From Workplace to Residence: The Spillover Effects of Minimum Wage Policies on Local Housing Markets*

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Abstract

The recent rise of sub-national minimum wage (MW) policies in the US has resulted in significant dispersion of MW levels within urban areas. In this paper, we study the spillover effects of these policies on local rental markets through commuting. To do so, for each USPS ZIP code we construct a "workplace" MW measure based on the location of its resident's jobs, and use it to estimate the effect of MW policies on rents. We use a novel identification strategy that exploits the fine timing of differential changes in the workplace MW across ZIP codes that share the same "residence" MW, defined as the same location's MW. Our baseline results imply that a 10 percent increase in the workplace MW increases rents at residence ZIP codes by 0.69 percent. To illustrate the importance of commuting patterns, we use our estimates and a simple model to simulate the impact of federal and city counterfactual MW policies. The simulations suggest that landlords pocket approximately 10 cents of each dollar generated by the MW across directly and indirectly affected areas, though the incidence on landlords varies systematically across space.

JEL codes: H70, J38, R21, R38.

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1 Introduction

Many US jurisdictions have recently enacted minimum wage policies surpassing the federal level of \$7.25, creating considerable variation in minimum wage (hereafter MW) levels across and even within metropolitan areas. These policies are inherently *place-based* in that they are tied to a location, and workers may live and work in locations under different statutory MW levels, suggesting the presence of spatially heterogeneous policy effects. 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 MW policies requires an understanding of how they affect different markets and how their effects spill over across neighborhoods. In fact, while the MW appears to lower income inequality through the labor market (Lee 1999; Autor et al. 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 (MaCurdy 2015).

In this paper, we study the effect of MW policies on local rental housing markets estimating their effects across neighborhoods within a metropolitan area. Consider the introduction of a new MW policy in certain neighborhoods of some metropolitan area. Given that low-wage workers are likely to live in particular neighborhoods with access to the (now better-paying) low-wage jobs, their disposable income will rise, causing a boost in housing demand and rental prices in residential areas rather than workplace ones. This effect, arising from the MW at the workplace, could undermine the distributional objective of the policy. Additionally, the MW hike may affect the jurisdiction that enacted the policy, for instance by increasing prices of non-tradable consumption. This effect, operating through the MW at the residence, will affect the demand for housing as well, and consequently rental prices. Thus, commuting patterns become an essential ingredient to understand the heterogeneous effects of local MW policies.

To operationalize this insight we collect granular data on commuting patterns and construct, for each USPS ZIP code (hereafter ZIP code) and month, the *workplace MW*, which we define as the log statutory MW where the average worker of the ZIP code works. We also define the *residence MW*, which is just the log statutory MW in the same ZIP code. Figure 1 visually represents these MW-based measures by illustrating their changes for the Chicago-Naperville-Elgin Core-Based Statistical Area (hereafter CBSA) in July 2019, when the city of Chicago and Cook County increased the MW from \$12 to \$13 and from \$11 to \$12, respectively. Even though the statutory MW only changed in some locations in the CBSA, the increase affected the workplace MW of most locations. We formulate a simple partial-equilibrium model that suggests that these measures are sufficient to determine the change in rents in a local housing market.

Studying the within-city spillover effects of the MW requires granular data on rents, which is why we employ a novel ZIP code-level panel dataset from Zillow. Our main rent variable is calculated as the median rental price per square foot for listings within a specific ZIP code-month for Single Family houses, Condominiums, and Cooperative units (SFCC).[1] This variable captures the posted price of newly available units, thereby avoiding tenure biases and more accurately reflecting current

[1]Single family houses are standalone housing units, while condominiums and cooperatives are multi-unit buildings with varying ownership structures (Zillow 2023).

market conditions (Ambrose et al. 2015). We find that low-wage households are more likely to be renters, tend to reside in these housing types, and that rents per square foot are surprisingly uniform across the income distribution. These findings suggest that the Zillow data can feasibly capture any MW effects. Moreover, the data varies monthly, aligning with the frequency of MW changes, thus allowing us to construct an estimation strategy that exploits the exact timing of hundreds of policy changes staggered across jurisdictions and months.

To estimate the spillover effects of MW policies on rents, we develop a novel difference-in-differences strategy that exploits our granular and high-frequency data to compare the evolution of rents across ZIP codes differentially exposed to workplace MW changes, conditional on the residence MW. To further illustrate the importance of commuting patterns in the propagation of MW shocks, we use our simple model and our main estimated elasticities to evaluate two MW policies: a federal MW increase and a local MW increase in the city of Chicago. We estimate the share of each dollar of extra income (generated by the MW) that accrues to landlords both combining all affected areas and in each particular location. We then discuss our results' implications for assessing the distributional impact of MW policies.

We start introducing a motivating partial equilibrium model of a ZIP code's rental market, which is part of a larger geography. The model is populated by workers who demand housing, and the interaction with a supply of rental units by absentee landlords determines the equilibrium rental price. Importantly, residents of the ZIP code can commute to work in other ZIP codes, possibly under a different MW policy. Workers' demand for square footage of homogeneous housing space is modelled as a function of prices of non-tradable consumption and income, both of which are influenced by the MW levels at residence and workplace locations. The model illustrates that the impact of a change in MW legislation would vary across ZIP codes, depending on whether it alters the MW at the workplace, the residence, or both. The model implies the impact of MW changes in certain ZIP codes on rents can be summarized by the workplace MW and the residence MW measures, emphasizing the need to control for the residence MW in the empirical analysis.

Guided by the theoretical model, we pose an empirical model where log rents in a location depend linearly on leads and lags of the workplace MW, the residence MW, ZIP code and time period fixed effects, and time-varying controls. This compares ZIP codes that are differentially exposed to the workplace MW but equally affected by the residence MW, conditional on other factors that affect the evolution of rents. The identification assumption is that, within a ZIP code, changes in the workplace MW are strictly exogenous with respect to unobserved changes in rents after partialing out the confounding variation generated by the residence MW. Given that MW policies are typically not enacted considering their spillover effects on local rental markets, we argue that this assumption is plausible. In an appendix, we discuss a general potential outcomes framework following Callaway et al. (2021). We demonstrate that, under the assumptions of *parallel trends* and *no selection on gains*, the effects of the workplace MW and residence MW are identified from the conditional slope of log rents with respect to each MW measure.

Our preferred specification implies that a 10 percent rise in the workplace MW (holding constant the residence MW) increases rents by 0.69 percent (SE=0.29). Failing to control for the residence MW results in an estimated effect of 0.45 (SE=0.16), and a model that uses the residence MW only results in an even lower estimate of 0.37 (SE=0.15). The reasons these estimates are lower are two-fold. First, by accounting for the difference between workplace and residence locations, the workplace MW is a better measurement of the change in the MW that is relevant for a ZIP code's wage income, thus reducing measurement error. Second, controlling for the residence MW removes confounding variation in rents that is generated by unobserved factors that may respond to it, such as prices of non-tradable consumption. Using a rough approximation to the share of MW workers in each ZIP code, we show that the elasticity of rents to the workplace MW is larger in locations with more MW residents, consistent with the fact that the effect operates by changing the income of low-wage workers. Likewise, we find a lower elasticity in locations with larger average incomes. These results imply that MW changes spill over spatially through commuting, affecting local housing markets in places beyond the boundary of the jurisdiction that originally enacted the policy.

When including both the workplace and residence MW in our model, we find that the coefficient on the residence MW is negative, consistent with the story that increases in local non-tradable consumption ameliorate the effect of the MW on rents, as in the theoretical model. However, the coefficient is not statistically significant in our baseline estimates, and the lack of data on prices of non-tradable consumption of a ZIP code's residents prevents us from drawing strong conclusions about this effect.

We provide support for our identification assumptions with a battery of additional analysis. First, we test for pre-period coefficients and construct a non-parametric analysis of the relationship between log rents and the MW measures. We find that future MW changes do not predict rents, and the conditional relationship of log rents with respect to each MW measure is nearly linear, suggesting that the identification assumptions are plausible. Second, we estimate our model using a rental index constructed by Zillow that controls for variation in the available housing stock at each time. This variable alleviates concerns that changes in the composition of available units, coinciding with MW changes, drive our estimates. Third, our estimates are robust to using commuting shares for different years, and they are stronger when we use shares based on jobs below a certain nominal income threshold or on younger workers, both of which are more likely to be affected by the MW. This is consistent with the view that identification arises from the "shares," as in Goldsmith-Pinkham et al. (2020). Finally, we construct a "stacked" regression model, similar to Cengiz et al. (2019), that explicitly compares ZIP codes within metropolitan areas where some but not all experienced a change in the statutory MW. This helps alleviate concerns that our estimates stem from undesired comparisons in difference-in-differences models with staggered treatment timing, as highlighted by recent literature (de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022).[2]

Our results remain robust across different sets of controls, alternative samples of ZIP codes, and reweighing observations to match demographics of the population of urban ZIP codes. We find similar (but noisier) results when we use median rents in different housing categories, which are available for smaller samples of ZIP codes.

In the final part of the paper, we use our motivating model and simulate counterfactual exercises to capture the incidence of MW policies on landlords. We compute the share pocketed by landlords

[2]We also estimate a model that includes the lagged first difference of rents as a control, and is estimated via instrumental variables following Arellano and Bond (1991).

in each ZIP code, and also compute the total incidence summing across locations. We simulate two counterfactual MW policies in January 2020, keeping all other MW policies in their 2019 levels. In the first scenario, we change the federal MW from $7.25 to $9. In the second, we propose a rise in the Chicago City MW from $13 to $14. We estimate that landlords capture 9.2 cents of each dollar across locations in affected CBSAs in the former, and 11.0 cents of each dollar across locations in the Chicago-Naperville-Elgin CBSA in the latter. We find systematic spatial variation in incidence, with the share pocketed usually being larger in locations that experience an increase in the workplace MW but not in the residence MW. These exercises illustrate that commuting patterns are essential to understanding the spatial incidence of MW policies within metropolitan areas.

Our analysis has some important limitations. First, while our workplace MW measure is consistent with a large body of work relying on variation generated by shift-share instruments (e.g., see recent work by Goldsmith-Pinkham et al. 2020; Borusyak et al. 2021), our formal justification in the theoretical model relies on strong constant-elasticity assumptions. We discuss their plausibility in the body of the paper. Second, while our model is useful to motivate our empirical strategy, it does not account for general equilibrium effects such as changes in commuting patterns. We discuss the potential consequences of relaxing this assumption for our empirical results in the context of our model. Additionally, we caution that our counterfactual simulations based on the model should be taken as an approximation to the effects of a small change in the MW. Third, our exercises do not capture the full welfare effect of MW policies in the US. Such an effort would require a general equilibrium model that accounts for responses to the MW across several margins. However, as low-wage households are more likely to rent and thus will be more affected by rent effects, our analysis suggest that such a model should consider the homeownership status of households.

Our findings contribute to the literature studying the effects of MW policies on the housing market. To our knowledge, the only papers whose goal is to estimate the effect of the MW on rents in the same location are Tidemann (2018) and Yamagishi (2019, 2021).[3] Agarwal et al. (2022) show that MW increases lower the probability of rental default, and also present estimates of the effect of the MW on rents using transactions data between 2000 and 2009. Our paper also relates to Hughes (2020), who studies the effect of MW policies on rent-to-income ratios. The key difference of our paper with this work is that we differentiate between residence and workplace MW levels, incorporating spillovers across regions. We highlight that this distinction is essential in a context of within-city variation in MW policies, such as the recent experience in the US. A second difference is the research design: we use high-frequency, high-resolution data that allows clean identification at the level of the local housing market.

We also contribute to the understanding of place-based policies and the spatial transmission of shocks. Kline and Moretti (2014) argue that place-based policies may result in welfare losses due to finite housing supply elasticites. In line with this insight, we show that landlords benefit from a place-based MW policy depending on their location. Allen et al. (2020) estimate the within-city transmission of expenditure shocks in Barcelona. We, on the other hand, study the within-city transmission of MW shocks.

[3]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.

More broadly, our paper relates to the large literature estimating the effects of MW policies on employment (see Dube 2019a and Neumark and Shirley 2021 for recent reviews of the literature), the distribution of income (e.g., Lee 1999; Autor et al. 2016; Dube 2019b), and the overall welfare effect of the MW (Ahlfeldt et al. 2022; Berger et al. 2022).[4] Our contributions are to incorporate spillovers across locations (as in the recent work by Jardim et al. 2022b) and to show that rent increases erode some income gains of low-wage workers. We also contribute by developing a novel panel dataset of MW levels at the ZIP code level for the entire US.

Finally, our paper relates to work in econometrics that focuses on spillover effects across units, both in the context of MW policies (Kuehn 2016; Jardim et al. 2022b), and more generally of any policy that spills over spatially (Delgado and Florax 2015; Butts 2021). Our approach is similar to Giroud and Mueller (2019): we specify a model for spillovers across units that allows us to estimate rich effect patterns of the MW on rents.

The rest of the paper is organized as follows. Section 2 introduces a motivating model of the rental market. In Section 3 we discuss the empirical relationship between income and housing and present our estimation data. In Section 4 we discuss our empirical strategy and identification assumptions. In Section 5 we present our estimation results. Section 6 discusses counterfactual MW policies, and Section 7 concludes.

2 A Partial-Equilibrium Model

In this section we lay out a simple demand and supply model of local rental markets. We use the model to motivate our research design and interpret our empirical findings. Specifically, we obtain two results. First, the model shows that a new MW legislation will have a different effect depending on whether it affects the workplace location, residence location, or both. Second, the model shows that under certain conditions the effect of a MW policy on rents can be summarized in two MW-based measures: the workplace MW and the residence MW. Derivations rely on several assumptions. We discuss their importance and the consequences of relaxing them in the last part of this section.

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 ZIP codes $z \in \mathcal{Z}(i)$, where $\mathcal{Z}(i) \subseteq \mathcal{Z}$. We let L_{iz} denote the number of i's residents who work in z and $L_i = \sum_{z\in\mathcal{Z}(i)} L_{iz}$ the number of residents in i.[5] Commuting shares are given by $\pi_{iz} = \frac{L_{iz}}{L_i}$. We assume that the vector of shares $\{\pi_{iz}\}_{z\in\mathcal{Z}(i)}$ is fixed, which we think is a good approximation for our empirical setting where we observe MW changes at a monthly frequency.[6] We discuss the consequences of relaxing this

[4]Our paper is also related to work studying the effects of local MW policies (e.g., Dube and Lindner 2021; Jardim et al. 2022a), the effect of MW policies on commuting and migration (e.g., Cadena 2014; Monras 2019; Pérez Pérez 2021), and prices of consumption goods (e.g., Aaronson 2001; Allegretto and Reich 2018; Leung 2021).

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

[6]Allen et al. (2020) study the within-city transmission of expenditure shocks by tourists within Barcelona over a period of two years. The authors maintain an analogous assumption of constant shares of income that each location in the city earns from every other location.

assumption later in this section.

Minimum Wages. Each ZIP code has a binding nominal minimum wage. The vector of binding MW levels relevant for i is $\{\underline{W}_z\}_{z\in\mathcal{Z}(i)}$.

Housing demand. Each group (i,z) consumes square feet of living space H_{iz}, a non-tradable good produced in their residence C_{iz}^{NT}, and a tradable good C_{iz}^{T}. A representative (i,z) worker chooses between these alternatives by maximizing a quasi-concave utility function $u_{iz} = u\left(H_{iz}, C_{iz}^{\text{NT}}, C_{iz}^{\text{T}}\right)$ subject to a budget constraint $R_i H_{iz} + P_i(\underline{W}_i)C_{iz}^{\text{NT}} + C_{iz}^{\text{T}} \leq Y_{iz}(\underline{W}_z)$. In this equation 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 specify the effect of MWs on these functions below.

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

The problem's structure, along with Assumption 1, is aligned with the prevailing literature. First, Miyauchi et al. (2021) show that individuals tend to consume close to home. Consequently, it's expected that they would be sensitive to local consumption prices within their own neighborhoods, justifying the assumption that workers consume non-tradables in the same ZIP code.[7] Second, MW hikes have been shown to increase prices of local consumption (e.g., Leung 2021), and also to increase wage income even for wages above the MW level (e.g., Cengiz et al. 2019; Dube 2019b).[8]

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 $\{h_{iz}(R_i, P_i, Y_z)\}_{z\in\mathcal{Z}(i)}$. The following assumption summarizes the properties of these functions.

Assumption 2 (Housing demand)**.** *Consider the set of functions* $\{h_{iz}(R_i, P_i, Y_z)\}_{z\in\mathcal{Z}(i)}$*. We assume that (i) housing is a normal good,* $\frac{dh_{iz}}{dY_z} > 0$ *for all* $z \in \mathcal{Z}(i)$*, and (ii) housing demand is decreasing in prices of non-tradable consumption,* $\frac{dh_{iz}}{dP_i} < 0$*.*

Using the first assumption, standard arguments imply that $\frac{dh_{iz}}{dR_i} < 0$. For the second assumption to hold, a sufficient (albeit not necessary) condition is that housing and non-tradable consumption are complements.[9] While direct empirical evidence on this particular channel is lacking, we view the

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

[8] 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, e.g., Draca et al. 2011; Harasztosi and Lindner 2019), then business income would depend negatively on the MW level.

[9] 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.

evidence of workers sorting towards locations with high housing costs and more expensive amenities as consistent with it (e.g., Couture et al. 2019).

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, $\frac{dS_i(R_i)}{dR_i} \geq 0$. Note that this formulation allows for an upper limit on the number of housing units 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

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

Given that per-capita housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.[10] 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 $z \in \mathcal{Z}(i) \setminus \mathcal{Z}_0$ binding MWs do not change, where $\mathcal{Z}_0$ is non-empty. Under the assumptions of unchanging $\{\pi_{iz}\}_{z \in \mathcal{Z}(i)}$ and Assumptions 1 and 2, we have that*

- *(a) for any $z' \in \mathcal{Z}_0 \setminus \{i\}$ for which $\frac{dY_{z'}}{d\underline{W}_{z'}} > 0$, the policy has a positive partial effect on rents, $\frac{d \ln R_i}{d \ln \underline{W}_{z'}} > 0$;*
- *(b) 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*
- *(c) as a result, the overall effect on rents is ambiguous if $i \in \mathcal{Z}_0$ and positive if $i \notin \mathcal{Z}_0$.*

Proofs are available in Online Appendix A.1.

The first part of Proposition 1 shows that, if at least some low-wage worker commutes to a ZIP code z' where the MW increased (so that $\frac{dY_{z'}}{d\underline{W}_{z'}} > 0$), then the MW hike will tend to increase rents. This follows from an increase in housing demand in i due to the increase in income of workers who commute to z'. The second part of Proposition 1 establishes that decreasing rents may follow if the minimum wage also increases in ZIP code i. This is because the increase in i lowers housing demand

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

by a substitution effect, so that the overall effect on rents is ambiguous. Consequently, the sign of the overall effect of the policy in i is not determined a priori.

As apparent from the proof of Proposition 1, the effect of the MW on rents at workplaces depends on the elasticities of per-capita housing demand to incomes $\xi_{iz}^{Y} = \frac{dh_{iz}}{dY_z} \frac{Y_z}{\sum_z \pi_{iz} h_{iz}}$ and on the elasticities of income to minimum wages $\epsilon_{iz}^{Y} = \frac{dY_z}{d\underline{W}_z} \frac{\underline{W}_z}{Y_z}$. These (i, z)-specific terms weigh the change in MW levels at workplaces, and their sum over z impacts rents. The following proposition establishes conditions under which we can reduce the dimensionality of the rent gradient.

Proposition 2 (Representation)**.** *Assume that for all ZIP codes $z \in \mathcal{Z}(i)$ we have (a) homogeneous elasticity of per-capita housing demand to incomes, $\xi_{iz}^{Y} = \xi_{i}^{Y}$, and (b) homogeneous elasticity of income to minimum wages, $\epsilon_{iz}^{Y} = \epsilon_{i}^{Y}$. Then, we can write*

$$dr_i = \beta_i d\underline{w}_i^{wkp} + \gamma_i d\underline{w}_i^{res} \tag{2}$$

where $r_i = \ln R_i$, $\underline{w}_i^{wkp} = \sum_{z \in \mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_z$ is ZIP code i's ***workplace MW****, $\underline{w}_i^{res} = \ln \underline{W}_i$ is ZIP code i's* ***residence MW****, and $\beta_i > 0$ and $\gamma_i < 0$ are parameters.*

Proposition 2 shows that, under a homogeneity assumption on the elasticities of per-capita housing demand to income and of income to the MW,[11] the change in rents following a small change 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 $z \in \mathcal{Z}(i)$, 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.

2.3 Summary and discussion

Under the stated assumptions, Proposition 1 shows that the effect of a MW increase on rents depends on whether it affects the ZIP code via the workplace or the residence. ZIP codes exposed via their workplace only will tend to experience an increase in rents. ZIP codes exposed to both via their workplace and residence will tend to experience lower increases, or even decreases, in rents. Controlling for the residence MW is thus important to characterize the spatial effects of MW policies. Proposition 2 provides guidance for the empirical analysis of spillover effects of the MW by formally justifying the workplace MW, our summary measure of the changes in the MW at workplaces.

In this subsection we discuss the plausibility of the assumptions that yield these results and the consequences of relaxing them.

The mechanics of rent adjustments. The model is static, in the sense that everyone chooses their housing demand simultaneously. Once a MW policy is enacted, a new equilibrium is reached where rents adjust to the new demand levels to clear the market. This adjustment takes place because workers can in principle move within ZIP codes, and even across ZIP codes, in search of housing that satisfies their demand, as long as the commuting shares remain unchanged. Agarwal

[11] The assumptions stated in Proposition 2 are actually stronger than needed. It is enough to have that the product $\xi_{iz}^{Y}\epsilon_{iz}^{Y}$ does not vary by z.

et al. (2022) finds an increase in the probability of moving after a MW increase. Online Appendix A.2 discusses an extension of the model with discrete time periods in which the process of workers moving within the ZIP code to renew expiring rental contracts is modelled explicitly. Therefore, we see that rents adjust because workers can demand more or less housing in response to the MW change.[12]

Homogeneity assumptions and the workplace MW. How likely are the assumptions that yield Proposition 2 to hold? The assumption that the elasticity of income to the MW is constant will fail if the income of some (i, z) groups is more sensitive to the MW than others. This would be the case if, for example, the share of low-wage workers within each π_{iz} varies strongly by workplace. The assumption that the elasticity of housing demand to income is constant will hold trivially for preferences $h_{iz} = g(R_i, P_i)\, Y_i$ for some $g(\cdot)$, such as those in Cobb-Douglas or Constant Elasticity of Substitution utility functions. However, one would expect the elasticity of (i, z) groups with many low-wage workers to be larger, suggesting that this type of preferences may not be appropriate.

We thus see that the homogeneity assumptions are strong and will likely not hold exactly. However, we expect our empirical model based on Proposition 2 to offer a decent approximation to study the spillover effects of MW policies on the housing market. In fact, unless the heterogeneity in $\{\xi^Y_{iz}\epsilon^Y_{iz}\}_{z\in\mathcal{Z}(i)}$ has a strongly asymmetric distribution across workplace locations, we expect to correctly capture the average contribution of the workplace MW on rents. In other words, the value of $\beta_i d\underline{w}^{\text{wkp}}_i$ is likely to be close to the value of the elasticity-weighted changes in workplace MW levels that, according to the model, determine rents.[13] Moreover, in our empirical exercises we allow for heterogeneity in elasticities based on observable characteristics of workers, such as the share of MW workers residing in each location, empirically exploring this channel.

Representation under changing commuting shares. Online Appendix A.3 discusses the consequences of relaxing the assumption of fixed commuting shares. In particular, we assume that the commuting shares are declining in the MW level at the workplace (as found by, e.g., Pérez Pérez 2021). In this case, Proposition 3 shows that MW hikes at workplace locations will affect rents through two channels: a positive channel via increases in housing demand, and a negative channel via decreases in commuting shares. Therefore, potential declines in commuting shares at the month of the MW change will tend to attenuate the positive effect of the workplace MW on rents, biasing our estimates towards not finding an effect. Alternatively, if one thinks that commuting shares adjust more slowly than posted rental prices, then an increase in the workplace MW will tend to lower

[12] This is a supply and demand story. Another story that would yield increasing rents as response to workplace MW hikes is one where landlords and tenants bargain over rents, and the MW improves the outside options of tenants.

[13] More precisely, say that $\xi^Y_{iz}\epsilon^Y_{iz} = \overline{\xi\epsilon}_i + \nu_{iz}$ where ν_{iz} has a mean of zero. In that case, a similar logic than the one in the proof of Proposition 2 will result in the following expression for rents changes:

$$dr_i = \gamma_i d\underline{w}^{\text{res}}_i + \frac{\overline{\xi\epsilon}_i}{\eta_i - \sum_z \pi_{iz}\xi^R_{iz}} \sum_z d\ln \underline{W}_z + \frac{1}{\eta_i - \sum_z \pi_{iz}\xi^R_{iz}} \sum_z \nu_{iz} d\ln \underline{W}_z.$$

The second term on the right-hand side is equivalent to $\beta_i \underline{w}^{\text{wkp}}_i$ in Proposition 2. The third term reflects the heterogeneity. If ν_{iz} has a symmetric distribution, and $d\ln \underline{W}_z$ is the same across workplaces (because it originates from a single jurisdiction), then this third term will equal zero.

rents after the MW change. We do not find evidence of this in our estimates, suggesting that the assumption of fixed commuting shares when focusing on monthly changes in rents is reasonable.

3 Context and Data

We begin the section by describing the construction of a ZIP code by month panel of MW levels in the US. We use our panel to describe trends in MW policies in the 2010s. Later, we discuss the relationship between income and housing consumption at the household level. The data suggests that rents are likely to respond to MW changes. We also explore how housing expenditure varies across ZIP codes. Finally, we document the construction of our analysis sample and discuss its strengths and limitations.

3.1 Minimum Wage Policies in the 2010s

We collected data on federal-, state-, county-, and city-level statutory MW levels from Vaghul and Zipperer (2016). We extended their data, available up to 2016, using data from UC Berkeley Labor Center (2022) and from official government offices for the years 2016–2020.[14] Most ZIP codes are contained within a jurisdiction, and for them the statutory MW is simply the maximum of the federal, state, and local levels. Some ZIP codes cross jurisdictions, and so are bound by multiple statutory MW levels. In these cases we assign a weighted average of the statutory MW levels in its constituent census blocks, exploiting an original correspondence table between blocks and ZIP codes detailed in Online Appendix B.1, where weights correspond to the number of housing units. The result is a ZIP code-month panel of statutory MW levels in the US between January 2010 and June 2020. More details on the construction of the panel can be found in Online Appendix B.2.

Online Appendix Figure 1 shows the different levels of binding MW policies over time in our data. Panel A focuses on state-level MW policies. There are 30 states with MW policies in 2010–2020, all of which started prior to January 2010. Panel B shows sub-state MW policies. In total, there are 37 counties and cities with some binding MW policy in this period. The number of new local jurisdictions instituting a MW policy increases strongly after 2013 and declines after 2018. Overall, we observe strong variations in MW levels across jurisdictions.

Figure 2 maps the percentage change in the statutory MW level from January 2010 to June 2020 in each ZIP code. We observe a great deal of spatial heterogeneity in MW levels within the US. Importantly, many metropolitan areas across and within state borders have differential MW changes, which will be central to distinguishing the effect of the two MW-based measures proposed in Section 2. We describe the construction of these measures later in this section.

[14]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 the tipped MW plus tips and the actual MW.

3.2 Households, Income, and Housing

We compare individuals and households within metropolitan areas using data from the 2011 and 2013 waves of the American Housing Survey (US Department of Housing and Urban Development 2020a). Figure 3 shows that low-income households are much more likely to rent. While only 12 percent of households in the top income quintile are renters, around 60 percent of them are when focusing on the bottom one. Online Appendix Figure 2 shows that, while low income individuals are less likely to be household heads, many of them are. The average probability for the bottom three income deciles is 50 percent. Online Appendix Figure 3 shows that, among households that rent, rents per square foot are surprisingly constant across household income levels. These figures suggest that the MW is likely to affect household income, at least for lower income households, and that rents per square foot can plausibly respond to MW changes. Online Appendix Figure 4 shows the type of building households live in by household income decile. Low-income households are more likely to live in buildings with more units, though they are spread across all building types.

We explore variations over space in housing expenditure. To do so, we collected Individual Income Tax Statistics aggregated at the ZIP code level from the IRS (Internal Revenue System 2022b), and Small Area Fair Market Rents (SAFMRs hereafter) data from the HUD (US Department of Housing and Urban Development 2020b).[15] For each ZIP code in 2018, we constructed a housing expenditure share dividing the average monthly wage per household from the IRS by the 2 bedroom SAFMR rental value from the HUD.[16,17] Online Appendix Figure 5 maps our estimates for the Chicago CBSA. There is considerable variation in housing expenditure over space, with poorer areas generally spending a higher share of their income on housing.

To get a sense of the spatial distribution of minimum wage earners we construct a proxy variable using the number of workers across income bins in the 5-year 2010-2014 American Community Survey (ACS; US Census Bureau 2022a). See details in Online Appendix B.2. Our variable for the share of MW workers is negatively correlated with median household income from the ACS (corr. = -0.26) and positively correlated with our estimate of the housing expenditure share (corr. = 0.30). This latter correlation also suggests that the MW is likely to affect rents.

3.3 Estimation Data and Samples

3.3.1 Rents Data

Zillow is the leading online real estate platform in the US, hosting more than 170 million unique monthly users in 2019 (Zillow 2020a). Zillow provides the median rental and sales price among units

[15] SAFMRs data are constructed by the HUD as an extension of the Fair Market Rents (FMRs) data using, for each year, ZIP code-level information from previous years' American Community Survey (US Department of Housing and Urban Development 2018, p. 35). SAFMRs are an estimate of the 40th percentile of the rents distribution based on constant housing quality (US Department of Housing and Urban Development 2018, p. 1). The FMRs data, available at the county and year levels, have been used to study the effect of the MW on rents in the US (Tidemann 2018; Yamagishi 2019).

[16] We impute a small share of missing values using a regression model where the ZIP code-level covariates include data from LODES and the US Census. See Online Appendix B.3 for details.

[17] This computation will be a good approximation for the housing expenditure share insofar total housing expenditure and total wage income are roughly proportional to their averages under the same constant of proportionality. This computation also assumes away differences in the number of bedrooms across ZIP codes.

listed on the platform for different house types and at different geographic and time aggregation levels (Zillow 2020b).[18] We collected the ZIP code level data, available from February 2010 to December 2019. There is variation in the entry of a ZIP code to the data, and locations with a small number of listings are omitted.

Our main analyses use the median rental price per square foot among housing units listed in the category Single Family houses, Condominium and Cooperative units (SFCC). This is the most populated time series, as it includes the most common US rental house types (Fernald 2020). We focus on rents *per square foot* to account for systematic differences in housing size. Online Appendix Figure 6 shows that this series follows a similar trend over time when compared to SAFMR. It is important to note that these data reflect rents of newly available units, for which new information is likely to be quickly incorporated into prices (Ambrose et al. 2015). As a result, we expect them to react quickly to economic shocks, such as changes in the MW. On the other hand, rents among the universe of leased units should react more slowly, as they are only updated when the lease is renewed. This is the pattern of results in Agarwal et al. (2022) who use data from contract rents.

We use Zillow's Observed Rental Index (ZORI) for a robustness check, computed following the repeat rent index methodology proposed by Ambrose et al. (2015). To compute the index, Zillow (2023) uses all units with more than one transaction in a ZIP code and estimates a weighted regression of the log of the change in rents between two months on year-month indicators, upweighting observations that correspond to housing types that are underrepresented in the Zillow sample relative to census data. The published index is a smoothed version of these coefficients, achieved through a three-month moving average that uses data from previous months. To account for this smoothing, we shift our MW measures when using the index as outcome in our regression models.

The Zillow data have several limitations. First, Zillow's market penetration dictates the sample of ZIP codes available. Online Appendix Figure 7 shows that the sample of ZIP codes with SFCC rents data coincides with areas of high population density. Second, we only observe the median rental value. No data on the distribution of rents, nor the number of units listed for rent, are available. Finally, we observe posted rents rather than contract rents. We did not find any information on how correlated posted rents and contract rents are, so we decided to ask this as a question in the online platform Quora. Online Appendix B.4 shows a selection of quotes from the replies, which overwhelmingly suggest that contract rents generally do not differ from posted rents. Some answers also suggest that rents of long-tenured landlords may not reflect current market conditions.

3.3.2 The residence and workplace minimum wage measures

Using the panel described in Section 3.1 at hand, computing the residence MW is straightforward. We define it as $\underline{w}_{it}^{\text{res}} = \ln \underline{W}_{it}$.

To construct the workplace MW we need commuting data, which we obtain 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 raw data are aggregated at the census block level. We further aggregate it to ZIP codes using the original

[18] As of the release of this article, the data are no longer available for download. See Internet Archive (2021) for a snapshot of the website as of February 2020, the last month the data were available.

correspondence between census blocks and USPS ZIP codes described in Online Appendix B.1. This results in residence-workplace matrices that, for each ZIP code and year, indicate the number of jobs of residents in every other ZIP code.

We use the 2017 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 $\{\pi_{iz}\}_{z\in\mathcal{Z}(i)}$ as $\pi_{iz} = N_{iz}/N_i$, where N_{iz} is the number of jobs with residence in i and workplace in z, and N_i is the total number of jobs originating in i. The workplace MW measure is defined as

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

The workplace MW has a shift-share structure. Our strategy, which exploits differential exposure to common shocks for identification, is most related to recent work in this area by Goldsmith-Pinkham et al. (2020).

While our baseline uses commuting shares from 2017, for robustness we present estimates in which the workplace MW measure is constructed using alternative set of weights. In particular, we use different years and alternative job categories, such as jobs for young or low-income workers.[19]

Figure 1, already discussed in the introduction, illustrates the difference in the MW-based measures mapping their change in the Chicago CBSA on July 2019. For completeness, Online Appendix Figure 8 shows the changes in our main median rents variable around the same date.

3.3.3 Other data sources

While our MW assignment recognizes that ZIP codes cross census geographies, we assign 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 geographic indicators in our estimates. Additionally, we collect ZIP code demographics from the ACS (US Census Bureau 2022a) and the 2010 US Census (US Census Bureau 2022b). We collect these data at the block or tract levels, and assign them to ZIP codes using the correspondence table described in Online Appendix B.1.

To proxy for local economic activity we collect data from the Quarterly Census of Employment and Wages (QCEW; US Bureau of Labor Statistics 2020b) at the county-quarter and county-month levels for several industrial divisions and from 2010 to 2019.[20] We use these data as controls for the state of the local economy in our regression models.

[19]The LODES data 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 (corr. $>$ 0.99 for every pair).

[20]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." We observe, for each county-quarter-industry cell, the number of establishments and the average weekly wage, and for each county-month-industry cell, the level of employment.

3.3.4 Estimation samples

We put together an unbalanced panel of ZIP codes available in Zillow in the SFCC category at the monthly date level from February 2010 to December 2019. This panel contains 7,626 MW changes at the ZIP code level, which arise from 82 state and 121 county and city changes. Online Appendix Figure 9 shows the distribution of positive MW increases among ZIP codes in the Zillow data. To prevent our estimates from being affected by changes in sample composition, we construct a "baseline" panel keeping ZIP codes with valid rents data starting on January 2015. The resulting fully-balanced panel contains 2,782 MW changes at the ZIP code level.[21]

Table 1 compares the sample of ZIP codes in the Zillow data to the population of ZIP codes along sociodemographic dimensions. The first and second columns report data for the universe of ZIP codes and for the set of urban ZIP codes, respectively. The third column shows the set of ZIP codes in the Zillow data with any non-missing value of rents per square foot in the SFCC category. Finally, the fourth column shows descriptive statistics for our estimation sample, which we call the "baseline" sample. While our baseline sample contains only 11.8 percent of urban ZIP codes, it covers 25.0 percent of their population and 25.8 percent of their households. With respect to demographics, ZIP codes in the baseline sample 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 ZIP code and the average urban ZIP code. This is so because Zillow is present in large urban regions, but it does not usually operate in smaller urban or rural areas. In an attempt to capture the treatment effect for the average urban ZIP code we conduct an exercise where we re-weight our sample to match the average of a handful of characteristics of those.

Finally, Online Appendix Table 1 shows statistics of our baseline panel. The distribution of the residence and workplace MW measures is, as expected, quite similar. We also show median rents in Zillow in the SFCC category. The average monthly median rent is \$1,757.9 and \$1.32 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 our models.

4 Empirical Strategy

In this section we discuss our empirical strategy. We start with an intuitive presentation of our identification argument, which is formalized in an appendix. Next, we specialize our discussion under the functional form suggested by the model in Section 2. We also discuss alternative estimation strategies, concerns related to the sample of ZIP codes we use, and heterogeneity of estimated effects.

4.1 Intuitive Identification Argument

Our data consist of rents, the residence and workplace MW measures, and economic controls. We can learn the effect of the workplace MW from the slope of the relationship between the workplace MW and rents conditioning to places with a similar change in the residence MW. Intuitively, we need

[21]To avoid losing observations in models with leads and lags we include six leads and lags of the MW measures, so the dataset actually runs from July 2014 to June 2020.

to condition on the residence MW to remove confounding variation as it may affect locations through other channels, such as changes in prices of non-tradable consumption. Likewise, a similar argument suggests that identifying the effect of the residence MW requires controlling for the workplace MW.

For these slopes to correspond to causal effects, we need to make two assumptions. The first one is a form of *parallel trends*: among ZIP codes with the same residence MW, ZIP codes with higher and lower worker MW levels would have had parallel trends in rents if not for the change in the workplace MW. The second one is *no selection on gains*: ZIP codes that receive different levels of the workplace MW must experience a similar treatment effect on average, conditional again on the residence MW. If these assumptions hold, the (conditional) slope of the relationship between the workplace MW and rents is actually the causal effect. We similarly need these assumptions to hold for the residence MW if we hope to give a causal interpretation to its coefficient. Online Appendix C formalizes these assumptions in a potential outcomes framework following Callaway et al. (2021). We discuss the plausibility of these assumptions later in this section.

4.2 Parametric Model

Consider the two-way fixed effects model relating rents and the MW measures given by

$$r_{it} = \alpha_i + \tilde{\delta}_t + \gamma \underline{w}_{it}^{\text{res}} + \beta \underline{w}_{it}^{\text{wkp}} + \mathbf{X}_{it}^{'} \eta + \upsilon_{it}, \tag{3}$$

where i and t index ZIP codes and time periods (months), respectively, r_{it} represents the log of rents per square foot, $\underline{w}_{it}^{\text{res}} = \ln \underline{W}_{it}$ is the ZIP code's residence MW, $\underline{w}_{it}^{\text{wkp}} = \sum_{z \in \mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_{zt}$ is the ZIP code's workplace MW, α_i and $\tilde{\delta}_t$ are fixed effects, and $\mathbf{X}_{it}$ is a vector of time-varying controls. Time runs from January 2015 ($\underline{T}$) to December 2019 $\left(\overline{T}\right)$. The parameters of interest are γ and β which, following the model in Section 2, we interpret as the elasticity of rents to the residence MW and the workplace MW, respectively.

By taking first differences in equation (3) we obtain

$$\Delta r_{it} = \delta_t + \gamma \Delta \underline{w}_{it}^{\text{res}} + \beta \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta \upsilon_{it}, \tag{4}$$

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. Online Appendix Table 2 shows strong evidence of serial auto-correlation in the error term of the model in levels. While estimated coefficients are similar in levels and in first differences, standard errors are seven to nine times larger in the former.

A standard requirement for a linear model like (4) to be estimable is a rank condition, which implies that both MW measures must have independent variation, conditional on the controls. 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). If so, γ and β could not be separately identified. We check in the data that the rank condition is satisfied.

The main results of the paper are obtained under the model in (4). In order to compare with the literature we also estimate versions of the model that exclude either one of the MW measures.

4.3 Validity of Identification Assumptions

The model in (3) imposes a linear functional form. This property rules out selection on gains, since then ZIP codes receiving a particular level of the MW measures will experience the same (constant) effect than ZIP codes that receive a different level. This is one of the assumptions required for identification according to Online Appendix C. We view this as a reasonable assumption. For it to not hold, workers would need to anticipate not only future MW policies but also how future rental markets would be affected by them given the commuting structure, and select their residence so that rents react differently to the MW in different ZIP codes with similar levels of the MW measures. We show in the results section that the (conditional) slope of log rents with respect to each of the MW measures appears linear, suggesting that the assumption of no selection on gains is plausible.

For estimates of β and γ from (3) to have a causal interpretation we need another assumption: the error term $\Delta\upsilon_{it}$ must be *strictly exogenous* with respect to the MW measures, and in particular with respect to the workplace MW. Namely, unobserved shocks to rents must be uncorrelated with past and present values of changes in our MW measures. This is in the spirit of parallel trends, the second assumption required for identification in the potential outcomes framework of Online Appendix C. This assumption implies that rents prior to a change in either MW measure must evolve in parallel. We test for pre-trends adding leads and lags of the workplace MW in (4),[22] though we also experiment with adding leads and lags of the residence MW. We only shift the workplace MW because estimating its effect is our focus, seeing the residence MW as a key control. In fact, Online Appendix C suggests that we only need to condition on one of the MW measures for parallel trends of the other measure to hold. Under the assumption of 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. Given the high frequency of our data and the focus on short windows around MW changes, the assumption of no anticipatory effects seems plausible.[23]

Another implication of the strict exogenity assumption is that it allows for arbitrary correlation between α_i and both MW variables. This means that our empirical strategy is robust to the fact that districts with more expensive housing tend to vote for MW policies.

We worry that unobserved shocks, such as those caused by local business cycles, may systematically affect both rent changes and MW changes, violating the strict exogeneity assumption. To account for common trends in the housing market we include time-period fixed effects δ_t, which in some specifications are allowed to vary by jurisdiction. To control for variation arising from unobserved trends in local labor markets we include economic controls from the QCEW in the vector $\mathbf{X}_{it}$.[24] Specifically, we control for average weekly wage and establishment counts at the county-

[22]Specifically, we estimate

$$\Delta r_{it} = \delta_t + \gamma \Delta \underline{w}_{it}^{\text{res}} + \sum_{k=-s}^{s} \beta_k \Delta \underline{w}_{ik}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \eta + \Delta \upsilon_{it},$$

where $s = 6$. Our results are very similar for different values of the window s.

[23]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 are consistent with both.

[24]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.

quarter level, and for employment counts at the county-month level, for the sectors "Professional and business services," "Information," and "Financial activities."[25] We also try models where we control for ZIP code-specific linear trends, which should account for time-varying heterogeneity at the ZIP code-level that follows a linear pattern.

A second worry is that changes in the composition of rentals may drive the results. For instance, if on the same month of the MW change more expensive listings go to the market, then what looks like a rent increase may actually be changes in quality.[26] We note that changes in housing size are accounted for, as we use rents per square foot. To more directly account for this threat we present evidence using Zillow's repeat rental index (ZORI), which is constructed using rental prices for the same housing unit in different moments in time. Given that the index is averaged using three lags, a regression analysis that relates period-t MWs would be expected to affect the ZORI index at $t-3$, and its first difference at $t-4$. To adjust for this we use the 4th lead of our MW measures in these models.

4.4 Alternative Strategies

Recent literature has shown that usual estimators in a difference-in-differences setting do not correspond to well-defined average treatment effects when the treatment roll-out is staggered and there is treatment-effect heterogeneity (de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022). While our setting does not correspond exactly to the models discussed in this literature, we worry about the validity of our estimator. To ease these concerns, in an appendix we construct a "stacked" implementation of equation (4) in which we take six months of data around MW changes for ZIP codes in CBSAs where some ZIP codes received a direct MW change and some did not, and then estimate the model on this restricted sample including event-by-time fixed effects. This strategy limits the comparisons used to compute the coefficients of interest to ZIP codes within the same metropolitan area and event.

In a separate exercise we relax the strict exogeneity assumption. We do so in an appendix as well, where we propose a model that includes lagged rents as an additional 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 requires innovations to rents to be uncorrelated only with past changes in the MW measures, and thus allows for feedback of rent shocks onto MW changes in future periods. We estimate this model using an IV strategy in which the first lag of the change in rents is instrumented with the second lag.

4.5 Heterogeneity and Sample Selection Concerns

We explore heterogeneity of our results with respect to pre-determined variables. Given the mechanism proposed in Section 2, we expect the effect of the residence MW to be stronger in locations

[25] We assume that these sectors are not affected by the MW. In fact, according to the US Bureau of Labor Statistics (2020a, Table 5), in 2019 the percent of workers paid an hourly rate at or below the federal MW in those industries was 0.8, 1.5, and 0.2, respectively. In comparison, 9.5 percent of workers in "Leisure and hospitality" were paid an hourly rate at or below the federal MW.

[26] We thank an anonymous referee for pointing out this concern.

where many workers earn close to the MW. 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 workers as residents since income would increase more strongly there. We then estimate the following model:

$$\Delta r_{it} = \Xi_t + \tilde{\gamma}_0 \Delta \underline{w}_{it}^{\text{res}} + \tilde{\gamma}_1 \iota_i \Delta \underline{w}_{it}^{\text{res}} + \tilde{\beta}_0 \Delta \underline{w}_{it}^{\text{wkp}} + \tilde{\beta}_1 \iota_i \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}^{'} \tilde{\eta} + \Delta \tilde{\upsilon}_{it},$$

where ι_i represents the standardized share of MW workers residing in i. Because we cannot estimate the share of MW workers working in a given location, we interact both the residence and workplace MW with the share of MW residents according to the MW in the location (estimated as explained in Section 3.2.) We conduct a similar exercise using median household income and the share of public housing units.

As explained in Section 3.3.4, the model in equation (4) is estimated using a balanced panel. In an alternative estimation exercise we use an unbalanced panel with all ZIP codes with Zillow rental data in the SFCC category from February 2010 to December 2019, controlling for time period by quarterly date of entry fixed effects. However, even all ZIP codes available in the Zillow data may be a selected sample of the set of urban ZIP codes. To approximate the average treatment effect in urban ZIP codes we follow Hainmueller (2012) and estimate our main models re-weighting observations to match key moments of the distribution of characteristics of those.

5 Estimation Results

In this section we present our empirical results. First, we show our baseline estimates and discuss our identifying assumptions and other robustness checks. Second, we present results of models that use alternative empirical strategies. Third, we discuss concerns that arise from the selectivity of our sample of ZIP codes and show heterogeneity analyses. Finally, we summarize our results and compare them with existing literature.

5.1 Baseline estimates

5.1.1 Main results

Table 2 displays our estimates using the baseline sample described in Section 3.3.4, using the parametric model in equation (4). Column (1) shows the results of a regression of the workplace MW on the residence MW, economic controls, and time fixed effects. We observe that a 10 percent increase in the residence MW is associated with an 8.63 percent increase in the workplace MW. While the measures are strongly correlated, this model shows that this correlation is far from exact, confirming the presence of independent variation that allows estimation of the effect of both variables on rents.

Columns (2) and (3) of Table 2 show estimates of models that include a single MW measure. Column (2) uses only the residence MW. In this model, only locations with a statutory MW change are assumed to experience effects, similar to the existing literature. The elasticity of rents to the MW is estimated to be 0.0372 ($t = 2.57$). Column (3) uses solely the workplace MW. The coefficient on the MW variable increases slightly to 0.0449 ($t = 2.88$), supporting the view that changes in the workplace MW are a better measure of the changes in the MW that are relevant for a ZIP code.

Column (4) of Table 2 show estimates of equation (4) including both MW measures. The coefficient on the workplace MW (β) increases to 0.0685 and is statistically significant ($t = 2.38$). These results suggest that omitting the residence MW generates a downward bias on the coefficient of the workplace MW. Consistent with the theoretical model in Section 2, the coefficient on the residence MW (γ) now turns negative and equals -0.0219, although it is not statistically significant ($t = -1.25$). We reject the hypothesis that $\gamma = \beta$ at the 10% significance level ($p = 0.051$). Finally, $\gamma + \beta$ is estimated to be 0.0466, which is highly significant ($t = 2.95$). Thus, our results imply that a 10 percent increase in both MW measures will increase rents by approximately 0.47 percent. However, our results also imply substantial heterogeneity across space. 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 likely be larger.

5.1.2 Identification assumptions

A central concern with these results is whether our identifying assumptions are likely to hold. Panel A of Figure 4 shows estimates of the parametric model including leads and lags of the workplace MW, so that the coefficients are $\{\{\beta_s\}_{s=-6}^{-1}, \beta, \{\beta_s\}_{s=1}^{6}, \gamma\}$. We cannot reject the hypothesis that $\beta_{-6} = ... = \beta_{-1} = 0$ ($p = 0.563$). Post-event coefficients $\{\beta_s\}_{s=1}^{6}$ are also estimated to be statistically zero. The only significant estimate is β at 0.0695 ($t = 2.45$). The estimate of γ is -0.0231 ($t = -1.28$). We now reject the hypothesis of equality of coefficients at the 5% significance level ($p = 0.045$). Our estimate of $\gamma + \beta$ is 0.0464. It is significant ($t = 2.90$) and almost identical to our baseline. Panel B shows the implied effects in levels of our first-differences models, assuming that pre- and post-coefficients are zero. Online Appendix Figure 10 shows that a similar story obtains when we add leads and lags of the residence MW only. We interpret these results as evidence in favor of the parallel trends assumption.

Online Appendix Figure 11 plots the relationship between log rents and each of the MW measures for ZIP codes in CBSAs and months in which at least one residence MW changed. Panel A displays the raw data, which shows a positive correlation between log rents and both MW measures. Panel B displays the same relationships after residualizing each variable on ZIP code fixed effects and indicators for different values of the other MW measure. We observe a positive slope for the workplace MW, and a negative one for the residence MW. This provides evidence in favor of the assumption of no selection on gains, and also of the linear functional form assumed in equation (3). Furthermore, the slopes in these figures show a similar magnitude to our baseline estimates of γ and β.

Online Appendix Figure 12 illustrates the identifying variation we use by mapping the residualized change in workplace MW and the residualized change in log rents.[27] Panel A of Online Appendix Figure 12, to be contrasted with the left panel of Figure 1, shows that the residualized change in the workplace MW is high outside of Cook County, where the statutory MW increased. For completeness, Panel B of Online Appendix Figure 12 shows residualized rents.

[27]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.

5.1.3 Zillow's repeat rental index

Online Appendix Figure 13 shows regression results using the ZORI index. Since the index has more missing values than our baseline before 2020, we use the entire sample of ZIP codes and set the window to 4.[28] Panel A controls for CBSA by year-month fixed effects, and shows a strong increase in rents following a change in the workplace MW. We observe that the effects lasts for a few months, which is to be expected as this variable is computed as an average over 3 months. The residence MW has a negative coefficient, although it is not statistically significant. We observe precisely estimated null pre-trends. Panel B controls for year-month fixed effects, and shows similar though weaker patterns. We see this evidence as supportive of the view that the estimated effects using our main rental variable are not driven by changes in the composition of listings that coincide with changes in the MW.

5.1.4 Robustness checks

Table 3 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 3 groups the results when varying the regression equation. Row (b) shows that our results are very similar when 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 conclusions. In all these cases our baseline point estimates are contained in relevant confidence intervals and, in the case of CBSA by month 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 non-significant when using state by monthly date fixed effects. While our baseline estimates are within relevant confidence intervals, the signs of the point estimates are flipped. However, these results are much noisier. In fact, a formal test does not reject that the coefficients on the workplace MW ($p = 0.2168$) and the residence MW ($p = 0.2668$) are identical to our baseline. Row (f) includes ZIP code fixed effects in the first differences model, which is equivalent to allowing for a ZIP code-specific linear trend in the model in levels. These results are also very similar to our baseline.

Panel B of Table 3 estimates the baseline model but computing the workplace MW using alternative commuting structures. Rows (g) and (h) use commuting shares from 2014 or 2018 instead of 2017. 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 with MW changes are unlikely to be the driver of the results. Furthermore, as discussed in the last subsection of Section 2.3, changing commuting shares would load on the workplace MW. However, we see no effects of the workplace MW beyond the month of the change, suggesting stable commuting shares as well. Finally, rows (j) and (k) use 2017 commuting shares for workers that earn

[28] While our baseline analysis in Table 2 uses 80,241 observations, the models that are estimated using the ZORI index use only 22,984.

less than \$1,251 per month and workers that are less than 29 years old, respectively. If anything, the results seem stronger and more significant in this case, consistent with the idea that these workers are more likely to earn close to the minimum wage.

5.1.5 County-level estimates

Can we rely on larger geographical units to estimate the effect of the workplace MW? In this subsection, we compare our results with estimates obtained from an alternative panel where the unit of observation is the county by month, using median rents from Zillow and commuting shares aggregated at this level. Online Appendix Figure 14 shows our estimates of a dynamic model. The point estimates on impact have the same sign as our main results, although they are much noisier. We also observe some evidence of pre-trends in the coefficients of this model, suggesting that estimates obtained at a larger geographical resolution may not rely on plausibly exogenous identifying variation. Finally, we note that these estimates would miss rich heterogeneity of effects within counties.

5.2 Alternative Strategies

Online Appendix Table 4 estimates our main models using a "stacked" sample, as discussed in Section 4.4. Our sample contains 184 "events," that is, CBSA-month pairs that had some strict subset of ZIP codes increasing the residence MW and had at least 10 ZIP codes. These estimates interact the year-month fixed effects with event ID indicators, limiting comparisons to ZIP codes in the same event. This is in line with recent literature that focuses on carefully selecting the comparison groups in difference-in-differences settings (de Chaisemartin and D'Haultfoeuille 2022; Roth et al. 2022). We find that our key MW-based measures have little predictive power on their own, but the model including both measures yields similar patterns as our baseline. If anything, results are stronger in this case. Now, both MW measures are strongly significant. A 10 percent increase in both MW measures is estimated to increase rents by 0.463 percent. Online Appendix Figure 15 shows the results of a similar model that includes leads and lags of the workplace MW. Estimates of leads and lags are statistically non-distinguishable from zero. However, they are noisier than in our baseline.

Online Appendix Table 5 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 MW measures. To avoid the well-known 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). Columns (1) and (2) show estimates of models in levels, both of which imply confidence intervals for the coefficients that include our preferred estimates. Columns (3) and (4) show preferred models in first differences, where results are very similar across strategies.

5.3 Heterogeneity and Sample Selection Concerns

Table 4 explores heterogeneity of our estimates. Column (1) reproduces the baseline results. Column (2) presents estimates interacting the MW measures with an estimated share of MW workers residing in each ZIP code. At the mean share of MW workers, our estimates indicate that the coefficient on the workplace MW is 0.097 (SE = 0.030). For a ZIP code that is one standard deviation above the average share of MW workers, the effect of the workplace MW is stronger at 0.181 (SE= 0.065). Housing more MW workers in a ZIP code implies that income is likely to be more sensitive to the MW and so, consistent with our model, the effect of the MW on rents is larger. The coefficient on the residence MW also presents significant heterogeneity.

Column (3) of Table 4 interacts both MW measures with the standardized median household income from the ACS. We find analogous patterns to Column (2), as a higher median income is correlated with a lower share of MW workers. Column (4) interacts the MW measures with the standardized share of public housing units. The effects for ZIP codes with more public housing seems larger, although the coefficient on the interaction is not statistically significant. This result suggests that public housing does not necessarily diminish the scope for landlords to increase rents. However, it is possible that this variable is capturing a high presence of low-wage residents and workers who, per our previous discussion, are more affected by the MW.

Online Appendix Table 3 explores the sensitivity of our estimates to the sample of ZIP codes used in estimation. Column (1) replicates our baseline estimates. In column (2) we estimate the same model but re-weighting observations to match pre-treatment characteristics of the sample of urban ZIP codes, defined in Table 1.[29] The coefficient on the workplace MW is somewhat smaller, but it remains strongly significant. Column (3) uses an unbalanced sample of ZIP codes and controls for quarter-of-entry by year-month fixed effects. The coefficient on the workplace MW is again somewhat smaller than our baseline, and now it is significant only at the 10% level.

5.4 Alternative rental categories

Online Appendix Table 6 shows how our results change when we use other rental categories available in the Zillow data. For each rental variable we use an unbalanced panel that controls for year-month fixed effects interacted with indicators for the quarter of entry to the data in the given rental category. We note that the number of observations varies widely across housing categories, and is always much lower than for our baseline SFCC variable.

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. We observe that the sum of the coefficients on our MW variables is statistically significant at conventional levels in the categories "Single Family" (SF), "Condominium and Cooperative Houses" (CC), and "Multifamily 5+ units." Online Appendix Figure 4 shows that low-wage households are likely to reside in these type of housing units. However, the coefficients on each of the MW measures are typically much noisier than baseline. We observe inconsistent results for the category "1 bedroom," for which the sign of the coefficients is flipped

[29]Our weights follow Hainmueller (2012) and are designed to match the averages of three variables from the 2010 US Census: the share of urban households, the share of renter-occupied households, and the shares of white households.

relative to baseline. However, these estimates are not statistically significant.

5.5 Summary and Discussion

We find strong evidence that increases in the MW at workplace locations increase rents, supporting the view that MW policies spill over across local housing markets through commuting. Our baseline estimated elasticity of rents to the MW is 0.0685. The magnitude of our estimates is similar to estimates of the elasticity of restaurant prices to the MW (Allegretto and Reich 2018 finds an average restaurant price elasticity of 0.058), and the elasticity of grocery store prices to the MW (Leung 2021 finds an elasticity ranging from 0.06 to 0.08; see also Renkin et al. 2020).

While our setting is somewhat different from existing research on the elasticity of wages to the MW, our estimates are consistent with it. For instance, estimates in Cengiz et al. (2019) (obtained using state MW events), and a share of MW workers of 0.14 (as in Table 4), imply an elasticity of income to the MW of 0.094.[30] Hughes (2020, Table 1) finds an elasticity of household income of affected households to the MW of 0.189. Dube (2019b) finds minimum wage elasticities of household income between 0.152 and 0.430 for households at the lower end of the distribution. Wiltshire et al. (2023) explores recent large MW increases in the US and finds an earnings elasticity of 0.18 for fast food workers. For a given elasticity of wages to the MW ε, and a share of housing expenditure s, our elasticity of rents to the workplace MW implies that $\gamma = 100 \times (s0.0685/\varepsilon)$ percent of the income generated by the MW is spent in housing. For instance, if we assume $s = 1/3$ and $\varepsilon = 0.1$, then $\gamma = 22.8$.

Next, we compare our estimates to existing research on the effect of the MW on rents. Exploiting heterogeneity in MW levels across Japanese prefectures, Yamagishi (2021) estimates an elasticity in 0.030–0.045, comparable to our estimated impact of an increase in both the residence and workplace MW. Using data at the county by year level for the US, Yamagishi (2019, Tables 1 and 2) obtains significant results using densely populated counties, reporting an elasticity of rents to the MW of 0.0365 in the first year, and of 0.1059 percent four years later. Our ZIP code-level estimates yield similarly sized elasticities.[31] Our results are also similar to Hughes (2020, Table 1), who uses cross-sectional survey data from the ACS and finds an elasticity of rents to the MW for affected households of 0.0543. However, our estimates are significantly smaller than those in Agarwal et al. (2022). While the main goal of this paper is to study eviction risk, the authors provide estimates of the effect of the MW on rents using individual-level transactions from 2000 to 2008 and state-level MW changes. Agarwal et al. (2022, Section 5.1) show an increase in nominal rents of 7.3%. Since the average MW change the authors use is about 10%, these results implies an elasticity of 0.73, an order of magnitude larger than our estimates. While it is hard to pin down the exact reason for this difference, we see this value as large given the existing literature on income, price, and rent effects. None of these papers account for the divergence between workplace and residence MW across granular geographical locations.

[30] Cengiz et al. (2019, Table I) find that a "MW event" increases wages by 6.8 percent, and their average MW event represents an increase of 10.1 percent. Assuming that 14 percent of workers in a location earn the MW results in an elasticity of $(6.8/10.1) \times 0.14 \approx 0.0943$. In their data, the authors find that "8.6% of workers were below the minimum wage." Our estimates of this share is likely larger because we account for local MW policies.

[31] Yamagishi (2019, Table 3) detects significant pre-trends in these estimates, questioning their validity.

6 Counterfactual Analysis

We use our empirical results to explore the incidence of counterfactual MW policies across space. We evaluate two policies: an increase in the federal MW from \$7.25 to \$9, and an increase in the local MW of the city of Chicago from \$13 to \$14. To measure incidence, we compute the share of the extra income generated by the policy that is pocketed by landlords.

6.1 Empirical Approach

Following the notation in Section 2, define the ZIP code-specific share pocketed by landlords as

$$\rho_i = \frac{\Delta H_i R_i}{\Delta Y_i} = \frac{H_i^{\text{Post}} R_i^{\text{Post}} - H_i^{\text{Pre}} R_i^{\text{Pre}}}{\Delta Y_i}$$

where "Pre" and "Post" denote moments before and after the MW change, $H_i R_i = \sum_{z \in \mathcal{Z}_i} H_{iz} R_{iz}$ denotes total housing expenditure in i, and $Y_i = \sum_{z \in \mathcal{Z}_i} Y_{iz}$ denotes total wage income in i.

Changes in rented square footage (if any) are unobserved. Therefore, we assume $H_i^{\text{Pre}} = H_i^{\text{Post}} = H_i$ and the share becomes

$$\rho_i = \frac{H_i^{\text{Post}} R_i^{\text{Post}} - H_i^{\text{Pre}} R_i^{\text{Pre}}}{\Delta Y_i} = H_i \frac{\Delta R_i}{\Delta Y_i}. \tag{5}$$

If $\Delta H_i > 0$ instead, then our estimates of ρ_i will be a lower bound.

We predict rent changes for all ZIP codes using the model in equation (4). Because we are interested in the partial effect of the policy, we hold constant common shocks affecting all ZIP codes, local economic trends reflected in the controls, and idiosyncratic shocks that show up in the error term. Then,

$$\Delta r_i = \beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}}. \tag{6}$$

We define the change in log total wages using a first-differenced model as well:

$$\Delta y_i = \varepsilon \Delta \underline{w}_i^{\text{wkp}}, \tag{7}$$

where $y_i = \ln Y_i$. The residence MW is excluded because we are considering the effect of the MW on nominal wages. We estimate ε using IRS data aggregated at the ZIP code level. Estimates of the income elasticity ε are not readily available in the literature. Given the discussion in Section 5.5, we set a baseline value of $\varepsilon = 0.1$. However, we show how our results change for different values of ε.

Assuming that we know the value of ε, we can substitute (6) and (7) into equation (5) to obtain

$$\begin{aligned} \rho_i &= H_i \left[\frac{\exp\left(\Delta r_i + r_i\right) - R_i}{\exp\left(\Delta y_i + y_i\right) - Y_i} \right] \\ &= s_i \left[\frac{\exp\left(\beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}}\right) - 1}{\exp\left(\varepsilon \Delta \underline{w}_i^{\text{wkp}}\right) - 1} \right] \end{aligned}$$

where $s_i = \left(H_i R_i\right) / Y_i$ is the share of i's expenditure in housing. As discussed in Section 3.2, we estimate this share as the ratio of the 2-bedroom SAFMR rental value, $\tilde{R}_i$, and monthly average wage per household, $\tilde{Y}_i$.

We also compute the total incidence of the policy on ZIP codes $i \in \mathcal{Z}_1$, for some subset $\mathcal{Z}_1 \subseteq \mathcal{Z}$, as follows:

$$\rho_{\mathcal{Z}_1} = \frac{\sum_{i \in \mathcal{Z}_1} \tilde{R}_i \left(\exp\left(\beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}} \right) - 1 \right)}{\sum_{i \in \mathcal{Z}_1} \tilde{Y}_i \left(\exp\left(\varepsilon \Delta \underline{w}_i^{\text{wkp}} \right) - 1 \right)}.$$

In words, total incidence is defined as the ratio of the total change in rents per household in $\mathcal{Z}_1$ to the total change in wage income per household in $\mathcal{Z}_1$.

6.2 Results

We use our estimates to compute the shares $\{\{\rho_i\}_{i \in \mathcal{Z}_1}, \rho_{\mathcal{Z}_1}\}$ for two counterfactual scenarios: an increase of the federal MW from \$7.25 to \$9 and an increase in the Chicago City MW from \$13 to \$14. In the federal case, we let $\mathcal{Z}_1$ be the set of ZIP codes located in urban CBSAs (as defined in Table 1) and exclude ZIP codes that are part of a CBSA where the average estimated increase in log total wages is less than 0.1%.[32] In the local case, we let $\mathcal{Z}_1$ represent ZIP codes in the Chicago-Naperville-Elgin CBSA, which are the most exposed to this policy.

6.2.1 Counterfactual increases in residence and workplace MW levels

We compute the counterfactual statutory MW in January 2020 at a given ZIP code by taking the max between (i) the state, county, and local MW in December 2019, and (ii) the assumed value for the federal or city MW in January 2020.[33] Then, we compute the counterfactual values of the residence MW and the workplace MW following the procedure outlined in Section 3.3.2. Like in our baseline estimates, we use commuting shares for all workers in 2017.

Federal increase. The distributions of counterfactual increases in the MW measures are displayed in Online Appendix Figure 16. Out of the 6,784 ZIP codes that satisfy our criteria, 1,043 (or 15.4%) experience no increase in the residence MW at all. The residence MW increases in 5,741 ZIP codes (or 84.6%), 3,616 of which were bound by the previous federal MW, and so the residence MW increases by $\ln(9) - \ln(7.25) \approx 0.2162$ in them. Correspondingly, we observe mass points in the distribution of the residence MW, with the two largest ones at 0 and 0.2162. Since many people reside and work under the same statutory MW, the mass points are still visible in the histogram of the workplace MW. However, we observe more places experiencing moderate increases in this measure.

[32] The goal of this restriction is to exclude metropolitan areas located in jurisdictions with a MW level above the new counterfactual federal level. Because all those ZIP codes experience a small and similar increase in the workplace MW, the estimated share pocketed will be equal to the estimated housing expenditure share times the constant $(\exp(\beta x) - 1) / (\exp(\varepsilon x) - 1)$, where x is the value of the workplace MW increase. These estimates, however, are not economically meaningful because the increase in income due to the policy is negligible.

[33] To be more precise, we take the maximum between the MW levels of different jurisdictions at the level of the block. Then, we aggregate up to ZIP codes using the correspondence table in Online Appendix B.1. We do so to account for the fact that the new MW policy may be partially binding in some ZIP codes.

Panel A of Online Appendix Figure 17 maps the changes in the residence and workplace MW in the Chicago-Naperville-Elgin CBSA. Unlike in Figure 1, we observe the MW increasing from the outside of Cook County and spilling over inside it.

Local increase. In our second counterfactual experiment we increase the Chicago City MW from $13 to $14 on January 2020, keeping constant other MW policies. Importantly, under this assumption the difference between the Chicago and Cook County MW levels increases by $1.

In this case, there are 62 ZIP codes whose residence MW are affected by this change and 323 that remain directly unaffected. Panel B of Online Appendix Figure 17 shows the changes in both MW measures after this policy. As expected, we observe large increases in the workplace MW in the city, which become smaller as one moves away from it.

6.2.2 The share of extra wage income pocketed by landlords

We couple the counterfactual increases in residence and workplace MW with estimates of β and γ. Following the results in Table 2, we take $\beta = 0.0685$ and $\gamma = -0.0219$. Based on previous discussion, we set $\varepsilon = 0.1$. We follow the procedure outlined in the previous subsection to estimate the incidence of the counterfactual policy.

Federal increase. Panel A of Figure 5 displays a histogram of the estimated shares $\{\rho_i\}_{i\in\mathcal{Z}_1}$. The median share is 0.101, which implies that at the median ZIP code landlords capture roughly 10 cents of each dollar. The distribution of the shares is skewed to the right. However, we observe a long left-tail with a few negative values which arise due to declines in rents in locations where the increase in the residence MW is much larger than the increase in the workplace MW.

Panel A of Figure 6 maps the estimated shares in the Chicago-Naperville-Elgin CBSA. Panel A of Online Appendix Figure 18 shows estimated increases in rents and wage income. We estimate a larger share pocketed in Cook County. The reason is that these ZIP codes experience the new policy only through their workplace MW and, as a result, rents increase relatively more than wage income. We also observe a larger incidence on landlords in the south of Cook County, where the housing expenditure share is larger.

The top rows of Panel A in Table 5 show the medians of the key estimated objects for two groups: ZIP codes where the residence MW did not change, and ZIP codes where it did. ZIP codes in the first group see rent increases that are moderated by the negative effect of the residence MW. The median incidence on landlords for this group is 9.6 cents of each dollar. Locations in the second group are only affected through changes in the workplace MW, so median incidence for this group is larger at 15.7 cents of each dollar. The bottom row of Panel A in Table 5 shows our estimate of total incidence of the policy, which is given by 0.092. The share is lower than the median values reported earlier because landlords capture more in locations with lower rent increases.

More generally, one can think of the average share for different values of the gap between the residence MW and the workplace MW, i.e., $\Delta\underline{w}_i^{\text{wkp}} - \Delta\underline{w}_i^{\text{res}}$. Figure 7 displays the average estimated share for each decile of that gap. We observe a positive and nearly monotonic relation. The share is lower in ZIP codes that had a low increase in the workplace MW relative to the residence MW, highlighting how the share pocketed depends on the incidence of the federal MW increase on the MW measures.

Local increase. Panel B of Figure 5 shows the distribution of the estimated shares in the Chicago-Naperville-Elgin CBSA. Panel B of Table 5 displays median values for ZIP codes inside the city and outside it. The incidence on landlords is of 9.1 cents of each dollar for the median directly treated ZIP code and of 15.6 cents for the median not directly treated one.

Panel B of Figure 6 maps the shares. Panel B of Online Appendix Figure 18 shows the estimated changes in rents and total wages. Unlike the previous exercise, the share pocketed by landlords is now higher right outside of Chicago City. Many commuters ~~to the city~~ reside there, and thus the workplace MW changes the most. This translates into higher rent increases, implying a large share pocketed.[34]

Sensitivity to ε. Our estimates of the incidence of the policy depend on the value of the income elasticity ε. Online Appendix Figure 19 displays total share pocketed by landlords for different values of ε, both for the federal and the local counterfactual policies. As expected, the share pocketed is decreasing in ε. For instance, for the federal policy the share pocketed would be somewhat larger about 0.16 cents of each dollar if $\varepsilon = 0.06$ (instead of 0.1).

6.3 Discussion

Overall, we observe that landlords capture a significant portion of the income generated by MW policies. We also found strong spatial heterogeneity of the incidence of the policy depending on commuting patterns. The share pocketed by landlords tends to be larger in ZIP codes located in jurisdictions where the MW policy did not change, particularly those located close to the MW change as many of their residents work under the new MW level and experience no change in the residence MW. According to the model in Section 2, the mechanism behind this result is the offsetting effect of increases in prices of non-tradable consumption in the same location.

Because of the housing market, the impact of the MW will be less equalizing in terms of the distribution of real incomes than nominal incomes. There are many reasons for this. First, poorer areas tend to have a higher share of expenditure in housing. Second, as we discussed in Section 3.2, low-wage households are more likely to rent. Finally, in the case of high-income cities enacting MW policies, affected low-wage individuals are more likely to live outside the city where rent increases will be larger, although the share pocketed by landlords will also be lower as the residence MW is not changing there.

[34] It is worth emphasizing that we estimate large increases in wage income inside the city due to the fact that our model in (7) excludes heterogeneity based on the share of MW workers. In a setting where this equation accounts for the share of MW workers we would not expect a strong effect on wages inside the city.

7 Conclusions

We explore whether minimum wage changes affect housing rental prices, and whether MW shocks propagate spatially through commuting. To answer this question we develop a theoretical approach that accounts for the fact that MW workers typically reside and work in different locations. Our model suggests that MW changes at workplaces will tend to increase rents, and highlights the importance of accounting for the MW at the residence location when estimating the effect of the workplace MW on rents.

We collect data on rents, statutory MW levels, and commuting flows, and estimate the effect of the residence and workplace MW on rents. We find evidence supporting the main conclusions of our model: the workplace MW increases rents, and thus MW policies spill over spatially through commuting. Our conclusions are robust to a variety of robustness checks, and suggest stronger effects in locations that are residence to more MW workers. Our two-parameter model is able to capture rich heterogeneity in the effect of the MW on rents depending on the prevailing commuting structure.

To explore the incidence of the MW on landlords, we explore two counterfactual MW policies. Our results suggest that landlords pocket a non-negligible portion of the newly generated wage income, and that this share varies spatially. Because low-wage households are more affected by MW policies and more likely to be renters, the omission of the housing market channel would lead to an overstatement of the equalizing effects of the MW on disposable income.

Our analysis takes a partial equilibrium perspective, exploring the incidence of small increases in the MW within metropolitan areas. However, one would expect general equilibrium adjustments to large changes in MW levels, such as worker mobility and changes in housing supply. Exploring these issues in the context of a spatial model with worker mobility that distinguishes between renters and homeowners appears as a fruitful avenue for future work.

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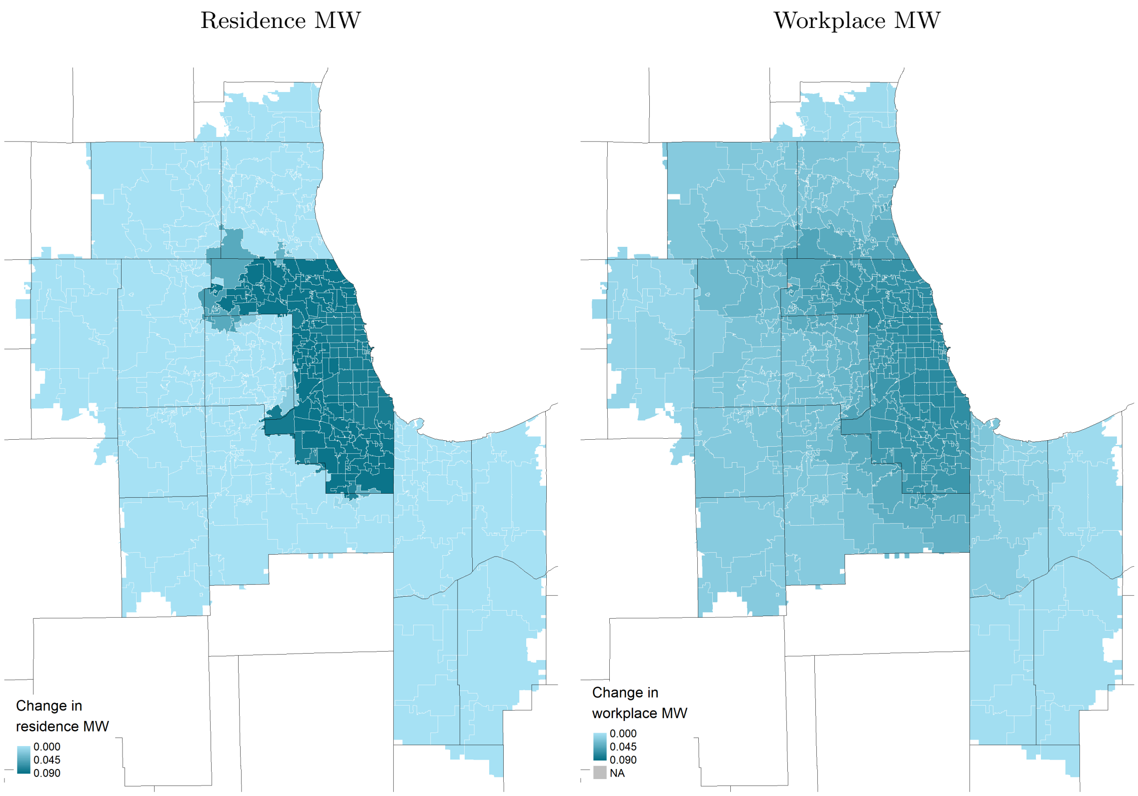
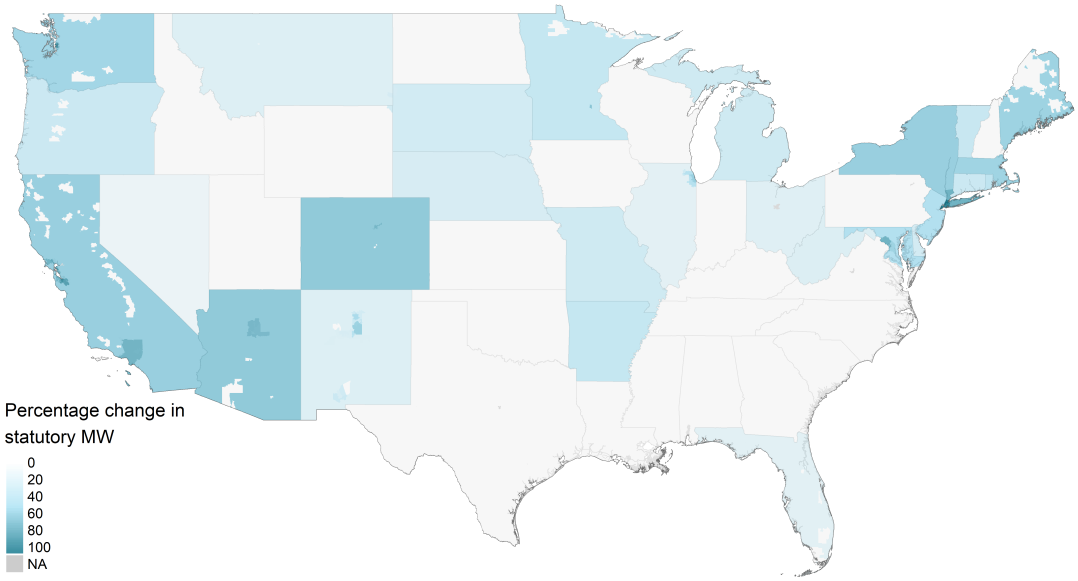
Figures and Tables

Figure 1: Changes in minimum wage measures in the Chicago-Naperville-Elgin CBSA, July 2019



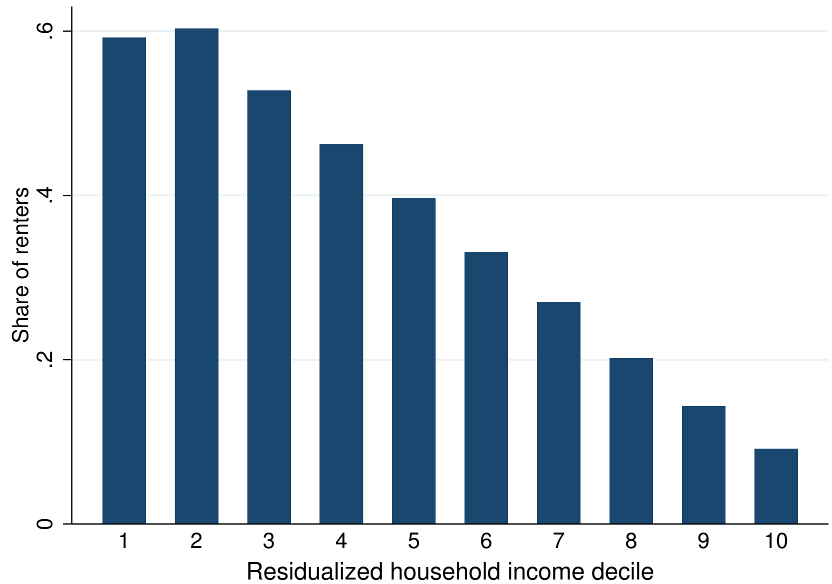
Notes: Data are from the MW panel described in Section 3.1 and from LODES. The figures show changes in the MW measures in July 2019 in the metropolitan area of Chicago. The figure on the left shows the change in the residence MW. The figure on the right shows the change in the workplace MW. The residence 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. Smaller colored polygons correspond to ZIP codes, and larger polygons correspond to counties.

Figure 2: Spatial distribution of minimum wage changes between January 2010 and June 2020, mainland US



Notes: Data are from the MW panel described in Section 3.1. The figure maps the percentage change in the statutory MW level in each ZIP code from January 2010 to June 2020.

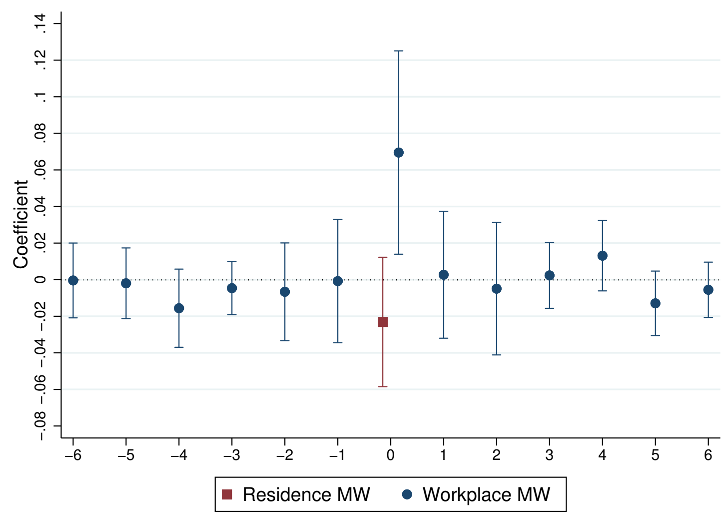
Figure 3: Probability of being a renter by household income decile, full sample



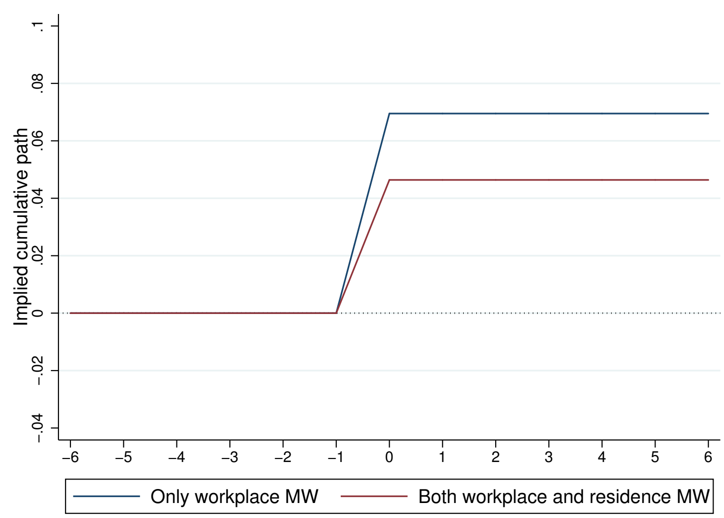
Notes: Data are from the 2011 and 2013 American Housing Surveys. The figure shows the probability of a household living in a rented unit by household income. We construct the figure as follows. First, we residualize an indicator for being a renter and household income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized household income variable. Finally, we take the average of the residualized renter indicator within each decile. We exclude from the calculation non-conventional housing units, such as mobile homes, hotels, and others.

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

Panel A: Estimates in first differences



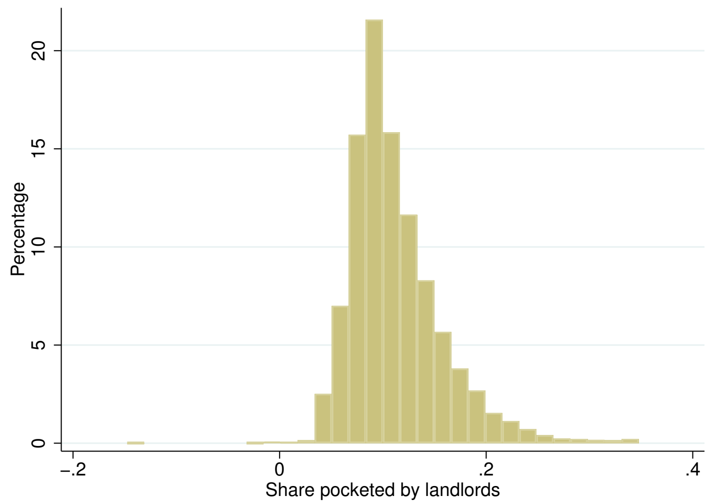
Panel B: Implied path in levels



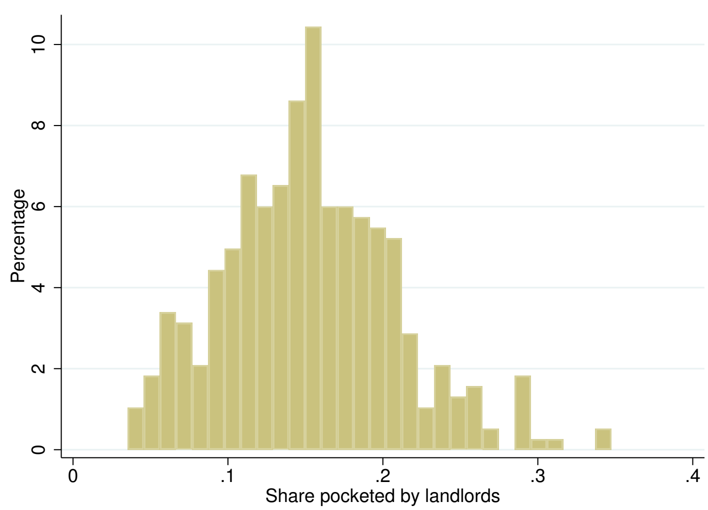
Notes: Data are from the baseline estimation sample described in Section 3.3.4. The top panel shows coefficients from regressions of the change in log of rents per square foot on leads and lags of the change in the workplace MW and the change in the residence MW. The bottom panel shows the implied paths in levels given the estimated coefficients. The regression includes time-period fixed effects and economic controls that vary at the county by month and county by quarter 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.

Figure 5: Estimated shares pocketed by landlords under counterfactual MW policies

Panel A: Increase in federal MW to $9, urban ZIP codes



Panel B: Increase in Chicago MW to $14, Chicago-Naperville-Elgin CBSA

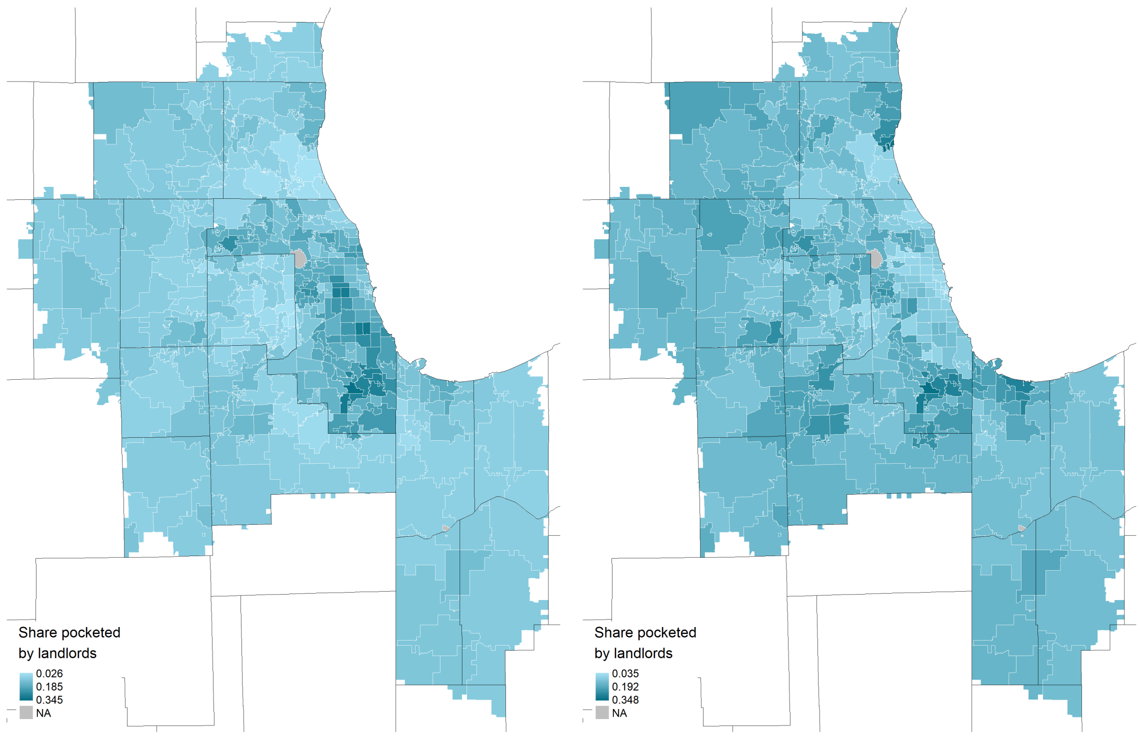


Notes: Data are from the MW panel described in section 3.1 and from LODES. The figures show the distribution of the estimated ZIP-code specific shares of additional income pocketed by landlords ("share pocketed") under different counterfactual policies. Panel A is based on a counterfactual increase to $9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. Panel B is based on a counterfactual increase from $13 to $14 in the Chicago City MW, also holding constant other MW policies. The unit of observation is the ZIP code. Panel A includes ZIP codes located in urban CBSAs where the estimated increase in income was higher than 0.1. Panel B includes ZIP codes in the Chicago-Naperville-Elgin CBSA. The share pocketed is defined as the ratio between the percent increase in rents and the percent increase in total wages multiplied by the share of housing expenditure in the ZIP code. To estimate it we follow the procedure described in Section 6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$.

Figure 6: Estimated shares pocketed by landlords under counterfactual MW policies, Chicago-Naperville-Elgin CBSA

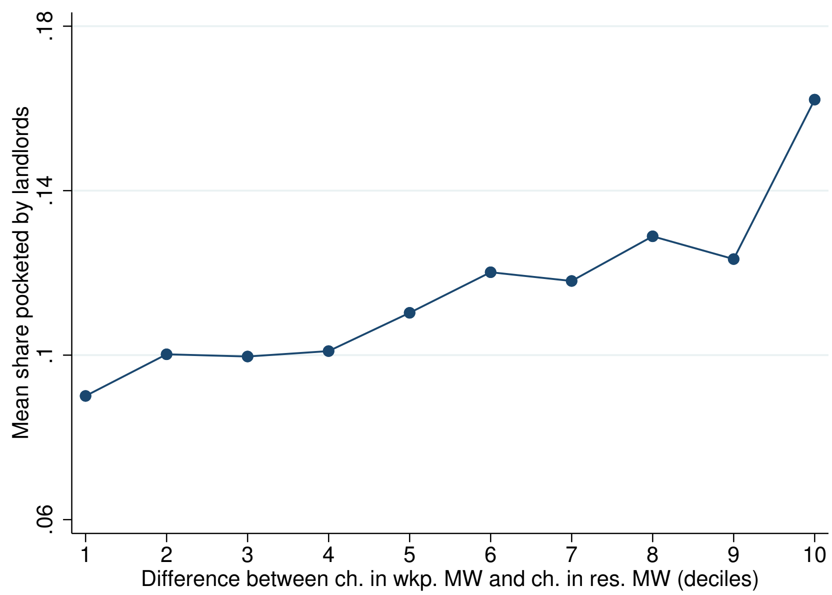
Panel A: Increase in federal MW from \$7.25 to \$9

Panel B: Increase in Chicago MW from \$13 to \$14



Notes: Data are from the MW panel described in Section 3.1 and from LODES. The figures map the estimated ZIP code-specific shares of additional income generated by the MW that are pocketed by landlords, for different counterfactual MW policies. Panel A is based on a counterfactual increase from \$7.25 to \$9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. Panel B is based on a counterfactual increase from \$13 to \$14 in the Chicago City MW, also holding constant other MW policies. The share pocketed is defined as the ratio between the percent increase in rents and the percent increase in total wages multiplied by the share of housing expenditure in the ZIP code. To estimate it we follow the procedure described in Section 6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$.

Figure 7: Share pocketed by landlords by intensity of treatment, urban ZIP codes under federal MW increase to $9



Notes: Data are from the MW panel described in Section 3.1 and from LODES. The figure shows the average estimate of the shares of additional income pocketed by landlords ρ_i for each decile of the difference $\Delta \underline{w}_i^{\text{wkp}} - \Delta \underline{w}_i^{\text{res}}$. Estimates for lower deciles correspond to ZIP codes where the increase in residence MW was relatively large. The unit of observation is the urban ZIP code, where we define a ZIP code as urban if it belongs to a CBSA with at least 80% of its population classified as urban by the 2010 Census. The share pocketed is defined as the ratio between the percent increase in rents and the percent increase in total wages multiplied by the share of housing expenditure in the ZIP code. To estimate it we follow the procedure described in Section 6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1013$. The figure excludes ZIP codes located in the 61 CBAs for which the average estimated change in log total wages was below 0.1.

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	204,585.8	111,709.2	51,181.1
Total number of households (thousands)	131,396.0	83,919.6	47,424.5	21,628.7
Mean population	9,681.7	18,018.8	33,687.9	38,052.9
Mean number of households	4,128.6	7,391.2	14,301.7	16,080.8
Share of urban population	0.391	0.725	0.960	0.972
Share of renter-occupied households	0.224	0.283	0.340	0.333
Share of black population	0.075	0.100	0.153	0.161
Share of white population	0.834	0.765	0.679	0.667
Panel B: 2014 IRS				
Share of households with wage income	0.820	0.830	0.836	0.843
Share of households with business income	0.152	0.161	0.176	0.182
Mean AGI per household (thousand $)	60.4	76.3	83.0	83.9
Mean wage income per household (thousand $)	39.7	49.8	53.2	55.2
Panel C: 2014 SAFMR				
Mean 40th perc. 2BR apt. rent ($)	936.17	1,028.33	1,087.42	1,131.95
Panel D: Minimum wage				
Min. in Dec. 2014 ($)	7.25	7.25	7.25	7.25
Mean in Dec. 2014 ($)	7.74	7.97	7.94	7.87
Max. in Dec. 2014 ($)	15.00	15.00	11.27	10.74
Min. in Dec. 2019 ($)	7.25	7.25	7.25	7.25
Mean in Dec. 2019 ($)	8.85	9.52	9.40	9.23
Max. in Feb. 2019 ($)	16.09	16.09	16.00	16.00
Panel E: Geographies				
Number of ZIP codes	31,826	11,354	3,316	1,345
Number of counties	3,135	605	487	244
Number of states	51	47	49	41

Notes: The table shows characteristics of different samples of ZIP codes. The first column uses all ZIP codes that are matched to a census block following Online Appendix B.1. The second column restricts to ZIP codes located in urban CBSAs, where we define a CBSA as urban if at least 80% of its population was classified as urban by the 2010 US Census. The third column uses ZIP codes with valid SFCC rents per square foot in any month. The fourth column uses our baseline estimation sample, as described in Section 3.3.4. Panel A uses data from the 2010 US Census (US Census Bureau 2022b). Panel B uses data from the 2014 IRS ZIP code-level aggregates (Internal Revenue System 2022b). AGI is an acronym for Average Gross Income. Panel C uses data from the 2014 Small-Area Fair Market Rents (SAFMR; US Department of Housing and Urban Development 2020b). Panel D uses data from the panel of MW levels described in Section 3.1. Panel E counts the number of different geographies present in each set of ZIP codes, assigned as explained in Section 3.3.3.

Table 2: Estimates of the effect of the minimum wage 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 $\Delta \underline{w}_{it}^{\text{res}}$	0.8627	0.0372		-0.0219
	(0.0374)	(0.0145)		(0.0175)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$			0.0449	0.0685
			(0.0156)	(0.0288)
Sum of coefficients				0.0466
				(0.0158)
Economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0514
R-squared	0.9444	0.0212	0.0213	0.0213
Observations	80,241	80,241	80,241	80,241

Notes: Data are from the baseline estimation sample described in Section 3.3.4. 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 include time-period fixed effects and economic controls that vary at the county by month and county by quarter levels. The measure of rents per square foot corresponds 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 parentheses are clustered at the state level.

Table 3: Robustness of estimates of the effect of the minimum wage on rents, baseline sample

	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	0.8627	-0.0219	0.0685	0.0466	80,241
	(0.0374)	(0.0175)	(0.0288)	(0.0158)	
Panel A: Vary specification					
(b) No controls	0.8632	-0.0200	0.0668	0.0468	80,692
	(0.0374)	(0.0180)	(0.0291)	(0.0162)	
(c) County by time FE	0.2857	-0.0606	0.1559	0.0953	75,593
	(0.0399)	(0.0511)	(0.1116)	(0.0811)	
(d) CBSA by time FE	0.5081	-0.0358	0.0944	0.0587	78,293
	(0.0387)	(0.0295)	(0.0610)	(0.0343)	
(e) State by time FE	0.5405	0.0142	-0.0076	0.0066	80,393
	(0.0629)	(0.0239)	(0.0526)	(0.0320)	
(f) ZIP code-specific linear trend	0.8596	-0.0217	0.0711	0.0494	80,241
	(0.0390)	(0.0167)	(0.0264)	(0.0132)	
Panel B: Vary workplace MW measure					
(g) 2014 commuting shares	0.8625	-0.0199	0.0662	0.0463	80,241
	(0.0377)	(0.0193)	(0.0299)	(0.0158)	
(h) 2018 commuting shares	0.8626	-0.0217	0.0683	0.0466	80,241
	(0.0372)	(0.0177)	(0.0292)	(0.0159)	
(i) Time-varying commuting shares	0.8806	-0.0292	0.0792	0.0500	64,236
	(0.0372)	(0.0207)	(0.0309)	(0.0166)	
(j) 2017 commuting shares, low-income workers	0.8566	-0.0348	0.0841	0.0493	80,241
	(0.0371)	(0.0221)	(0.0341)	(0.0160)	
(k) 2017 commuting shares, young workers	0.8569	-0.0332	0.0822	0.0490	80,241
	(0.0390)	(0.0180)	(0.0294)	(0.0156)	

Notes: Data are from the baseline estimation sample described in Section 3.3.4. 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 on the change in the residence MW. 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 SFCC category in Zillow. Row (a) repeats the results of Table 2, including fixed effects for each year month and economic controls from the QCEW. 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). Row (i) uses data from 2015 to 2018 only. Standard errors in parentheses are clustered at the state level.

Table 4: Heterogeneity of estimates of the effect of the minimum wage on rents, baseline sample

	Change log rents Δr_{it}			
	(1)	(2)	(3)	(4)
Change res. MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0219	-0.0483	-0.0377	-0.0158
	(0.0175)	(0.0196)	(0.0254)	(0.0150)
Change res. MW $\times$ Std. share of MW workers		-0.0801		
		(0.0392)		
Change res. MW $\times$ Std. median household income			0.0506	
			(0.0266)	
Change res. MW $\times$ Std. share of public housing				-0.0336
				(0.0330)
Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0685	0.0969	0.0862	0.0645
	(0.0288)	(0.0300)	(0.0356)	(0.0258)
Change wkp. MW $\times$ Std. share of MW workers		0.0841		
		(0.0445)		
Change wkp. MW $\times$ Std. median household income			-0.0608	
			(0.0344)	
Change wkp. MW $\times$ Std. share of public housing				0.0306
				(0.0374)
Mean heterogeneity variable		0.1497	60,457	0.0044
Std. dev heterogeneity variable		0.0468	22,923	0.0173
R-squared	0.0213	0.0212	0.0211	0.0213
Observations	80,241	75,329	77,197	79,701

Notes: Data are from the baseline estimation sample described in Section 3.3.4. In all columns we report the results of regressions of the log of median rents per square foot on our MW-based measures. Column (1) reproduces estimates our baseline results from Table 2. In column (2) the changes in residence and workplace MW levels are interacted with the standardized share of MW workers residing in the ZIP code, estimated as in Online Appendix B.2. In column (3) they are interacted with standardized median household income from the ACS (US Census Bureau 2022a). In column (4) they are interacted with the standardized share of public housing units. To construct this share we use total units of public housing in 2017 (US Department of Housing and Urban Development 2022a), and the number of households in the 2010 US Census (US Census Bureau 2022b). Standard errors in parentheses are clustered at the state level.

Table 5: Median effect of counterfactual minimum wage policies by treatment status

Panel A: Increase in federal MW to \$9, urban ZIP codes

	N	Change in res. MW	Change in wkp. MW	Share of housing exp.	Share Pocketed
Effect in ZIP codes with...					
previous MW $\leq$ \$9	5,741	0.216	0.204	0.214	0.097
previous MW $>$ \$9	1,043	0.000	0.013	0.232	0.159
Total incidence	6,784				0.093

Panel B: Increase in Chicago MW to \$14, Chicago-Naperville-Elgin CBSA

	N	Change in res. MW	Change in wkp. MW	Share of housing exp.	Share Pocketed
Effect in ZIP codes with...					
previous MW $\geq$ \$13	62	0.074	0.046	0.252	0.092
previous MW $<$ \$13	323	0.000	0.009	0.231	0.158
Total incidence	385				0.112

Notes: Data are from LODES origin-destination statistics, Small Area Fair Market Rents, IRS ZIP code aggregate statistics, and the MW panel described in Section 3.1. The table shows the median of the estimated ZIP code-specific shares of the additional income pocketed by landlords ("Share pocketed"), defined as the ratio of the increase in income to the increase in rents, for different groups of ZIP codes. Panel A is based on a counterfactual increase from \$7.25 to \$9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. Panel B is based on a counterfactual increase from \$13 to \$14 in the Chicago City MW, also holding constant other MW policies. In the last row of each panel, we report the total incidence of the counterfactual policy. We also report the median change in residence MW, change in workplace MW, and share of ZIP code-specific housing expenditure "Share of housing exp.") defined in Online Appendix B.3. Increases in income and rents are simulated following the procedure described in Section 6. We assume the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$. Panel A includes urban ZIP codes only and excludes ZIP codes located in 61 CBAs for which the average estimated change in log total wage income was below 0.1. Panel B includes all ZIP codes with valid data in the Chicago-Naperville-Elgin CBSA.

Online Appendix for "Minimum Wage as a Place-Based Policy: Evidence from US Housing Rental Markets"

A Model Appendix

A.1 Proofs

Proof of Proposition 1. Fully differentiate the market clearing condition with respect to $\ln R_i$ and $\ln \underline{W}_z$ for all $z \in \mathcal{Z}(i)$. Using (1) and appropriate algebraic manipulations, one can show that

$$\Big(\eta_i - \sum_z \pi_{iz}\xi_{iz}^R\Big) d\ln R_i = \sum_z \pi_{iz}\left(\xi_{iz}^P \epsilon_i^P d\ln \underline{W}_i + \xi_{iz}^Y \epsilon_{iz}^Y d\ln \underline{W}_z\right), \tag{8}$$

where $\xi_{iz}^x = \frac{dh_{iz}}{dx_i}\frac{x_i}{\sum_z \pi_{iz}h_{iz}}$ for $x \in \{R, P\}$ is the elasticity of the per-capita housing demand with respect to x evaluated at the average per-capita demand of ZIP code i, $\xi_{iz}^Y = \frac{dh_{iz}}{dY_z}\frac{Y_z}{\sum_z \pi_{iz}h_{iz}}$ represents the analogous elasticity with respect to income Y from each workplace z, $\epsilon_i^P = \frac{dP_i}{d\underline{W}_i}\frac{\underline{W}_i}{P_i}$ and $\epsilon_{iz}^Y = \frac{dY_z}{d\underline{W}_z}\frac{\underline{W}_z}{Y_z}$ are elasticities of prices and income to the MW, and $\eta_i = \frac{dS_i}{dR_i}\frac{R_i}{S_i}$ is the elasticity of housing supply in ZIP code i.

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

$$\frac{d\ln R_i}{d\ln \underline{W}_{z'}} = \left(\eta_i - \sum_z \pi_{iz}\xi_{iz}^R\right)^{-1} \pi_{iz'}\xi_{iz}^Y\epsilon_{iz'}^Y.$$

Because $\eta_i \geq 0$ and $\xi_{iz}^R < 0$ for all $z \in \mathcal{Z}(i)$, the first factor is positive. From Assumptions 1 and 2, $\epsilon_{iz}^Y \geq 0$ and $\xi_{iz}^Y > 0$. Therefore, the effect is positive if for z' we have $\frac{dY_{z'}}{d\underline{W}_{z'}} > 0$ (or $\epsilon_{iz'}^Y > 0$), and the effect is zero otherwise.

For ZIP code i the partial effect is given by

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

By Assumption 1 we have that $\epsilon_i^P > 0$ and that $\epsilon_{ii}^Y \geq 0$. By Assumption 2 we have that $\xi_{ii}^Y > 0$ and that, for all $z \in \mathcal{Z}(i)$, $\xi_{iz}^P < 0$. Then, the second parenthesis has an ambiguous sign. The third statement of the Proposition follows directly. □

Proof of Proposition 2. Under the stated assumptions we can manipulate (8) to write

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

where $\beta_i = \frac{\xi_i^Y \epsilon_i^Y}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} > 0$ and $\gamma_i = \frac{\sum_{z\in\mathcal{Z}(i)} \pi_{iz}\xi_{iz}^P \epsilon_i^P}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} < 0$ are parameters, which signs can be verified using Assumptions 1 and 2. $\square$

A.2 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 workers 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. We assume that all contracts last for one year, so that the stock is composed of contracts starting at different calendar months. We impose 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 levels at the time of contract sign-up. We assume that this demand function is decreasing in the residence MW and increasing in the 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.[35] 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, and by assumption $\sum_{\tau=t-11}^{t} \lambda_{i\tau} = 1$. It is convenient to define the stock of contracted square feet excluding the ones that were signed 12 months ago:

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

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

Within-period equilibrium

We assume the following timing: (1) At the beginning of period t, a share λ_{it} of 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}. Next, we develop each of these steps more formally.

At 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

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

Note that this differs from $S_i - H_{i,t-1}$, the non-rented square feet as of $t-1$. We denote by $V_{it}(R_{it}, \lambda_t)$

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

the supply of housing, increasing in R_{it}. A feasibility 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 workers go to the market and may desire to rent more square feet given changes in their income.

The market in period t clears if

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

Given statutory MW levels 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. The results in Section 2 can be extended to a dynamic setting if the demand and supply functions in t only depend on MW levels in t.

A.3 A static model with flexible commuting shares

In this section we use a simplified version of the partial-equilibrium model in Section 2. We do so to focus on the implications of relaxing the assumption of fixed commuting shares.

Assume that housing demand depends directly on the workplace and residence MW, abstracting away from the mediation channels of prices and income. Let commuting shares depend on the MW at the respective workplace location. Then, the housing market equilibrium can be written as

$$L_i \sum_{z\in\mathcal{Z}(i)} \pi_{iz}(\underline{W}_z) h_{iz} \left(R_i, \underline{W}_i, \underline{W}_z\right) = S_i(R_i). \tag{11}$$

Following empirical results in Pérez Pérez (2021), we assume that an increase in the workplace MW may decrease the share of workers traveling to a given destination.

Assumption 3 (Endogenous commuting shares)**.** *Commuting shares of location i's residents are given by $\{\pi_{iz}\left(\underline{W}_z\right)\}_{z\in\mathcal{Z}(i)}$, where $d\pi_{iz}/d\underline{W}_z \leq 0$.*

With this assumption we are ready to prove the following result.

Proposition 3 (Representation with endogenous shares)**.** *Assume that for all ZIP codes $z \in \mathcal{Z}(i)$ we have (a) homogeneous elasticity of per-capita housing demand to the MW at workplace locations, $\xi_{iz}^{wkp} = \xi_i^{wkp}$, and (b) homogeneous elasticity of commuting shares to the MW, $\zeta_{iz} = \zeta_i$. Then,* *approximating* $\pi_{iz}h_{iz}/\sum_{z'}\pi_{iz'}h_{iz'} \approx \pi_{iz}$*, we can write*

$$dr_i = (\beta_i + \zeta_i)\, d\underline{w}_i^{wkp} + \gamma_i d\underline{w}_i^{res}$$

where $r_i = \ln R_i$*,* $\underline{w}_i^{wkp} = \sum_{z\in\mathcal{Z}(i)} \pi_{iz} \ln \underline{W}_z$ *is ZIP code* i*'s* ***workplace MW****,* $\underline{w}_i^{res} = \ln \underline{W}_i$ *is ZIP code* i*'s* ***residence MW****, and* $\beta_i > 0$*,* $\zeta_i < 0$*, and* $\gamma_i < 0$ *are parameters.*

Proof. Fully differentiate the market clearing condition with respect to $\ln R_i$ and $\ln \underline{W}_z$ for all $z \in \mathcal{Z}(i)$. Using (11) and appropriate algebraic manipulations, one can show that

$$\left(\eta_i - \sum_z \pi_{iz}\xi_{iz}^R\right) d\ln R_i = \left(\sum_z \pi_{iz}\xi_{iz}^{\text{res}}\right) d\ln \underline{W}_i + \sum_z \pi_{iz}(\xi_{iz}^{\text{wkp}} + \zeta_{iz}) d\ln \underline{W}_z, \tag{12}$$

where we also impose the approximation that $\pi_{iz}h_{iz}/\sum_{z'}\pi_{iz'}h_{iz'} \approx \pi_{iz}$. In this expression $\xi_{iz}^R = \frac{dh_{iz}}{dR_i}\frac{R_i}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of housing demand to rents, $\xi_{iz}^{\text{res}} = \frac{dh_{iz}}{d\underline{W}_i}\frac{\underline{W}_i}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of housing demand to the MW at i, $\xi_{iz}^{\text{wkp}} = \frac{dh_{iz}}{d\underline{W}_z}\frac{\underline{W}_z}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of housing demand to the MW at workplace z, and $\zeta_{iz} = \frac{d\pi_{iz}}{d\underline{W}_z}\frac{\underline{W}_z}{\sum_z \pi_{iz}h_{iz}}$ is the elasticity of commuting shares to the MW at z.

Under the stated assumptions we can manipulate (12) to write

$$dr_i = (\beta_i + \zeta_i) d\underline{w}_i^{\text{wkp}} + \gamma_i d\underline{w}_i^{\text{res}} \tag{13}$$

where $\beta_i = \frac{\xi_i^{\text{wkp}}}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} > 0$, $\zeta_i = \frac{\zeta_i}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} \leq 0$, and $\gamma_i = \frac{\sum_z \pi_{iz}\xi_{iz}^{\text{res}}}{\eta_i - \sum_z \pi_{iz}\xi_{iz}^R} < 0$ are parameters. The sign of ζ_i is given by Assumption 3. □

This result implies that, up to an approximation, the response of rents to the workplace MW includes the negative effect of the MW on commuting shares.

B Data Appendix

B.1 Matching census blocks to USPS ZIP codes

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

First, we collected the GIS map of 11,053,116 blocks from US Census Bureau (2012) and computed their centroids. Second, we assigned each block to a unique ZIP code using the GIS map from ESRI (2020) based on assigning to each block the ZIP code that contains its centroid. If the centroid falls outside the block, we pick a point inside it at random. We assigned 11,013,203 blocks using the spatial match (99.64 percent of the total).[36] Third, for the blocks that remain unassigned we used the tract-to-ZIP-code correspondence from US Department of Housing and Urban Development (2022b). Specifically, for each tract we keep the ZIP code where the largest number of houses of the tract fall, and we assign it to each block using the tract identifier. We assigned 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 ZIP codes.

In the end, there are 11,036,022 blocks which are assigned to 31,754 ZIP codes, implying an average of 347.55 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.

B.2 Assigning minimum wage levels to USPS ZIP codes

Our main rents data is aggregated at the level of the USPS ZIP code. To match this geographical level, we assign statutory MW levels to ZIP codes. ZIP codes usually cross jurisdictions, and as a result parts of them are subject to different statutory MW levels. Trying to overcome this problem, we assign averages of the relevant MW levels to each ZIP code.

We proceed as follows. First, we collect a census crosswalk constructed by US Census Bureau (2021) that contains, for each 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 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 the original correspondence table described in Online Appendix B.1, we assign a ZIP code to each block. Finally, we define *the statutory MW* for 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.[37] For ZIP codes that have no housing units in them, such as those corresponding to universities or airports, we use a simple average instead.

[36] 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).

[37] ZIP codes between 00001 and 00199 correspond to federal territories. Thus, we assign as statutory MW the federal level.

Locating minimum wage earners

We approximate the share of people that earn at or below the MW as follows. First, we collect data on the number of workers in each tract from the 5-year 2010-2014 American Community Survey (US Census Bureau 2022a). Using our assignment of hourly statutory MW levels in January 2014 we compute the total yearly wage of a full-time worker earning the MW in each tract, which we denote by $\underline{YW}$.[38] We keep track of what wage bin $\underline{YW}$ falls into. We estimate the number of MW earners in a tract as the total number of workers in all bins below the one where $\underline{YW}$ falls plus a fraction of the total number of workers in the bin $\underline{YW}$ falls 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.[39] Finally, we compute the share of MW workers who reside in each ZIP code dividing our estimate of the number of MW workers by the total number of workers in the data.

Due to limitations in the ACS data, it is not possible to use the MW at workplace locations in the computation, nor to estimate the share of MW workers by workplace.

B.3 Measuring housing expenditure at the ZIP code level

For our counterfactual exercises we require several pieces of information. First, to estimate the overall incidence of a MW policy we need the levels of total wages and total housing expenditure in each location. Second, to estimate the ZIP code-specific incidence, we require a housing expenditure share that varies by ZIP code. We construct these measures for 2018 using data from the Internal Revenue System (2022b) and the US Department of Housing and Urban Development (2020b).

To construct these data we proceed as follows. We approximate the levels of total wages and housing expenditure using per household variables. From the IRS we obtain annual wage per household, which we divide by 12 to obtain a monthly measure. From the HUD, we use the 2-bedroom SAFMR series as our monthly housing expenditure variable.[40] We define the ZIP code-specific housing share as the ratio of these two variables.

The computed variables have several missing values across the entire US, and small percentage of missing values within urban CBSAs (as defined in Table 1). We impute missing values independently for each variable using an OLS regression based on sociodemographic characteristics of each ZIP code (including data from the US Census and LODES) and CBSA by county fixed effects. To limit the influence of outliers, we winsorize the results at the 0.5th and 99.5th percentiles. The percentage of urban ZIP codes with non-imputed housing expenditure shares is 93.2.

[38]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.

[39]More precisely, we compute a tract-to-ZIP-code correspondence from the LODES correspondence between blocks and tracts, available in US Census Bureau (2021), and the geographical match between blocks and ZIP codes from Online Appendix B.1. For each tract, we compute 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 MW workers in the tract.

[40]Average rents in a location would be better approximated as a weighted average of rents for houses with different number of bedrooms, weighted by the share of households that rent each type of housing. However, these data are not publicly available.

B.4 Posted rents and contracted rents: Quora question

We asked the following question: "How different is the rent paid by a tenant and the rent posted online of the same housing unit? Do tenants have space to bargain the posted price, or is it common for tenants to just accept the posted price?" The question can be accessed at this link.

Landlords

- *As a landlord for over 40 years, I have never agreed to negotiate the rental price. I would rather lose that renter and later lower the list price and rent to someone else. ...*
- *... The rent posted should be the actual rent available. As far as comparing the rent to a newly available listed property and one that is being occupied by another existing tenant, they will likely differ as the market continues to evolve and inflation has an impact on everything as well as the law of supply and demand. ...*
- *I've been a landlord for over 35 years, in three states and in three countries. I do NOT "bargain," and anyone trying would find their application in the round file, instantly.*

Tenants

- *Where I am, you accept the posted price unless there is something very odd about the unit or you have a needed skill to offer. Both are very rare.*

  The payoff is that landlords often increase rent for renewing tenants at a slightly lower rate than they set for a similar empty unit. If you stay many years, you may have noticeably lower rent. ...
- *Presumably, the landlord has done at least minimal research to figure out how much the market is charging. That market price should bring in multiple potential tenants wanting to rent. Given that, it would be very rare for a landlord to accept lower offers. ...*

From a Real Estate Transaction Coordinator (boldface added by us):

- *That's entirely up to the landlord.*

  When dealing with a property management company or the manager of an apartment complex, they may have limits on what they can do as far as negotiations. I'm not familiar with any in my city who negotiate rent. ***What's posted is the price. Period.***

  We mainly deal with private landlords who, of course, have 100% control over whether they are willing to negotiate the rent. ***In 15 years, I've only seen it happen twice.*** *But our clients post the rent at a fair price—generally right in the center of the "fair market value range"—so they have no reason to negotiate.*

  What they WILL negotiate, and I've seen it done many times, is how the deposits are collected. ...

C Identification in a Potential Outcomes Framework

Following Section 2, we assume that the effect of MW policies across locations can be summarized in the residence and workplace MW measures. Thus, we consider the following causal model

$$r_{it} = r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}). \tag{14}$$

For this section we represent our dataset as $\left\{\{r_{it}, \underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}\}_{t=\underline{T}}^{\overline{T}}\right\}_{i\in\mathcal{Z}}$. Monthly dates run from $\underline{T}$ to $\overline{T}$ for every unit, and $\mathcal{Z}$ is the set of ZIP codes. We assume that the data are *iid*. We impose no anticipation, so units do not change their pretreatment outcome given future changes in the MW measures.

Every month in which some jurisdiction changes the level of the MW there will be units that are treated directly and units that are treated indirectly. We follow Angrist and Imbens (1995) and Callaway et al. (2021) to define the treatment effects of interest. We denote a unit's causal response to the residence MW as $\partial r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}})/\partial \underline{w}_{it}^{\text{res}}$, and to the workplace MW as $\partial r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}})/\partial \underline{w}_{it}^{\text{wkp}}$. Let the federal MW level be $\underline{w}^{\text{fed}}$.

Definition 1 (Treatment Effects)**.** *Consider a group with a residence MW level of w^{res} and a workplace MW level of w^{wkp}. Focus on the effect of the workplace MW. The average treatment effect on that group is*

$$ATT^{wkp}(w^{wkp}|w^{res}, w^{wkp}) = \mathbb{E}\left[r_{it}(w^{res}, w^{wkp}) - r_{it}(w^{res}, \underline{w}^{fed})\middle|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = w^{wkp}\right].$$

The average causal response of the same group to the workplace MW is given by

$$ACRT^{wkp}(w^{wkp}|w^{res}, w^{wkp}) = \left.\frac{\partial\,\mathbb{E}\left[r_{it}(w^{res}, l)|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = w^{wkp}\right]}{\partial \underline{w}^{wkp}}\right|_{l=w^{wkp}}.$$

These treatment effects may be heterogeneous across the distribution of $(\underline{w}_{it}^{res}, \underline{w}_{it}^{wkp})$. The average causal response across all groups treated with different levels of the workplace and residence MW is

$$ACR^{wkp}(w^{wkp}) = \frac{\partial\,\mathbb{E}\left[r_{it}(w^{res}, w^{wkp})\right]}{\partial w^{wkp}}.$$

Analogously, for the residence MW we define: ATT^{res}, $ACRT^{res}(w^{res}|w^{res}, w^{wkp})$, and $ACR^{res}(w^{res})$.

Our main interest lies in the rent gradient to the MW, i.e., the average causal response of rents to each of the MW measures. For that, we make a parallel trends assumption.

Assumption 4 (Parallel trends)**.** *We assume that, for all levels of w^{res} and w^{wkp},*

$$\begin{aligned}
&\mathbb{E}\left[r_{it}(\underline{w}^{fed}, \underline{w}^{fed}) - r_{i,t-1}(\underline{w}^{fed}, \underline{w}^{fed})\middle|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = w^{wkp}\right] \\
&\qquad = \mathbb{E}\left[r_{it}(\underline{w}^{fed}, \underline{w}^{fed}) - r_{i,t-1}(\underline{w}^{fed}, \underline{w}^{fed})\middle|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = \underline{w}^{fed}\right] \\
&\qquad = \mathbb{E}\left[r_{it}(\underline{w}^{fed}, \underline{w}^{fed}) - r_{i,t-1}(\underline{w}^{fed}, \underline{w}^{fed})\middle|\underline{w}_{it}^{res} = \underline{w}^{fed}, \underline{w}_{it}^{wkp} = w^{wkp}\right].
\end{aligned}$$

Assumption 4 states that the untreated outcomes evolve in parallel between ZIP codes experiencing treatment levels $(w^{\text{res}}, w^{\text{wkp}})$ and (a) ZIP codes with the same level of the residence MW but unchanged workplace MW and (b) ZIP codes with the same level of the workplace MW but unchanged residence MW. We further maintain a second assumption.

Assumption 5 (No selection on gains)**.** *We assume that*

$$\frac{\partial ATT^{wkp}(w^{wkp}|w^{res}, l)}{\partial \underline{w}^{wkp}}\bigg|_{l=w^{wkp}} = 0 \quad \text{and} \quad \frac{\partial ATT^{res}(w^{res}|l, w^{wkp})}{\partial \underline{w}^{res}}\bigg|_{l=w^{res}} = 0.$$

To identify $ACRT^{\text{wkp}}$ we will compare ZIP codes that received similar levels of the residence MW and different levels of the workplace MW. Analogous comparisons of ZIP codes with different residence MW and similar workplace MW will idenfity $ACRT^{\text{res}}$.

Proposition 4 (Identification)**.** *Under Assumption 4 we have that*

$$\begin{aligned}\frac{\partial\, \mathbb{E}\left[r_{it}(w^{res}, w^{wkp})|\underline{w}_{it}^{res} = w^{res}, \underline{w}_{it}^{wkp} = w^{wkp}\right]}{\partial \underline{w}^{wkp}} &= ACRT^{wkp}(w^{wkp}|w^{res}, w^{wkp}) \\ &+ \frac{\partial ATT^{wkp}(w^{wkp}|w^{res}, l)}{\partial \underline{w}^{wkp}}\bigg|_{l=w^{res}}.\end{aligned}$$

Furthermore, if Assumption 5 holds, then

$$\frac{\partial\, \mathbb{E}\left[r_{it}(w^{res}, w^{wkp})|w^{res}, w^{wkp}\right]}{\partial w^{wkp}} = ACRT^{wkp}(w|w^{res}, w).$$

Analogous expressions hold for the residence MW.

Proof. The setting is analogous to Callaway et al. (2021) but with two treatment variables. The proof is analogous as well, with the only difference being that one must condition on the residence MW when deriving the expression for the workplace MW, and viceversa. □

As extensively discussed by Callaway et al. (2021), Assumption 4 is not enough to identify the average causal response in the context of continuous treatments. The gradient of our rents function for the group $(w^{\text{res}}, w^{\text{wkp}})$ is a mix of the average causal response of interest and a "selection bias" term that captures the fact that the treatment for the particular group that received $(w^{\text{res}}, w^{\text{wkp}})$ may be different for other groups at that level of treatment. Assumption 5 imposes that those selection bias terms are zero.[41] We discuss the plausibility of these assumptions in Section 4.

Consider now a functional form for (14) like the one used in the main analysis:

$$r_{it} = \alpha_i + \tilde{\delta}_t + \gamma \underline{w}_{it}^{\text{res}} + \beta \underline{w}_{it}^{\text{wkp}} + \epsilon_{it}$$

where we exclude the controls for simplicity. It is easy to see, if $\mathbb{E}[\epsilon_{it}|\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}] = 0$, then both Assumptions 4 and 5 hold under this linear functional form with constant effects. Furthermore, in

[41]There are several alternatives to this assumption. See Callaway et al. (2021, Section 3.3) and discussion therein.

this case we have that

$$ACRT^{\text{wkp}}(w^{\text{wkp}}|w^{\text{res}}, w^{\text{wkp}}) = ACR^{\text{wkp}}(w^{\text{wkp}}|w^{\text{res}}, w^{\text{wkp}}) = \beta$$

and that

$$ACRT^{\text{res}}(w^{\text{res}}|w^{\text{res}}, w^{\text{wkp}}) = ACR^{\text{res}}(w^{\text{res}}|w^{\text{res}}, w^{\text{wkp}}) = \gamma$$

for any $w^{\text{res}} \geq \underline{w}^{\text{fed}}$ and $w^{\text{wkp}} \geq \underline{w}^{\text{fed}}$.

D Additional Tables and Figures

Online Appendix Table 1: Summary statistics of baseline panel

	N	Mean	St. Dev.	Min	Max
Minimum wage variables					
Statutory MW $\underline{W}_{it}$	80,700	8.56	1.58	7.25	16.00
Residence MW $\underline{w}_{it}^{\text{res}}$	80,700	2.132	0.168	1.981	2.773
Workplace MW $\underline{w}_{it}^{\text{wkp}}$	80,700	2.136	0.163	1.981	2.694
Workplace MW, low-income workers	80,700	2.134	0.161	1.981	2.681
Workplace MW, young workers	80,700	2.135	0.163	1.981	2.707
Median Rents					
SFCC	74,012	1,757.89	901.50	625.00	30,000.00
SFCC per sqft.	80,700	1.32	1.01	0.47	22.20
Log(SFCC per sqft.)	80,700	0.14	0.47	-0.76	3.10
Economic controls					
Avg. wage Business services	80,700	11.19	1.38	6.02	13.39
Employment Business services	80,700	8.71	1.25	4.36	10.96
Estab. count Business services	80,700	7.14	0.31	5.73	8.18
Avg. wage Financial services	80,352	9.01	1.57	2.40	12.39
Employment Financial services	80,700	6.13	1.35	1.61	9.53
Estab. count Financial services	80,352	7.33	0.36	5.89	8.91
Avg. wage Information services	80,688	10.23	1.43	4.75	12.90
Employment Information services	80,700	8.01	1.21	3.66	10.34
Estab. count Information services	80,688	7.31	0.37	6.33	9.16

Notes: This table shows summary statistics of the panel of ZIP codes used in our baseline results, constructed as explained in Section 3.3.4. All workplace MW variables use 2017 commuting data from LODES. The workplace MW variables "Workplace MW, low-income workers" and "Workplace MW, young workers" are constructed using data for workers who earn less $1,251 and are aged less than 29, respectively.

Online Appendix Table 2: Estimates of the effect of the minimum wage on rents in levels and first differences, baseline sample

	Log rents	
	Levels (1)	First Differences (2)
Residence MW	-0.0432	-0.0199
	(0.1751)	(0.0195)
Workplace MW	0.0376	0.0687
	(0.2033)	(0.0298)
Economic controls	Yes	Yes
P-value autocorrelation test		< 0.0001
R-squared	0.9924	0.0216
Observations	80,340	78,912

Notes: Data are from the baseline estimation sample described in Section 3.3.4. 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 ϕ. 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 $\phi = -0.5$. The row "P-value autocorrelation test" reports the p-value of a Wald test of that hypothesis. Standard errors in parentheses are clustered at the state level.

Online Appendix Table 3: Estimates of the effect of the minimum wage on rents, different samples

	Change log rents Δr_{it}		
	Baseline (1)	Reweighted (2)	Unbalanced (3)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0219	-0.0039	-0.0274
	(0.0175)	(0.0128)	(0.0237)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0685	0.0558	0.0528
	(0.0288)	(0.0240)	(0.0299)
P-value equality	0.0514	0.1026	0.1325
R-squared	0.0213	0.0209	0.0309
Observations	80,241	79,701	193,239

Notes: Data are from Zillow, the statutory MW panel described in Section 3.1, LODES origin-destination statistics, and the QCEW. Every column shows 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 by month and county by quarter levels. The measure of rents per square foot corresponds to the Single Family, Condominium and Cooperative houses from Zillow. Columns (1) and (2) use our baseline sample defined in Section 3.3.4. Column (3) uses the unbalanced sample of all ZIP codes with Zillow rents data at any point in time, and controls for quarter-year of entry to the panel by year-month fixed effects. Column (2) re-weights observations so that the sample of ZIP codes in the data (column 3 of Table 1) matches the averages of the set of ZIP codes located in urban CBSAs (column 2 of Table 1) in the following census variables: share of urban population share of renter-occupied households, and share of white population. Weights for each sample are computed following Hainmueller (2012). Standard errors in parentheses are clustered at the state level.

Online Appendix Table 4: Estimates of the effect of the minimum wage on rents, stacked sample

	Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	Change log rents Δr_{it}		
	(1)	(2)	(3)	(4)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	0.5461	0.0051		-0.0444
	(0.0316)	(0.0109)		(0.0174)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$			0.0242	0.0906
			(0.0216)	(0.0391)
Sum of coefficients				0.0463
				(0.0266)
Economic controls	Yes	Yes	Yes	Yes
P-value equality				0.0208
R-squared	0.9763	0.0539	0.0540	0.0540
Observations	98,326	98,326	98,326	98,326

Notes: Data are from Zillow, the MW panel described in Section 3.1, LODES origin-destination statistics, and the QCEW. The table mimics 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. We drop events that have less than 10 ZIP codes. For each of the selected CBSA-months we assign a unique event ID. Second, for each event ID we take a window of 6 months, 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 ID by year-month fixed effects.

Online Appendix Table 5: Estimates of the effect of the minimum wage on rents including one lag of the dependent variable, baseline sample

	Log rents			
	Levels		First Differences	
	Baseline (1)	Arellano Bond (2)	Baseline (3)	Arellano Bond (4)
Residence MW	-0.0432	-0.0055	-0.0219	-0.0221
	(0.1751)	(0.0298)	(0.0175)	(0.0234)
Workplace MW	0.0376	0.0065	0.0685	0.0702
	(0.2033)	(0.0346)	(0.0288)	(0.0390)
Lagged log rents		0.8421		0.3299
		(0.0179)		(0.0177)
Economic controls	Yes	Yes	Yes	Yes
P-value equality	0.8264	0.8481	0.0514	0.1378
Observations	80,340	80,321	80,241	80,217

Notes: Data are from the baseline estimation sample described in Section 3.3.4. All columns show the results of regressions of the log of median rents per square foot on the residence and workplace MW measures. Columns (1) and (2) estimate two-way fixed-effects regressions in levels that include ZIP code and year-month fixed effects. Columns (3) and (4) estimate models in first differences that include year-month fixed effects. All regressions include economic controls (in levels or first differences, respectively) that vary at the county by month and county by quarter levels. Odd-numbered columns are estimated under OLS. Even-numbered columns include the lagged variable of the dependent variable as control, and are estimated using an IV strategy where the first lag is instrumented with the second lag, following Arellano and Bond (1991). The measure of rents per square foot corresponds to the SFCC category from Zillow. 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 parentheses are clustered at the state level.

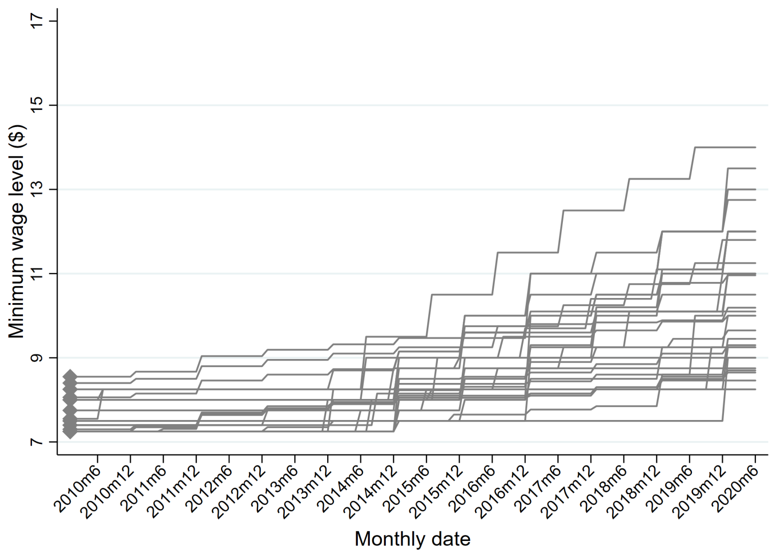
Online Appendix Table 6: Comparison of estimates of the effect of the minimum wage on rents across Zillow categories, unbalanced samples

	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) Unbalanced (SFCC)	0.8476	-0.0263	0.0479	0.0216	193,292
	(0.0297)	(0.0213)	(0.0302)	(0.0157)	
(b) Single family (SF)	0.8588	-0.0169	0.0429	0.0260	140,750
	(0.0315)	(0.0399)	(0.0477)	(0.0138)	
(c) Condo/Cooperatives (CC)	0.8019	-0.0648	0.0968	0.0320	29,817
	(0.0288)	(0.0266)	(0.0417)	(0.0199)	
(d) Studio	0.8330	-0.0669	0.0776	0.0107	22,746
	(0.0287)	(0.0520)	(0.0570)	(0.0206)	
(d) 1 Bedroom	0.7879	0.0287	-0.0327	-0.0039	53,538
	(0.0300)	(0.0269)	(0.0456)	(0.0208)	
(e) 2 Bedroom	0.8022	-0.0069	0.0063	-0.0006	89,635
	(0.0296)	(0.0232)	(0.0285)	(0.0114)	
(f) 3 Bedroom	0.8113	-0.0645	0.0920	0.0275	64,916
	(0.0322)	(0.0475)	(0.0682)	(0.0328)	
(g) Multifamily 5+ units	0.8072	-0.0133	0.0369	0.0236	142,759
	(0.0314)	(0.0260)	(0.0362)	(0.0115)	

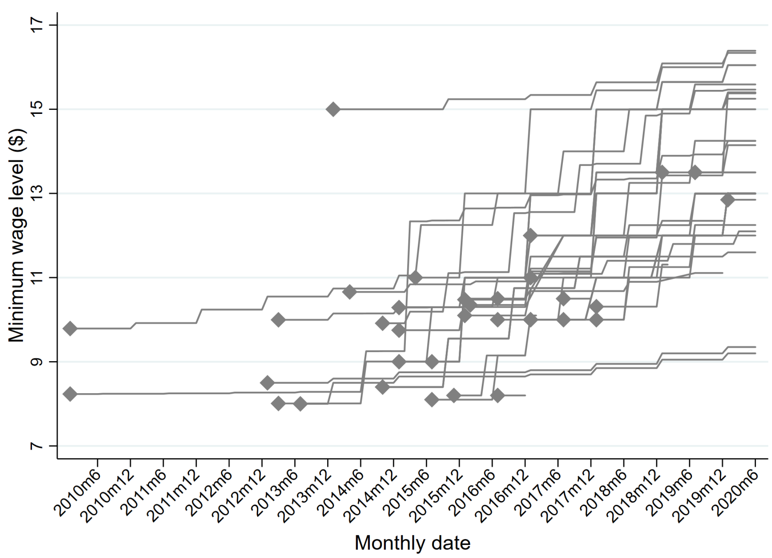
Notes: Data are from Zillow, the statutory MW panel described in Section 3.1, LODES origin-destination statistics, and the QCEW. Each row of the table shows two estimations on the same sample of ZIP codes and months. The first column shows the results of a regression of the change in the workplace MW 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 quarter of entry to Zillow by year-month fixed effects and economic controls from the QCEW. Row (a) repeats the results of column (5) of Table 3, using the Single Family, Condominium and Cooperative Houses category. Rows (b) through (g) estimate the same regression for different Zillow categories. We exclude the rental categories “4 bedroom,” “5 bedroom,”, and “Duplex and triplex,” all of which contain less than 15 thousand ZIP code by month observations. Standard errors in parentheses are clustered at the state level.

Online Appendix Figure 1: Minimum wage levels in the US by jurisdiction between January 2010 and June 2020

Panel A: State policies

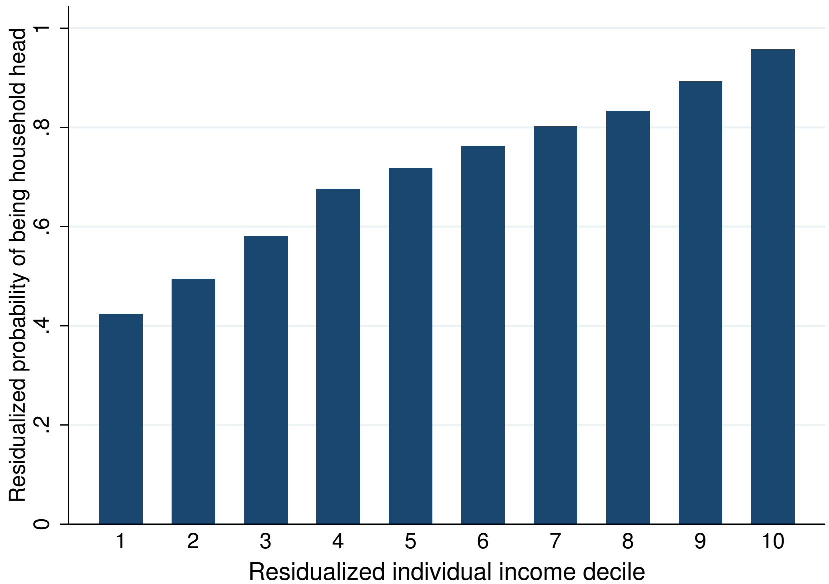


Panel B: Sub-state policies



Notes: Data are from the MW panel described in Section 3.1. Lines show the levels of the MW for jurisdictional policies that were binding for at least one ZIP code inside them in some month between January 2010 and June 2020. Diamonds indicate the first month the MW policy became operational within the same period. Panel A reports state level policies. Panel B reports sub-state level policies.

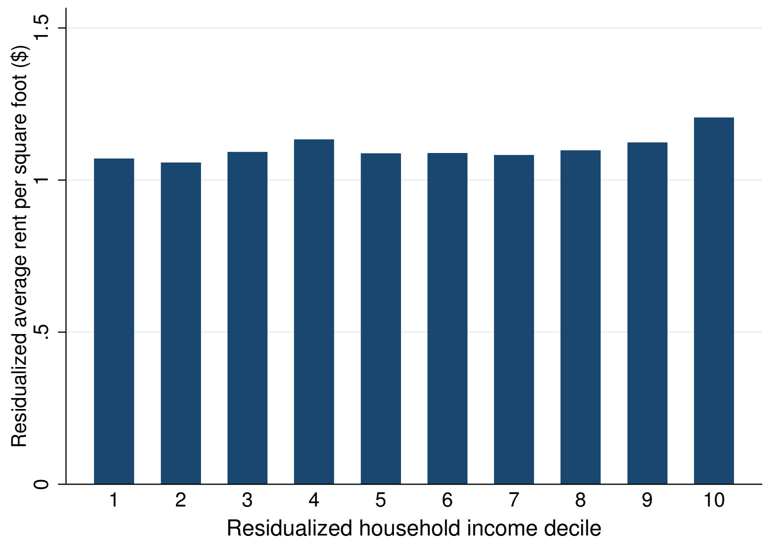
Online Appendix Figure 2: Probability of being a head of household, by income decile



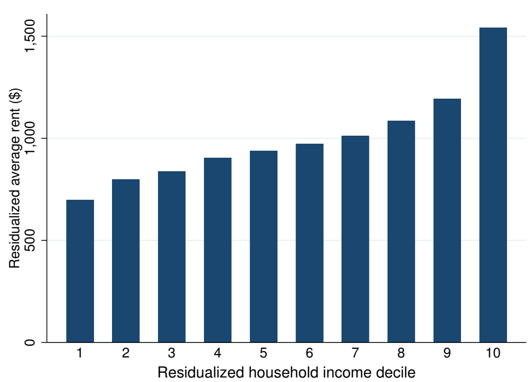
Notes: Data are from the 2011 and 2013 American Housing Surveys. The figure shows the probability that an individual is a head of household, by individual income decile. We construct the figure as follows. First, we residualize the variable in the y-axis and individual income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized individual income variable. Finally, we take the average of the residualized y-variable within each decile. Individuals that do not work are excluded from the figure.

Online Appendix Figure 3: Average rent, square footage, and rent per square foot by household income decile, renters sample

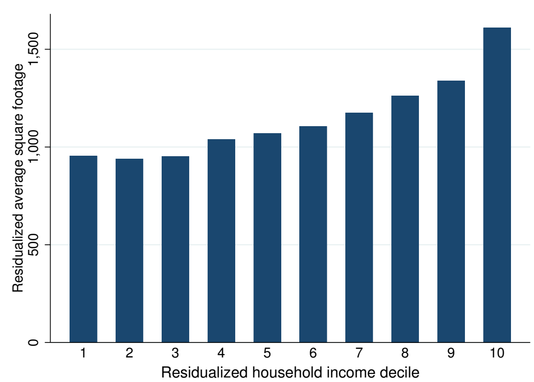
Rents per square foot



Rents



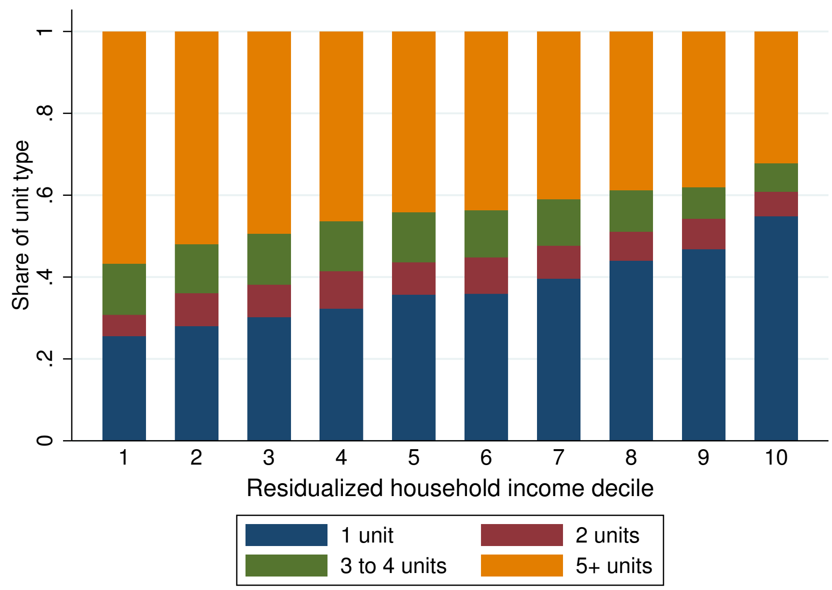
Square footage



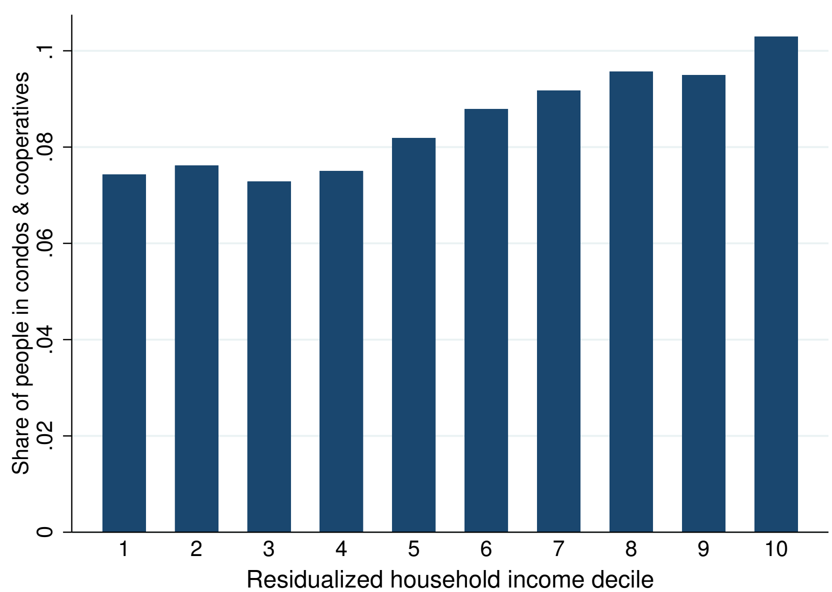
Notes: Data are from the 2011 and 2013 American Housing Surveys. The top figure shows average rents per square foot by household income. The bottom left figure shows average rents by household income. The bottom right figure shows average square feet by household income. The variable rent per square foot is defined as total rental payments divided by total square feet. We construct the figure as follows. First, we residualize the variable in the y-axis and household income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized household income variable. Finally, we take the average of the residualized y-variable within each decile. The sample includes households with non-missing values for square footage and rental payments. We exclude from the calculation non-conventional housing units, such as mobile homes, hotels, and others.

Online Appendix Figure 4: Properties of building where household unit is located by household income decile, full sample

Distribution of the number of units in building

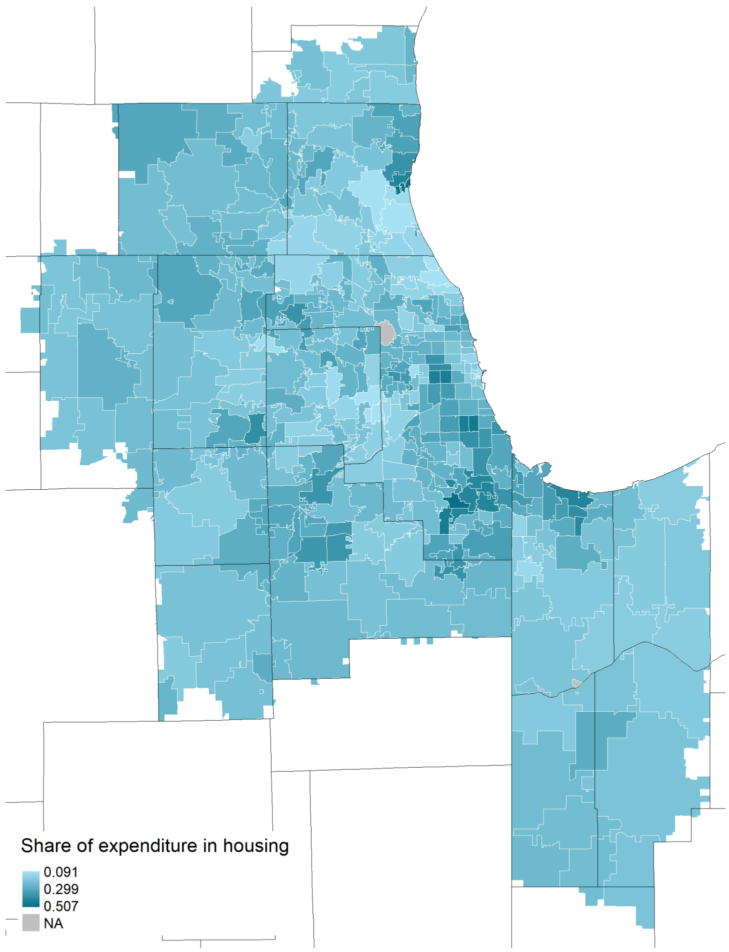


Probability building is a condominium or cooperative housing



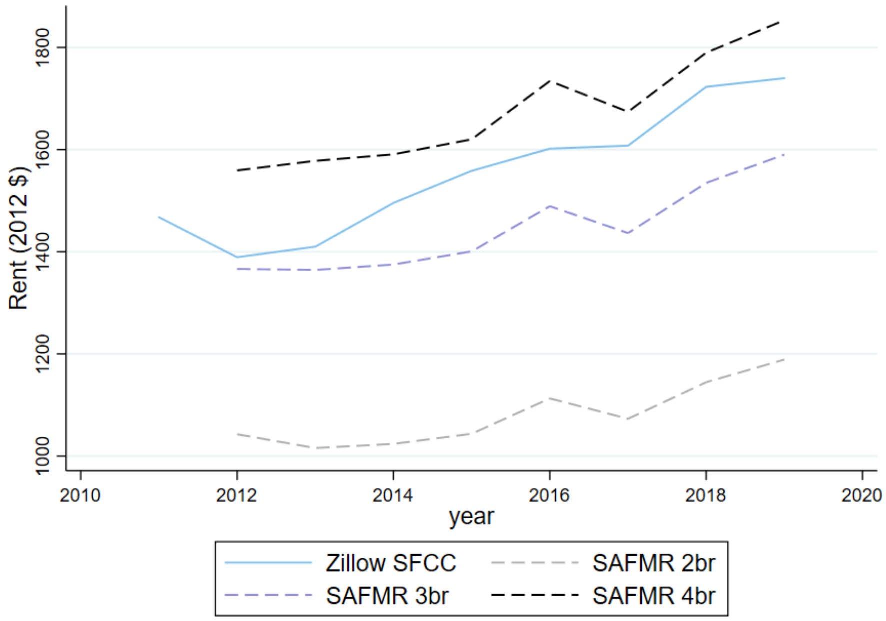
Notes: Data are from the 2011 and 2013 American Housing Surveys. The top figure shows the number of housing units in the building where the household is located, and the bottom figure shows the share of housing units located in condominiums or cooperative housing, both by household income. We construct the figure as follows. First, we residualize the variable in the y-axis and household income by SMSA indicators, the closest analogue of CBSAs available in the data. Second, we construct deciles of the residualized household income variable. Finally, we take the average of the residualized y-variable within each decile. We exclude from the calculation non-conventional housing units, such as mobile homes, hotels, and others.

Online Appendix Figure 5: Estimated housing expenditure shares in 2018, Chicago-Naperville-Elgin CBSA



Notes: Data are from the Internal Revenue System (2022b) and the US Department of Housing and Urban Development (2020b). The figure shows housing expenditure shares as computed in Online Appendix B.3, namely, by dividing the SAFMR 40th percentile rental value for a 2-bedroom apartment by average monthly wage per household divided, both for 2018. We include ZIP codes located in the Chicago-Naperville-Elgin CBSA.

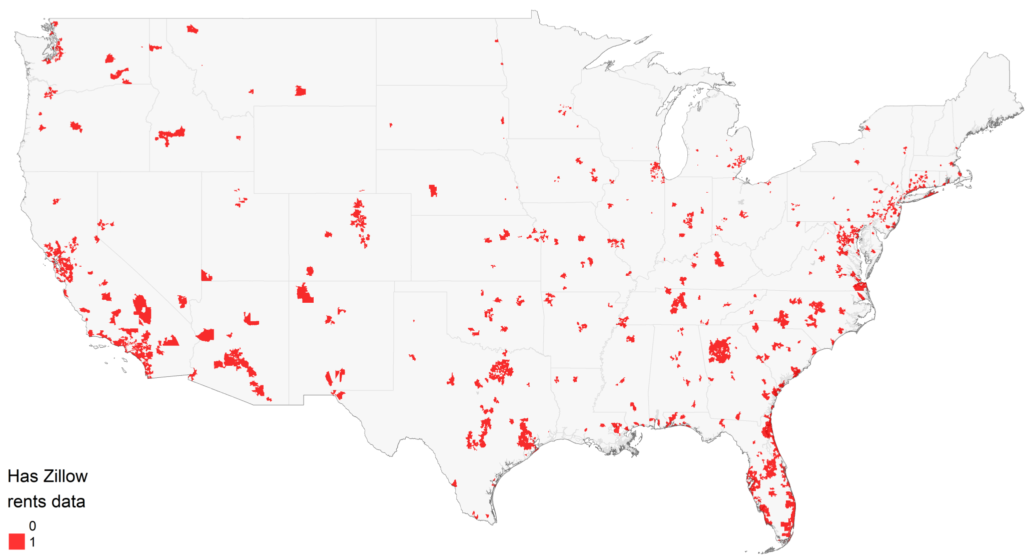
Online Appendix Figure 6: Time trends in rents according to Zillow and SAFMR



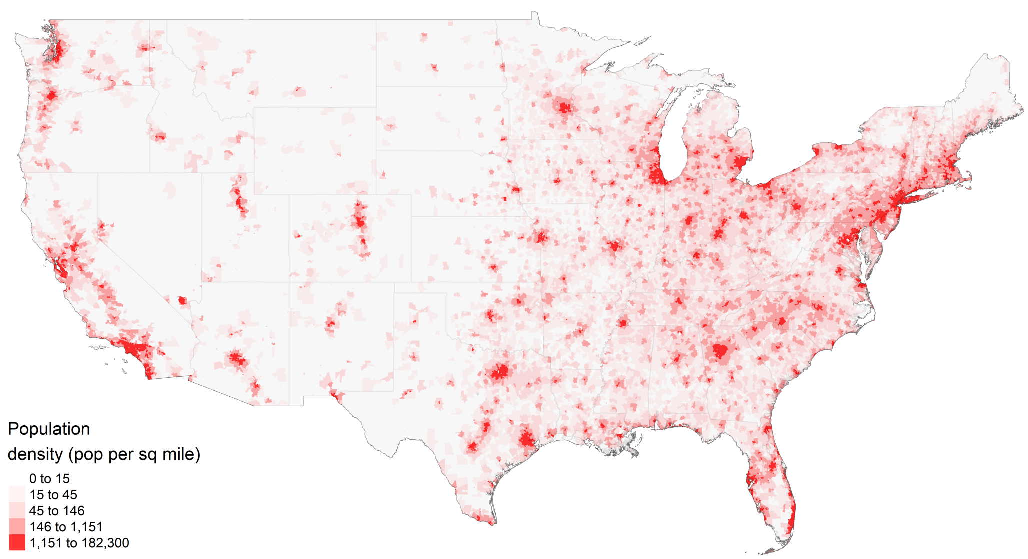
Notes: Data are from Zillow (2020b) and Small Area Fair Market Rents (2020). The figure compares the evolution of the median rental value in Zillow to three SAFMRs series, for 2, 3, and 4 or more bedrooms. SAFMR data generally corresponds to the 40th percentile of the distribution of paid rents in a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

Online Appendix Figure 7: Sample of ZIP codes in Zillow data and population density, mainland US

Zillow ZIP codes

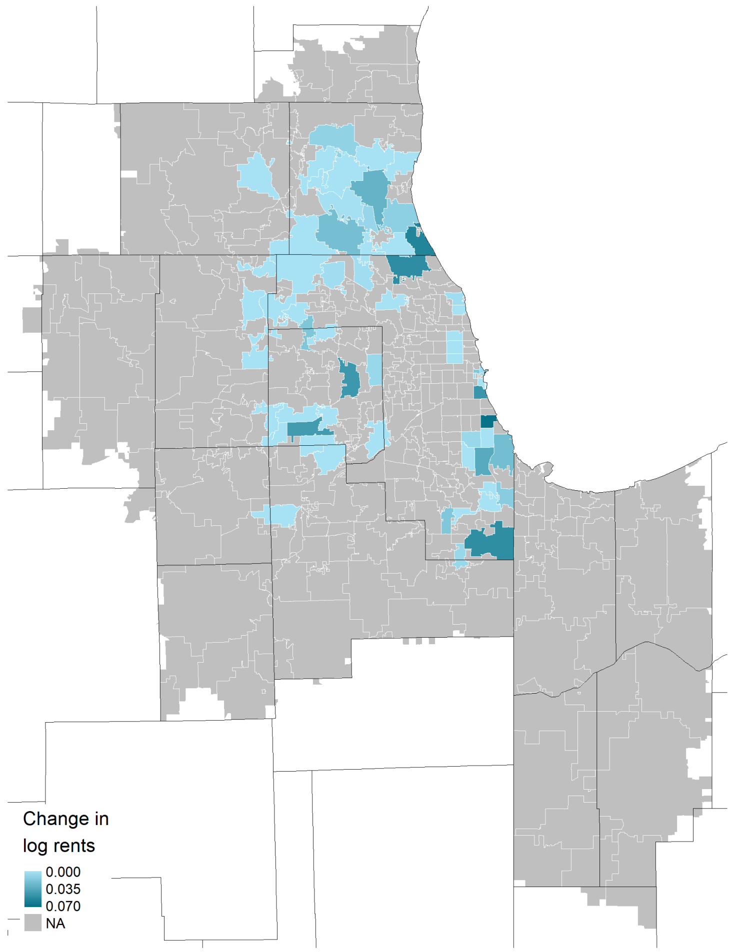


Population Density



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

Online Appendix Figure 8: Changes in log rents in the Chicago-Naperville-Elgin CBSA, July 2019

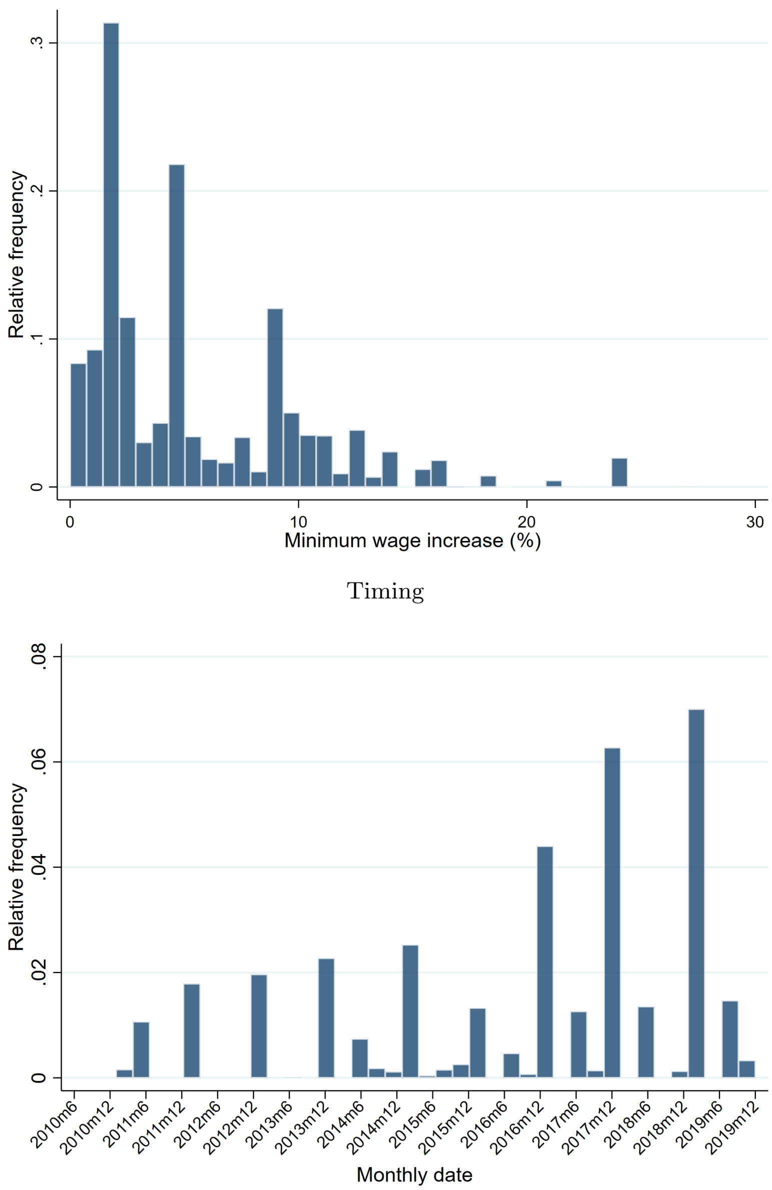


Notes: Data are from Zillow (2020b). The figure shows the change in the log of median rents per square foot in the SFCC category in the month of June 2019 in ZIP codes located in the Chicago-Naperville-Elgin CBSA.

Online Appendix Figure 9: Distribution of statutory minimum wage changes, Zillow sample

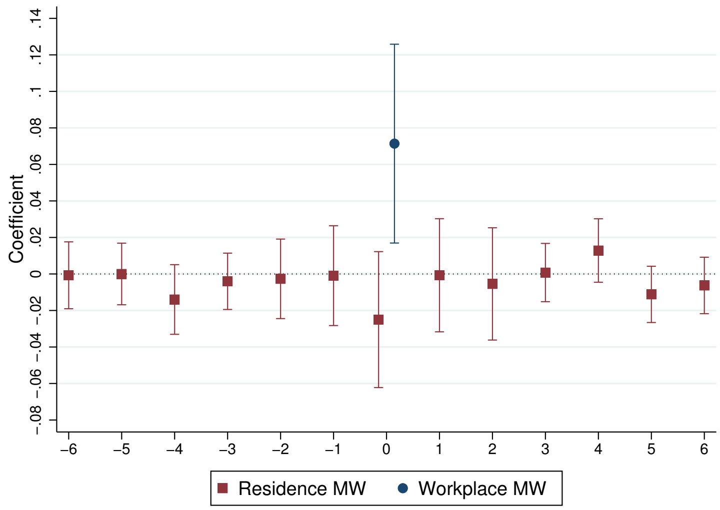
Intensity

Timing



Notes: Data are from the MW panel described in Section 3.1. The histograms show the distribution of positive MW changes in the sample of ZIP codes available in the Zillow data. We exclude a few negative changes for expository purposes. The top figure ("Intensity") reports the intensity of the changes in percentage terms. The bottom figure ("Timing") reports the distribution of such changes over time.

Online Appendix Figure 10: Estimates of the effect of the minimum wage on rents, baseline sample including leads and lags of the residence MW

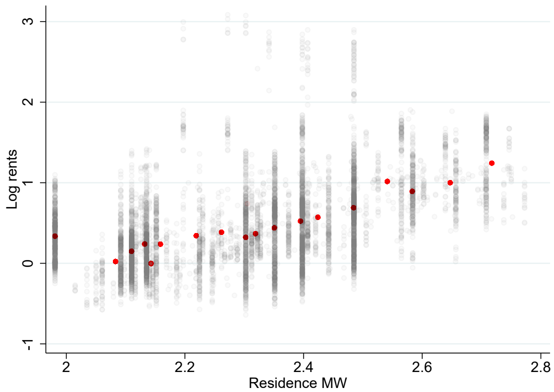


Notes: Data are from the baseline estimation sample described in Section 3.3.4. The figure shows coefficients from regressions of the log of rents per square foot on the residence and workplace MW measures, including six leads and lags of the residence MW. All regressions include time-period fixed effects and economic controls that vary at the county by month and county by quarter 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.

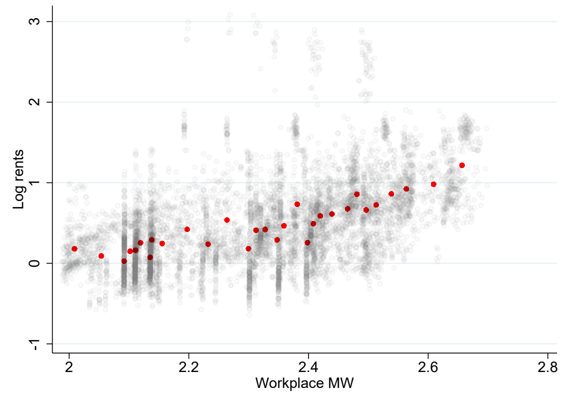
Online Appendix Figure 11: Relationship between log rents and the minimum wage measures, sample of affected ZIP code-months

Panel A: Raw data

Residence MW

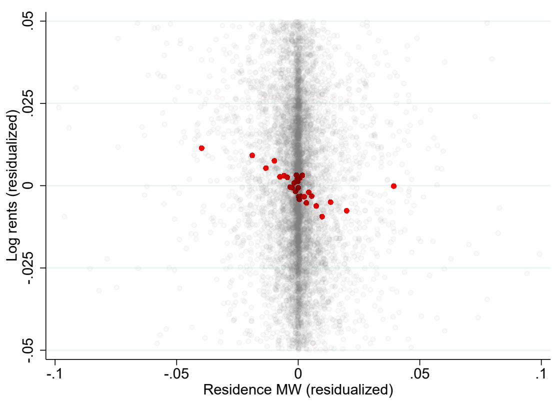


Workplace MW

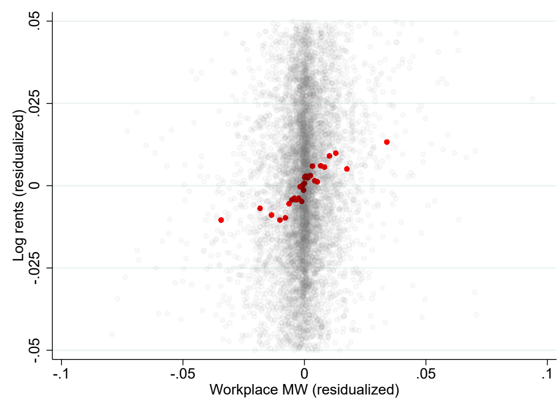


Panel B: Conditional on ZIP code FE and the other MW measure

Residence MW

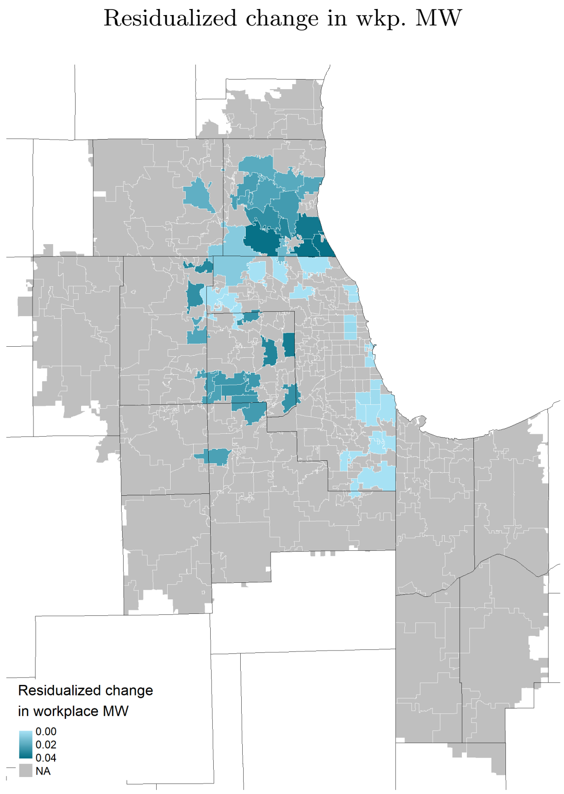


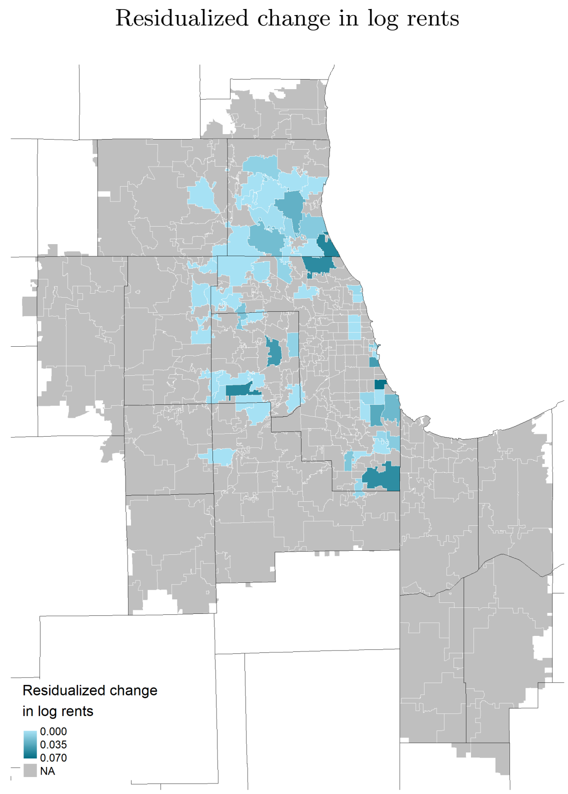
Workplace MW



Notes: Data are from Zillow and LODES. The plot shows the unconditional and conditional relationship between log rents and the MW measures. The sample is composed of ZIP code-month observations located in CBSAs where there was some statutory MW increase in the month of interest. The rents variable correspond to log rents per square foot in the SFCC category in Zillow. The workplace MW measure is constructed using commuting data from the closest prior year. Panel A shows the raw relationship between log rents and workplace and residence MW levels. Panel B shows the same relationship using residuals from regressions on ZIP code indicators and 100 indicators of the other MW measure. Red dots correspond to 30 equally-sized bins of the x-axis variable. Gray dots correspond to all data points in Panel A, and only those data points that fall within the range of the plot axes in Panel B.

Online Appendix Figure 12: Residualized changes in the workplace minimum wage and log rents, Chicago-Naperville-Elgin CBSA on July 2019

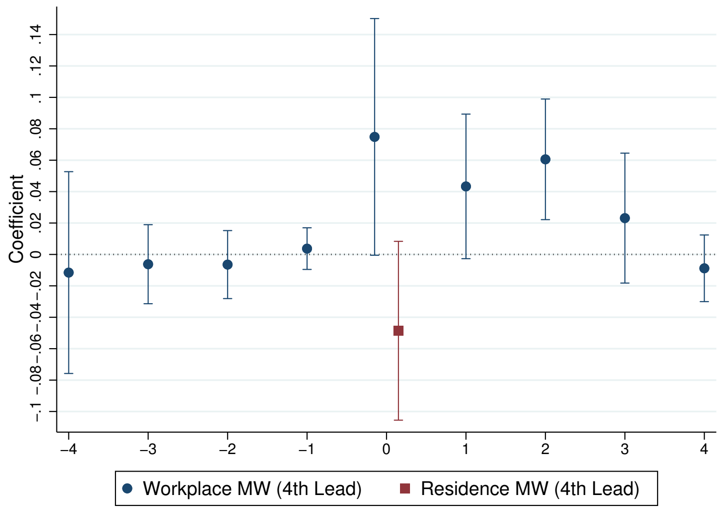




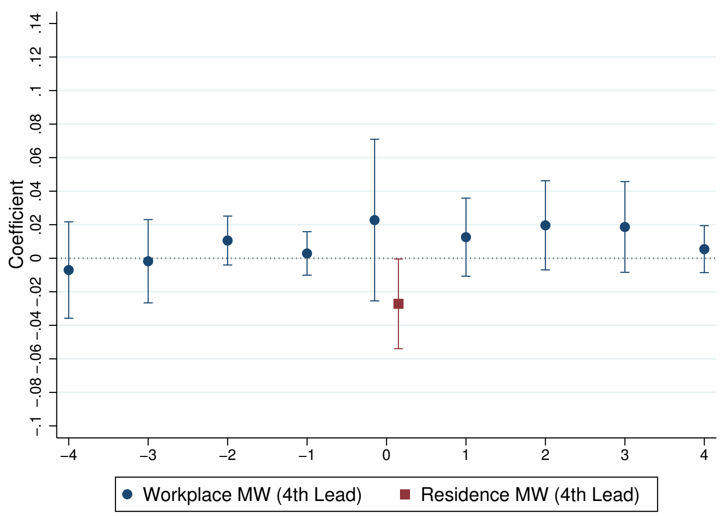
Notes: Data are from the unbalanced estimation panel described in Section 3.3.4. The left figure maps the residuals of a regression of the change in the workplace MW measure on the change in the residence MW measure, including economic controls and year-month fixed effects. The right figure maps the 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."

Online Appendix Figure 13: Estimates of the effect of the minimum wage on rents, Zillow rental index

Panel A: Control for year-month by CBSA fixed effects

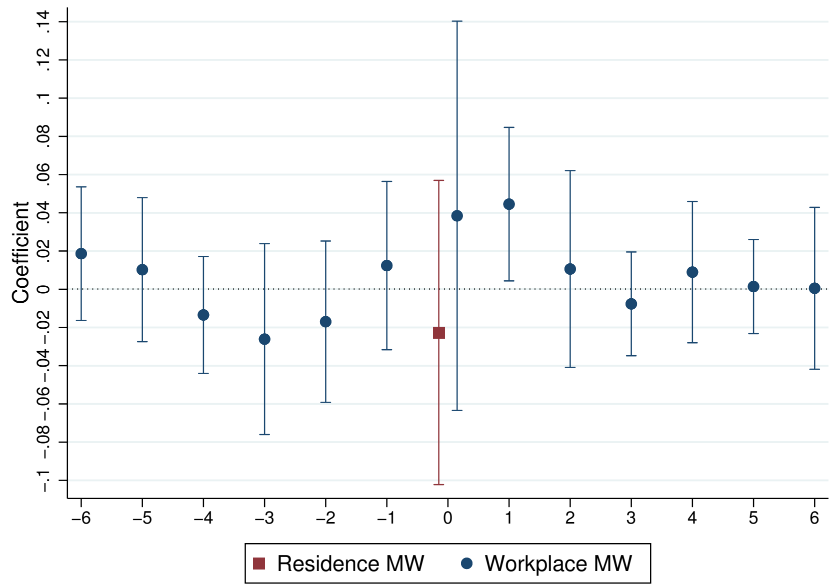


Panel B: Control for year-month fixed effects



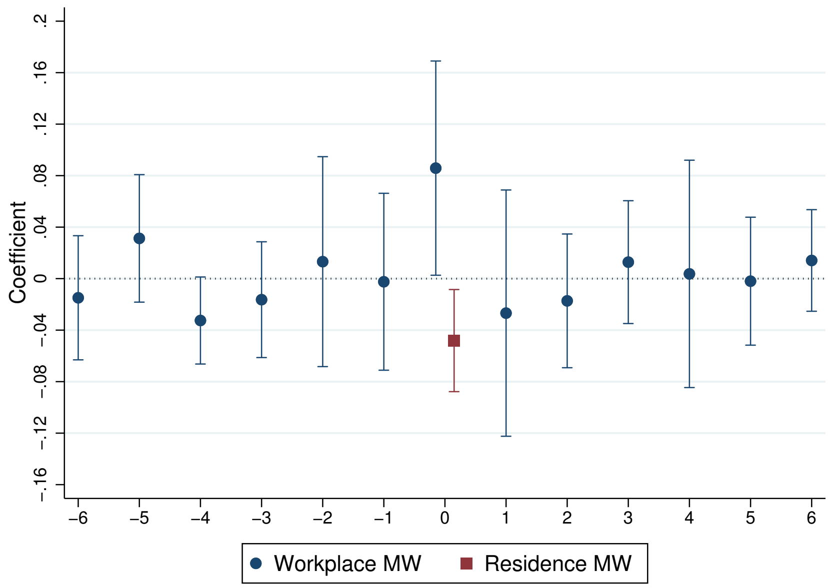
Notes: Data are from the baseline estimation sample described in Section 3.3.4. The figures show coefficients from regressions of the change in log of Zillow rental index on leads and lags of the change in the workplace MW and the change in the residence MW, using the 4th lead of the MW-based measures. The top panel includes CBSA by year-month fixed effects, whereas the bottom panel includes year-month fixed effects. Both regressions include economic controls that vary at the county by month and county by quarter levels, which 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.

Online Appendix Figure 14: Estimates of the effect of the minimum wage on rents, county by month data



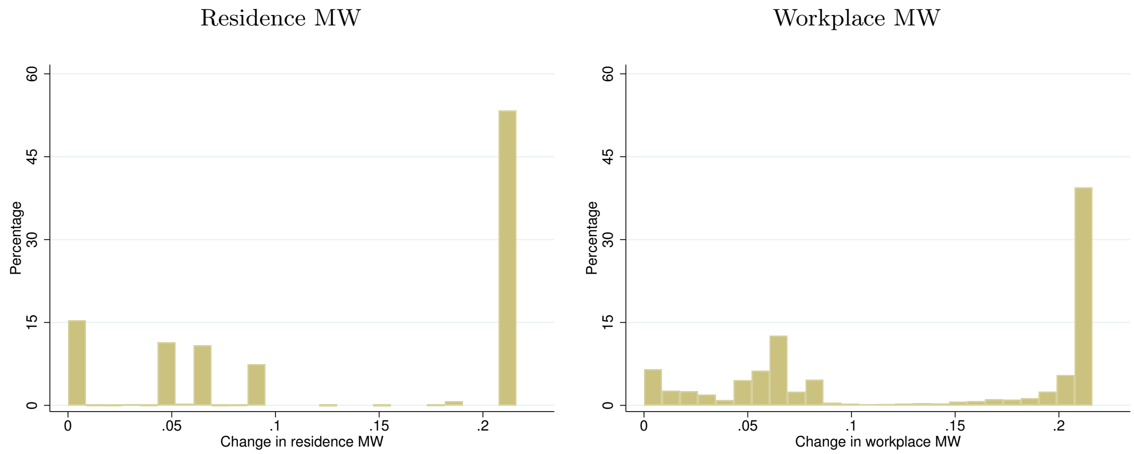
Notes: Data are from the county-by-month panel described in Section 3.3.4. We plot coefficients from regressions of the log of rents per square foot on the residence MW and workplace MW, including six leads and lags of the workplace MW measure. All regressions are estimated in first differences and include time-period fixed effects and economic controls that vary at the county by month and county by quarter levels. The measure of rents per square foot corresponds 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.

Online Appendix Figure 15: Estimates of the effect of the minimum wage on rents, stacked sample including leads and lags



Notes: Data are from Zillow, the MW panel described in Section 3.1, LODES origin-destination statistics, and the QCEW. The figure mimics estimates in Figure ?? using a "stacked" sample. We construct the sample as explained in Online Appendix Table 4. 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

Online Appendix Figure 16: Distribution of changes in minimum wage measures under a counterfactual federal minimum wage of $9, urban ZIP codes

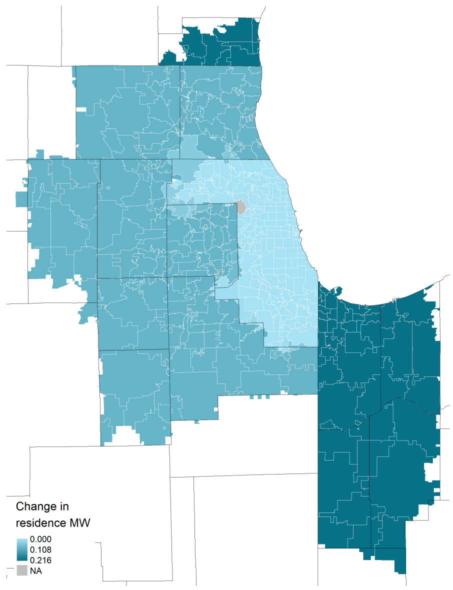


Notes: Data are from LODES and the MW panel described in Section 3.1. The figures show the distribution of changes in the residence and workplace MW measures generated by a counterfactual increase to $9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. The unit of observation is the urban ZIP code, where we define a ZIP code as urban if it belongs to a CBSA with at least 80% of its population classified as urban by the 2010 Census. We exclude ZIP codes located in CBSAs where the estimated increase in income was higher than 0.1.

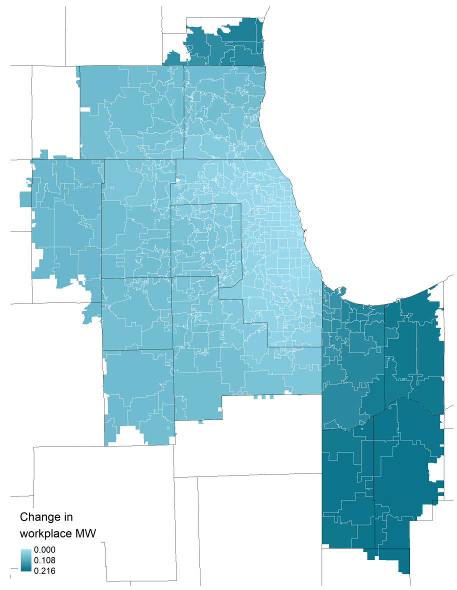
Online Appendix Figure 17: Changes in the minimum wage measures under counterfactual minimum wage policies, Chicago-Naperville-Elgin CBSA

Panel A: Increase in federal MW from \$7.25 to \$9

Changes in residence MW

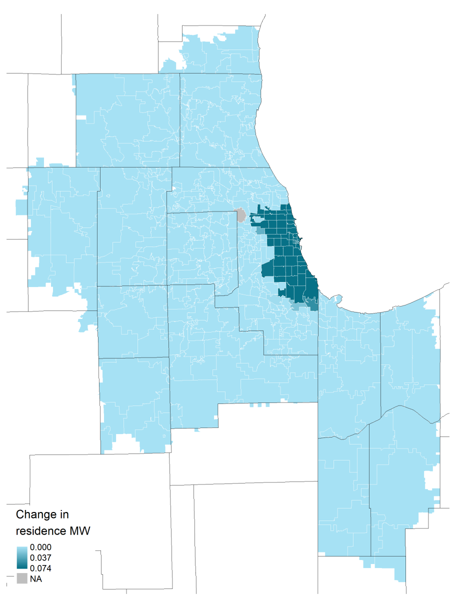


Changes in workplace MW

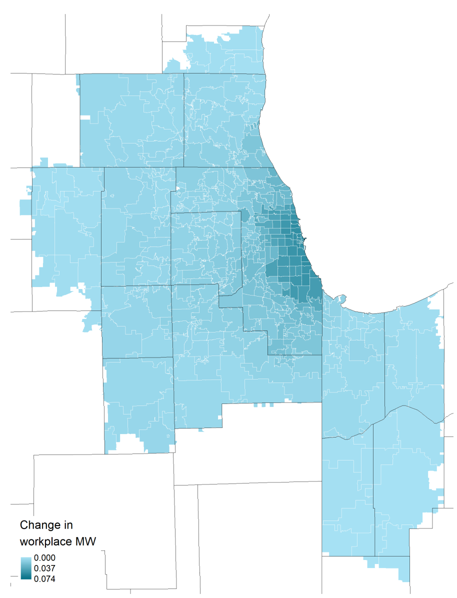


Panel B: Increase in Chicago MW from \$13 to \$14

Changes in residence MW



Changes in workplace MW

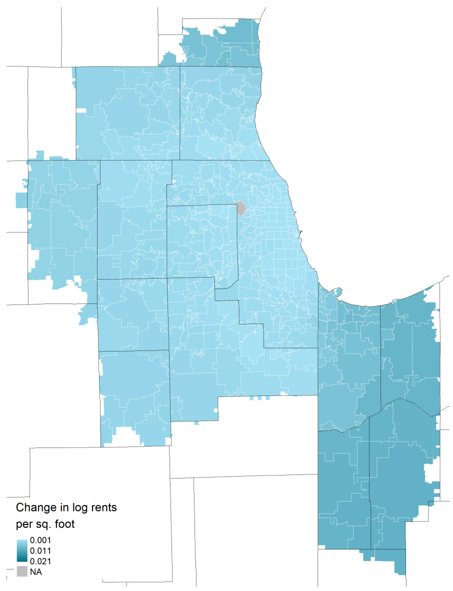


Notes: Data are from the MW panel described in Section 3.1 and from LODES. The figures map changes in the residence and workplace MW measures by counterfactual MW policies in the Chicago-Naperville-Elgin CBSA. Panel A shows a policy where the federal MW increases from \$7.25 to \$9 in January 2020, holding constant other MW policies at their December 2019 levels. Panel B shows a policy where the city of Chicago increases its MW from \$13 to \$14 in January 2020, holding constant other MW policies at their December 2019 levels as well.

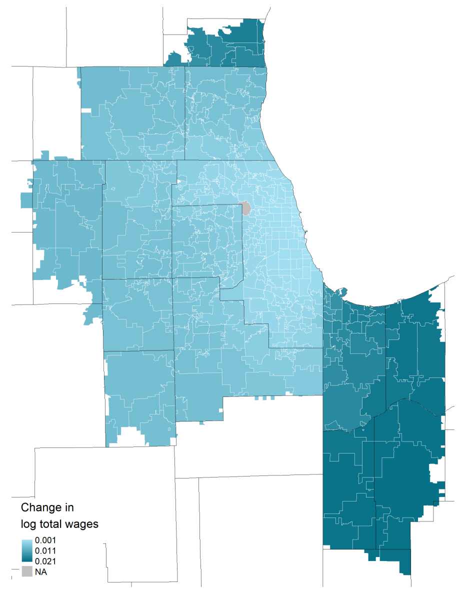
Online Appendix Figure 18: Changes in log rents and log total wages under counterfactual minimum wage policies, Chicago-Naperville-Elgin CBSA

Panel A: Increase in federal MW from $7.25 to $9

Changes in log rents

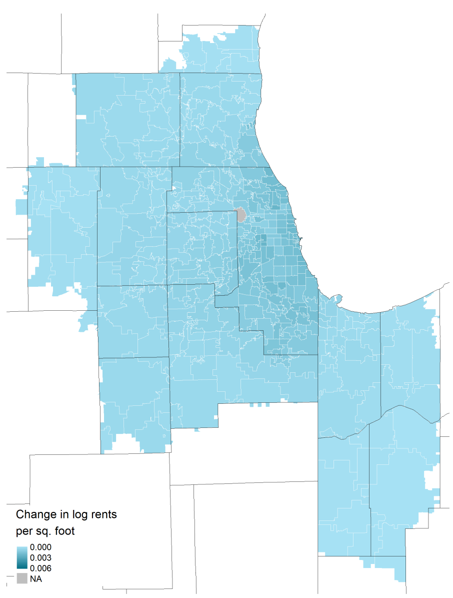


Changes in log total wages

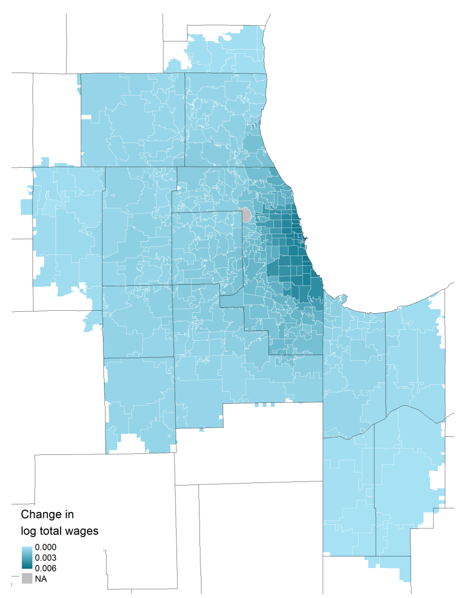


Panel B: Increase in Chicago MW from $13 to $14

Changes in log rents

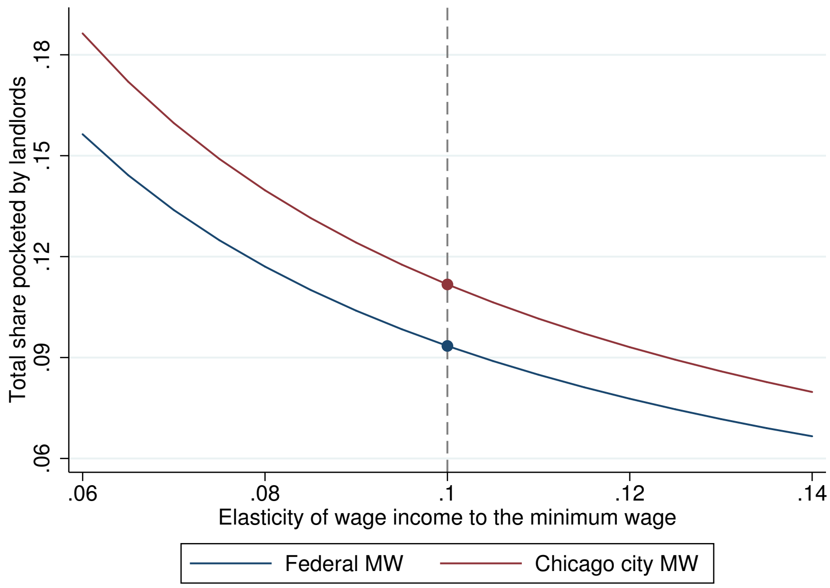


Changes in log total wages



Notes: Data are from the MW panel described in section 3.1 and from LODES. The figures map the estimated changes in log total rents per square foot and log total wage income under different counterfactual MW policies in the Chicago-Naperville-Elgin CBSA. Panel A is based on a counterfactual increase to $9 in the federal MW in January 2020, and Panel B on a counterfactual increase from $13 to $14 in the Chicago City MW, both holding constant other MW policies. The color scale has been standardized within each panel. To estimate the changes we follow the procedure described in Section 6 assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$, and $\varepsilon = 0.1$.

Online Appendix Figure 19: Estimated shares pocketed by landlords for different values of the elasticity of wage income to the MW



Notes: Data are from the MW panel described in section 3.1 and from LODES. The figures show the estimated ZIP-code specific share of additional income pocketed by landlords ("share pocketed") under different counterfactual policies: an increase to $9 in the federal MW in January 2020, and an increase from $13 to $14 in the Chicago City MW, both holding constant other MW policies. The unit of observation is the ZIP code. To estimate it we follow the procedure described in Section 6, assuming the following parameter values: $\beta = 0.0685$, $\gamma = -0.0219$. The x-axis shows a range of values for the elasticity of wage income to the minimum wage ε. The line at $\epsilon = 0.1$ corresponds to the estimates reported in Table 5.