{Z}_0]}{\Delta\underline{w}} - \beta\left(E\left[\sum_{z\in\mathcal{Z}_0} \pi_{iz}\middle| i \in \mathcal{Z}_0\right] - E\left[\sum_{z\in\mathcal{Z}_0} \pi_{iz}\middle| i \notin \mathcal{Z}_0\right]\right). \tag{14}$$

Since β is known per equation 13, γ is identified as well. $\square$

C Appendix Tables

D Appendix Figures