

# Minimum Wage as a Place-based Policy: Evidence from US Housing Rental Markets <sup>\*</sup>

Diego Gentile Passaro      Santiago Hermo      Gabriele Borg <sup>†</sup>

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## 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 place-based nature of MW policies. We construct a novel measure of exposure to MW policies based on commuting shares, which we call a ZIP code’s “workplace MW.” We write down a partial-equilibrium model and show that the workplace MW increases rents, whereas the “residence MW” (i.e., the statutory one) has a negative effect on rents. 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 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 statutory MW increases, landlords pocket between 5 and 9 cents on the extra dollar. In ZIP codes where the binding MW does not change, landlords pocket between 9 and 16 cents.

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<sup>†</sup>Gentile Passaro: Department of Economics, Brown University (email: [diego\\_gentile\\_passaro@brown.edu](mailto:diego_gentile_passaro@brown.edu)); Hermo: Department of Economics, Brown University (email: [santiago\\_hermo@brown.edu](mailto: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 (hereafter MW) policies are *place-based* in that they are tied to a location, and workers may work and live in locations under different MW levels, crucially impacting the effects of changes in these policies across space. While most research on the effects of the minimum wage has focused on employment and wages irrespective of location (e.g., Card and Krueger 1994; Autor, Manning, and Smith 2016; Cengiz et al. 2019), a full account of the welfare consequences of the MW requires an understanding of how it affects different markets and how its effects spill over across different neighborhoods (as recently emphasized by Dube and Lindner 2021).

In this paper, we study the short-run effect of MW policies across local rental housing markets. One approach to answer this question would be to relate the MW of a region with some measure of housing rents of people residing in that region. This approach, which is the one that was taken in the existing literature, appears sensible only if all low-wage workers live and work in the same place. However, if workers are subject to different wage floors in their workplace and residence locations, and residence and workplace minimum wages have different effects on housing demand, this approach will yield confounded results. For instance, if the MW increases in the city center, it will likely affect the rents of low-wage workers that commute there from other neighborhoods. Figure 1 shows that the share of workers residing and working on the same geographical unit is low, especially at the local level. As a result, we expect the effects of a local MW increase on rents—and thus on welfare of workers and homeowners—to be heterogeneous across space, both in places within the jurisdiction that passed the new legislation and in affecting other neighborhoods depending on prevailing commuting patterns.

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).<sup>1</sup> 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. This is challenging because it requires to define an appropriate measure for workplace MW, which in turn requires data on commuting patterns at the local level. Second, estimation at the local level requires spatially disaggregated 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) is positive.<sup>2</sup> In addition, as MW changes are unlikely to be set considering the dynamics of local rental markets, when using small geographic units the exogeneity assumptions required for identification appear more plausible.

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<sup>1</sup>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.

<sup>2</sup>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.

Third, even if using local variation in MW changes, the effects estimated over longer time horizons may conflate changes in migration, housing demand, and housing supply.

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 devise 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 exploit the monthly frequency of our data to focus on effects immediately around the month of MW changes. Finally, 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 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 they face at residence and workplace locations, respectively. This short-run model imposes fixed commuting patterns and fully flexible prices.<sup>3</sup> Motivated by the evidence of the effect of MW policies on income and prices (Allegretto and Reich 2018; Leung 2021), we assume that MW hikes in the workplace increase disposable income and MW hikes in the residence increase local prices.<sup>4</sup> The model illustrates that, under these conditions, 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 an increase in some workplace MW will cause rents to go up, whereas an increase in residence MW will (conditional on workplace MW) lower rents. We also show that, under the assumption that workers who work in different locations have a similar elasticity of housing demand with respect to 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*. 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 policies. Our main rent variable comes from Zillow, the largest online real estate platform in the 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),

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<sup>3</sup>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 2019, for a review). Relatedly, Pérez Pérez (2021) finds small elasticities of commuting to MW policies in a time horizon of several years.

<sup>4</sup>We also assume that housing is increasing in income and decreasing in local consumption prices.

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 official 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).<sup>5</sup> We use our MW data coupled with commuting origin-destination matrices obtained from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics US Census Bureau (LODES; 2020b) database to construct the workplace MW for each ZIP code, defined as a weighted average of the log statutory MW in each other ZIP code where its residents work, where the weights are the share of population that work in each one of them.

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 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.<sup>6</sup> 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 tease out the comparisons that pin down the coefficients of our model, and how they relate to the treatment effects of interest.

Our preferred specification implies that a 10 percent increase in the workplace MW only increases rents by 0.65 percent (SE=0.27). If the residence MW also increases, then rents would increase by 0.34 percent instead (SE=0.15). The reason is that the residence MW is estimated to have a negative partial effect on rents. 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 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 ...

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<sup>5</sup>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 **MissouriCenter**.

<sup>6</sup>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.

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-weighting 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.

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).<sup>7</sup> Similarly, several papers study the consequences of minimum wage policies on income inequality (Lee 1999; Autor, Manning, and Smith 2016). We contribute to this literature by focusing on a relatively less studied channel through which minimum wage policies may affect welfare: the housing market.

Second, this paper is similarly 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.

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<sup>7</sup>See Neumark and Wascher (2006) for an earlier review of this literature, and Dube (2019) and Neumark and Shirley (2021) for more recent reviews.

Second, we use data at a more detailed geography and higher frequency.<sup>8</sup> 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.<sup>9</sup> 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 elasticities 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.

## 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

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<sup>8</sup>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.

<sup>9</sup>Another related paper is Agarwal, Ambrose, and Diop (2019) who show that MW increases lower the probability of rental default.

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.<sup>10</sup> 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  $u_{iz} = u(h_{iz}, c_{iz}^{NT}, c_{iz}^T)$  subject to a budget constraint

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

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,  $\frac{dP_i}{dW_i} > 0$ , and (ii) income is weakly increasing in  $z$ 's MW, and strictly 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 in their same neighborhood, justifying the inclusion of  $c_{iz}^{NT}$  in the utility function.<sup>11</sup> 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).<sup>12</sup>

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

<sup>10</sup>To simplify we assume that all of  $i$ 's residents work, so that the number of residents equals the number of workers.

<sup>11</sup>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.

<sup>12</sup>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.

prices  $P_i$ .<sup>13</sup> 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

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

Given that housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.<sup>14</sup> 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 dimensionality 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  $\{L_{iz}\}_{z \in \mathcal{Z}(i)}$  and Assumption 1, we have that (1) for  $z \in \mathcal{Z}_0 \setminus \{i\}$  for which  $\frac{dY_z}{dW_z} > 0$ , 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  $i \in$

<sup>13</sup>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.

<sup>14</sup>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.



$\mathcal{Z}(i)$ . Dividing by 1 and each of the variables appropriately, one can show that

$$\left(\eta_i - \sum_z \pi_{iz} \xi_{iz}^r\right) d \ln R_i = \sum_z \pi_{iz} (\xi_{iz}^p \epsilon_i^p d \ln \underline{W}_i + \xi_{iz}^y \epsilon_z^y d \ln \underline{W}_z), \quad (2)$$

where  $\pi_{iz} = \frac{L_{iz}}{L_i}$  represents the share of  $i$ 's residents working in  $z$ ;  $\xi_{iz}^x = \frac{dh_{iz}}{dx_i} \frac{x_i}{\sum_z \pi_{iz} h_{iz}}$  for  $x \in \{R, P, Y\}$  is the elasticity of housing demand at the average demand of ZIP code  $i$ ;  $\epsilon_i^p = \frac{dP_i}{dW_i} \frac{W_i}{P_i}$  and  $\epsilon_z^y = \frac{dY_z}{dW_z} \frac{W_z}{Y_z}$  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

$$\left(\eta_i - \sum_z \pi_{iz} \xi_{iz}^r\right)^{-1} \pi_{iz} \xi_{iz}^y \epsilon_z^y d \ln \underline{W}_z.$$

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

For ZIP code  $i$  the partial effect is given by

$$\left(\eta_i - \sum_z \pi_{iz} \xi_{iz}^r\right)^{-1} \left(\epsilon_i^p \sum_z \pi_{iz} \xi_{iz}^p + \pi_{ii} \xi_{ii}^y \epsilon_i^y\right) d \ln \underline{W}_i.$$

Because  $\epsilon_i^p > 0$ ,  $\xi_{iz}^p < 0 \forall z \in \mathcal{Z}(i)$ , and  $\epsilon_i^y \geq 0$ , 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  $\frac{dY_z}{dW_z} > 0$ ) 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

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

where  $r_i = \ln R_i$  represents 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  $\beta_i = \frac{\sum_{z \in \mathcal{Z}(i)} \pi_{iz} \xi_{iz}^p \epsilon_i^p}{\eta_i - \sum_z \pi_{iz} \xi_{iz}^r} > 0$  and  $\gamma_i = \frac{\sum_z \xi_i^y \epsilon^y}{\eta_i - \sum_z \pi_{iz} \xi_{iz}^r} < 0$  are parameters.  $\square$

Proposition 2 shows that, under an homogeneity assumption,<sup>15</sup> 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 explain where we obtained our data and the steps we take to put them together in a USPS ZIP code by month panel. First, we describe the sources and construction of our residence and workplace MW measures. We describe in detail the trends, timing, and geographic patterns of the statutory MW changes that give raise to rich variation in our MW variables. Second, we describe the Zillow data on rents. We explore how the sample of ZIP codes available in Zillow compares to the US sample of ZIP codes. Third, we detail sources of other data. Finally, we guide the reader through the construction of the baseline sample we use in estimation.

### 3.1 Rents Data from Zillow

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

Zillow is the leader online real estate and rental platform in the US, hosting more than 110 million homes and 170 million unique monthly users in 2019 (Zillow 2020a). Zillow provides the median rental and sale price among homes listed on the platform for different house types and at geographic and time aggregation levels (Zillow 2020b).<sup>16</sup> We collect the USPS ZIP code level monthly time series. The timespan of the data varies at the ZIP code level, and units with a small number of listings are omitted.<sup>17</sup> As explained below, we construct a balanced panel to address the changing composition of the sample.

We focus our primary analysis on a single housing category: *single-family* houses, *condominium*, and *cooperative* units (SFCC). This is 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 (Fernald 2020). To account for systematic differences in house size across locations we focus on *per square foot* rents. Our main

<sup>15</sup>We acknowledge that this simplifying 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.

<sup>16</sup>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, and Archive (2021) for a snapshot of the website as of February 2020.

<sup>17</sup>Two related notes: (i) once a ZIP code enters our panel, it remains until the final month of our data (December 2019); (ii) the threshold used by Zillow to censor the data is not made public.

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. Appendix Table XXX shows that our results are similar when we use other rental variables.

Zillow data has several limitations. First, we do not observe the underlying number of units listed for rent in a given month. We do observe the number of houses listed *for sale*, which we use as a proxy for the number of rentals in robustness analysis. A second limitation is that Zillow’s market penetration dictates the sample of ZIP codes available. Appendix Figure D.1 shows that the sample of ZIP codes we observe in Zillow typically coincide with high population-density areas. We conduct several exercises to account for this issue.

To ensure that our data correctly captures the price evolution of the US rental market, we compare Zillow’s median rental price in the SFCC category with three SAFMRs series for houses with a different number of bedrooms (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.<sup>18</sup> Appendix Figure D.2 shows that these series evolve very similarly over time.

## 3.2 Minimum Wage

### The Statutory Minimum Wage

We collect data on federal-, state-, county-, and city-level statutory MW levels from Vaghul and Zipperer (2016). We supplement their data, available up to 2016, with data from UC Berkeley Labor Center (2020) for the years 2016–2019. We assign MW levels to USPS ZIP codes by taking the following steps. First, we assign a single census-based ZIP Code Tabulation Area (ZCTA) to each USPS ZIP code using a correspondence from UDS Mapper (2020). Then, based on the ZCTAs we assign to each USPS ZIP code all possible place-county combinations using data from the Missouri Census Data Center (2018), and we keep only the place-county combination with the highest number of housing units in the 2010 census. Then, based on place and county codes we assign a unique state and local MW level to each USPS ZIP code. We define *the statutory MW* at ZIP code  $i$  and month  $t$ ,  $W_{it}$ , as the maximum between the federal, state, county, and city statutory MWs.<sup>19</sup> As a result, we only use MW changes that are binding, meaning that they actually modify that maximum.

When restricting to the sample of ZIP codes available in Zillow, and to our sample period, our data reports 18,689 statutory MW changes at the ZIP code-month level. These, in turn, arise from 151 state-level and 182 county and city-level changes. Figure 2 shows the distribution of positive increases in our statutory MW variable among all ZIP codes available in the Zillow data.<sup>20</sup> Panel (a) shows the distribution of the intensity of our residence MW changes. The average percent change among Zillow ZIP codes is 5.5%. Our estimation strategy exploits the intensity of MW changes. On

<sup>18</sup>For more information on how SAFMRs are calculated, see US HUD (2017, page 41641).

<sup>19</sup>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.

<sup>20</sup>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.

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, where our rents data is more abundant. Statutory MW changes have also been concentrated geographically. Appendix Figure D.3 shows the percentage change in the statutory MW levels from January 2010 to December 2019. There exist many areas across and within state borders that have differential MW changes, which will be central to distinguish the effect of the two MW-based measures proposed in Section 2. We describe these measures in the next subsection.

### The Residence and Workplace Minimum Wage Measures

In this subsection we define the minimum wage variables we use in our analysis. Both follow the definitions in Proposition 2. With our MW panel at hand, computing the residence MW is straightforward. We define it as

$$\underline{w}_{it}^{\text{res}} = \ln \underline{W}_{it} .$$

We also construct the workplace MW, which captures the spillover effects of statutory MW policies in other locations. To construct this measure we need to know, for each ZIP code, where workers residing in that location work. We obtain this information from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES; US Census Bureau 2020b) for the years 2009 through 2018. The data come at the block-group level. We aggregate it to construct a ZIP code origin-destination matrix where we observe, for each location, the number of residents working in every other location.

We then use this ZIP code residence-workplace matrix to build exposure weights. Let  $\mathcal{Z}(i)$  be the set of ZIP codes in which  $i$ 's residents work (including  $i$ ). We construct the set of weights  $\{\omega_{iz}\}_{z \in \mathcal{Z}(i)}$  as

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

where  $N_{iz}$  is the number of workers who reside in  $i$  and work in  $z$ , and  $N_i$  is the total working-age population of  $i$ . Appendix table XXX shows how our results change when we use young workers and low-income workers as weights.<sup>21</sup> We define the workplace minimum wage measure as

$$\underline{w}_{it}^{\text{wkp}} = \sum_{z \in \mathcal{Z}(i)} \omega_{iz} \ln \underline{W}_{zt} . \quad (4)$$

Figure 3

### 3.3 Other Data Sources

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

<sup>21</sup>The LODES data additionally reports origin-destination matrices for the number of workers 29 years old and younger, and the number of workers earning less than \$1,251 per month. The resulting workplace MW measures with any set of weights are highly correlated among each other ( $\rho > 0.99$  for every pair).

division and from 2010 to 2019.<sup>22</sup> For each county-quarter-industry, 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 use this data for descriptive purposes and as controls for the state of the local economy in our regression models.

We collect socio-demographic information from the 5-years 2007-2011 American Community Survey (ACS) ?? at the ZCTA level. We assign it to our USPS ZIP code monthly panel using the crosswalk from using again the crosswalk from ??. We collect data on the following characteristics: population, number of housing units, median income, African-American population, number of unemployed, and number of college students. We use this information to display descriptive statistics of our ZIP codes.

We collect Individual Income Tax Statistics at the USPS ZIP code level from the IRS ?? from 2010 to 2019. For each ZIP code year we observe the number of households, population, adjusted gross income, total wage bill, total business income, number of households that receive a wage, number of households that have business income, number of households with farm income. We use this data to describe our ZIP codes but also in heterogeneity and counterfactual exercises.

We collect rental prices at the ZIP code and year level from the US Housing and Urban Development (HUD). The data comes from the Small Area Fair Market Rents (SAFMR) ?? reports that are a crucial input for designing housing voucher programs. We gather data from 2012 to 2016 and we use it for comparison with the Zillow data and for describing the ZIP codes in our sample.

### 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.<sup>23</sup> 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 Figure ?? 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.

Table ?? 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 set of urban ZIP codes, respectively. In column 3 we show the set of ZIP codes in the Zillow data that have rental per square feet data in the SFCC category. We refer to this as the Zillow

<sup>22</sup>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”.

<sup>23</sup>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.

sample. Finally, column 4 shows descriptive statistics 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 exercise.

Finally, Table ?? 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 Section ?. 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 Table ?? 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 and Identification

In this section, we present the empirical strategy adopted to study the effect of the MW on rents and we discuss the assumptions required for identification.

### 4.1 First-differenced model

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

$$r_{it} = \alpha_i + \hat{\delta}_t + \gamma \underline{w}_{it}^{\text{res}} + \beta \underline{w}_{it}^{\text{wkp}} + \mathbf{X}_{c(i)t}' \eta + \varepsilon_{it} .$$

In this equation,  $r_{it}$  represents the log of rents per square foot, in ZIP code  $i$  at month  $t$ ,  $\underline{w}_{it}^{\text{res}}$  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  $\sum_{z \in \mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_{zt}$ ,  $\alpha_i$  and  $\hat{\delta}_t$  are ZIP code and month fixed effects, respectively, and  $\mathbf{X}_{c(i)t}$  is a vector of county-level time-varying controls. Time runs from  $\underline{T}$  = February 2010 to  $\bar{T}$  = December 2019. The parameters of interest are  $\gamma$  and  $\beta$ , which we interpret as the elasticity of rents to the residence and workplace MW, respectively.

By taking first differences on 4.1 we obtain the following model In first differences:

$$\Delta r_{it} = \delta_t + \gamma \Delta \underline{w}_{it}^{\text{res}} + \beta \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{c(i)t}' \eta + \Delta \varepsilon_{it} . \quad (5)$$

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.

## 4.2 Identification

Estimates of the parameters  $\gamma$  and  $\beta$  can be interpreted causally under the assumption of *strict exogeneity*. That is, we require the unobserved shocks to rents to be uncorrelated with past and present values of minimum wage changes (Wooldridge 2010, chapter 10). Formally, we assume that the set of conditions

$$E[\Delta \varepsilon_{ict} \Delta \ln \underline{w}_{ict} | \alpha_i, \delta_t, \Delta \mathbf{X}_{ct}] = 0 \quad \forall \tau \in \{\underline{T}, \overline{T}\}$$

holds for all periods  $t$ . This assumption has two important implications. First of all, it implies no pre-trends in rents leading up to a minimum wage change (conditional on controls). We will test this implication more formally by including leads of the minimum wage. Secondly, it rules out feedback effects from current values of rents on our controls, i.e., MW changes are assumed not to be influenced by past values of rents. While we think this is a reasonable assumption—minimum wages are usually not set by considering their effects on the housing market—, we allow this type of feedback effects in the panel specifications described in the upcoming subsection. Importantly, our identifying assumption allows for arbitrary correlation between ZIP code effects  $\alpha_i$  and minimum wages —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. We deploy two strategies to account for that. First of all, we include economic controls from the QCEW. These data are aggregated at the county level, and represent a second best given the unavailability of controls at the ZIP code level. Specifically, we use average weekly wages, employment and establishment count for the sectors “Professional and business services,” “Information,” and “Financial activities.” We assume that these sectors are not affected by the minimum wage. 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.<sup>24</sup> We believe that these controls plausibly capture variation arising from unobserved trends in local markets. Our second strategy to deal with unobserved heterogeneity is to control for ZIP code-specific linear and quadratic trends. These models, presented in the Appendix, should account for time-varying heterogeneity not captured by our economic controls that follows this pattern.

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<sup>24</sup>In Section C we show suggestive evidence that they are not affected by the MW by using them as dependent variable in our models.

We can test the identification assumption using models that include leads and lags of the MW variables.

$$\Delta \ln y_{ict} = \delta_t + \sum_{r=-s}^s \beta_r \Delta \ln \underline{w}_{ic,t+r} + \Delta \mathbf{X}_{ct}' \eta + \Delta \varepsilon_{ict} , \quad (6)$$

where  $s$  is the number of months of a symmetric window around the MW change. Our baseline specification sets  $s = 5$ . We show in the appendix that our results are very similar for  $s \in \{3, 6, 9\}$ . Results hold for larger values of  $s$  as well; however, increasing  $s$  implies dropping observations at the start and end of the panel, which decreases our sample size and makes the results less reliable.

Importantly, this model allows us to test whether  $\beta_{-s} = \beta_{-s+1} = \dots = \beta_{-1} = 0$ . Under the assumption that there is no anticipatory effects in the housing market, we interpret the absence of pre-trends as evidence against the presence of unobserved economic shocks driving our results.<sup>25</sup> 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. We further present evidence in favor of this assumption by showing that MW changes do not predict the number of listings of houses for sale in Zillow.<sup>26</sup> Specifically, we can track the number of houses listed for sale in selected ZIP codes during the period 2013-2019 for our preferred house type (SFCC). We use such series to run placebo regressions where we estimate both the static and dynamic models using the change in (log) listings as dependent variable.

Appendix Table XX shows that we obtain similar results under a weaker *sequential exogeneity* assumption (Arellano and Honoré 2001).

### 4.3 Estimation concerns

Appendix B shows that under parallel trends assumptions the model in equation 5 identifies parameters  $\gamma$  and  $\beta$ .

Inspired by this approach, we use a stacked sample in Appendix Table XX.

## 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.

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<sup>25</sup>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 does is consistent with both.

<sup>26</sup>Ideally, we would run this regression on the number of rental units. Unfortunately, as described in Section 3, this information is not available.



## 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 commuting 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 years 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.

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## Tables and Figures

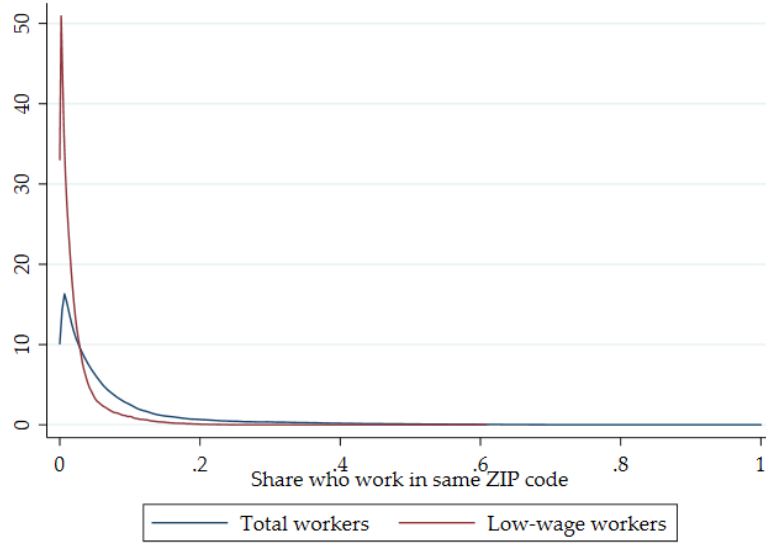
Table 1: Static model

	Change wrk. MW	Change log rents		
	(1)	(2)	(3)	(4)
Change residence minimum wage	0.8683 (0.0298)	0.0257 (0.0137)		-0.0302 (0.0169)
Change workplace minimum wage			0.0321 (0.0150)	0.0645 (0.0274)
Sum of coefficients				0.0342 (0.0151)
County-quarter economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0332
R-squared	0.9449	0.0209	0.0209	0.0209
Observations	131,196	131,196	131,196	131,196

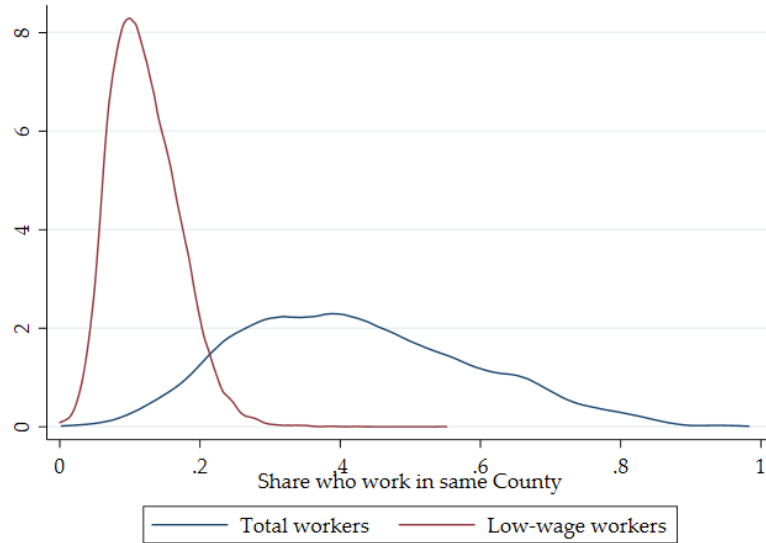
Notes:

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

(a) ZIP code



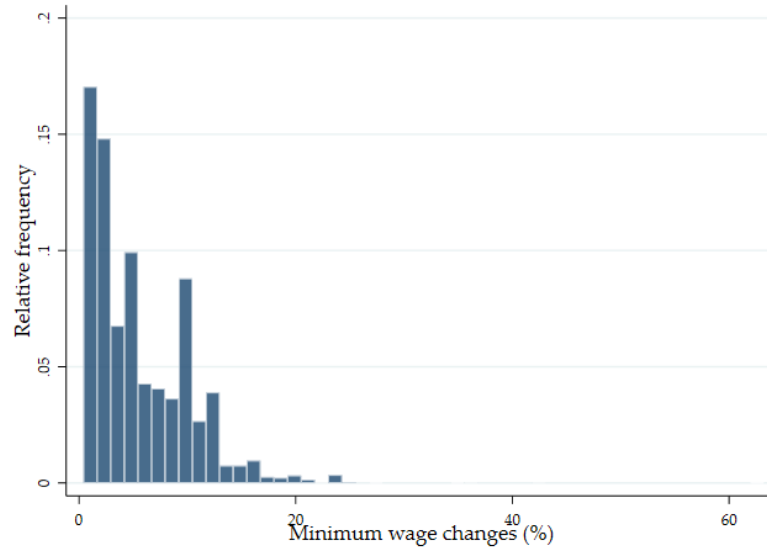
(b) County



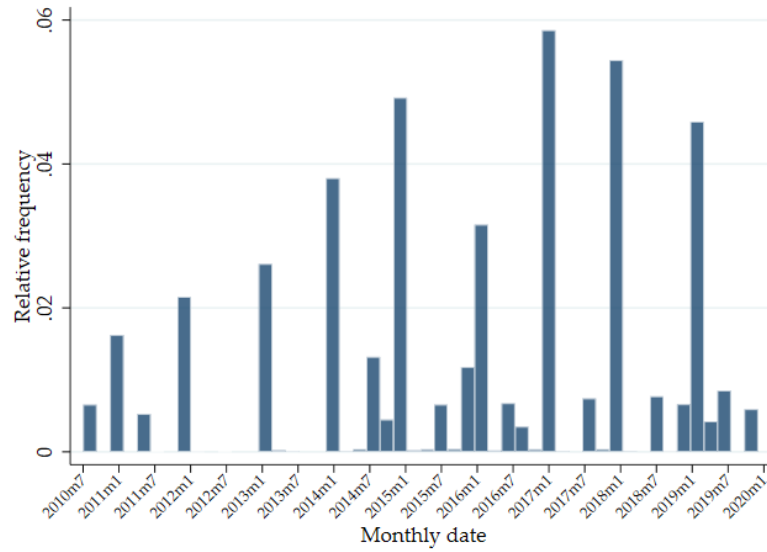
Notes: Data are from LODES origin-destination matrices (US Census Bureau 2020b), aggregated at the ZIP code and county levels. The figure shows the estimated distribution of the share of workers who reside and work in the same geographical unit, at the ZIP code (panel a) and County (panel b) levels.

Figure 2: Distribution of Minimum Wage Changes

(a) Intensity



(b) Timing



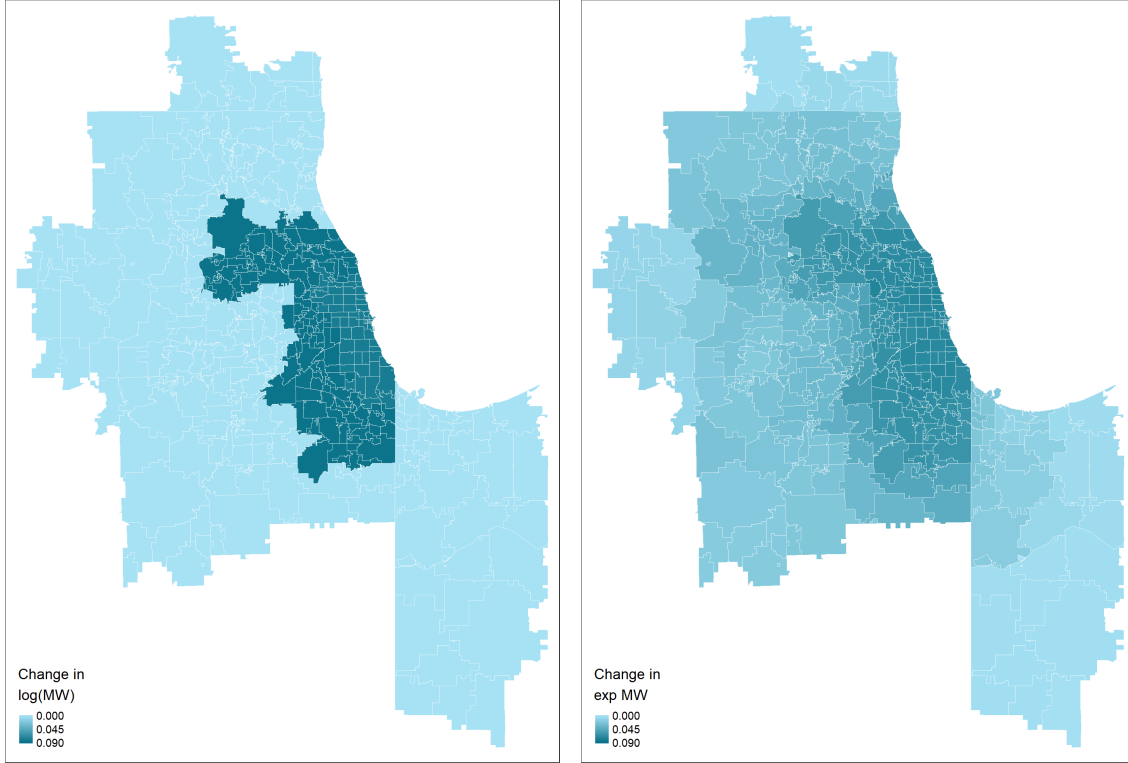
Notes: The histograms show the distribution of positive MW changes in the full sample of ZIP codes available in the Zillow data. Panel (a) reports the intensity of the changes in percentage terms. Panel (b) plots the distribution of such changes over time.



Figure 3: Spatial Distribution of Minimum Wage Changes

(a) Residence MW

(b) Workplace MW



Notes: Data are from the MW panel defined in Section and from LODES (US Census Bureau 2020b). The figure shows the change in the residence MW (panel a) and workplace MW (panel b) on July 2019 in the metropolitan area of Chicago. The residence MW is defined as the log of the statutory MW of the given ZIP code. The workplace MW is defined as the weighted average of the log of the statutory MW levels in workplace locations of a ZIP code's residents, where weights are given by commuting shares.

## 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  $H_{it} \leq D_i$  for all  $t$ , where  $D_i$  denotes the total number of available square feet in  $i$ .

We further decompose  $H_{it}$  as follows. Let  $h_{izt} = h_{iz}(R_{it}, \underline{W}_{it}, \underline{W}_{zt})$  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  $\lambda_{it}$  denote the share of  $i$ 's residents who started their contracts in period  $t$ .<sup>27</sup> 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  $\tau$ . 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  $\lambda_{it}$  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

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

---

<sup>27</sup>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 feasibility constraint is that

$$V_{it}(R_{it}, \lambda_t) \leq D_i - \tilde{H}_{i,t-1}. \quad (7)$$

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

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

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  $t$  clears if

$$\lambda_t \sum_z L_{iz} h_{izt}(R_{it}, \underline{W}_{it}, \underline{W}_{zt}) = V_{it}(R_{it}, \lambda_t). \quad (8)$$

Given minimum wages in  $t$ ,  $\{\underline{W}_{it}\}_{i \in \mathcal{Z}}$ , the share of workers looking to rent in period  $t$ ,  $\lambda_t$ , and a number of vacancies that satisfies (7), equation (8) 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 (8) 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:

$$\Delta r_{it} = \gamma \Delta \underline{w}_{it} + \beta \sum_{z \in \mathcal{Z}} \pi_{iz} \Delta \underline{w}_{zt} + \delta_t + \Delta \varepsilon_{it} \quad (9)$$

where  $\delta_t$  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  $\beta$  and  $\gamma$  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  $\beta$  and  $\gamma$  in the structural model 9 are identified.*

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

$$E[\Delta r_{it} | i \notin \mathcal{Z}_0] = \begin{cases} \delta_t + E[\Delta \varepsilon_{it} | z \notin \mathcal{Z}_0] & \text{if } t \neq \bar{t} \\ \beta \sum_{z \in \mathcal{Z}_0} \pi_{iz} \underline{w}_{zt} + \delta_t + E[\Delta \varepsilon_{it} | z \notin \mathcal{Z}_0] & \text{if } t = \bar{t}, \end{cases} \quad (10)$$

Now, rank these ZIP codes according to their exposure to the policy  $\Pi_i = \sum_{z \in \mathcal{Z}_0} \pi_{iz}$ . 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 10 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  $t = \bar{t}$ ,

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

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

$$\beta = \frac{E[\Delta r_{it}|i \in \mathcal{Z}_h] - E[\Delta r_{it}|i \in \mathcal{Z}_l]}{(E[\sum_{z \in \mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_h] - E[\sum_{z \in \mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_l]) \Delta \underline{w}}. \quad (11)$$

Assumption (1) guarantees that  $(E[\sum_{z \in \mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_h] - E[\sum_{z \in \mathcal{Z}_0} \pi_{iz}|i \in \mathcal{Z}_l]) \neq 0$ , thus  $\beta$  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 10 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

$$\gamma = \frac{E[\Delta r_{it}|i \in \mathcal{Z}_0] - E[\Delta r_{it}|i \notin \mathcal{Z}_0]}{\Delta \underline{w}} - \beta \left( E \left[ \sum_{z \in \mathcal{Z}_0} \pi_{iz} \middle| i \in \mathcal{Z}_0 \right] - E \left[ \sum_{z \in \mathcal{Z}_0} \pi_{iz} \middle| i \notin \mathcal{Z}_0 \right] \right). \quad (12)$$

Since  $\beta$  is known per equation 11,  $\gamma$  is identified as well.  $\square$

## C The Rationale for Selecting Economic Controls

Throughout the paper we use wages, employment, and establishment-count from the QCEW to control for the local business cycle. However, those variables may themselves be impacted directly by changes in the MW. As a result, incorporating those controls raises the concern of a “bad control” problem (Angrist and Pischke 2009). In the scenario that the MW affects the controls used in the regression, this approach will not unveil the average treatment effect of the MW on rents even if the policy is known to be *randomly assigned*. The reason is that conditioning on these variables opens a potential alternative channel through which the MW affects rents, introducing bias in our estimates.

To avoid the “bad controls” problem, while at the same time include control variables that proxy for the local economic conditions, we select QCEW county-level time-series for the sectors that we think are unlikely to be affected by MW legislation: “Professional and Business Services”, “Information”, and “Finance”. Our interpretation of the effects of the MW on rents as causal relies on the assumption that these controls are not influenced by the MW. In this appendix we provide evidence in favor of this assumption.

We start by noting that these sectors employ a rather small portion of MW labor. 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. These low percentages make direct impacts unlikely. However, these sectors may be influenced by MW legislation indirectly. We test this possibility by using a version of the dynamic model to analyze whether MW affects either wages, employment, or establishment-count in these sectors. The presence of significant pre-treatment trends would suggest that these sectors react to MW, and cast doubt on the identification strategy discussed in the paper.

For each industry, we observe employment at the county and month levels, and average weekly wages and establishment-count at the county and quarter levels. As a result, estimation of the dynamic model defined at the zipcode and month level is not straightforward. To be able to estimate our model, we aggregate MW zipcode-month information at the county-month level by taking, for each period, the weighted average of MW levels in each zipcode associated with a given county, using the number of housing units as weights. We call this our MW variable  $\underline{w}_{ct}$ , where  $(c, t)$  is a county and monthly date cell.<sup>28</sup>

Given that we observe employment with monthly frequency, we are able to estimate the following model:

$$\Delta \ln y_{ct} = \delta_t + \sum_{r=-s}^s \beta_r \Delta \ln \underline{w}_{c,t+r} + \Delta \nu_{ct}, \quad (13)$$

where  $y_{ct}$  is the outcome variable (employment) in county  $c$  and month  $t$ , and  $s$  defines the number of leads and lags in the model.

Because the QCEW provides average weekly wages and establishment-count at the county-quarter level, we estimate the model for these variables in quarterly averages of monthly obser-

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<sup>28</sup>For counties without city level ordinances this procedure would simply reflect the state- or county-level MW. For places with a MW at the city level our MW variable corresponds to a weighted average between places affected by the city MW and places not affected by it.

uations. We compute quarterly average of QCEW measures as  $\overline{\Delta y_{cq}} = \frac{1}{3}\Delta y_{cq}$ . As for MW data, we define  $\underline{w}_{cq}$  as the third month in each quarter  $q$ . We are then able to compute the quarterly average for MW changes,  $\overline{\Delta \ln \underline{w}_{cq}} = \frac{1}{3}\Delta \ln \underline{w}_{cq}$ . The estimating model then becomes

$$\overline{\Delta \ln y_{cq}} = \bar{\delta}_q + \sum_{r=\tau}^{\tau} \rho_r \overline{\Delta \ln \underline{w}_{c,q+r}} + \overline{\Delta \nu_{ct}}, \quad (14)$$

where  $\tau$  defines the quarterly window. We interpret the  $\rho_r$  coefficients in Equation 14 as averages of the monthly coefficients  $\beta_r$  in Equation 13. The reason is that the average change over a quarter is a linear combination of monthly changes.<sup>29</sup>

?? shows the results of our estimation for the three industries selected as controls. Even though some of the coefficients are significant, we interpret the results as suggestive of a noisy zero effect. The most worrisome sector is “Professional and Business Services”, which shows a significant same quarter coefficient of average weekly wages, and a slight negative pre-trend in employment. While we decided to keep this sector as control in our main models, we stress that our results are virtually identical when dropping it.

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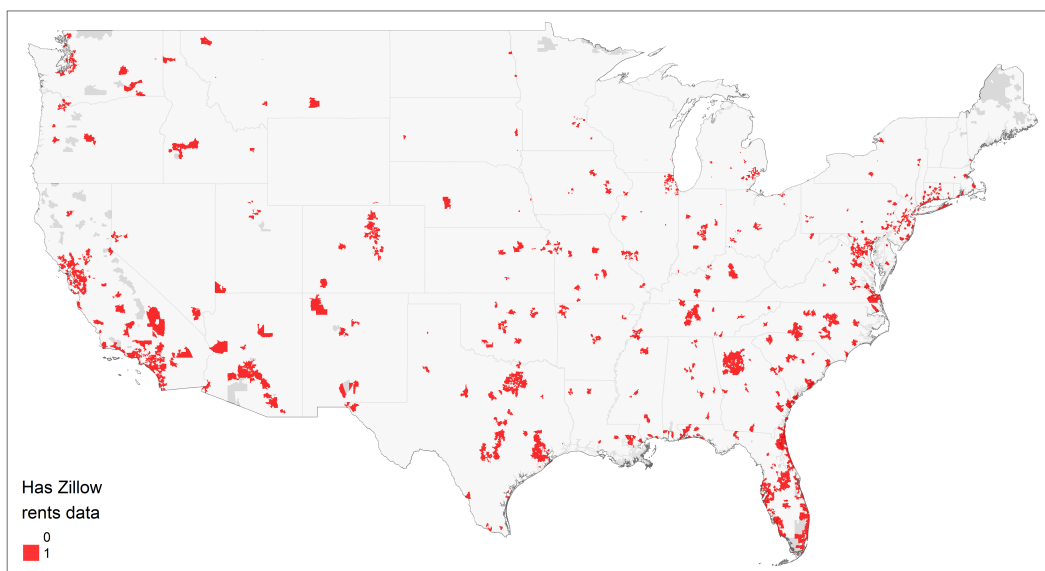
<sup>29</sup>To see this, let's define  $q_1, q_2, q_3$  as the first, second, and third months in a quarter  $q$ , respectively. Then, for any variable  $x$ ,

$$\begin{aligned} \frac{1}{3}\Delta x_{cq} &= \frac{1}{3}(x_{cq} - x_{c,q-1}) = \frac{1}{3}(x_{cq_3} - x_{c,q_3-1}) \\ &= \frac{1}{3}(x_{cq_3} - x_{cq_2} + x_{cq_2} - x_{cq_1} + x_{cq_1} - x_{c,q_3-1}) \\ &= \frac{1}{3}(\Delta x_{cq_3} + \Delta x_{cq_2} + \Delta x_{cq_1}). \end{aligned}$$

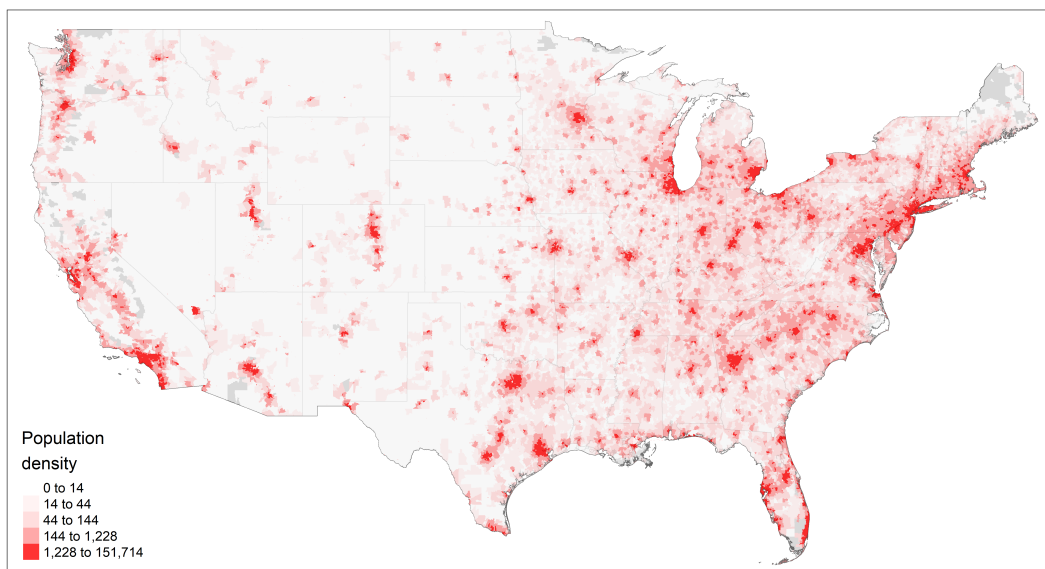
## D Additional Tables and Figures

Figure D.1: Sample of ZIP codes in Zillow data vs. population density

(a) Zillow ZIP codes



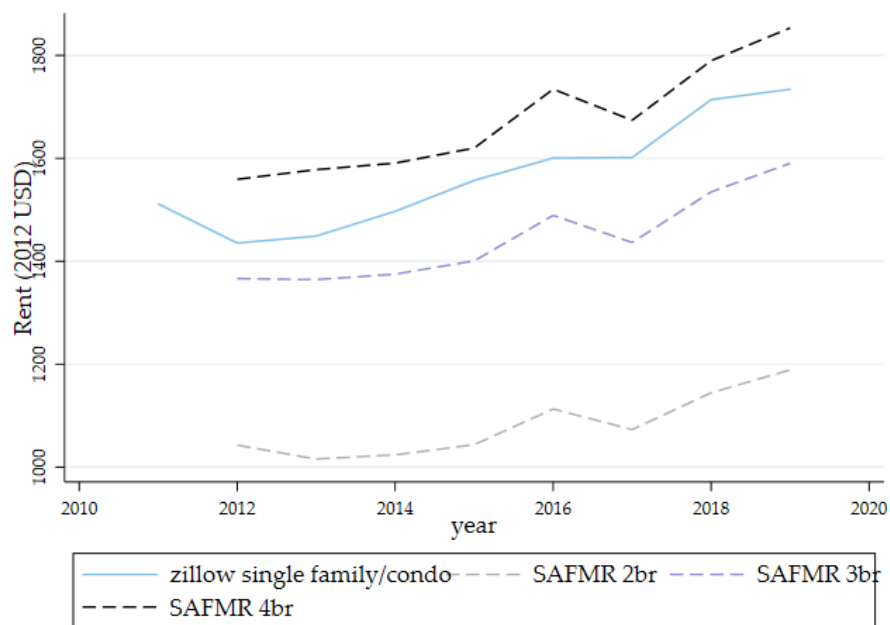
(b) Population Density



Notes: Data are from Zillow (2020b) and CENSO (YYYY). The figure compares the USPS ZIP codes available in Zillow to the population density. Panel (a) shows the sample of the ZIP codes that have rents data in the SFCC category at any point in the period 2010–2019. Panel (b) shows quintiles of population density, measured in people per square kilometer.

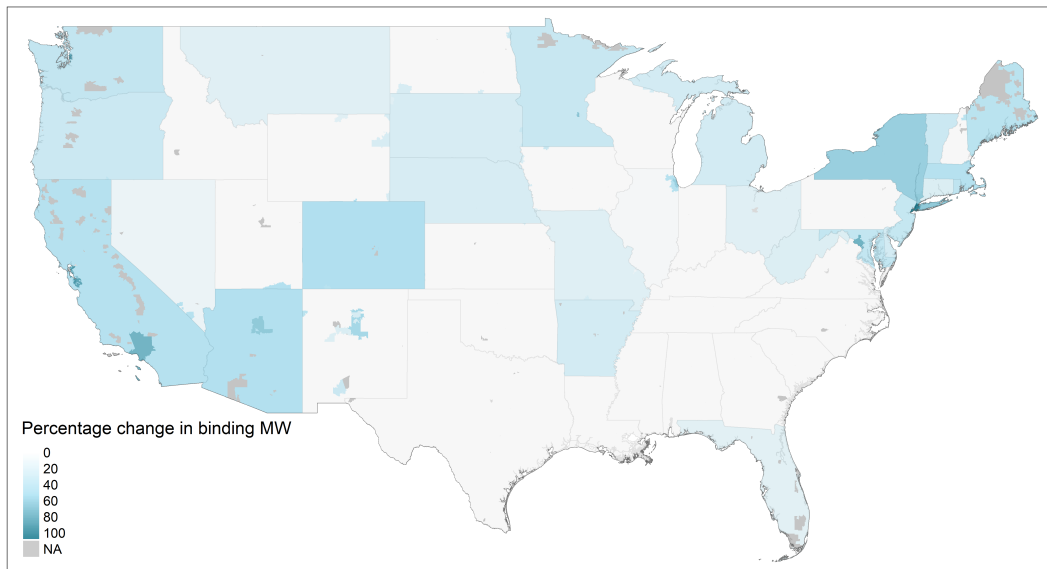


Figure D.2: Time trends in rents according to Zillow and SAFMR



Notes: Data are from Zillow (2020b) and Small Area Fair Market Rents (2020). The figure compares the evolution of the median rental value in Zillow to three SAFMRs series, for 2, 3, and 4 or more bedrooms.

Figure D.3: Spatial Distribution of Minimum Wage Changes



Notes: The figure maps the percentage change in the statutory minimum wage level in each USPS ZIP code from January 2010 to December 2019.