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

Diego Gentile Passaro Santiago Hermo Gabriele Borg †

December 16, 2021

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. 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.

*We are thankful to Jesse M. Shapiro, John N. Friedman, Matthew Turner, Kenneth Chay, Neil Thakral, Matthew Pecenco, 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 (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 worke 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 neighbordhoods 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).[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. 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 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) is positive.[2] 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.

[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] 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 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 (2020). 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, where workers who reside there can commute to work into ZIP codes elsewhere within the metropolitan area, potentially facing a different MW level. In the model workers demand square feet of housing as a function of local prices and income, which in turn depend on the MWs they face at residence and workplace locations, respectively. This short-run model imposes fixed commuting patterns and fully flexible prices.[3] 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.[4] 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),

[3]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.

[4]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 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 US Census Bureau (LODES; 2020) 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 the US Census Bureau US Census Bureau (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.[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 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 XX percent (SE=XX). If the residence MW also increases, then rents would increase by XX percent instead (SE=XX). 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 ...

[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.

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 earnings 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. Due to data constraints, we assume a range of values for the share of housing expenditure at the ZIP code level following the literature (CITE, DIAMOND?). We focus on studying the consequences of a counterfactual increase in the federal MW from \$7.25 to \$9. We find large variations in the effects of this policy on rents 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, as the pass through of MW to prices (other than rents) is likely to be small.

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).[7] Similarly, several papers study the consequences of minimum wage policies on the distribution of income and 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.

This paper is similarly related to the literature estimating 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 when compared

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

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.[8] Both of these facts enrich our understanding of the estimated effects and make the required identification assumption more plausible. Our paper also relates to Hughes (2020) who uses a triple difference-in-differences design 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. [9] 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 these insight, we show in our counterfactual MW increase that landlords may benefit from this policy eroding some of the extra disposable income of low wage workers.

Finally, our paper relates to the literature on the econometric issues of 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 simple and intuitive functional form assumptions 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 5 we present our main 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. We show how, under reasonable assumptions, the short-term effects of MW changes in log rents can be expressed as a function of the changes in two MW-based measures that take into account residence and workplace locations. The model is informative in itself but it will also guide our empirical strategy.

The model is purposefully stylized. Because we study the consequences of MW changes in the

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

[9]Another related paper is Agarwal, Ambrose, and Diop (2019) who shows that MWs decrease the probability of rental default.

very-short run, our model is static. We discuss the addition of the time dimension in Appendix A. We also assume an exogenous distribution of workers across residence and workplace locations. 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 $\mathcal{Z}$ composed of a finite number of ZIP codes. 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.[10] Each ZIP code has a binding minimum wage, which we denote by $\{\underline{w}_z\}_{z \in \mathcal{Z}(i)}$.

Housing demand In this simple static model all workers have to rent a house in a common market, where the rental rate is r_i. We assume that group (i, z)'s demand of square feet per person is given by $h_{iz}(r_i, \underline{w}_i, \underline{w}_z)$, where the second argument corresponds to the *residence* MW, and the third to the *workplace* MW. We characterize the properties of this set of functions below.

Assumption 1 (Housing demand)**.** *For all residence-workplace pairs, the housing demand function $h_{iz}(r_i, \underline{w}_i, \underline{w}_z)$ is: (i) continuously differentiable in its three arguments; (ii) decreasing in rental prices r_i; (iii) non-increasing in residence minimum wage $\underline{w}_i$; (iv) non-decreasing in workplace minimum wage $\underline{w}_z$. Furthermore, for at least one $z \in \mathcal{Z}(i)$, the inequalities in points (iii) and (iv) are strict.*

Points (i) and (ii) simply say that h_{iz} is a "smooth" demand function. Point (iv) follows from the fact that housing is a normal good. Given that, under negligible employment effects, workplace MW increases income, it should also increase housing demand for workers with earnings close to the MW, unless that there is no MW workers in a ZIP code. Point (iii) is a bit more subtle. Residence MW, while increasing the income of people working and residing in the same ZIP code, will also increase the cost of production of local non-tradable goods (assuming that workers are an input in non-tradable production). The higher cost of non-tradables will translate into a lower demand of housing if the substitution effect of a change in the price of non-tradables on local demand of housing is smaller than the corresponding income effect. A sufficient condition for that is that housing and local consumption are complements.[11] Another possibility is to introduce firms that produce non-

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

[11] We can formalize this discussion with a simple choice problem. Say a representative (i, z) worker chooses between housing demand h_{iz}, non-tradable consumption c_{iz}^{NT}, and tradable consumption c_{iz}^{T}, by maximizing

$$u_{iz} = u\left(h_{iz}, c_{iz}^{\mathrm{NT}}, c_{iz}^{\mathrm{T}}\right)$$

subject to $r_i h_{iz} + p_i(\underline{w}_i) c_{iz}^{\mathrm{NT}} + c_{iz}^{\mathrm{T}} \leq y_{iz}(\underline{w}_z)$, where $p_i(\underline{w}_i)$ gives the price of local consumption, which is increasing in residence MW; the price of tradable consumption is normalized to one; and $y_{iz}(\underline{w}_z)$ is an income function that depends tradable local goods, and that use MW workers as an input. Under perfect competition, after a MW increase, the firms will charge a higher price to hit the zero profit condition and not go out of business. Now the residents that don't work in that ZIP code will pay a higher price for their local good and they will have less disposable income for housing.

We think that the interpretation underlying point (iii) is plausible for several reasons. 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. Second, MWs have been shown to increase prices of local consumption (e.g., Allegretto and Reich 2018; Leung 2021). These empirical facts suggest that residence MW changes might (conditional on workplace) negatively affect incomes and thus demand for housing.

Housing supply We assume a simple supply side. Denote by $D_i(r_i)$ the supply of square feet in i, which is 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

$$\sum_{z \in \mathcal{Z}(i)} L_{iz} h_{iz}(r_i, \underline{w}_i, \underline{w}_z) = D_i(r_i). \tag{1}$$

Given that housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.[12] We denote equilibrium rents as $r_i^* = f(\{\underline{w}_i\}_{i \in \mathcal{Z}(i)})$.

Note that equilibrium rents are a function of the entire vector of minimum wages. 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?

Proposition 1 (Comparative Statics)**.** *Under the assumptions of (i) exogenously given distribution of workers across workplace and residence pairs, (ii) housing demand equation satisfying Assumption 1, and (iii) continuously differentiable and increasing housing supply, we have that workplace-MW hikes increase rents, and residence-MW hikes, holding constant workplace-MW hikes, decrease rents.*

positively on the workplace MW. Let h_{iz}^* and c_{iz}^* denote Marshallian demands, and $\tilde{h}_{iz}^*$ denote the Hicksian housing demand. By assumption, the price of the MW will increase prices of non-tradable consumption. Thus, consider the effect of an increase in p_i on housing demand. The Slutsky equation implies that

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

We have that $\frac{\partial h_{iz}^*}{\partial p_i} < 0$ if and only if $\frac{\partial \tilde{h}_{iz}^*}{\partial p_i} < \frac{\partial h_{iz}^*}{\partial y_{iz}} c_{iz}^*$.

[12] Assume $D_i(0) - \sum_{z \in \mathcal{Z}(i)} L_{iz} h_{iz}(0, \underline{w}_i, \underline{w}_z) < 0$ and apply the intermediate value theorem.

Proof. Fully differentiate the market clearing condition with respect to $\ln r_i$ and $\ln \underline{w}_i$ for all $i \in \mathcal{Z}(i)$ and re-arrange terms to get

$$\Big(\eta_i - \sum_z \pi_{iz}\xi_{iz}\Big) d\ln r_i = \sum_z \pi_{iz}\left(\epsilon^i_{iz} d\ln \underline{w}_i + \epsilon^z_{iz} d\ln \underline{w}_z\right), \tag{2}$$

where $\pi_{iz} = \frac{L_{iz}}{L_i}$ represents the share of i's residents working in z; $\xi_{iz} = \frac{dh_{iz}}{dr_i}\frac{r_i}{\sum_z \pi_{iz} h_{iz}}$ is the elasticity of housing demand at the average per-capita demand of ZIP code i; $\epsilon^i_{iz} = \frac{dh_{iz}}{d\underline{w}_i}\frac{\underline{w}_i}{\sum_z \pi_{iz} h_{iz}}$ and $\epsilon^z_{iz} = \frac{dh_{iz}}{d\underline{w}_z}\frac{\underline{w}_z}{\sum_z \pi_{iz} h_{iz}}$ are the elasticities of housing demand to workplace and residence MWs also at the average per-capita demand of ZIP code i; and $\eta_i = \frac{1}{L_i}\frac{dD_i}{dr_i}\frac{r_i}{D_i}$ is the elasticity of housing supply in ZIP code i.

Because $\xi_{iz} < 0$ and, for at least some workplace, $\epsilon^i_{iz} < 0$ and $\epsilon^z_{iz} > 0$, it is apparent from (2) that an increase in workplace MW unambiguously increases rents, whereas the effect of an increase in residence MW on rents is generally ambiguous (as long as some residents of i also work in i) as it is composed of a direct negative effect and an indirect positive effect through changing the experienced MW.[13] Holding constant workplace MWs, the effect of the residence MW is negative. □

Proposition 1 shows that, under conditions on the direction of the effect of MW changes and regularity conditions on the demand function, we can unequivocally establish the influence of the MW on rents. Crucially, increases in MW changes in a set of ZIP codes other than i will affect rents at i if some of i's residents work in some of those ZIP codes.

Proposition 2 (Representation)**.** *Under the assumption of constant elasticity of housing demand (across workplace locations) to workplace minimum wages, we can write the change in log rents as a function of the change in two MW-based measures: the* ***experienced log MW*** *and the* ***statutory log MW****.*

Proof. Under the assumption that $\epsilon^z_{iz} = \epsilon^z_i$ for all $z \in \mathcal{Z}(i)$ we can manipulate (2) to write

$$d\ln r_i = \beta_i \sum_i \pi_{iz} d\ln \underline{w}_z + \gamma_i d\ln \underline{w}_i \tag{3}$$

where $\beta_i = \frac{\epsilon^z_i}{\eta_i - \sum_z \pi_{iz}\xi_{iz}} > 0$ and $\gamma_i = \frac{\sum_z \pi_{iz}\epsilon^i_{iz}}{\eta_i - \sum_z \pi_{iz}\xi_{iz}} < 0$. □

Proposition 2 shows that, under an homogeneity assumption,[14] 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 workplace MW, and another one summarizing the effect of residence MW. This motivates our empirical strategy, where we regress log rents on the empirical counterparts of the measures.

[13] The sign of the overall partial effect depends on the sign of $\pi_{ii}\epsilon^z_{ii} + \sum_z \pi_{iz}\epsilon^i_{iz}$.

[14] 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.

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 in a given period. Time series are provided for different house types and at several geographic and time aggregation levels (Zillow 2020b).[15] 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.[16] 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.

[15] 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.

[16] 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.

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.[17] 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.[18] 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.[19] 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 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.[20] As a result, we only use MW changes that are binding, meaning that they actually

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

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

[19] 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.

[20] 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.

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. Figure 2 shows the distribution of positive increases in our statutory MW variable among all ZIP codes available in the Zillow data.[21] 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.

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.[22] 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.[23] 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

[21] 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.

[22] 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.

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

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 subsection 4.4 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.[24] 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 types of workers.[25] 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.[26] This panel contains 5,302 MW increases, which arise from 124 state changes and 99 county and local level

[24]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".

[25]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).

[26]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.

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.

Table 1 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 exercise.

Finally, Table 2 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 Table C.1 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. We begin with a *static* difference-in-differences specification that imposes no dynamics in the effects. To ease concerns of contemporaneous shocks systematically affecting both changes in rents and the MW within a ZIP code, we directly control for several county-level time-varying proxies of the health of the local labor market.

This static model has several shortcomings that motivate the inclusion of leads and lags of MW changes. The *dynamic* model both allows the effect to persist for more than one period and permits a test of the underlying parallel-trends assumption. One may also worry that the dependent variable presents auto-correlation, which would imply bias in our estimates. To account for this possibility we present a panel-specification that includes the lagged dependent variable as control following Arellano and Bond (1991) and related literature. We test the robustness of our results by adding ZIP code level linear and quadratic trends as alternative controls of local dynamics in the housing market.

Our specifications are distinct from classic event-study models used commonly in the literature (discussed in, e.g., Borusyak and Jaravel 2017; Abraham and Sun 2018). Rather, they share features with empirical work estimating the impact of high-frequency and changing-intensity events (see, e.g., Fuest, Peichl, and Siegloch 2018; Suárez Serrato and Zidar 2016). First, our models allow for units treated more than once. Secondly, our models allow for the inclusion of never-treated units, which aid in the identification of fixed effects and other parameters, diminishing concerns of under-identification (Borusyak and Jaravel 2017). Finally, our specifications identify the treatment effect of minimum wages on median rents exploiting not only the timing of a MW change but also its intensity.

Importantly, all regressions cluster standard errors at the state level so as to match the main source of variation of MW changes.

4.1 Static Model

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

$$\ln y_{ict} = \alpha_i + \hat{\delta}_t + \beta \ln \underline{w}_{ict} + \mathbf{X}_{ct}^{'}\eta + \varepsilon_{it} \ .$$

In this equation, y_{ict} represents our main outcome variable, rents per square foot for the Zillow SFCC series, in ZIP code i county c month t, $\underline{w}$ is the statutory minimum wage, α_i and $\hat{\delta}_t$ are ZIP code and time period (month) fixed effects, respectively, and $\mathbf{X}_{ct}$ is a vector of county-level time-varying controls. Time runs from $\underline{T}$ = February 2010 to $\overline{T}$ = December 2019. By taking first differences on this expression we obtain what we label as our *static* model, which is given by

$$\Delta \ln y_{ict} = \delta_t + \beta \Delta \ln \underline{w}_{ict} + \Delta \mathbf{X}_{ct}^{'}\eta + \Delta\varepsilon_{ict} \ , \tag{5}$$

where $\delta_t = \hat{\delta}_t - \hat{\delta}_{t-1}$, and β can be interpreted as the elasticity of rents to the MW. We spell out the model in first differences because it is reasonable to expect unobserved shocks to rental prices to be persistent over time. Both the first differences and the level models are consistent under similar assumptions, but the model in first differences is more efficient if shocks are serially correlated. In Appendix **??**, we test for AR(1) auto-correlation in the shocks following Wooldridge (2010, chapter 10) and we comfortably reject the no auto-correlation hypothesis.

Estimates of the parameter β 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

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

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 α_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.[27] 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.

4.2 Dynamic Models

One concern with the static model is that, despite controlling for economic factors or ZIP code-specific trends, preexisting time paths of rents per square foot in anticipation of MW changes may be different in treated and non-treated ZIP codes due to either residual demand shocks or anticipatory effects in the supply of rentals. In order to assess whether pre-trends are present, we extend our static model with leads and lags of our minimum wage variable. In addition, one may believe that the effect of MW changes on rents is not a one-time discrete level jump but that it has persistence. In such cases the estimated coefficient β from equation (5) might only have limited relevance for policy evaluation purposes (Callaway and Sant'Anna 2021). Extending the model with leads and lags of the MW allows us to explore this possibility as well. The *dynamic* difference-in-differences model is

[27] In Section B we show suggestive evidence that they are not affected by the MW by using them as dependent variable in our models.

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

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 $\beta_{-s} = \beta_{-s+1} = ... = \beta_{-1} = 0$. 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.[28] 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.[29] 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.

This model allows us to estimate the dynamics of the logarithm of the rent per square foot around changes in the MW. We define the cumulative elasticity of rents to the MW as $\sum_{r=0}^{s} \beta_r$. We intend to obtain reliable estimates of this statistic. However, because each coefficient is estimated with noise, the cumulative sum is likely to show large confidence intervals. Imposing the assumption of no pre-trends, we can gain efficiency by estimating a model with distributed lags only. We present estimates of dynamic models for both the individual coefficients and the cumulative sum in the results section.

Finally, we consider a dynamic panel specification that includes the lagged dependent variable as a control. Such a model has two important advantages. First and most important, it accounts for the possibility of structural auto-correlation in the rents variable that would bias our results. This is in fact a possibility, given that some listings likely remain in the Zillow data for more than one month. Secondly, it allows us to relax the strict exogeneity assumption (Arellano and Honoré 2001). The less stringent *sequential exogeneity* assumption required for identification allows for feedback effects of minimum wages on rents.[30] The model with can be written as

$$\Delta \ln y_{ict} = \delta_t + \sum_{r=0}^{s} \beta_r \Delta \ln \underline{w}_{ic,t+r} + \gamma \Delta \ln y_{ic,t-1} + \Delta \mathbf{X}_{ct}^{'} \eta + \Delta \varepsilon_{ict} \ . \tag{7}$$

The model can also be estimated using pre-trends, which we show to be zero.

We are also interested in the parameter γ since, per the properties of time series, the long run

[28]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.

[29]Ideally, we would run this regression on the number of rental units. Unfortunately, as described in Section 3, this information is not available.

[30]More precisely, the sequential exogeneity assumption states that, conditional on controls and fixed effects, unobserved shocks to rents are uncorrelated with *past* MW changes.

effect of minimum wages on rents is given by $(\beta_0 + \beta_1 + \cdots + \beta_s)/(1-\gamma)$. Estimation is not that straightforward, however, because by construction we now have that $\Delta y_{ic,t-1}$ is correlated with the error term. To address this issue, we follow the literature and instrument $\Delta y_{ic,t-1}$ with $\Delta y_{ic,t-2}$ (Arellano and Honoré 2001). To obtain a more precise estimate of the long run effect, we shut down some lagged coefficients by imposing a short window s in the model.

4.3 Heterogeneity by ZIP code Characteristics

Consider some demographic characteristics of ZIP codes (say, the share of college graduates). We extend the baseline panel static model defined in equation (5) by interacting the local MW change variable with dummy variables that indicate whether the ZIP code belongs to each quartile of the distribution of such characteristic. This specification allows us to test for heterogeneous effects of MW changes based on ZIP code characteristics, therefore assessing whether our effects are driven by ZIP codes expected to have more MW workers. Formally, the model is

$$\Delta \ln y_{ict} = \theta_t + \sum_{q=1}^{4} \beta_q \mathbb{1}\{i \in q\} \Delta \ln \underline{w}_{ict} + \Delta \mathbf{X}_{ct}^{'} \eta + \Delta \varepsilon_{ict} \ , \tag{8}$$

where q identifies quartiles of some ZIP code level characteristic, and $\mathbb{1}\{\cdot\}$ is the indicator function.

To minimize any type of concern about ZIP code characteristics being endogenous to MW changes, we use use socio-demographic data from the 2010 Census and the 5-years 2008-2012 ACS that predate our panel.

4.4 Experienced Minimum Wage

We run versions of the static and dynamic models both using the experienced MW instead of the statutory one and including both measures together. These models are valuable for two main reasons. First, as long as MW workers commute across ZIP codes, their income will be determined by the MW level set at their workplace location, not by the one in place at their residential address. As a consequence, ZIP codes with no changes in the statutory MW may nevertheless record changes in their residents' income, whereas ZIP codes with statutory changes may experience minimal variation in residents' income. By a basic supply-and-demand logic, we expect rents to increase in locations where residents have more income due to the MW changes. In this light, classical measurement error arguments will imply that, using the statutory MW only, the effect of interest will be downward biased (Angrist and Pischke 2009). Our results support that view.

Secondly, MW changes may be thought of as income transfers across ZIP codes. The experienced MW better captures variation relevant for those ZIP codes where, as a result of the policy, residents earn more money. By directly controlling for both MW measures in the baseline static model, we argue that the residual variation of the statutory MW will be related to places where residents see a negative income shock following the MW increase, since these are the ZIP codes that "pay" for the transfer to ZIP codes where low-income workers reside. As a result we would expect to see a stronger positive effect for the experienced MW (i.e., ZIP codes that experience a positive income shock), and a negative effect for the actual MW (i.e., ZIP codes that experience a negative shock).

Identification now requires the timing of within-ZIP code experienced MW changes to be orthogonal with respect to dynamics of rent unobservables. Given that the experienced MW is constructed as a weighted average of the statutory MWs of close-by zipcodes, this restriction is similar to imposing unobserved determinants of rents in one zipcode to be orthogonal to MW changes in other zipcodes where its residents work.[31] This restriction seems unquestionable for state-wide changes, since they tend to affect near-by ZIP codes in the same fashion, and thus are unlikely to be correlated with specific ZIP codes' unobservables. For local changes the assumption could be questioned if local policy-makers set MW changes at the same time as rents in places where MW workers reside are rising. Considering the hard-to-predict commuting patterns across zipcodes and the fact that MW changes tend to be set without consideration of housing market dynamics in general, we think this identifying assumption is likely to hold. On the other hand, because the weights are fixed over time we are not worried about changes in commuting patterns that hamper identification. However, if those weights are inaccurately measured or change over time significantly following MW changes our estimations will be biased. For example, if MW workers move to places with low rents that are close to cities with regular MW changes the effect of experienced MW on rents will be understated by our model.

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.

[31] They are not *exactly* equal because we use the natural logarithm of the experienced MW in our model.

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

Abraham, Sarah and Liyang Sun (2018). "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects". In: *Available at SSRN 3158747*.

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.

Angrist, Joshua D and Jörn-Steffen Pischke (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton university press.

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.

Borusyak, Kirill and Xavier Jaravel (2017). "Revisiting event study designs". In: *Available at SSRN 2826228*.

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.

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 (2019). "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*.

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*.

Fuest, Clemens, Andreas Peichl, and Sebastian Siegloch (2018). "Do higher corporate taxes reduce wages? Micro evidence from Germany". In: *American Economic Review* 108.2, pp. 393–418.

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`.

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.

Suárez Serrato, Juan Carlos and Owen Zidar (2016). “Who benefits from state corporate tax cuts? A local labor markets approach with heterogeneous firms”. In: *American Economic Review* 106.9, pp. 2582–2624.

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 (2019). *American Community Surve 5-year Data*. https://www.census.gov/data/developers/data-sets/acs-5year.html. Accessed: 2020-10-03.

— (2020). *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.

Wooldridge, Jeffrey M (2010). *Econometric analysis of cross section and panel data*. MIT press.

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: Descriptive Statistics of Different Sets of ZIP Codes

	U.S.	Top 100 CBSA	Full Panel	Est. Panel
Population (millions) (2010)	311.18	189.71	110.17	50.62
Population as share of U.S.	1	0.61	0.35	0.16
Housing Units (millions) (2010)	132.83	78.74	46.72	21.32
Housing Units as share of U.S.	1	0.59	0.35	0.16
Urban Share (2010)	0.46	0.75	0.96	0.97
College Share (2010)	0.31	0.39	0.44	0.44
African-American Share (2010)	0.09	0.12	0.15	0.17
Hispanic Share (2010)	0.10	0.14	0.17	0.19
Elder Share (2010)	0.15	0.13	0.12	0.11
Poor Share (2010)	0.15	0.14	0.14	0.13
Unemployed Share (2010)	0.09	0.09	0.09	0.09
Mean HH income (2010)	52,492.56	62,773.64	65,475.16	66,919.72
Rent House Share (2010)	0.29	0.35	0.38	0.38
Unique zipcodes	38,893	14,583	3,315	1,305
Mean SFCC psqft rent			1.30	1.27

Notes: The table shows characteristics of four sets of U.S. postal service ZIP codes. Column 1 reports demographic statistics for the universe of USPS ZIP code we were able to map. Column 2 shows the same statistics for the top 100 Core-Based Statistical Areas (CBSA). Column 3 shows the characteristics of the set of ZIP codes available in the Zillow data. Finally, column 4 shows the restricted balanced sample we use as baseline in our empirical analysis. All demographic information comes from the 2010 Census and the 5-years 2008-2012 ACS.

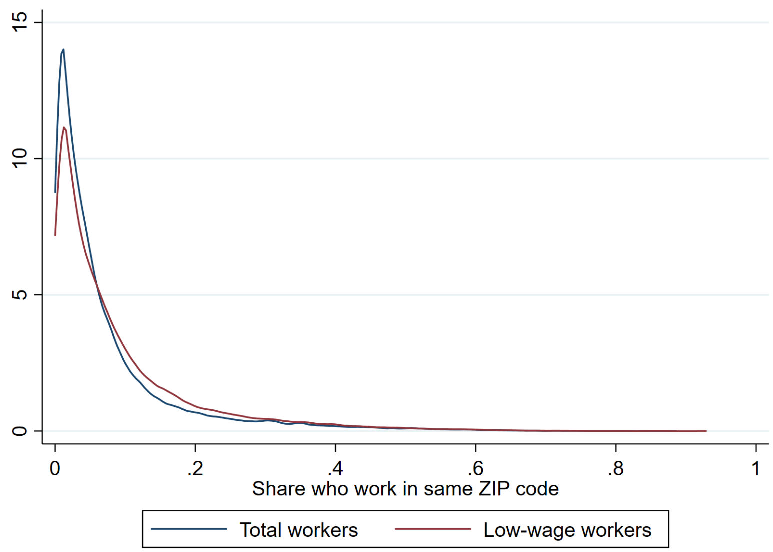
Table 2: Descriptive Statistics of Estimating Panel

Statistic	N	Mean	St. Dev.	Min	Max
Statutory MW	155,295	8.08	1.21	7	16
Experienced MW	155,295	8.06	1.21	6.29	14.98
Median rent psqft. SFCC	113,375	1.27	0.83	0.47	7.25
Median rent SFCC	125,644	1,651.10	702.99	595.00	6,595.00
Avg. wage Fin. activities	151,032	1,561.71	961.88	0.00	9,557.00
Employment Fin. activities	151,032	59,595.23	75,840.23	0.00	397,839.00
Estab. count Fin. activities	151,032	5,105.58	5,201.89	31.00	30,405.00

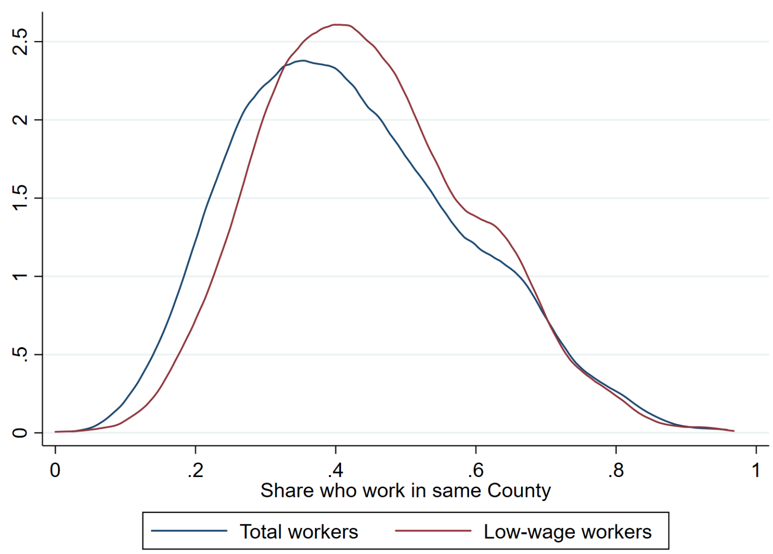
Notes: The table shows descriptive statistics of our baseline estimating panel. Variables included are the statutory and experienced MW, constructed as explained in subsection 3.2; the average of median monthly rents per square foot and total in the SFCC category, taken directly from Zillow; and average weekly wage, employment and establishment count in the Financial Sector from the QCEW.

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

(a) ZIP code



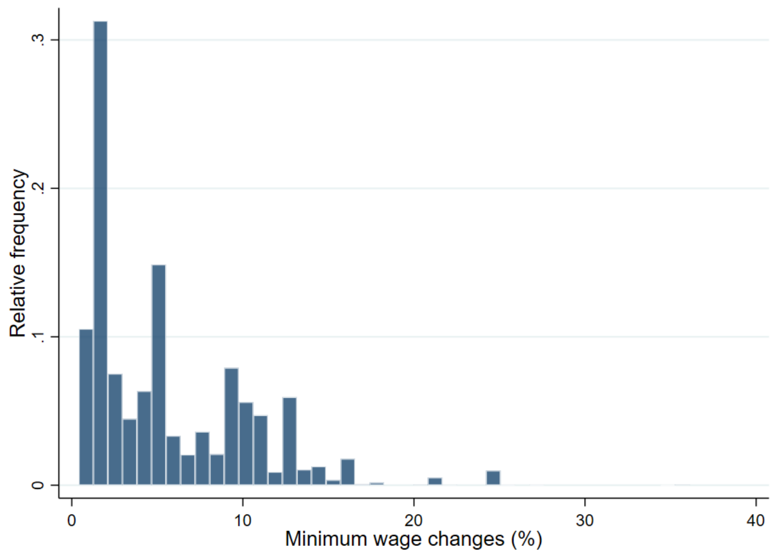
(b) County



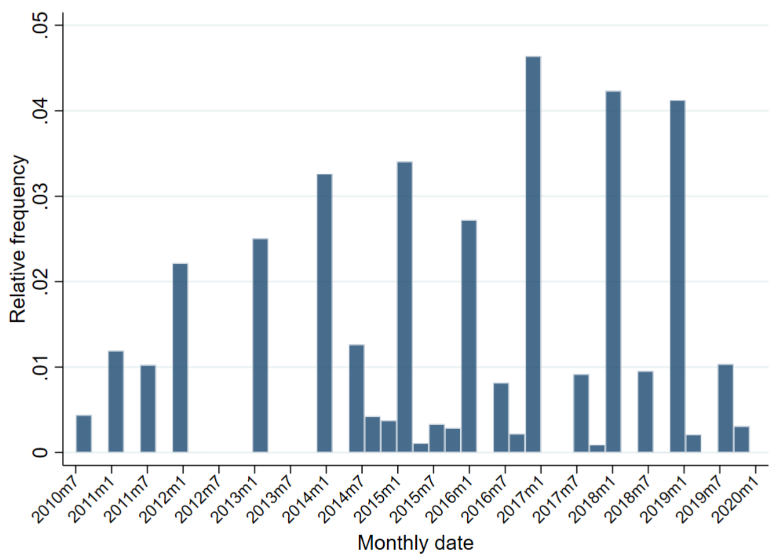
Notes:

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 ZIP code 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 has the properties given in Assumption 1. Let λ_{it} denote the share of i's residents who started their contracts in period t.[32] 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

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

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

[32] For simplicity, we assume that these shares don't vary by workplace.

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 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.[33]

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

$$\Delta \ln y_{ct} = \delta_t + \sum_{r=-s}^{s} \beta_r \Delta \ln \underline{w}_{c,t+r} + \Delta \nu_{ct}, \tag{11}$$

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-

[33] 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.

vations. 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

$$\overline{\Delta \ln y}_{cq} = \overline{\delta}_q + \sum_{r=\tau}^{\tau} \rho_r \overline{\Delta \ln \underline{w}}_{c,q+r} + \overline{\Delta\nu}_{ct}, \tag{12}$$

where τ defines the quarterly window. We interpret the ρ_r coefficients in Equation 12 as averages of the monthly coefficients β_r in Equation 11. The reason is that the average change over a quarter is a linear combination of monthly changes.[34]

?? 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.

[34]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,

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

C Appendix Tables

Table C.1: Extended Descriptive Statistics of Estimating Panel

Placeholder

Notes: The table shows summary statistics of our baseline estimating panel. Variables included are the statutory and experienced MW, constructed using different sets of weights as explained in subsection 3.2; median monthly rents per square foot in the categories 2 bedroom (2BR), multi family houses with 5 or more units (MFR5plus), and SFCC, and absolute median rents for the SFCC category, all taken directly from Zillow; and average weekly wage, employment and establishment count for three industries used in our estimation, obtained from the QCEW.

D Appendix Figures