

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

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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. For each location we define both the log minimum wage where the average resident works (the “workplace MW”) and the log minimum wage in the location itself (the “residence MW”). We write down a partial-equilibrium model of a housing market in which minimum wage levels in each location affect housing demand by changing the income of commuters and the prices of non-tradable consumption. We show that, in the model, the workplace MW has a positive effect on rents whereas the residence MW has a negative effect. We take our model to the data by constructing a ZIP code monthly panel using rents data from Zillow. We use a difference-in-differences design to estimate the effect of residence and workplace MW changes on log 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 the residence MW also increases by 10 percent, then the increase in rents will be lower—between 0.35 and 0.9 percent—. We use our results to study the consequences of a counterfactual increase in the federal MW from \$7.25 to \$9. We estimate that, in ZIP codes where the residence MW increases, landlords pocket between 5 and 9 cents on the extra dollar. In ZIP codes where the residence MW does not change, landlords pocket between 9 and 16 cents.

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1 Introduction

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

In this paper we take a step in this direction by studying the short-run effect of MW policies on local rental housing markets. Consider a new MW policy in some locations within a metropolitan area. Because low-wage workers tend to reside in specific neighborhoods with access to those (now better-paying) low-wage jobs, one would expect an increase in disposable income and subsequent rise in demand for housing and rental prices there. This effect, which operates through the MW at the workplace, will undermine (at least partially) the distributional objective the policy. Similarly, the MW hike will translate into higher prices of non-tradable consumption that use low-wage workers intensively as an input. As a result, the demand for housing and rental prices will also be affected. This effect, which operates through the MW at the residence, in turn, will also have distributional consequences. Commuting patterns thus become an essential ingredient to understand the heterogeneous effects of local MW policies on the housing market when there is a divergence in the workplace and residence locations of workers. Figure 1 illustrates by showing the share of low-income workers and residents in each ZIP code in the Chicago-Naperville-Elgin CBSA.

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 for several reasons. First, as opposed to assessing effects on pure labor market outcomes where jobs and wages are tied to the workplace, when evaluating the housing market it is crucial to account for the fact that people may reside and work under different MW levels.² This is challenging because accounting for changes in the MW where residents of a location work requires data on commuting patterns at the local level. Second, estimation at the local level requires spatially 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) on some local housing markets is positive.³ Even if the effects in the large geographies may be of

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

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

³Rents in neighborhoods where low-wage workers live are likely to increase, whereas elsewhere they are likely not

interest, they will mask substantial heterogeneity and therefore miss the fact that some people will be paying higher rents due to the policy change. 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.

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 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 ZIP code-level IRS data, we estimate the share on each dollar of extra income (caused by the MW) that accrues to landlords in each ZIP code. We discuss the implications of our results for assessing the distributional impact of MW policies.

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

to change or even decrease, as those residents “pay” for the higher MW through higher prices and lower profits.

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

We construct a panel at the USPS ZIP code and monthly levels with rental prices and binding MW levels. Our main rent variable comes from Zillow, the largest online real estate platform in the US (PDX 2020; Investopedia 2020), and corresponds to the median rent price per square foot across Zillow listings in the given ZIP code-month cell of the category Single Family, Condominiums and Cooperative Houses (SFCC). This is the most popular housing category in the US (Fernald 2020), and also the most populated series in the Zillow data. We collect data on MW changes from Vaghul and Zipperer (2016) for the period 2010–2016, which we update until January 2020 using data from UC Berkeley Labor Center (2020) and cross-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). We use our MW data coupled with commuting origin-destination matrices obtained from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES; US Census Bureau 2020b) database. These data provide workplace locations for the residents of all the US census blocks, and we use it to construct the workplace MW measure.

We also collect data on county-level economic indicators from the Quarterly Census of Employment and Wages US Bureau of Labor Statistics (QCEW; 2020); wage and business income at the ZIP code-year levels from Internal Revenue System (2022); and ZIP code level sociodemographic characteristics from the US Census Bureau (2020a).

Guided by the theoretical model, we pose an empirical model where log rents in a location depend linearly on (1) residence MW—the log of the statutory MW at that location—, (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 MW 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 and the controls. 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.⁵ Using an argument akin to the recent difference-in-differences literature (e.g., Callaway, Goodman-Bacon, and Sant’Anna 2021), in an appendix we unpack our identification argument beyond the residence and workplace MW. We state clearly the conditions required on the commuting shares and the unobservable determinants of rents under a MW policy that increases the MW in a subset of ZIP codes only.

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

⁵We use all ZIP codes with valid rents data as of July 2015.

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 understanding the distributional consequences of the rich pattern of changes in rent gradients generated by this policy.

Heterogeneity analyses show that ...

We conduct several robustness checks to test the validity of our results. First, we test our identifying assumption estimating our model adding leads and lags of each MW variable. Reassuringly, we find 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; and using an unbalanced panel with full set of ZIP codes and “cohort-by-time” fixed effects. Our results are robust to these exercises. Trying to approximate the average treatment effect beyond our selected sample of ZIP codes, we estimate our model using weights constructed to match key moments of the distribution of urban ZIP codes, finding similar results.

In the final part of the paper, we develop a simple extension to our baseline model to estimate the ZIP code-specific share on each dollar that accrues to landlords following a MW increase. This parameter depends on the change in the total wage bill of a ZIP code, and the share of a ZIP code’s total income spent in housing. We posit a model for the wage bill similar to our baseline, and estimate an elasticity of wages to the minimum wage that is in line with the literature (e.g., Cengiz et al. 2019). Due to data constraints, we assume a range of values for the share of housing expenditure at the ZIP code level. We focus on studying the consequences of a counterfactual increase in the federal MW from \$7.25 to \$9 on January 2020. We find large variation in the estimated resulting rent changes across ZIP codes. We estimate that, in ZIP codes where both the residence and workplace MWs increase due to the policy, landlords pocket between 5 and 8 cents on the dollar. However, in ZIP codes where the residence MW does not change, the share pocketed by landlords is higher. These results imply that a share of the extra income of the low-wage workers due to the policy is actually captured by landlords due to increased housing demand and a finite elasticity of housing supply.⁶ Ignoring this fact will lead to an overstatement of the gains of low-wage workers following a MW increase.

This paper is related to several strands of literature. First, our paper relates to the large literature

⁶This result is consistent with the mechanism proposed by Kline and Moretti (2014), whereby place-based policies generate welfare losses due to inefficiencies in the housing market. However, in our model we do not allow for migration responses that may mitigate this coefficient in the medium run.

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

Second, this paper is related to the literature studying the effects of MW policies on behaviors beyond the labor market. We already mentioned the scant literature estimating the effects of MW policies on rental housing prices (Tidemann 2018; Yamagishi 2021). We innovate in several ways relative to these papers. First, while these papers estimate the effect of same-location MW on rents, we differentiate between residence and workplace MW levels, fully incorporating spillovers across regions. Second, we use data at a more detailed geography and higher frequency.⁸ 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.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 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

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

⁸Both 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.

⁹Another related paper is Agarwal, Ambrose, and Diop (2021) who show that MW increases lower the probability of rental default.

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 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.¹⁰ Each ZIP code has a binding minimum wage, which we denote by $\{W_z\}_{z \in \mathcal{Z}(i)}$.

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

$$R_i h_{iz} + P_i(W_i) c_{iz}^{NT} + c_{iz}^T \leq Y_{iz}(W_z),$$

where R_i gives the rental price of housing per square feet, $P_i(W_i)$ gives the price of local consumption, the price of tradable consumption is normalized to one, and $Y_{iz}(W_z)$ 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, $\frac{dY_{iz}}{dW_z} \geq 0$, with strict inequality 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

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

consume close to home. As a result, we expect them to be sensitive to prices of local consumption in their same neighborhood, justifying the inclusion of c_{iz}^{NT} in the utility function.¹¹ 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).¹²

The solution to the worker's problem for each z yields a set of continuously differentiable housing demand functions $\{h_{iz}(R_i, P_i, Y_z)\}_{z \in \mathcal{Z}(i)}$. Standard arguments imply that this function is decreasing in its own price R_i . We assume conditions that ensure that housing demand is decreasing in local prices P_i .¹³ 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 absentee landlords supply square feet in i according to the 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

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

Given that housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.¹⁴ Equilibrium rents are a function of the entire set of minimum wages, formally, $R_i^* = f(\{\underline{W}_i\}_{i \in \mathcal{Z}(i)})$.

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

¹¹An 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.

¹²An 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.

¹³To 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.

¹⁴To see this, assume that $D_i(0) - \sum_{z \in \mathcal{Z}(i)} L_{iz} h_{iz}(0, P_i, Y_z) < 0$ and apply the intermediate value theorem.

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

Proposition 1 (Comparative Statics). *Consider residence ZIP code i and a change in MW policy such that for $z \in \mathcal{Z}_0 \subseteq \mathcal{Z}(i)$ binding MWs increase, and for $z' \in \mathcal{Z}(i) \setminus \mathcal{Z}_0$ binding MWs do not change, where \mathcal{Z}_0 is non-empty. Under the assumptions of unchanging $\{L_{iz}\}_{z \in \mathcal{Z}(i)}$ 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 $\ln W_i$ for all $i \in \mathcal{Z}(i)$. Dividing by 1 and each of the variables appropriately, one can show that

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

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

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

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

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

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 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

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

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

Proposition 2 shows that, under an homogeneity assumption,¹⁵ 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 Department of Housing and Urban Development (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

¹⁵We 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.

and time aggregation levels (Zillow 2020b).¹⁶ 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.¹⁷ 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 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.¹⁸ 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 2016–2019.¹⁹ We assign MW levels to USPS ZIP codes by taking the following steps. First, we collect a crosswalk constructed by (US Census Bureau 2020b) that contains, for each census block, identifiers for block group, tract, county, state, and “place” (i.e.,

¹⁶The 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.

¹⁷Two 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.

¹⁸For more information on how SAFMRs are calculated, see US Department of Housing and Urban Development (2017, page 41641).

¹⁹Some 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.

census designated place, which includes cities). Second, we assign the MW level of each jurisdiction to the relevant census block. We use the state code for state MW policies, and we match local MW policies based on the names of the county and the place. We define the statutory MW at each census block as the maximum of the federal, state, county and place levels. Then, based on an original correspondence table described in Appendix B, we assign a USPS ZIP code to each census block. Finally, we define *the statutory MW* at ZIP code i and month t , \underline{W}_{it} , as the weighted average of the statutory MW levels in its constituent blocks, where the weights are given by the number of housing units.

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.²⁰

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 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 across 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 aggregated at the census block level. We aggregate it to USPS ZIP codes using the original correspondence between census blocks and USPS ZIP codes described in Appendix B. This results in USPS ZIP code residence-workplace matrices that, for each location and year, tell 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

²⁰There 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.

$\{\omega_{iz}\}_{z \in \mathcal{Z}(i)}$ 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.²¹ We define the workplace minimum wage measure as

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

Figure 3 illustrates the difference in these measures by plotting the change in the residence and workplace MW in the metropolitan area of Chicago on July 2019. On that month, both Cook County and the city of Chicago increased the statutory MW from \$12 to \$13. We observe how the increase affects ZIP codes far beyond the limits of the county, suggesting that rents may be affected there as well. For completeness, Appendix Figure D.4 shows the changes in our main rents variable around the same date.

3.3 Other Data Sources

To proxy for local economic activity we collect data from the Quarterly Census of Employment and Wages (QCEW; US Bureau of Labor Statistics 2020) at the county-quarter and county-month levels for every main industrial division and from 2010 to 2019.²² 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 Individual Income Tax Statistics aggregated at the USPS ZIP code level for the period 2010–2019 (Internal Revenue System 2022). 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, and the number of households with farm income. We use this data in our counterfactual exercises.

Finally, in order to describe our sample of ZIP codes we use data from the the 5-year 2007-2011 American Community Survey (ACS; US Census Bureau 2020a) and from Small Area Fair Market Rents (SAFMR; US Department of Housing and Urban Development 2020).

3.4 Estimation Samples

We put together an unbalanced 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

²¹The LODS 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).

²²The 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”.

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. This partially balanced panel contains 5,302 MW increases, which arise from 124 state-level changes and 99 county- and city-level changes. 4,224 of those changes take place after ZIP codes already entered the panel, and thus are used in estimation. We also construct a fully balanced panel since July 2015 that delivers similar though noisier results.

Table 2 further compares the Zillow sample and our baseline panel to the population of ZIP codes along several demographic dimensions. The first and second columns report data for the whole universe of US ZIP codes and for the set of urban ZIP codes, respectively. The third column shows the set of ZIP codes in the Zillow data, i.e., those that have some value of rents per square foot in the SFCC category. We refer to this as the Zillow sample. Finally, the fourth column shows descriptive statistics for our baseline estimating sample described above.

Our baseline ZIP codes tend to be more populated, with a higher share of Black and Hispanic inhabitants, richer, and with a higher share of renter households than both the average US ZIP code and the average urban US ZIP code. This is the case because Zillow is present in almost every large urban market, but it does not operate as often in small urban or rural areas. Therefore, our results can be interpreted as relevant for large urban areas. However, even in this sample our ZIP codes are richer than the average (i.e., arguably less influenced by MW changes), so we expect to underestimate the true parameters. In an attempt to capture the treatment effect for the average US urban ZIP code we conduct an estimation exercise where we re-weight our sample to match the average of a handful of sociodemographic characteristics of those.

Finally, Appendix Table D.1 shows some sample statistics of our baseline estimating panel. As suggested in the table, the distribution of the residence and workplace MW measures is similar. However, we show in the next section that they do show independent variation in our model. We also show summary statistics of median rents in the SFCC category. The average of monthly median rents is \$1,665 in absolute values and \$1.23 per square foot, although these variables show a great deal of variation. Finally, we show average weekly wage, employment and establishment count for the QCEW industries we use as controls in some models.

4 Empirical Strategy

4.1 First-differences model

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

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

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

By taking first differences on the previous equation we obtain

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

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 of the model in levels. The main results of the paper are obtained under the model in (5). However, to compare to results in the literature we also estimate versions of the model that exclude either one of the MW measures.

4.2 Identification and Causality

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

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

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

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

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

We worry that unobserved shocks, such as those caused by local business cycles, may systematically affect both rents and minimum wage changes. To account for common trends in the housing market we include the time-period fixed effects. In some specifications we allow these trends to vary by CBSA. To control for variation arising from unobserved trends in local markets we include economic controls from the QCEW.²³ Specifically, we use average weekly wages, employment and

²³These data are aggregated at the county level, and represent a second best given the unavailability of controls at

establishment counts for the sectors “Professional and business services,” “Information,” and “Financial activities.” We assume that these sectors are not affected by the minimum wage.²⁴ We also try models where we control for ZIP code-specific linear and quadratic trends, which should account for time-varying heterogeneity not captured by our economic controls that follows this pattern.

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

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

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

Under the assumption that there are no anticipatory effects in the housing market, we interpret the absence of pre-trends as evidence against the presence of unobserved economic shocks driving our results. We think that, given the high frequency of our data and that we focus on short windows around MW changes, the assumption of no anticipatory effects is reasonable.²⁵ We further present evidence in favor of this assumption by showing that our MW measures do not predict the number of listings of houses for sale in Zillow.²⁶

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

4.3 Identification beyond the MW measures

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

the ZIP code level.

²⁴In fact, according to U.S. Bureau of Labor Statistics (2020, table 5), in 2019 the share of workers earning at or below the minimum wage in those industries was 0.8, 1.5, and 0.2, respectively.

²⁵We 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}_{it} capture all relevant unobserved heterogeneity arising from local business cycles. While we find the interpretation given in the text more palatable, the data is consistent with both.

²⁶Ideally, we would run this regression on the number of rental units. Unfortunately this information is not available in the data. Specifically, we track the number of houses listed for sale in a sample of ZIP codes during the period 2013-2019 for our preferred house type (SFCC).

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

4.4 Heterogeneity and Representativeness of the Results

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

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

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

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

4.5 Alternative Strategies

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

Regardless of the estimation strategy, our results still rely on the strict exogeneity assumption.

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

5 Results

In this section we...

5.1 Baseline Results

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

5.2 Robustness checks

Alternative rents measure Discuss results with SAFMR

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

5.3 The Heterogeneous Impact of MW Changes on Rents

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

6 Counterfactual Analysis

Discuss pass-through estimates. Stress that they depend on geography of prevailing MWs across the 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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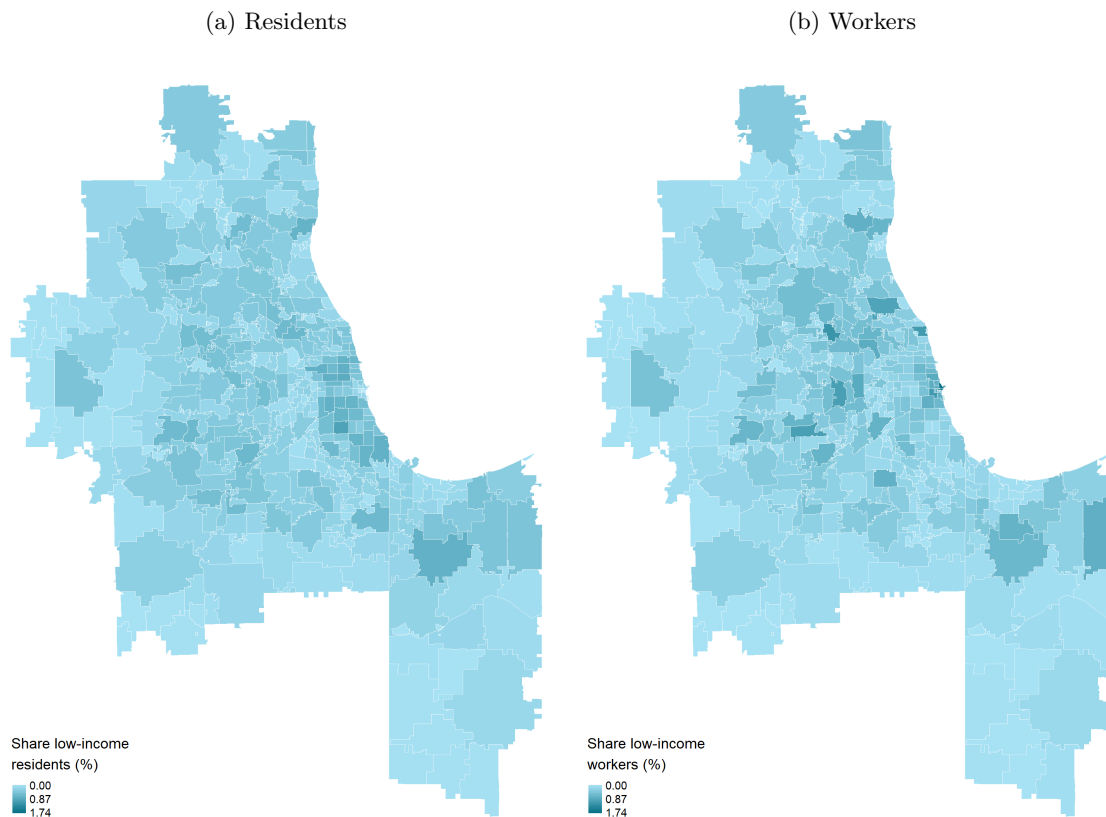
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Figures and Tables

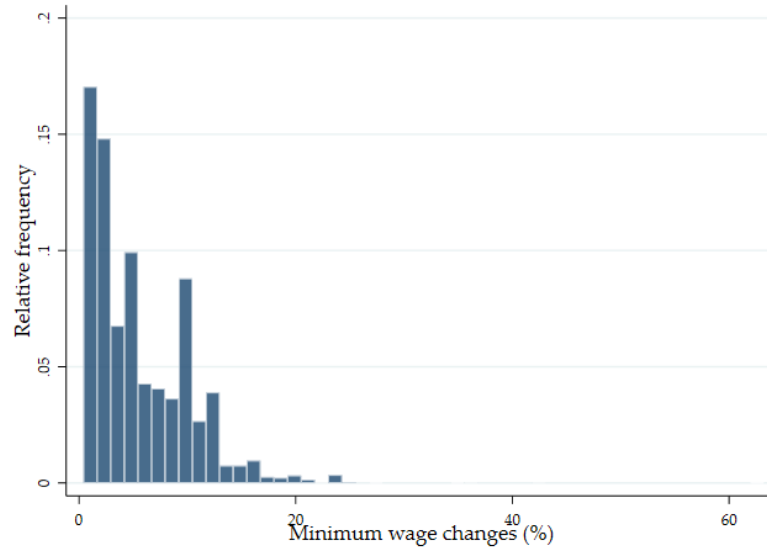
Figure 1: Share of low-income residents and workers in the Chicago-Naperville-Elgin CBSA, 2018



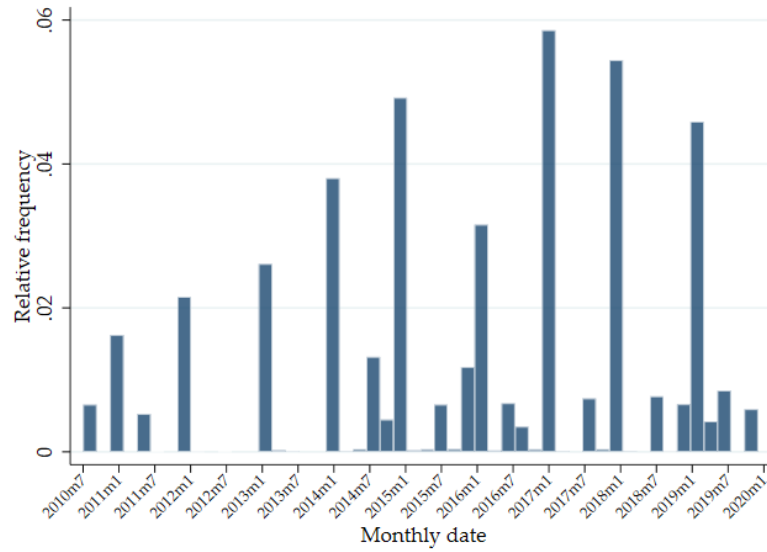
Notes: Data are from LODES (US Census Bureau 2020b) aggregated at the ZIP code level. The figure shows the share of low-income residents and workers out of the CBSA total in each ZIP code. Low-income workers are defined as those earning less than \$1,251 per month.

Figure 2: Distribution of Minimum Wage Changes

(a) Intensity

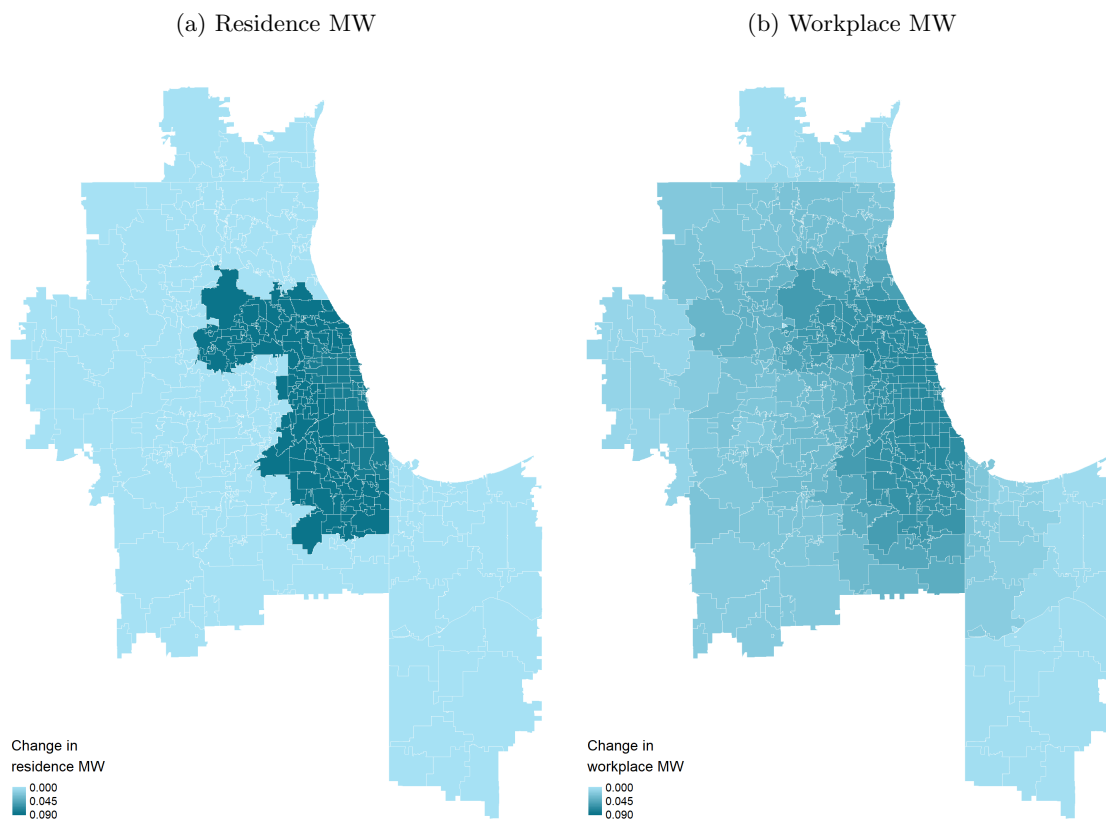


(b) Timing



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: Changes in MW measures in the Chicago-Naperville-Elgin CBSA, July 2019



Notes: Data are from the MW panel described in Section 3.2 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.

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:

Table 2: Descriptive statistics of different samples of ZIP codes

	All ZIP codes	Urban ZIP codes	Zillow sample	Baseline sample
(a) Mean population, 2011	12,161.2	16,147.1	33,635.9	37,965.9
(b) Mean number of households, 2011	4,600.5	6,114.7	12,709.3	14,238.3
(c) Mean household size, 2011	2.91	3.09	2.64	2.66
(d) Share urban ZIP codes, 2020	0.555	1.000	0.843	0.845
(e) Share renter households, 2011	0.288	0.347	0.371	0.361
(f) Share black population, 2011	0.091	0.111	0.153	0.160
(g) Share hispanic population, 2011	0.093	0.126	0.169	0.184
(h) Share households with wage income, 2010	0.822	0.827	0.833	0.840
(i) Share households with business income, 2010	0.159	0.156	0.166	0.170
(j) Mean AGI per household (\$), 2010	53,237.6	56,934.8	70,264.4	71,150.6
(k) Mean wage income per household (\$), 2010	36,241.8	38,347.5	47,469.6	49,287.6
(l) Mean 40th perc. 2BR apt. rent (\$), 2012	897.85	947.72	1,081.59	1,122.35
(m) Min statutory MW (\$), Feb. 2010	7.25	7.25	7.25	7.25
(n) Mean statutory MW (\$), Feb. 2010	7.42	7.45	7.48	7.43
(o) Max statutory MW (\$), Feb. 2010	12.67	12.67	12.67	9.79
(p) Number of ZIP codes	40,487	22,484	3,316	1,345

Notes: The table shows characteristics of different samples of ZIP codes. The first column uses all valid USPS ZIP codes. The second restricts to urban ZIP codes, according to Missouri Census Data Center (2018). The third and fourth columns use the ZIP codes with valid SFCC rents data, and the ones used in the baseline estimation, as described in Section 3. Each row shows a given characteristic obtained from different a data source. Rows (a)–(c) and (e)–(g) are constructed from the 2011 ACS US Census Bureau (2020a). Row (d) is based on an indicator obtained from Missouri Census Data Center (2018). Rows (h)–(k) are obtained from 2011 IRS ZIP code-level statistics Internal Revenue System (2022). AGI is an acronym for Average Gross Income. Row (l) is obtained from SAFMR US Department of Housing and Urban Development (2020). Rows (m)–(o) are computed using the panel of minimum wage levels described in Section 3.2.

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 λ_{it} denote the share of i 's residents who started their contracts in period t .²⁷ 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

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

²⁷We 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

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

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

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

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

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

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 Matching census blocks to USPS ZIP codes

One challenge of this project is that LODES data on commuting patterns are aggregated at the census-block level of the 2010 US Census. However, Zillow data are aggregated at the level of *USPS ZIP codes*, and census blocks and USPS ZIP codes are not nested. In this appendix we describe the steps we took to construct a correspondence table between these geographies.

First, we collected the GIS map of 11,053,116 census blocks from US Census Bureau (2012) and compute their centroids. Second, we assigned each block to a unique USPS ZIP code using the GIS map from ESRI (2020) based on assigning to each block the USPS ZIP code that contains its centroid. If the centroid falls outside of the block, we pick a point inside it at random. We assign 11,013,203 using the spatial match (99.64 percent of the total).²⁸ Third, for the blocks that remain unassigned we use the tract-to-USPS-ZIP-code correspondence from US Department of Housing and Urban Development (2022). Specifically, for each tract we keep the USPS ZIP code where the largest number of houses of the tract fall, and we assign it to each block using the tract identifier. We assign 22,819 blocks using this approach (0.21 percent). There remain 17,094 unassigned blocks (0.15 percent), which we drop from the analysis. This creates a unique mapping from census blocks to USPS ZIP codes.

In the end, there are 11,036,022 census blocks which are assigned to 31,754 USPS ZIP codes, implying an average of 347.55 census blocks per ZIP code. Thus, even though there may be blocks that go beyond one ZIP code, we expect the error introduced by this process to be very small.

²⁸545,566 of ZIP codes assigned via spatial match use a point of the census block picked at random (4.94 percent of the total).

C 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:

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

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

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

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

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

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 12 we can compute, for $t \neq \bar{t}$,

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

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

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

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

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

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 β is identified.

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

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

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

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

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

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

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

D Additional Tables and Figures

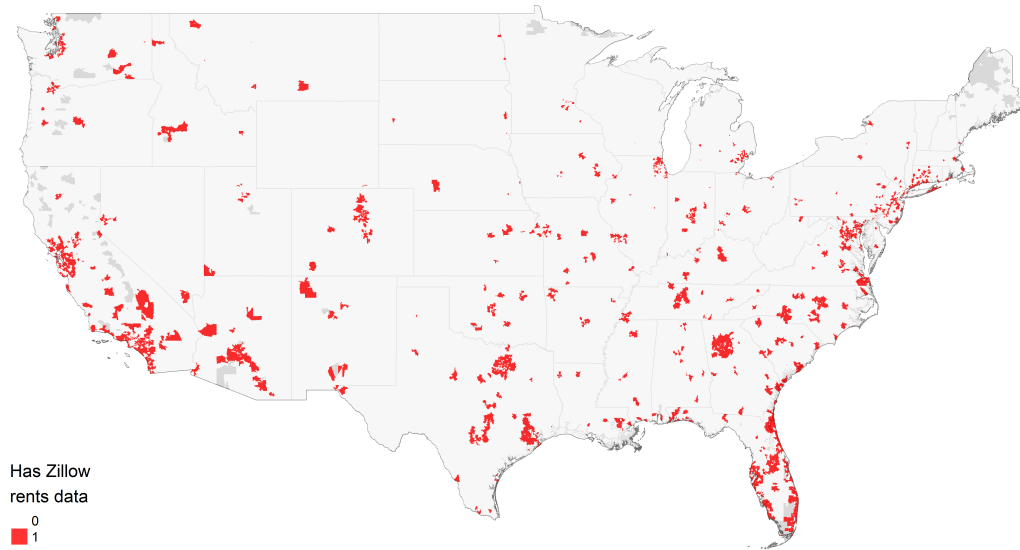
Table D.1: Summary statistics of baseline panel

	N	Mean	St. Dev.	Min	Max
Statutory MW \underline{W}_{it}	161,400	8.07	1.27	7.25	16.00
Residence MW $\underline{w}_{it}^{\text{res}}$	161,400	2.08	0.14	1.98	2.77
Workplace MW $\underline{w}_{it}^{\text{wkp}}$	161,400	2.07	0.13	1.87	2.70
Median SFCC rents	116,333	1,665.61	827.21	595.00	30,000.00
Median SFCC rents per sqft.	133,600	1.23	0.90	0.42	22.20
Log(median SFCC rents per sqft.)	133,600	0.08	0.46	-0.87	3.10
Median 2BR rent	46,056	1,815.5	921.6	480.0	6,910.0
Median 2BR rent per sqft.	44,196	1.70	0.84	0.50	6.51
Avg. wage Business services	161,400	11.10	1.41	5.58	13.39
Employment Business services	161,400	8.64	1.28	3.93	10.96
Estab. count Business services	161,400	7.08	0.32	5.42	8.43
Avg. wage Financial services	160,362	8.98	1.56	1.95	12.39
Employment Financial services	161,400	6.07	1.37	0.69	9.53
Estab. count Financial services	160,362	7.25	0.35	5.69	8.91
Avg. wage Information services	161,307	10.16	1.46	4.45	12.90
Employment Information services	161,400	7.94	1.23	3.43	10.34
Estab. count Information services	161,307	7.23	0.38	6.18	9.16

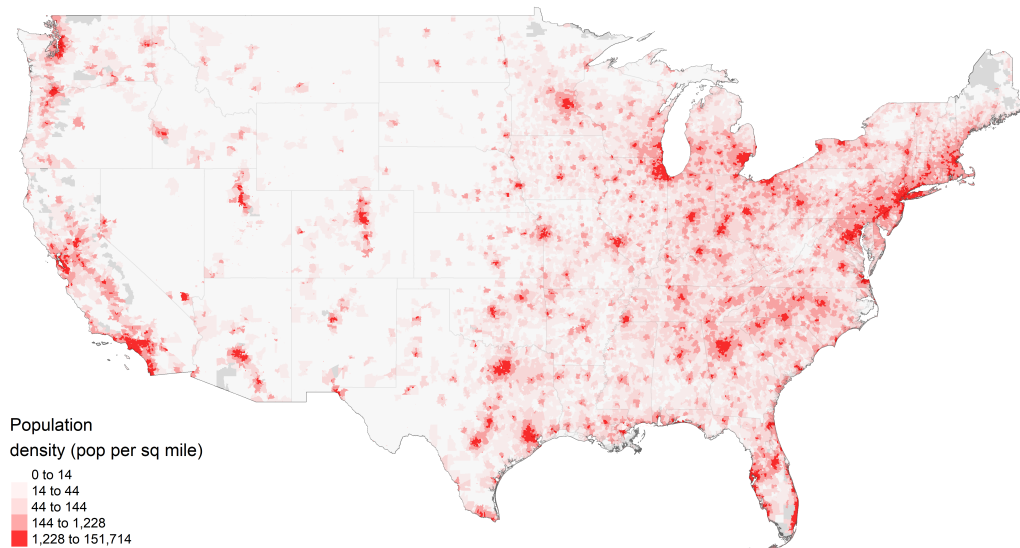
Notes: This table shows summary statistics of the panel of ZIP codes used in our baseline results, as described in Section XX.

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

(a) Zillow ZIP codes

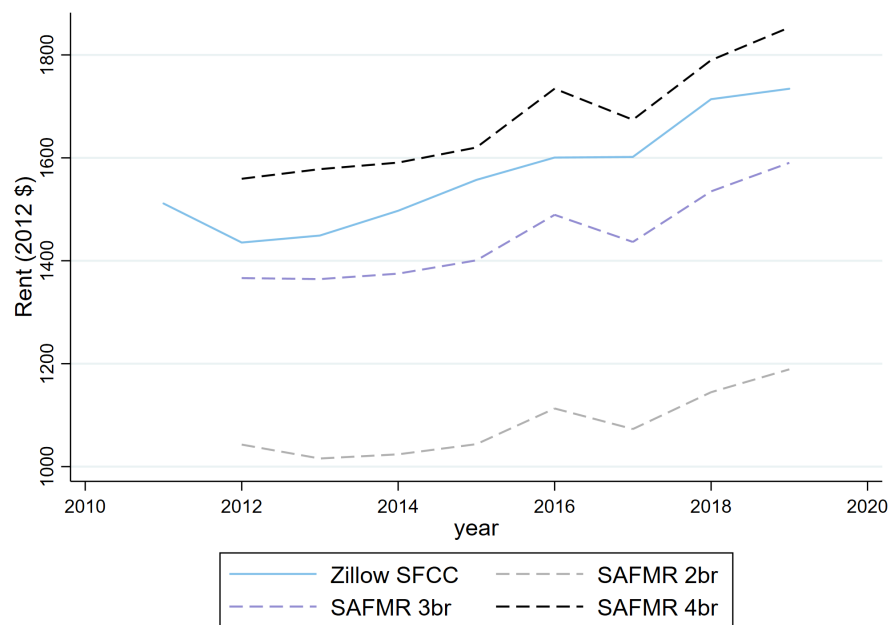


(b) Population Density



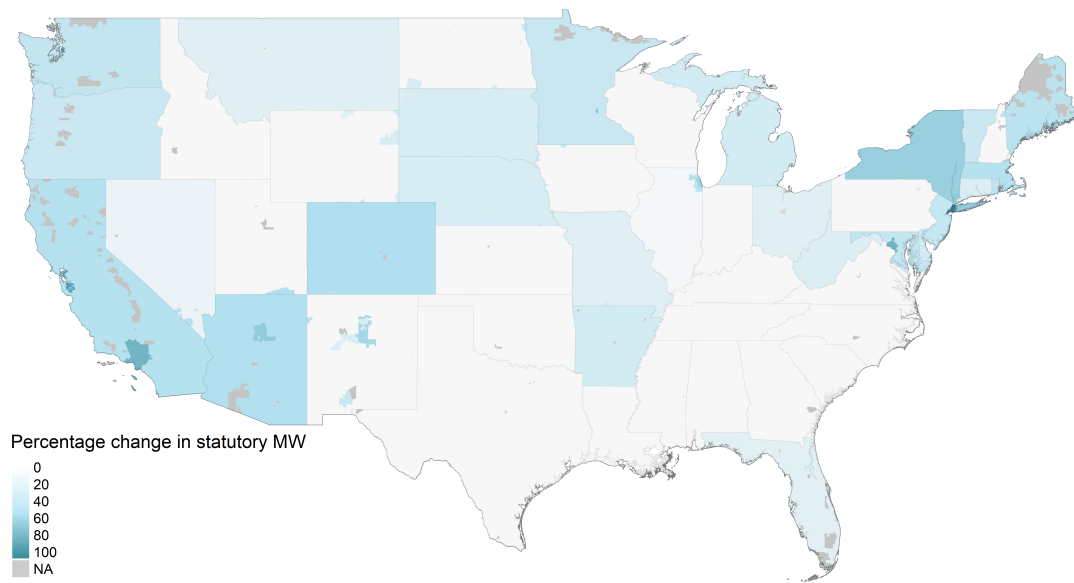
Notes: Data are from Zillow (2020b) and ESRI (2020). The figure compares the USPS ZIP codes available in Zillow to the population density. Panel (a) shows the sample of the ZIP codes that have rents data in the SFCC category at any point in the period 2010–2019. Panel (b) shows quintiles of population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

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



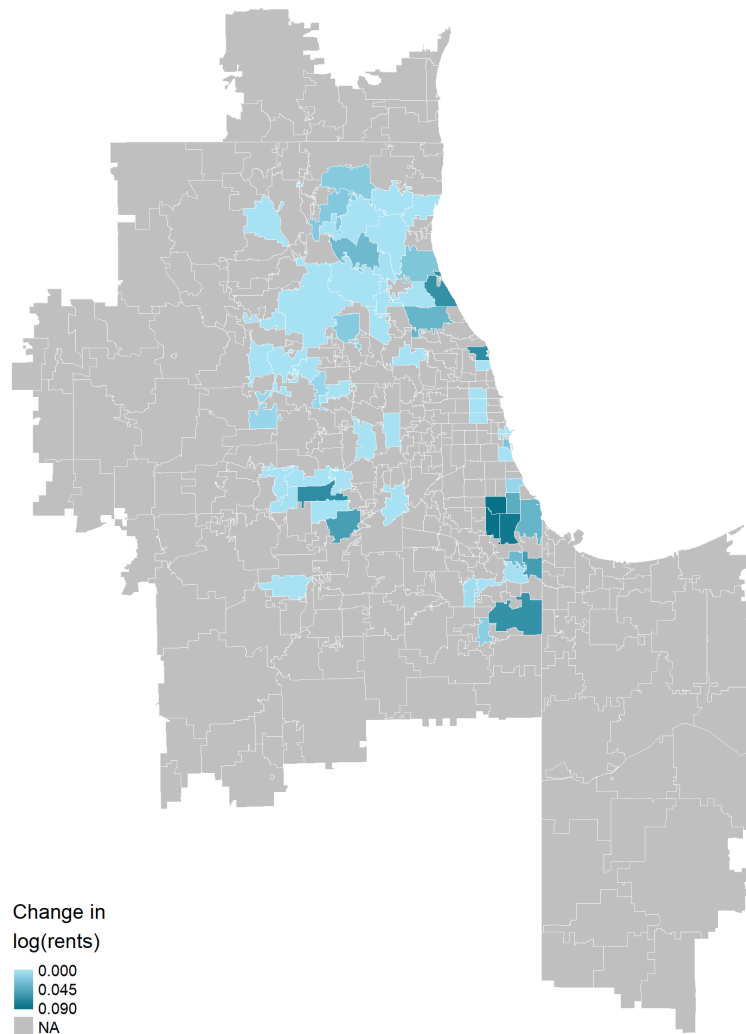
Notes: Data are from Zillow (2020b) and Small Area Fair Market Rents (2020). The figure compares the evolution of the median rental value in Zillow to three SAFMRs series, for 2, 3, and 4 or more bedrooms.

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.

Figure D.4: Changes in rents in the Chicago-Naperville-Elgin CBSA, July 2019



Notes: Data are from Zillow (Zillow 2020b). The figure shows the change in the log of median rents in the SFCC category in the month of June 2019 in ZIP codes located in the metropolitan area of Chicago.