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 short-run effect of MW changes on local housing rental markets exploiting the placed-based nature of MW policies. We argue that commuting patterns are key to understand the effect of these policies in local housing markets within metropolitan areas. For each location we define both the log MW where the average resident works (the "workplace MW") and the log MW in the location itself (the "residence MW"). We derive a partial-equilibrium model of a housing market in which MW levels in each location affect housing demand by changing the income of commuters and the prices of non-tradable consumption. The model shows that 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 by month 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. Our baseline results imply that a ZIP code experiencing a 10 percent increase in the workplace MW and no change in the residence MW will have an increase in rents of 0.53 percent. If the residence MW also increases by 10 percent, then the rents will increase by 0.34 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 overall effect on income for low-wage workers may be smaller if there is a significant pass-through from MW changes to prices, including housing.

In this paper we study 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 of the policy. Similarly, the MW hike will translate into higher prices of non-tradable consumption that use low-wage workers intensively as an input inside the jurisdiction that passed the new policy. As a result, the demand for housing and rental prices will also be affected. This effect, which operates through the MW at the residence, will have distributional consequences as well. 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. In Figure 1 we display, as an example, the geographical distribution of low-income workers by residence and workplace in the Chicago-Naperville-Elgin CBSA. We observe a clear divergence between the most common residence and workplace locations for these workers. This pattern is ubiquitous in our data.

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), Yamagishi (2019; 2021),[1] and Agarwal, Ambrose, and Diop (2021).[2] Estimating 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.[3] 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

[1] In 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.

[2] While the main goal of Agarwal, Ambrose, and Diop (2021) is to study the effect of the MW on eviction risk, the author also presents estimates of the MW on rents using individual transactions.

[3] However, 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).

local level requires spatially dissagregated data on rents. Using large geographies might result in null or even negative effects on average, even if no one commutes outside of this region and the actual effect (of workplace MW) on some local housing markets is positive.[4] Even if the effects in the large geographies may be of interest, they may mask substantial heterogeneity and therefore miss the fact that some people may 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. Finally, identification of the price effects requires high-frequency data, as otherwise the results may reflect changes in commuting and migration.

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 panel dataset of binding minimum wages at the ZIP code level, and commuting origin-destination matrices from the US Census Bureau (2021). As a result, we are able to estimate the effect of MW policies on rents using variation from hundreds of policy changes staggered across small jurisdictions and months that generate plausibly exogenous variation of workplace and residence MW levels. We show that our results are robust to using commuting shares of different years and for different worker categories, suggesting that commuting patterns are stable and thus unlikely to affect the results.

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 housing quality, alongside fully flexible prices. We argue that this assumption is consistent with the literature. In fact, several recent papers find null or small effects of MW policies on employment (Cengiz et al. 2019; Dustmann et al. 2022), and small elasticities of commuting to MW policies in a time horizon of several years (Pérez Pérez 2021).[5] 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

[4]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.

[5]This assumption is also motivated by our dataset, which varies at the monthly level. Thus, we are assuming that the first order effects of MW changes do not affect where agents live and work in a window of a few months around MW changes.

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.

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 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). 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 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 2021) database. These data provide workplace locations for the residents of all the US census blocks, and we use it–along with a novel correspondence table between blocks and ZIP codes–to construct our workplace MW measure. We also collect data on county-level economic indicators from the QCEW (US Bureau of Labor Statistics 2020); wage and business income at the ZIP code-year level from the IRS (Internal Revenue System 2022b); and ZIP code level sociodemographic characteristics from the Census (US Census Bureau 2022b; US Census Bureau 2022a).

Guided by the theoretical model, we pose an empirical model where log rents in a location depend linearly on (1) the residence MW, defined as the log of the statutory MW at that location; (2) the workplace MW, defined as 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.[6] 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 (holding

[6] We use all ZIP codes with valid rents data as of July 2015.

constant residence MW) increases rents by 0.53 percent (SE=0.28). A 10 percent increase in the residence MW (holding constant workplace MW) decreases rents by 0.20 percent (SE=0.17). As a result, if both measures increase simultaneously by 10 percent then rents would increase by 0.34 percent instead (SE=0.16). 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 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, we show that our results are very similar when computing the workplace MW with commuting shares for different years and worker categories. Furthermore, in a specification we allow the commuting shares to vary by year, the frequency with which we observe them in the data. The fact that results are very similar should aliviate concerns that commuting patterns change as response of MW changes, biasing our results. Third, 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 match key moments of the distribution of urban ZIP codes, finding similar results as well.

Finally, in the appendix we estimate under two alternative models. We construct a "stacked" regression model that compares ZIP codes within metropolitan areas where some but not all experienced a change in the statutory MW. Results are very similar but also more noisy (as one should expect given that this model contains more fixed effects and less observations). This should alleviate concerns that our estimates actually stem from undesired comparisons, as highlighted by recent literature (de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022). The second alternative model includes the lagged first difference of rents as a control, and is estimated via instrumental variables following Arellano and Bond (1991) and Meer and West (2016). This model is valid under a weaker identification assumption, and allows that past values of rents affect current MW variables. This model also yields similar but less precise 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.[7] 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 estimating the effects of minimum wage policies on labor market outcomes (Card and Krueger 1994; Neumark, Wascher, et al. 2007; Meer and West 2016; Cengiz et al. 2019).[8] 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, Naidu, and Reich 2007; Schmitt and Rosnick 2011; 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. This yields a more plausible identification and enrichs our understanding of the estimated effects. Second, we use data at a more detailed geography and higher frequency.[9] Both of these facts enrich our understanding of the estimated effects and make the required identification assumptions more plausible. Our paper also relates to Hughes (2020) who uses a triple difference strategy to study the effect of MW policies on rent-to-income ratios. Like us, the author explicitly mentions disentangling general equilibrium effects from effects on rental markets as a motivation for his approach.Another related paper is Agarwal, Ambrose, and Diop (2021) who show that MW increases lower the probability of rental default. 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

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

[8]See Dube (2019a) and Neumark and Shirley (2021) for recent reviews of the literature.

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

the aggregate cost of housing constraints. In line with this insight, we show in our counterfactual analysis that landlords may benefit from a MW increase, eroding some of the rise in low-wage workers' income generated by the policy.

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

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

2 A Partial-Equilibrium Model

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

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 a 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 workers along residence-workplace pairs is fixed.[10] This assumption is intended as an approximation to our empirical setting where we look at the effects of MW changes at a monthly frequency. This assumption is consistent with estimates of small effects of the MW on employment, as in Cengiz et al. (2019) and Dustmann et al. (2022), and on migration, as in Pérez Pérez (2021), in a time-frame of several years.

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

Housing demand

Each group (i, z) consume square feet of living space H_{iz}, a non-tradable good produced in their residence C_{iz}^{NT}, and a tradable good C_{iz}^{T}. A representative (i, z) worker chooses between these

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

alternatives by maximizing a quasi-concave utility function $u_{iz} = u\left(H_{iz}, C_{iz}^{\text{NT}}, C_{iz}^{\text{T}}\right)$ subject to a budget constraint

$$R_i H_{iz} + P_i(\underline{W}_i) C_{iz}^{\text{NT}} + C_{iz}^{\text{T}} \leq Y_{iz}(\underline{W}_z),$$

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

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

We think that the structure of the problem and Assumption 1 are consistent with the literature. First, recent evidence by Miyauchi, Nakajima, and Redding (2021) shows that individuals tend to consume close to home. As a result, we expect them to be 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; Dube 2019b).[12]

For convenience we define the per-capita housing demand function as $h_{iz} \equiv \frac{H_{iz}}{L_{iz}}$. The solution to the worker's problem for each z then yields a set of continuously differentiable per-capita housing demand functions $\{h_{iz}(R_i, P_i, Y_z)\}_{z \in \mathcal{Z}(i)}$. First, we assume that housing is a normal good, so that housing demand is increasing in income Y_z. Standard arguments then imply that this function is decreasing in its own price R_i. Finally, we assume that housing demand is decreasing in local prices P_i. A sufficient (albeit not necessary) condition is that housing and non-tradable consumption are complements.[13] The fact that families are willing to pay higher rents for better amenities, as found by Couture et al. (2019), is consistent with this assumption.

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)—.

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

[12] An extension would allow separate wage income and business income in the budget constraint. If firm owners tend to live where they work, and MW increases damage profits (as found by Draca, Machin, and Van Reenen 2011, among others), then business income would depend negatively on the MW level.

[13] To formalize the required condition, let h_{iz} and c_{iz} denote per-capita Marshallian demands resulting from the choice problem, and $\tilde{h}_{iz}$ denote the corresponding 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.

Housing supply

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

2.2 Equilibrium and Comparative Statics

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

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

Given that the per-capita 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. First, 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? Second, under what conditions can we reduce the dimensionality of the rents function and represent the effects of MW changes on equilibrium rents in a simpler way? We start with the first question.

Proposition 1 (Comparative Statics)**.** *Consider residence ZIP code i and a change in MW policy at a larger jurisdiction such that for $z \in \mathcal{Z}_0 \subset \mathcal{Z}(i)$ binding MWs increase, and for $z' \in \mathcal{Z}(i)\backslash\mathcal{Z}_0$ 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*

(i) for $z' \in \mathcal{Z}_0\backslash\{i\}$ for which $\frac{dY_{z'}}{d\underline{W}_{z'}} > 0$, the policy has a positive partial effect on rents, $\frac{d\ln R_i}{d\ln \underline{W}_{z'}} > 0$;

(ii) the partial effect of the MW increase in i on rents is ambiguous, $\frac{d\ln R_i}{d\ln \underline{W}_i} \lesseqgtr 0$; and

(iii) as a result, the overall effect on rents is ambiguous if $i \in \mathcal{Z}_0$ and weakly positive if $i \notin \mathcal{Z}_0$.

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

$$\Big(\eta_i - \sum_z \pi_{iz}\xi_{iz}^R\Big) d\ln R_i = \sum_z \pi_{iz}\left(\xi_{iz}^P \epsilon_i^P d\ln \underline{W}_i + \xi_{iz}^Y \epsilon_z^Y d\ln \underline{W}_z\right), \tag{2}$$

where $\pi_{iz} = \frac{L_{iz}}{L_i}$ represents the share of i's residents working in z; $\xi_{iz}^x = \frac{dh_{iz}}{dx_i}\frac{x_i}{\sum_z \pi_{iz}h_{iz}}$ for $x \in \{R, P, Y\}$ is the elasticity of the per-capita housing demand evaluated at the average per-capita demand of ZIP code i; $\epsilon_i^P = \frac{dP_i}{d\underline{W}_i}\frac{W_i}{P_i}$ and $\epsilon_z^Y = \frac{dY_z}{d\underline{W}_z}\frac{W_z}{Y_z}$ are elasticities of prices and income to minimum wages; and $\eta_i = \frac{dS_i}{dR_i}\frac{R_i}{S_i}$ is the elasticity of housing supply in ZIP code i.

[14] To see this, assume that $S_i(0) - \sum_{z\in\mathcal{Z}(i)} L_{iz}h_{iz}(0, P_i, Y_z) < 0$ and apply the intermediate value theorem. Intuitively, we require that at low rental prices demand exceeds supply.

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

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

Because $\eta_i > 0$ and $\xi_{iz}^R < 0 \ \forall z \in \mathcal{Z}(i)$, the first factor is positive. Since we also assumed $\epsilon_z^Y > 0$ this effect is positive, as desired.

For ZIP code i the partial effect is given by

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

Because $\epsilon_i^P > 0$, $\xi_{iz}^P < 0 \ \forall z \in \mathcal{Z}(i)$, and $\epsilon_i^Y > 0$, then the sign of this partial effect is ambiguous. The third statement of the Proposition follows directly. □

Proposition 1 (i) shows that, if at least some low-wage worker (for whom $\frac{dY_z}{d\underline{W}_z} > 0$) commutes to a ZIP code where the MW increased, then the MW hike will tend to increase rents. Proposition 1 (ii) 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)**.** *Assume that for all ZIP code $z \in \mathcal{Z}(i)$ we have (i) homogeneous elasticity of per-capita housing demand to incomes $\{Y_z\}$, $\xi_{iz}^Y = \xi_i^Y$, and (ii) homogeneous elasticity of income to minimum wages $\{\underline{W}_z\}$, $\epsilon_z^y = \epsilon^y$. Then, 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****, defined as $\sum_{z\in\mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_z$, and ZIP code i's* ***residence MW****, defined as $\ln \underline{W}_i$. Furthermore, the workplace MW has a positive effect on rents, whereas the residence MW has a negative effect.*

Proof. Under the stated assumptions we can manipulate (2) to write

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

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

Proposition 2 shows that, under an homogeneity assumption,[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.

[15] 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 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 levels. The higher frequency of the Zillow data is an advantage since it allows us to explore the effects of MW changes on rents exploiting their precise timing.

Zillow is the leader online real estate and rental platform in the US, hosting more than 110 million homes and 170 million unique monthly users in 2019 (Zillow 2020a). Zillow provides the median rental and sale price among homes listed on the platform for different house types and at geographic and time aggregation levels (Zillow 2020b).[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 US 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. However, we show results using median rents per square foot in other rental categories available in the data as well.

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 1 shows that the sample of ZIP codes we observe in Zillow typically coincides with high population-density areas.

[16] The availability of different time series changed over time, so not all series used for the analysis might be still available to download. See Zillow (2020b) for more details on the data shared by Zillow, and Archive (2021) for a snapshot of the website as of February 2020.

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

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 distribution of rents. Appendix Figure 2 shows that these series evolve very similarly over time.

3.2 Minimum Wage

3.2.1 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.[18] We assign MW levels to USPS ZIP codes by taking the following steps. First, we collect a crosswalk constructed by (US Census Bureau 2021) that contains, for each census block, identifiers for block group, tract, county, CBSA (i.e., core-based statistical area), place (i.e., census designated place), and state. 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 7565 statutory MW changes at the ZIP code-month level. These, in turn, arise from 81 state-level and 121 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.[19]

Panel (a) shows the distribution of the intensity of the MW changes. The average percent change among Zillow ZIP codes is 5.44. 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 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.

[18] 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.

[19] There are a few cases of decreases 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.

3.2.2 The Residence and Workplace Minimum Wage Measures

In this subsection we define the minimum wage variables we use in our analysis, which follow 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 2021) for the years 2009 through 2018. We collected the datasets for "All Jobs." The data is aggregated at the census block level, and is aggregated to ZIP codes using the original correspondence between census blocks and USPS ZIP codes described in Appendix B. This results in ZIP code residence-workplace matrices that, for each location and year, tell the number of jobs of residents in every other location.

We then use the 2017 ZIP code residence-workplace matrix to build exposure weights. Let $\mathcal{Z}(i)$ be the set of ZIP codes in which i's residents work (including i). We construct the set of weights $\{\omega_{iz}\}_{z\in\mathcal{Z}(i)}$ as

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

where N_{iz} is the number of workers who reside in i and work in z, and N_i is the total working population of i. Appendix table 4 shows how our results change when we use young workers and low-income workers to construct the weights.[20] We define the workplace minimum wage measure as

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

Figure 3 illustrates the difference in these measures by plotting the change in the residence and workplace MW in the metropolian 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 4 shows the changes in our main rents variable around the same date.

3.3 Other Data Sources

3.3.1 Time-varying data

We complement the origin-destination LODES matrices with block level aggregates on residence and workplace area characteristics from LODES (US Census Bureau 2021) for the years 2009 through

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

2018. We aggregate these data to the USPS ZIP code level using the correspondence table discussed in Appendix B. While in principle these data can be constructed from aggregating the origin-destination matrices, in practice they contain counts of workers broken by more detailed categories, such as NAICS industrial aggregates and schooling levels.

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 several industrial divisions and from 2010 to 2019.[21] 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 2022b). 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.

3.3.2 ZIP code characteristics

Our sample of ZIP codes consists of those that are matched to some census block in Appendix B. While our MW assignment recognizes that many of these ZIP codes cross census geographies, we asign to each ZIP code a unique geography based on where the largest share of its houses fall. We do this for descriptive purposes and also to use geography indicator variables in our empirical models.

In order to describe our sample of ZIP codes we collect data from the the 2010 US Census (US Census Bureau 2022b), the 5-year 2007-2011 American Community Survey (ACS; US Census Bureau 2022a), and from Small Area Fair Market Rents (SAFMR; US Department of Housing and Urban Development 2020). We collect most of these data at the block level, and aggregate it to ZIP codes using the correspondence in Appendix B. Finally, we collect data on the number of workers in several income bins at the tract level. For each census tract, we observe the number of workers that in the last 12 months earned current dollar wages within certain bins.[22]

3.3.3 Locating Minimum Wage Earners

An important piece of information for our analysis requires knowledge of the residence and workplace locations of workers likely to earn the minimum wage. To compute this variable one would need to know the distribution of income by residence and workplace in each ZIP code. Unfortunately, such figures are not readily available in public data.[23] Thus, to get a sense of the spatial distribution of

[21]The QCEW covers the following industrial aggregates: “Natural resources and mining,” “Construction,” “Manufacturing,” “Trade, transportation, and utilities,” “Information,” “Financial activities” (including insurance and real state), “Professional and business services,” “Education and health services,” “Leisure and hospitality,” “Other services,” “Public Administration,” and “Unclassified.”

[22]The bin categories are: less than \$10,000, between \$10,000 and \$14,999, between \$15,000 and \$24,999, between \$25,000 and \$34,999, between \$35,000 and \$49,999, between \$50,000 and \$64,999, between \$65,000 and \$74,999, and more than \$75,000.

[23]These data cannot be constructed from usual sources. Both for the Current Population Survey (CPS) and the 5% Census samples available in IPUMS the smaller geographical unit is the PUMA (IPUMS CITE PENDING).

minimum wage earners we proceed in several ways.

To estimate of the residence location of minimum wage workers we proceed as follows. Using our assignment of hourly statutory MW in January 2011 we compute the total yearly wage of a full-time worker earning the MW in each census tract, which we denote $\underline{YW}$.[24] Then, we compute in which wage bin $\underline{YW}$ falls. We estimate the number of MW earners in a tract as the total number of workers in all bins below that one plus a fraction of the total number of workers in that bin given by $\left(\underline{YW} - b_\ell\right) / \left(b_h - b_\ell\right)$, where b_h and b_ℓ represent the upper and lower limits of the bin. We impute the tract estimates to ZIP codes proportionally to the share of houses in each tract that fall in every ZIP code the tract overlaps with.[25] Finally, we compute the share of minimum wage workers who reside in each ZIP code dividing our estimate of the number of minimum wage workers to the total number of workers in the data.

It is not possible to use the same approach to estimate the workplace location of workers. Thus, in this case we solely rely on workplace characteristics data from LODES. We exploit use the intuition that ZIP codes where, for example, many workers belong to the industry "Accommodation and Food Services" (NAICS sector 72), are likely to be the workplace location of many MW earners.

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. This panel contains 7565 MW changes, which arise from 81 state-level changes and 121 county- and city-level changes. 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. This partially balanced panel contains 5090 MW changes at the level of the ZIP code. We also construct a fully balanced panel by dropping dates before July 2015 from our baseline panel, which contains 2764 MW changes at the ZIP code level.

Table 1 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 non-missing value of rents per square foot in the SFCC category. Finally, the fourth column shows descriptive statistics for our baseline estimating sample described above. Throughout the paper we refer to this set of ZIP codes as the baseline sample.

Our baseline sample contains ZIP codes tend to be more populated, richer, with a higher share of Black and Hispanic inhabitants, 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

[24] We use the definition of full-time workers from Internal Revenue System (2022a). Specifically, we assume that a full-time employee works for 130 hours per week for 12 months.

[25] More precisely, we compute a tract to ZIP code correspondence from the LODES correspondence between block and tract, available in (US Census Bureau 2021), and the geographical match between blocks and ZIP codes in Appendix B. We compute for each tract the share of houses that fall in each ZIP code, and we assume that the share in the tract-ZIP code combination equals the share of houses times the estimated number of minimum wage workers in the tract.

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. 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 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.

For some estimations we construct analogous panels where the units of observation is the county by month and ZIP code by year. In the county by month panel we define the statutory MW in an analogous fashion as for ZIP codes, and we use Zillow data that is already aggregated at this level. We also define a baseline sample keeping counties with Zillow data as of July 2015. In the ZIP code by year panel we compute the monthly difference in the log rents and MW measures and compute their yearly averages.

4 Empirical Strategy

4.1 First-differences model

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

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

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

By taking first differences on the previous equation we obtain

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

where $\delta_t = \tilde{\delta}_t - \tilde{\delta}_{t-1}$. We estimate the model in first differences because we expect unobserved shocks to rental prices to be serially autocorrelated over time, making the levels model less efficient. Appendix Table 2 shows strong evidence of serial 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,

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

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

This assumption has two important implications. First, it implies parallel 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 often set at a much larger jurisdiction than the ZIP code and they are not usually set considering their effects on the local 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 time-period fixed effects. In some specifications we allow these trends to vary by different geographic jurisdictions. To control for variation arising from unobserved trends in local markets we include economic controls from the QCEW.[26] Specifically, we control for average weekly wage and establishment counts at the county-quarter, and for employment counts at the county-month, for the sectors "Professional and business services," "Information," and "Financial activities." We assume that these sectors are not affected by the minimum wage.[27] We also try models where we control for ZIP code-specific linear trends, which should account for time-varying

[26]These data are aggregated at the county level, and represent a second best given the unavailability of local business cycle data at the ZIP code level.

[27]In 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.

heterogeneity not controlled by our economic controls that follows a linear pattern.

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

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

In this equation s is the number of months of a symmetric window around the MW change. We use $s = 6$ as baseline but our results are very similar for windows of 3 or 9 months.

Because the MW measures are strongly correlated, adding leads and lags of both leads to a decline in precision. Thus, we try models with leads and lags of only one of the MW measures as well.

Under the assumption that there are no anticipatory effects in the housing market, we interpret 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 the focus on short windows around MW changes, the assumption of no anticipatory effects seems plausible.[28] 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.

[29]

As explained in Section 3.4, the model in (5) is estimated using a partially balanced panel. This estimation is valid if sample selection is orthogonal to treatment assignment. However, if MW levels 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 and under an unbalanced panel where we include time-period by year-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.

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

[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 $\mathbf{X}_{it}$ 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.

[29] Ideally, we would run this regression on the number of rental units. Unfortunately this information is not available in the Zillow 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).

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 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 (de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022). One 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 different jurisdictions; 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 restricted 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.

4.5 Sample Selection Concerns and Heterogeneity

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.

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:

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

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

5 Results

In this section we present our main results. First, we show our baseline estimates and conduct several robustness checks to explore the strength of our results. Second, we present results of models that use alternative empirical strategies. Third, we show heterogeneity analysis based on the residence location of MW workers and discuss other concerns that arise from the selectivity of our sample of ZIP codes. Finally, we summarize our results and compare them with existing literature.

5.1 Main Results

Table 2 displays our estimates using the baseline sample described in Section 3.4. Column (1) shows the results of a first-differenced regression of the workplace MW measure on the residence MW measure, economic controls and monthly date fixed effects. We observe that a 10 percent increase in the residence MW induces an 8.6 percent increase in the workplace MW. While the measures are strongly correlated, this model shows that this correlation is far from exact, suggesting that there is independent variation to estimate the effect of both measures on rents.

Columns (2) through (4) of Table 2 show estimates of equation (5), varying the set of included MW measures. Column (2) shows the results of estimating a model that does not include the workplace MW. In this model, only locations with a statutory MW change are assumed to experience effects, similar to much of the MW literature (e.g., Dube, Lester, and Reich 2010; Meer and West 2016; Yamagishi 2021). In this case, we estimate that a 10 percent increase in the MW is associated with a statistically significant 0.27 percent increase in median rents. Column (3) shows the results of a model that does not include the residence MW. The coefficient on the MW variable seems to increase slightly, consistent with the view that changes in the workplace MW are a better proxy of the changes in disposable income generated by MW increases. Column (4) estimates the model using both MW measures. Consistent with the theoretical model in Section 2, the coefficient on the residence MW (γ) now turns negative and equals -0.0204, although it is not statistically significant ($t = -1.21$). The coefficient on the workplace MW (β) increases to 0.0545 and is statistically significant ($t = 1.93$). We reject the hypothesis that $\gamma = \beta$ at the 10% significance level ($p = 0.096$). Finally, $\gamma + \beta$ is estimated to be 0.0342 ($t = 2.21$), very similar in magnitude of coefficients on MW variables in columns (2) and (3). Thus, our results imply that a 10 percent increase in both MW measures will increase rents by 0.34 percent. However, if only the residence MW increases then rents are expected to decline, and if only the workplace MW goes up then the rents increase will be larger.

A central concern with these results is whether our identifying assumptions is likely to hold. Figure 4 shows estimates of equation (7) under the baseline sample. Panel (a) adds leads and lags of the workplace MW measure only, so that our set of coefficients is now $\{\{\beta_s\}_{s=-6}^{-1}, \beta, \{\beta_s\}_{s=1}^{6}, \gamma\}$. We cannot reject the hypothesis that $\beta_{-6} = ... = \beta_{-1} = 0$ ($p = 0.856$). Estimates of post-event coefficients $\{\beta_s\}_{s=1}^{6}$ are also estimated to be zero. The only significant estimates are those of β and γ, and they imply larger and more significant effects of both the residence and workplace MW measures.

Our estimate of γ is now -0.0306, and is statistically significant at the 10% level ($t = -1.82$). Our estimate of β_0 equals 0.0683 and is statistically significant at the 1% level ($t = 2.52$). Furthermore, in this case we can reject the hypothesis of equality of coefficients more precisely ($p = 0.025$). Our estimate of $\gamma + \beta$ is now 0.0377 and is highly significant ($t = 2.45$), impying that a 10 percent increase in both measures would increase rents by 0.38 percent.

Panel (b) of Figure 4 shows that a similar story obtains when we add leads and lags of the residence MW only. Panel (c) of Figure 4 adds leads and lags of both MW measures. In this case, the width of confidence intervals for pre- and post-event coefficients increases between 2 to 4 times. We attribute this fact to the strong correlation of the MW measures in our panel, which implies that these coefficients are weakly identified. However, the event-period coefficients are similar to those of panels (a) and (b).

Appendix Figure 5 illustrates the identifying variation that we use by mapping the residualized workplace MW and residualized log rents.[30] Panel (a) of Appendix Figure 5, to be contrasted with Panel (a) of Figure 3, shows that residualized workplace MW is high outside of Cook County, the jurisdiction that increased the MW. Panel (b) of Appendix Figure 5 shows that residualized rents are very noisy. The correlation of these residualized variables across our sample identifies the effect of the workplace MW.

Robustness Checks

Table 4 shows how our results change when we vary the specification of the regression model and the commuting shares used to construct the workplace MW measure. Each row of the table shows estimates analogous to those of columns (1) and (4) of Table 2.

Panel A of Table 4 groups the results when varying the regression model. Row (b) shows that our results are very similar when we we exclude the economic controls from the QCEW. Rows (c) and (d) show that interacting our time fixed effects with indicators for county or CBSA yields similar results. In all these cases our baseline estimates are contained in relevant confidence intervals and, in the case of CBSA $\times$ monthly date fixed effects, the results seem even larger. This supports the view that our results are not caused by regional trends in housing markets correlated with our MW variables. Row (e) shows that the results are different when using state $\times$ monthly date fixed effects. While our baseline estimates are within relevant confidence intervals, the signs of the point estimates are flipped. We think that within-state comparisons are not appropriate because they fully identify coefficients from local MW changes which, in turn, are more likely to be passed by cities or counties that have more dynamic rental markets. On the other hand, within-CBSA and within-county comparisons use ZIP codes that are likely to experience similar trends in rental markets. Row (f) includes ZIP code fixed effects in the first-differenced model, which is equivalent to allowing for a linear trend in the model in levels. The fact that our results are very similar implies that potential ZIP code level linear trends correlated with MW changes are unlikely to be the cause of our results.

Panel B of Table 4 estimates the baseline model but computing the workplace MW using alternative commuting structures. Rows (g) and (h) uses commuting shares from 2014 or 2018 instead

[30] To maximize the number of ZIP codes with valid data on this map we use the results of the unbalanced panel discussed in Section 5.3.

of 2017 as the baseline estimates. Row (i) allows the commuting shares to vary by year, introducing additional cross-year variation in the workplace MW measure that does not arise from changes in the statutory MW. The fact that these specifications yield very similar results suggests that changes in commuting correlated to MW changes are unlikely to be the driver of the results. Rows (j) and (l) use 2017 commuting shares for workers that earn less than $1,251 per month and workers that are less than 29, respectively. If anything, the results seem to be stronger in this case, consistent with the idea that these workers are more likely to earn close to the minimum wage.

Alternative Rental Categories

Appendix Table 5 shows how our results change when we use other rental categories available in the Zillow data. The sample in each row is constructed starting from the baseline sample used in Table 2 and keeping only ZIP codes that have valid data in the given rental category. We note that the number of observations varies widely and is always much lower than in our baseline.

Given the reduced precision of these estimates is hard to obtain strong conclusions on what type of housing is reacting more strongly to MW changes. Results in the categories "Condominium and Cooperative Houses" and "3 bedroom" resemble our baseline estimates the most. We observe inconsistent results for the category "1 bedroom" where the sign of the coefficients is flipped relative to baseline. However, these are not statistically significant.

Other geographies and time frames

In this subsection, we compare our results with estimates obtained from alternative panels where the unit of observation is either the county by month or the ZIP code by year. The reason to show these results is twofold. First, it allows us to emphasize how important is the ZIP code and monthly resolution of our data for the plausibility of our identification assumption. Namely, that the no pre-trends assumption at the ZIP code level is more plausible than at the county level. Second, it allows us to compare our results with the previous literature estimating the effects of MW on housing rents. Because none of the previous papers distinguish between workplace and residence MW measures, we compare them to our short model that regresses log rents on the workplace MW only. The results for each dataset are summarized in Appendix Table 6, where Panel A repeats the results in Table 2 for convenience.

Panel B of Appendix Table 6 shows our results based on a county by month panel. Overall, the results are similar in magnitude to our baseline but are not statistically significant. In Appendix Figure 6 we extend the model that includes both MW measures adding leads and lags of the workplace MW, as in panel (a) of Figure 4. We observe considerable pre-trends in the rental prices in this model, suggesting that estimates obtained at a larger geographical resolution may not use plausibly exogenous identifying variation.

Panel C of Appendix Table 6 show results estimated using a ZIP code by year panel. We estimate models that are yearly averages of their monthly equivalents, so in principle they should be valid under the same identifying assumption. However, in practice we find that estimates are very imprecise, with standard errors 3 to 4 times larger. Our rental changes occur right at the month of the MW change, thus using yearly variation lacks the power to detect them. Therefore, the usage of monthly date appears central to estimate the effect of MW changes on rents.

5.2 Alternative Strategies

Appendix Table 3 estimates our main models using a "stacked" sample, as discussed in Section 4.4. Our sample contains 618 "events," that is, CBSA-month pairs that had some strict subset of ZIP codes increasing the residence MW. These estimates interact the year-month fixed effects with event-id indicators, and thus compare ZIP codes within the same CBSA. This is in line with recent difference-in-differences literature that focuses on carefully selecting the comparison groups (Callaway, Goodman-Bacon, and Sant'Anna 2021; de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022). Relative to Table 2 this model has less observations and includes many more fixed effects. Nevertheless, we find very similar albeit less precise results. We reject the hypothesis of equality of the residence and workplace MW at the 10% significance level. In this case, a 10 percent increase in both MW measures is estimated to increase rents by 0.375 percent, in line with the results of the previous subsection.

Appendix Table 4 shows estimates of a model that includes the lagged difference in log rents as a covariate. This specification relaxes the strict exogeneity assumption and allows for feedback effects of rent increases on the minimum wage variables. To avoid the endogeneity problem of including this covariate the models are estimated using an IV strategy where we instrument the first lag of the change in rents with the second lag of this variable (Arellano and Bond 1991; Arellano and Honoré 2001). This estimation strategy also yields very similar but less precise results when compared to our baeline in the previous subsection.

5.3 Sample Selection Concerns and Heterogeneity

Table 3 explores the senstivity of our estimates to the sample of ZIP codes used in estimation. Columns (1), (3), and (5) use our baseline sample of ZIP codes, a fully-balanced sample dropping all data prior to July 2015, and an unbalanced sample of ZIP codes where we control for year-of-entry by year-month fixed effects. While the coefficient on the residence MW is very stable across specifications, the one on the workplace MW seems to increase slightly when using the fully-balanced sample, and to decrease slightly under the unbalanced sample. Our baseline sample achieves more precision at the cost of allowing the composition of ZIP codes to change before 2015. We also worry that our ZIP codes might be a selected sample in ways that affect our estimated effects. In columns (2), (4), and (6) we estimate the same models but reweighting observations to match relevant characteristics of the sample of urban ZIP codes.[31] The re-weighting seems not to affect the estimated coefficients.

Table XX shows heterogeneity...

[31] In particular, samples are reweighted to match the share of renter households, the estimated share of MW residents, the share of workers aged less then 29, and the share of workers in the "Accomodation and Food Services" industry.

5.4 Summary and Discussion

Faced with an increase of the statutory MW at some jurisdiction, our results indicate that its spatial effect across rental markets will be determined by its incidence on each of the MW measures. Consistent with the theoretical model in Section 2, we find that increases in the MW at the residence tend to lower rents, whereas increases in the MW at workplace locations tend to increase rents. Our estimates appear robust to several specification tests. Furthermore, the magnitude of our estimates is similar to estimates of the elasticity of restaurant prices to the MW (Allegretto and Reich 2018), and the elasticity of grocery store prices to the MW (Renkin, Montialoux, and Siegenthaler 2020; Leung 2021).

We compare our estimates with those in Yamagishi (2019) and Agarwal, Ambrose, and Diop (2021). Using Fair Market Rents data at the county by year level, Yamagishi (2019, Tables 1 and 2) use a long-differences specification and obtains null results using all counties and statistically significant results using densely populated counties. In the latter case, he reports that a 10 percent increase in the MW increases rents by 0.0365 percent in the first year, and 0.1059 percent four years later. Our ZIP code-level estimates using only the workplace MW imply a one-time increase in rents of a similar magnitude as Yamagishi's (2019) one-year estimates. While our results are consistent in this sense, (Yamagishi 2019, table 3) detects significant pre-trends, questioning the validity of the longer-run results.[32]

Our results are consistent with Agarwal, Ambrose, and Diop (2021). While the main goal of the paper is to estimate the effect of the MW on eviction risk, the authors provide estimates of the effect of the MW on rents using individual level transactions from 2000 to 2009. Agarwal, Ambrose, and Diop (2021, Figure 4) suggests that a 10 percent hike in the MW (at residence) increases rents paid by individuals by 0.5 percent. The authors estimate an increasing that fully shows itself after 6 months. This result is consistent with our estimates that show how rents of housing units in the rental market (which we observe in the Zillow data) jump discreetly on the month of the MW change.

6 Counterfactual Analysis

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

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

Discuss policy implications.

[32] Tidemann (2018) uses the same data at the state level and reports the paradoxical result that MW hikes decrease monthly rents. Yamagishi (2019, Appendix C.1.3.) compares his results with Tidemann (2018) and concludes that for densely populated areas Tidemann's result turns positive and that clustering the standard errors at the state level renders his results insignificant.

7 Conclusions

In this paper, we ask whether minimum wage changes affect housing rental prices. To answer this question we develop a theoretical approach that accounts for the fact that MW workers typically reside and work in different locations. We show in a partial-equilibrium model that one should expect different effects on rental prices in a given location depending on whether they arise from the residence or the workplace location of its residents.

We collect data on rents, minimum wage levels and commuting patterns and estimate the effect of residence and workplace MW on rents. 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. We find evidence supporting the main conclusions of our model: the effect of the MW is larger when it arises from the workplace. Minimum wage changes appear to spill over spatially through commuting.

Our results suggest that homeowners pocket some portion of the increase in income of some workers generated by the MW. The omission of this channel leads to an overstatement of the equalizing effects of the MW on disposable income.

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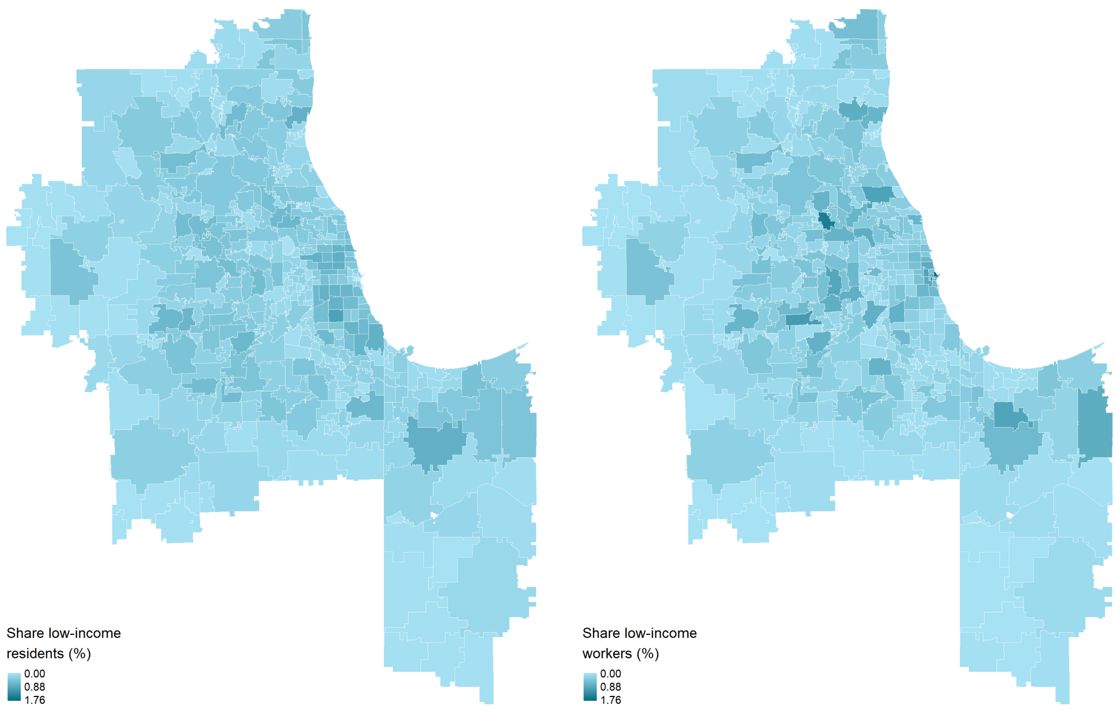
Figures and Tables

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

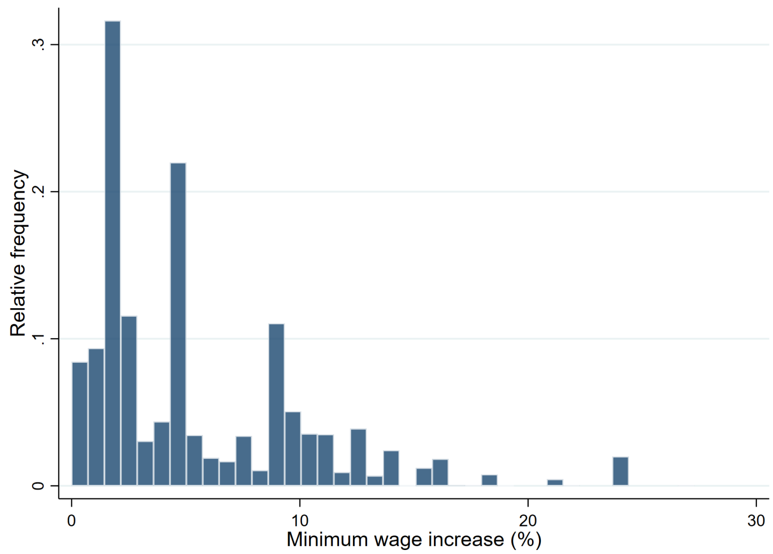
(a) Residents (b) Workers



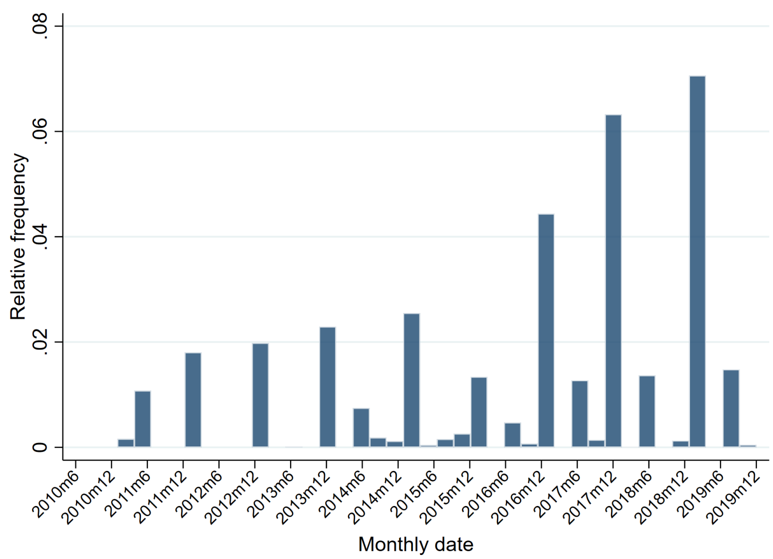
Notes: Data are from LODES (US Census Bureau 2021) aggregated at the ZIP code level using the procedure described in Section 3.2. The figure shows the share of low-income residents and workers out of the CBSA total in each ZIP code. Low-income workers are defined as those earning less than $1,251 per month.

Figure 2: Distribution of Minimum Wage Changes

(a) Intensity



(b) Timing

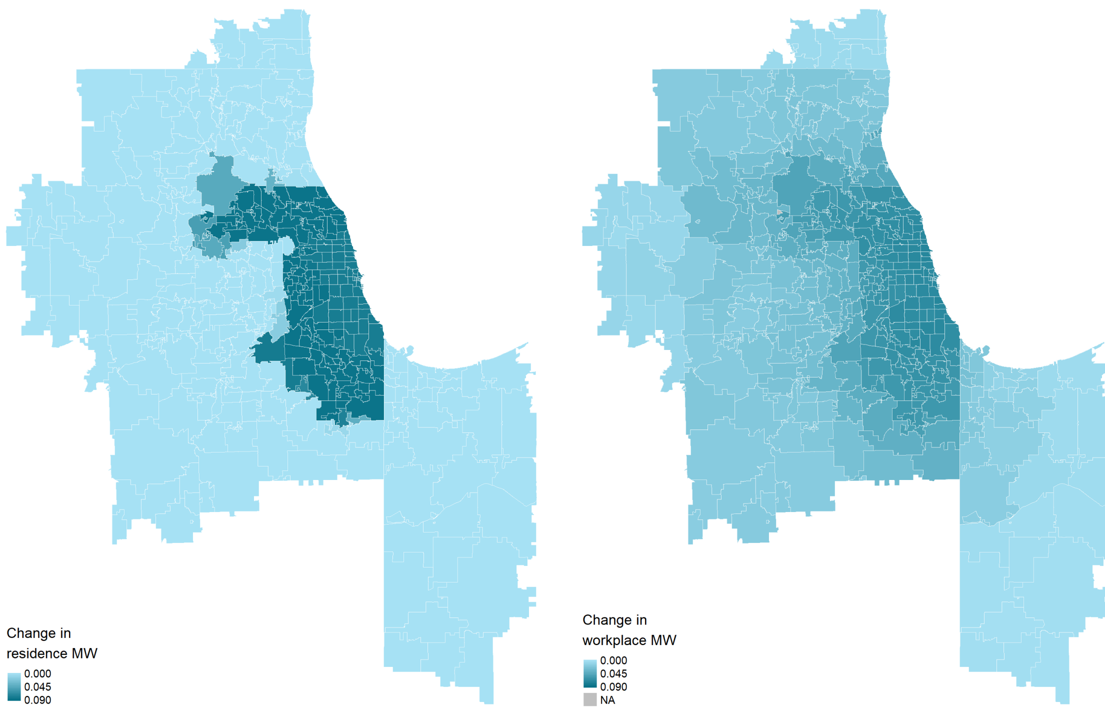


Notes: The histograms show the distribution of positive MW changes in the full sample of ZIP codes available in the Zillow data. Panel (a) reports the intensity of the changes in percentage terms. Panel (b) plots the distribution of such changes over time.

Figure 3: Changes in MW measures in the Chicago-Naperville-Elgin CBSA, July 2019

(a) Residence MW

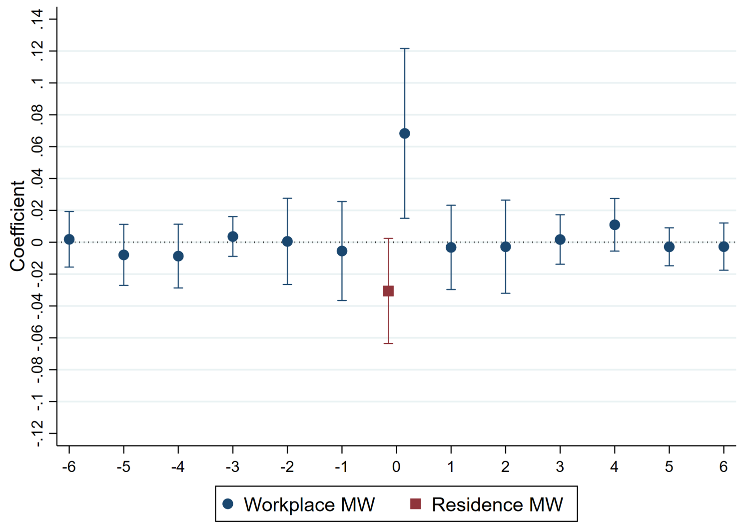
(b) Workplace MW



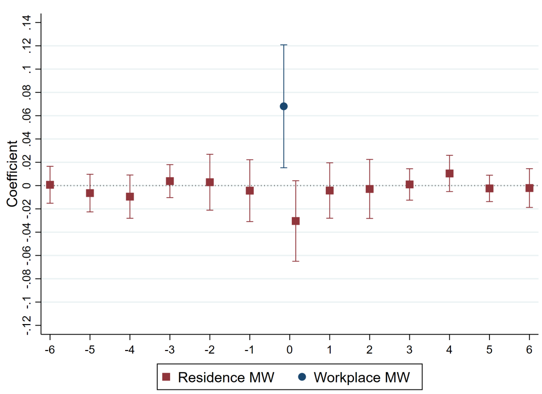
Notes: Data are from the MW panel described in Section 3.2 and from LODES (US Census Bureau 2021). The figure shows the change in the residence MW (panel a) and workplace MW (panel b) on July 2019 in the metropolitan area of Chicago. The residence MW is defined as the log of the statutory MW of the given 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.

Figure 4: Estimates of the effect of the MW on rents, baseline sample including leads and lags

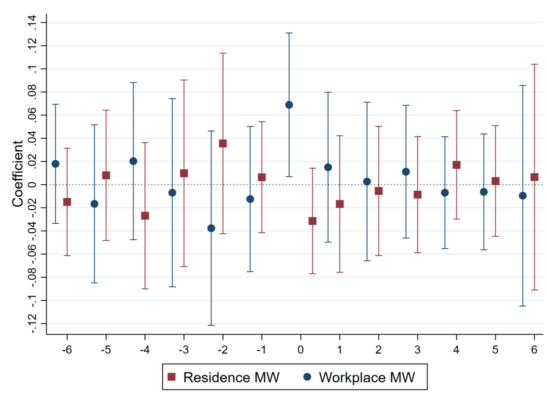
(a) Leads and lags of the workplace MW



(b) Leads and lags of the residence MW



(c) Leads and lags of both wkp. and res. MW



Notes: Data are from the baseline estimation sample described in Section 3.4. All panels plot coefficients from regressions of the log of rents per square foot on the residence and workplace MW measures, varying the number of leads and lags of each MW variable included. Panel (a) includes six leads and lags of the workplace MW measure. Panel (b) includes six leads and lags of the residence MW measure. Panel (c) includes six leads and lags of both MW measures. All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county and month levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the log statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors “Information,” “Financial activities,” and “Professional and business services.” 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

Table 1: Descriptive statistics of different samples of ZIP codes

	All ZIP codes	Urban ZIP codes	Zillow sample	Baseline sample
Panel A: 2010 Census				
Total population, thousands	308,129.6	218,568.3	111,709.2	51,181.1
Total number of households, thousands	131,396.0	90,781.9	47,424.5	21,628.7
Mean population	9,681.7	22,626.1	33,687.9	38,052.9
Mean number of households	4,128.6	9,397.7	14,301.7	16,080.8
Share urban population	0.391	0.967	0.960	0.972
Share renter households	0.224	0.341	0.340	0.333
Share black population	0.075	0.133	0.153	0.161
Share white population	0.834	0.719	0.679	0.667
Panel B: 2010 IRS				
Share households with wage income	0.822	0.834	0.833	0.840
Share households with business income	0.159	0.152	0.166	0.170
Mean AGI per household, thousand $	53.2	65.2	70.3	71.2
Mean wage income per household, thousand $	36.2	44.3	47.5	49.3
Panel C: 2012 SAFMR				
Mean 40th perc. 2BR apt. rent ($)	887.23	995.82	1,039.50	1,076.72
Panel D: Minimum wage				
Min in Feb. 2010, $	7.25	7.25	7.25	7.25
Mean in Feb. 2010, $	7.41	7.47	7.48	7.43
Max in Dec. 2010, $	10.10	10.10	10.10	9.79
Min in Dec. 2019, $	7.25	7.25	7.25	7.25
Mean in Dec. 2019, $	8.83	9.47	9.38	9.21
Max in Feb. 2019 $	16.00	16.00	16.00	16.00
Panel E: Geographies				
Number of ZIP codes	31,826	9,660	3,316	1,345
Number of counties	3,135	1,176	487	244
Number of states	51	50	49	41

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, where we define a ZIP code as urban if at least 80% of its population was classified as urban by the 2010 US Census (US Census Bureau 2022b). 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)–(g) are constructed from the 2010 US Census (US Census Bureau 2022b). Rows (h)–(k) are obtained from 2011 IRS ZIP code-level statiscs (Internal Revenue System 2022b). AGI is an acronym for Average Gross Income. Row (l) is obtained from SAFMR (US Department of Housing and Urban Development 2020). Rows (m)–(s) are computed using the panel of minimum wage levels described in Section 3.2.

Table 2: Estimates of the effect of the MW on rents, baseline sample

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}		
	(1)	(2)	(3)	(4)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	0.8648	0.0268		-0.0204
	(0.0300)	(0.0135)		(0.0169)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$			0.0326	0.0545
			(0.0151)	(0.0283)
Sum of coefficients				0.0342
				(0.0155)
County-quarter economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0965
R-squared	0.9467	0.0209	0.0209	0.0209
Observations	131,383	131,383	131,383	131,383

Notes: Data are from the baseline estimation sample described in Section 3.4. Column (1) shows the results of a regression of the workplace MW measure on the residence MW measure. Column (2) through (4) show the results of regressions of the log of median rents per square foot on our MW-based measures. All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county and month levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the log of the average wage, the log of employment, and the log of the establishment count from the sectors “Information”, “Financial activities”, and “Professional and business services”. Standard errors in parenthesis are clustered at the state level.

Table 3: Estimates of the effect of the MW on rents, different samples

	Change log rents Δr_{it}					
	Baseline (1)	Baseline Reweighted (2)	Fully-balanced (3)	Fully-balanced Reweighted (4)	Unbalanced (5)	Unbalanced Reweighted (6)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0204	-0.0243	-0.0194	-0.0213	-0.0240	-0.0209
	(0.0169)	(0.0183)	(0.0198)	(0.0214)	(0.0200)	(0.0215)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0545	0.0559	0.0678	0.0674	0.0461	0.0411
	(0.0283)	(0.0278)	(0.0308)	(0.0296)	(0.0305)	(0.0300)
P-value equality	0.0965	0.0819	0.0826	0.0813	0.1586	0.2193
R-squared	0.0209	0.0204	0.0216	0.0207	0.0160	0.0160
Observations	131,383	122,984	78,912	73,787	193,292	181,933

Notes: Data are from Zillow (Zillow 2020b), the statutory MW panel described in Section 3.2, LODES origin-destination statistics (US Census Bureau 2021), and the QCEW (US Bureau of Labor Statistics 2020). Every column show the results of regressions of the log of median rents per square foot on our MW-based measures. All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county and month levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. Columns (1) and (2) use our baseline sample defined in Section 3.4. Columns (3) and (4) use a fully-balanced panel that, starting from the baseline panel, drops all data before July 2015. Column (5) and (6) use the unbalanced sample of all ZIP codes with Zillow rents data at any point in time, and controls for year of entry to the panel $\times$ year-month fixed effects. Even numbered columns re-weight observations so that the sample of ZIP codes in the data matches the averages of the set of urban ZIP codes in the following variables: share of renter-occupied households (US Census 2010), share of minimum wage residents in 2011 (computed as in Section 3.3.3), share of workers in the "Accommodation and Food Services" industry in 2013 (NAICS sector 72), and share of workers under 29 years old in 2013. Weights for each sample are computed following Hainmueller (2012). We define urban ZIP codes as in the second column of Table 1.

Table 4: Estimates of the effect of the MW on rents, robustness

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}			
	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Sum of coefficients	N
(a) Baseline	0.8648	-0.0204	0.0545	0.0342	131,383
	(0.0300)	(0.0169)	(0.0283)	(0.0155)	
Panel A: Vary specification					
(b) No controls	0.8656	-0.0184	0.0523	0.0339	132,255
	(0.0299)	(0.0173)	(0.0288)	(0.0159)	
(c) County $\times$ time FE	0.2851	-0.0256	0.0315	0.0059	123,674
	(0.0397)	(0.0399)	(0.0604)	(0.0500)	
(d) CBSA $\times$ time FE	0.5073	-0.0373	0.0908	0.0535	128,240
	(0.0341)	(0.0289)	(0.0592)	(0.0321)	
(e) State $\times$ time FE	0.5284	0.0135	-0.0175	-0.0040	131,726
	(0.0591)	(0.0213)	(0.0444)	(0.0268)	
(f) ZIP code-specific linear trend	0.8628	-0.0216	0.0561	0.0345	131,383
	(0.0308)	(0.0157)	(0.0272)	(0.0147)	
Panel B: Vary workplace MW measure					
(g) 2014 commuting shares	0.8658	-0.0202	0.0542	0.0341	131,383
	(0.0309)	(0.0186)	(0.0296)	(0.0154)	
(h) 2018 commuting shares	0.8660	-0.0209	0.0550	0.0342	131,383
	(0.0305)	(0.0175)	(0.0293)	(0.0155)	
(i) Time-varying commuting shares	0.8794	-0.0286	0.0649	0.0362	115,378
	(0.0303)	(0.0195)	(0.0301)	(0.0161)	
(j) 2017 commuting shares, low-income workers	0.8588	-0.0296	0.0656	0.0360	131,383
	(0.0297)	(0.0207)	(0.0339)	(0.0161)	
(l) 2017 commuting shares, young workers	0.8615	-0.0312	0.0673	0.0361	131,383
	(0.0308)	(0.0167)	(0.0290)	(0.0155)	

Notes: Data are from the baseline estimation sample described in Section 3.4. Each row of the table shows two estimations on the same sample of ZIP codes and months. The first column shows the results of a regression of the change in the workplace MW measure on the change in the residence MW measure. The second through fourth columns show the results of a regression of the change in log rents on the change in the residence MW and the workplace MW, with the fifth column showing the sum of the coefficients on the MW measures. The rents variable corresponds to the median rent per square foot in the Zillow data. Row (a) repeats the results of Table 2, including fixed effects for each year month and economic controls at the county $\times$ quarter level. Specifications in Panel A vary the set of fixed effects included in the regression relative to row (a). Row (f) includes ZIP code fixed effects in the first-differenced model, which in the level model can be interpreted as a ZIP-code specific linear trend. Specifications in Panel B vary the commuting shares used to construct the workplace MW measure relative to row (a). Standard errors in parenthesis are clustered at the state level.

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

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

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

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

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

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

Within-period equilibrium

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

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

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

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

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

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

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 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).[34] 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.

[34] 545,566 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:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

D Additional Tables and Figures

Appendix Table 1: Summary statistics of baseline panel

	N	Mean	St. Dev.	Min	Max
Minimum wage variables:					
Statutory MW $\underline{W}_{it}$	133,600	8.20	1.34	7.25	16.00
Residence MW $\underline{w}_{it}^{\text{res}}$	133,600	2.09	0.14	1.98	2.77
Workplace MW $\underline{w}_{it}^{\text{wkp}}$	133,600	2.09	0.14	1.98	2.69
Median Rents:					
SFCC	114,525	1,658.78	822.41	595.00	30,000.00
SFCC per sqft.	133,600	1.23	0.90	0.42	22.20
Log(SFCC per sqft.)	133,600	0.08	0.46	-0.87	3.10
Log(single family (SF) per sqft.)	99,783	-0.04	0.37	-0.87	3.10
Log(condo/cooperatives (CC) per sqft.)	24,735	0.63	0.47	-0.54	1.92
Log(Studio per sqft.)	14,545	0.31	0.75	-0.85	1.97
Log(1 Bedroom per sqft.)	25,947	0.73	0.40	-0.31	1.90
Log(2 Bedroom per sqft.)	43,770	0.43	0.43	-0.69	1.87
Log(3 Bedroom per sqft.)	48,672	0.05	0.39	-0.68	1.87
Log(multifamily 5+ units per sqft.)	55,640	0.44	0.44	-0.67	1.90
Economic controls:					
Avg. wage Business services	133,600	11.15	1.36	5.80	13.39
Employment Business services	133,600	8.69	1.24	4.13	10.96
Estab. count Business services	133,600	7.09	0.31	5.42	8.43
Avg. wage Financial services	132,909	9.00	1.53	2.40	12.39
Employment Financial services	133,600	6.11	1.33	1.39	9.53
Estab. count Financial services	132,909	7.27	0.34	5.89	8.91
Avg. wage Information services	133,576	10.20	1.42	4.75	12.90
Employment Information services	133,600	7.98	1.20	3.50	10.34
Estab. count Information services	133,576	7.25	0.37	6.22	9.16

Notes: This table shows summary statistics of the panel of ZIP codes used in our baseline results, constructed as explained in Section 3.4. We exclude rental categories with less than 10,000 non-missing ZIP code by month observations. Excluded categories are “4 Bedroom,” “5 bedroom,” and “Duplex and triplex.”

Appendix Table 2: Autocorrelation

	Log rents	
	Levels//(1)	First Differences//(2)
Residence minimum wage	0.0862	-0.0204
	(0.2144)	(0.0169)
Workplace minimum wage	-0.0660	0.0545
	(0.2159)	(0.0283)
County-quarter economic controls	Yes	Yes
P-value autocorrelation test		< 0.0001
R-squared	0.9889	0.0209
Observations	132,897	131,383

Notes: Data are from the baseline estimation sample described in Section 3.4. Both columns report the results of regressions of the log of median rents per square foot on our MW-based measures. Column (1) presents estimates of a model in levels, including ZIP code and year-month fixed effects. Column (2), presents estimates of a model in first differences, including year-month fixed effects (note that the ZIP code fixed effect drops out). For the model in first differences, we also report the results of an AR(1) auto-correlation test. We proceed as in (Wooldridge 2010, Section 10.6.3). First, we compute the residuals of the model estimated in column (2), and we regress those residuals on their lag. Let the auto-correlation coefficient of this model be ρ. The model in levels is efficient assuming no auto-correlation in the error term, which would imply that the residuals of the first-differenced model are auto-correlated with $\rho = -0.5$. The row "P-value autocorrelation test" reports the p-value of a Wald test of that hypothesis.

Appendix Table 3: Estimates of the effect of the MW on rents including one lag of the dependent variable, stacked sample

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change in log rents Δr_{it}		
	(1)	(2)	(3)	(4)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	0.5331	0.0076		-0.0265
	(0.0337)	(0.0129)		(0.0204)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$			0.0237	0.0639
			(0.0226)	(0.0349)
Sum of coefficients				0.0375
				(0.0238)
County-quarter economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0932
R-squared	0.9769	0.0817	0.0817	0.0817
Observations	110,239	110,239	110,239	110,239

Notes: Data are from Zillow (Zillow 2020b), the statutory MW panel described in Section 3.2, LODES origin-destination statistics (US Census Bureau 2021), and the QCEW (US Bureau of Labor Statistics 2020). The table mimicks the estimates in Table 2 using a "stacked" sample. To construct the sample we proceed as follows. First, we define a CBSA-month as treated if in that month there is at least one ZIP code that had a change in the binding MW. For each of the selected CBSA-months we assign a unique event ID. Second, for each event we take a window $w = 6$, and we keep all months within that window for the ZIP codes that belong to the treated CBSA. If a ZIP code has missing data for some month within the window, we drop the entire ZIP code from the respective event. For each column, we estimate the same regression as the analogous column in Table 2 but include event indicators $\times$ year-month fixed effects.

Appendix Table 4: Estimates of the effect of the MW on rents including one lag of the dependent variable, baseline sample

	Change log rents Δr_{it}	
	Baseline (1)	Arellano-Bond (2)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0204	-0.0254
	(0.0169)	(0.0220)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0545	0.0623
	(0.0283)	(0.0375)
Lagged change log rents $\Delta r_{i,t-1}$		0.4381
		(0.0184)
County-quarter economic controls	Yes	Yes
P-value equality	0.0965	0.1391
Observations	131,383	128,718

Notes: Data are from the baseline estimation sample described in Section 3.4. Both columns show the results of regressions of the log of median rents per square foot on our MW-based measures. Column (1) repeats the results of Column (4) in Table 2. Column (2) extends the estimate of Column (2) including the lagged change in log rents as a control, and is estimated using an instrumental variables strategy that uses the second lag of rents as an instrument for the first lag following Arellano and Bond (1991). All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county and month levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the log of the average wage, the log of employment, and the log of the establishment count from the sectors "Information", "Financial activities", and "Professional and business services". Standard errors in parenthesis are clustered at the state level.

Appendix Table 5: Comparison of estimates of the effect of the MW on rents, different Zillow categories

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}			
	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Sum of coefficients	N
(a) Baseline (SFCC)	0.8648	-0.0204	0.0545	0.0342	131,383
	(0.0300)	(0.0169)	(0.0283)	(0.0155)	
(b) Single family (SF)	0.8766	0.0083	0.0321	0.0404	97,808
	(0.0358)	(0.0469)	(0.0531)	(0.0129)	
(c) Condo/Cooperatives (CC)	0.7955	-0.0319	0.0961	0.0642	24,315
	(0.0426)	(0.0277)	(0.0532)	(0.0285)	
(d) Studio	0.8311	-0.0474	0.0422	-0.0052	14,290
	(0.0445)	(0.0437)	(0.0549)	(0.0258)	
(d) 1 Bedroom	0.7773	0.0318	-0.0460	-0.0142	25,277
	(0.0467)	(0.0248)	(0.0435)	(0.0218)	
(e) 2 Bedroom	0.8035	-0.0110	0.0024	-0.0086	42,732
	(0.0335)	(0.0391)	(0.0495)	(0.0183)	
(f) 3 Bedroom	0.7938	-0.1008	0.1471	0.0464	47,426
	(0.0482)	(0.0396)	(0.0663)	(0.0389)	
(g) Multifamily 5+ units	0.8230	0.0053	0.0038	0.0091	54,520
	(0.0317)	(0.0250)	(0.0373)	(0.0174)	

Notes: Data are from the baseline estimation sample described in Section 3.4. Each row of the table shows two estimations on the same sample of ZIP codes and months. The first column shows the results of a regression of the change in the workplace MW measure on the change in the residence MW measure. The second through fourth columns show the results of a regression of the change in log rents on the change in the residence MW and the workplace MW, with the fifth column showing the sum of the coefficients on the MW measures. All rent variables correspond to the median per square foot rent in a Zillow category. All estimated regressions include fixed effects for each year-month and economic controls at the county $\times$ quarter level. Row (a) repeats the results of Table 2, using the Single Family, Condominium and Cooperative Houses category. Rows (b) through (g) estimate the same regression using an analogous rent variable for different Zillow categories. Excluded categories are "4 Bedroom", "5 bedroom", and "Duplex and triplex".

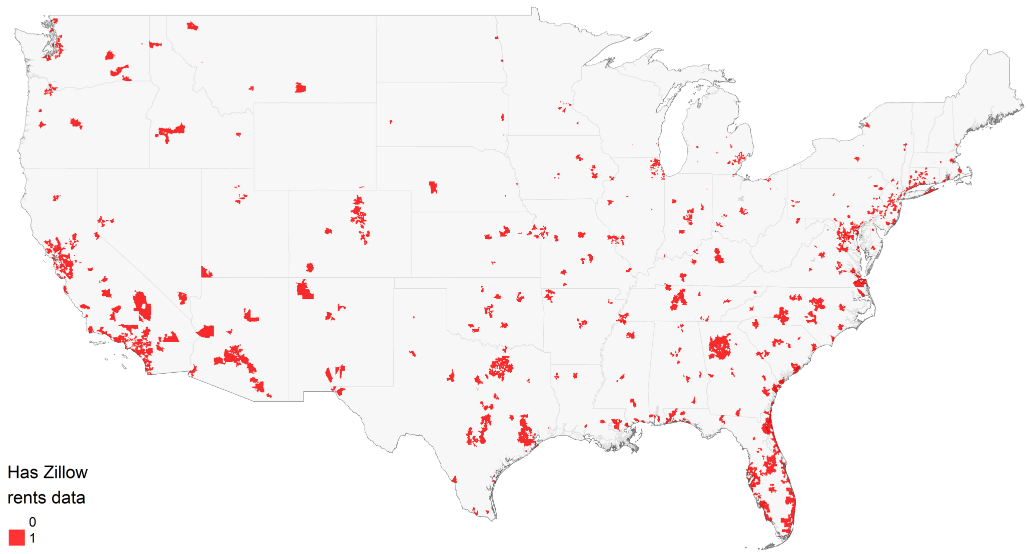
Appendix Table 6: Comparison of estimates of the effect of the MW on rents across geographies and time frames

	Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}			
	Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Sum of coefficients	N
Panel A: Baseline (ZIP code × Month)					
(i) Residence MW only		0.0268			131,383
		(0.0135)			
(ii) Workplace MW only			0.0326		131,383
			(0.0151)		
(iii) Both residence and workplace MW	0.8648	-0.0204	0.0545	0.0342	131,383
	(0.0300)	(0.0169)	(0.0283)	(0.0155)	
Panel B: County × Month					
(i) Residence MW only		0.0177			47,938
		(0.0169)			
(ii) Workplace MW only			0.0227		47,938
			(0.0193)		
(iii) Both residence and workplace MW	0.8820	-0.0406	0.0661	0.0255	47,938
	(0.0204)	(0.0279)	(0.0371)	(0.0196)	
Panel C: ZIP code × Year					
(i) Residence MW only		0.0172			11,071
		(0.0476)			
(ii) Workplace MW only			0.0178		11,071
			(0.0517)		
(iii) Both residence and workplace MW	0.8999	0.0238	-0.0074	0.0164	11,071
	(0.0217)	(0.0980)	(0.1087)	(0.0514)	

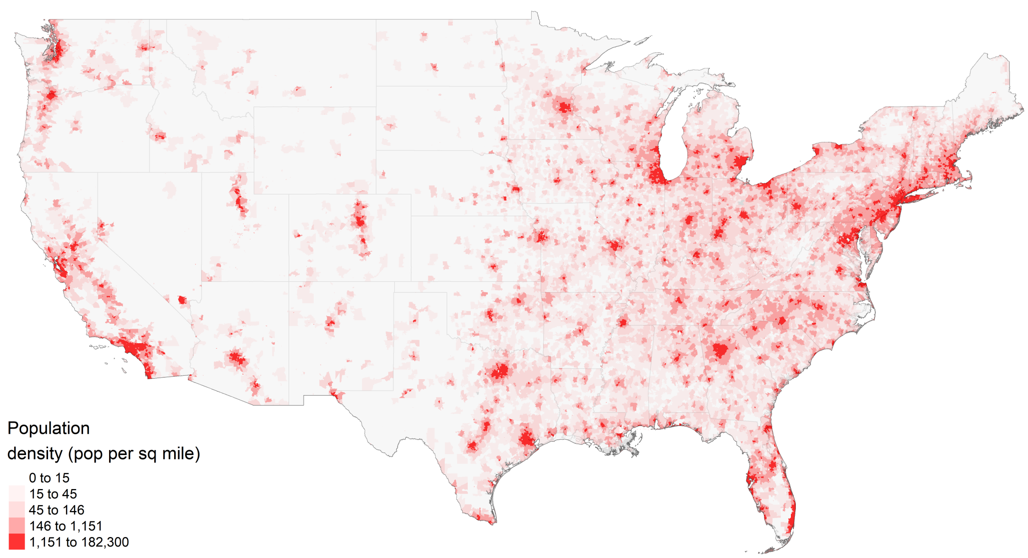
Notes: Data are from the baseline estimation sample described in Section 3.4, where we select ZIP codes and counties based on whether they had non-missing values of median rents per square foot in the SFCC category in Zillow as of July 2015. The first column and rows labeled (iii) show the results of a regression of the change in the workplace MW measure on the change in the residence MW measure. The second through fourth columns show the results of regressions of the change in log rents on either the change in the residence MW—rows (i)— or the workplace MW—rows (ii)— or both—rows (iii)—, with the fifth column showing the sum of the coefficients on the MW measures. The last column shows the number of observations, fixed within each row. All regresions include economic controls from the QCEW, as defined in Table 2. Regressions estimated at a yearly frequency use the yearly average of the change in the MW measures and the change in the economic controls. Panel A repeats our baseline results from Table 2, where the unit of observation is the ZIP code × month. Panel B shows results for a panel where the unit of observation is the county × month. Panel C shows results for a panel where the unit of observation is the ZIP code × year. In all panels, (i) displays the results of a regression of the change in log rents on the residence MW only; (ii) displays the results of a regression of the change in log rents on the workplace MW only; and (iii) displays the results of a regression of the change in workplace MW on the change in residence MW (column 1), and of the change in log rents on both MW measures (columns 2–5). Standard errors in parenthesis are clustered at the state level.

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

(a) Zillow ZIP codes

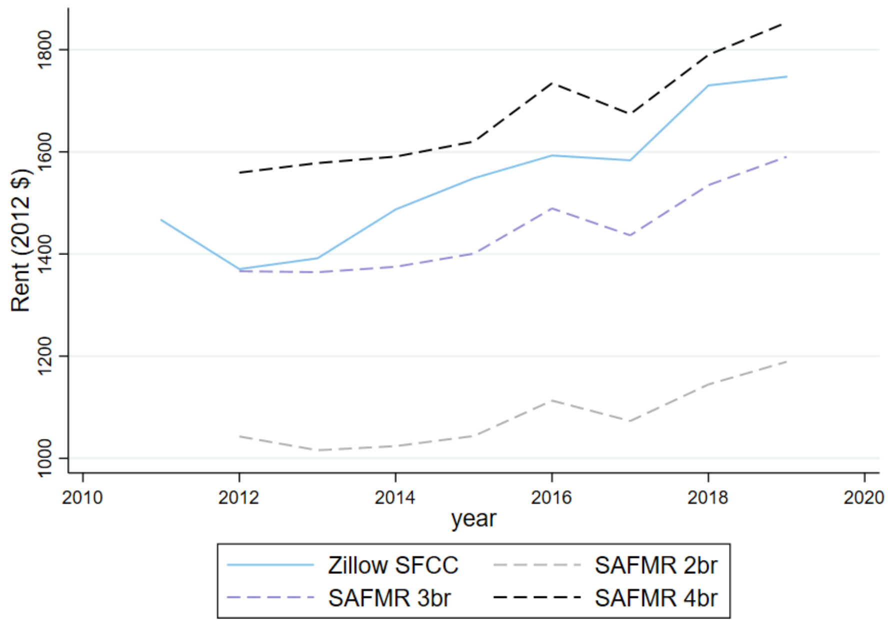


(b) Population Density



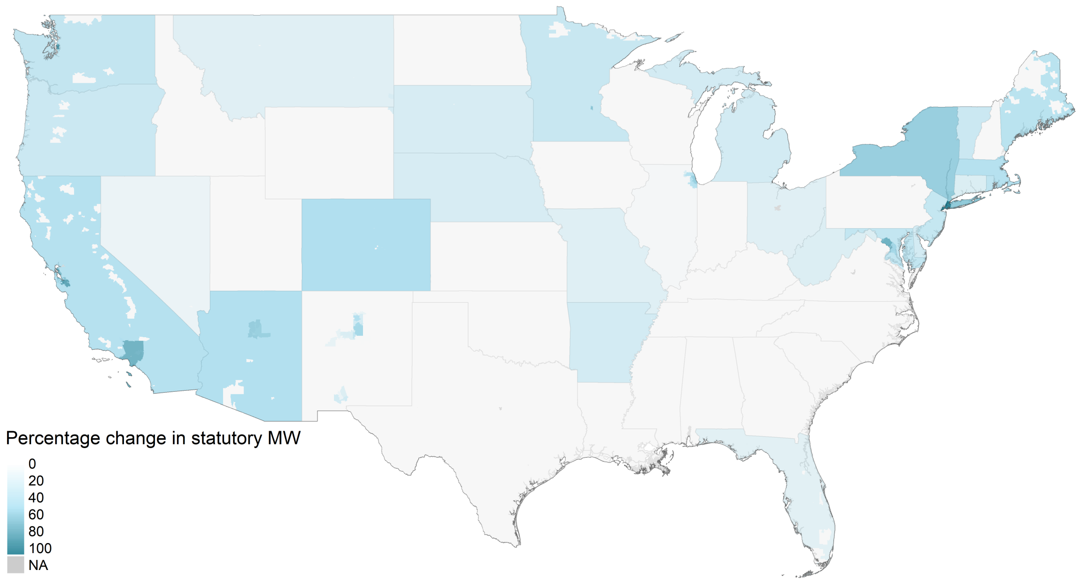
Notes: Data are from Zillow (2020b) and ESRI (2020). The figure compares the USPS ZIP codes available in Zillow to the population density. Panel (a) shows the sample of the ZIP codes that have rents data in the SFCC category at any point in the period 2010–2019. Panel (b) shows quintiles of population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

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



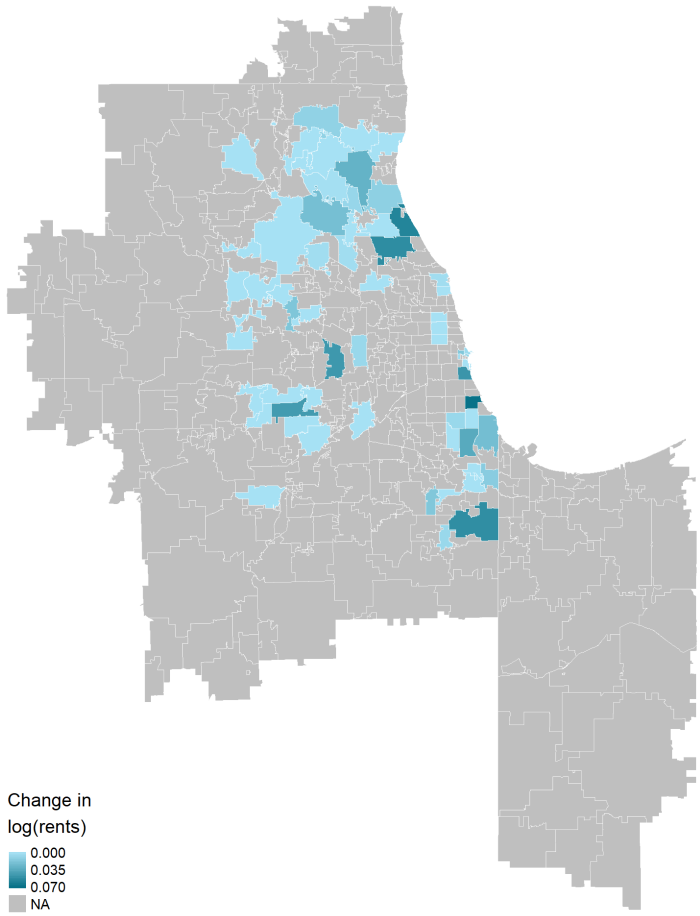
Notes: Data are from Zillow (2020b) and Small Area Fair Market Rents (2020). The figure compares the evolution of the median rental value in Zillow to three SAFMRs series, for 2, 3, and 4 or more bedrooms. SAFMR data generally corresponds to the 40th percentile of the distribution of paid rents in a given ZIP code. For more information on how SAFMRs are calculated, see US Department of Housing and Urban Development (2017, page 41641).

Appendix Figure 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.

Appendix Figure 4: Changes in log rents in the Chicago-Naperville-Elgin CBSA, July 2019

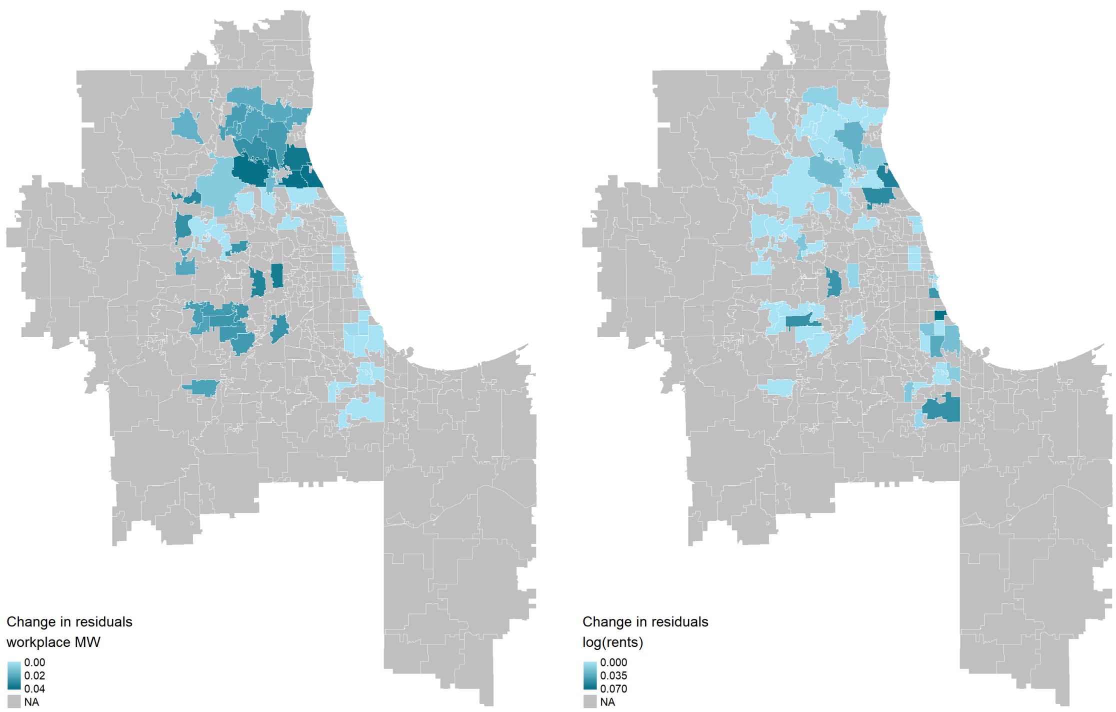


Notes: Data are from Zillow (Zillow 2020b). The figure shows the change in the log of median rents in the SFCC category in the month of June 2019 in ZIP codes located in the metropolitan area of Chicago.

Appendix Figure 5: Changes in residuals of baseline estimates in the Chicago-Naperville-Elgin CBSA, July 2019

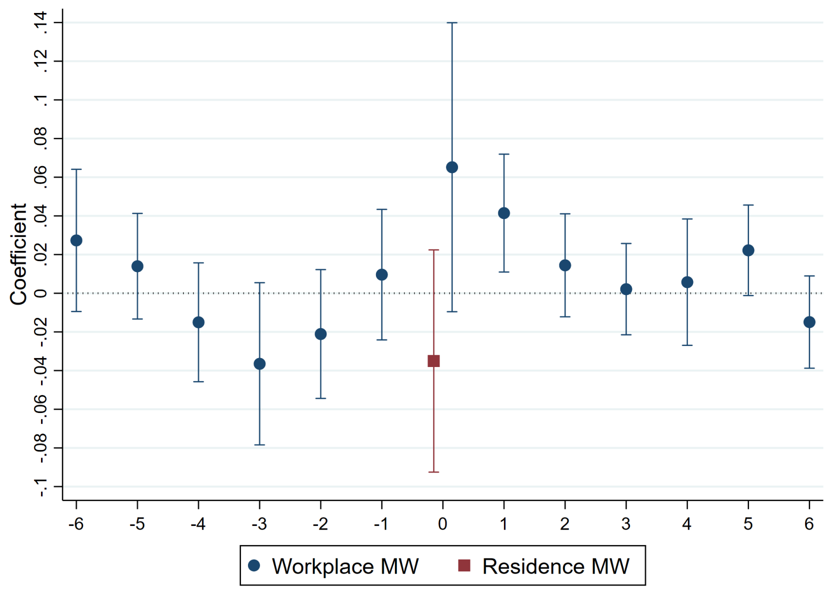
(a) Residualized workplace MW

(b) Residualized log rents



Notes: Data are from unbalanced estimation panel described in Section 3.4. Panel (a) maps the residuals of a regression of the change in the workplace MW measure on the change in the residence MW measure, including economic controls and year month fixed effects. Panel (b) maps the residuals of a regression of the change in log rents on economic controls and year month fixed effects. The residence MW is defined as the log statutory MW in the same ZIP code. The workplace MW is defined as the statutory MW where the average resident of the ZIP code works, constructed using LODES origin-destination data. Economic controls from the QCEW include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors "Information," "Financial activities," and "Professional and business services."

Appendix Figure 6: Estimates of the effect of the MW on rents, county by month data



Notes: Data are from the a county by month panel described in Section 3.4. We plot coefficients from regressions of the log of rents per square foot on the residence and workplace MW measures, including six leads and lags of the workplace MW measure. All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county and month levels. The measure of rents per square foot correspond to the Single Family, Condominium and Cooperative houses from Zillow. The residence MW is defined as the log statutory MW at the County. The workplace MW is defined as the log statutory MW where the average resident of the county works, constructed using LODES origin-destination data. Economic controls from the QCEW include the change of the following variables: the log of the average wage, the log of employment, and the log of the establishment count for the sectors "Information," "Financial activities," and "Professional and business services." 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.