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

Gabriele Borg   Diego Gentile Passaro   Santiago Hermo†

July 17, 2022

Abstract

The recent rise of sub-state minimum wage (MW) policies in the US has resulted in significant dispersion of MW levels within metropolitan areas. In this paper, we study the effect of MW changes on local housing rental markets exploiting the placed-based nature of MW policies. For each location we define both the log MW where the average resident works (the "workplace MW") and the log MW in the location itself (the "residence MW"). We derive a partial-equilibrium model of a housing market in which MW levels in each location affect housing demand by changing the income of commuters and the prices of non-tradable consumption. The model shows that the workplace MW has a positive effect on rents whereas the residence MW has a negative effect. We construct a ZIP-code-by-month panel using rents data from Zillow, and use a difference-in-differences design to estimate the effect of residence and workplace MW changes on log median housing rents. Our baseline results imply that a ZIP code experiencing a 10 percent increase in the workplace MW and no change in the residence MW will experience an increase in rents of 0.69 percent (SE=0.29). If the residence MW also increases by 10 percent, then rents will increase by 0.47 percent (SE=0.16). We use our results to study the incidence of two counterfactual MW policies: a federal MW increase and a city MW increase. We estimate that landlords pocket 9.2 and 11.0 cents for every dollar increase in worker income in areas affected by these policies. However, the incidence varies systematically across space.

*We are thankful to Jesse Bruhn, Kenneth Chay, John N. Friedman, Peter Hull, Matthew Pecenco, Jonathan Roth, Jesse M. Shapiro, Neil Thakral, and Matthew Turner for valuable feedback and exchanges during the development of this project. We also thank seminar participants at the Applied Micro Lunch at Brown University and the Montevideo Graduate Workshop at Universidad Católica del Uruguay for valuable comments. We acknowledge support from the James M. and Cathleen D. Stone Wealth and Income Inequality Project and the Orlando Bravo Center for Economic Research, both at Brown University. Finally, we thank Martín Gallardo for excellent research assistance. All errors are our own.

†Borg: Amazon Web Services; Gentile Passaro: Department of Economics, Brown University (email: diego_gentile_passaro@brown.edu); Hermo: Department of Economics, Brown University (email: santiago_hermo@brown.edu)

1 Introduction

In recent years, many US jurisdictions have introduced minimum wages above the federal level of $7.25, resulting in minimum wage levels that vary substantially across space and even within metropolitan areas. Minimum wage policies (hereafter MW) are *place-based* in that they are tied to a location, and workers may live and work in locations under different 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 on rents and the subsequent pass-through to landlords. Consider a new MW policy in some locations within a metropolitan area. Because low-wage workers tend to reside in specific neighborhoods with access to the (now better-paying) low-wage jobs, one would expect an increase in disposable income and a subsequent rise in demand for housing and rental prices in their residence instead of their workplace. This effect, which operates through the MW at the workplace, will undermine the distributional objective of the policy. Similarly, the MW hike will translate into higher prices of non-tradable consumption that use low-wage workers intensively as an input inside the jurisdiction that passed the new policy. As a result, the demand for housing and rental prices will also be affected. This effect, which operates through the MW at the residence, will have distributional consequences as well. Commuting patterns thus become an essential ingredient to understand the heterogeneous effects of local MW policies.

We operationalize this insight constructing, 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 shows the change in the two MW-based measures for the Chicago-Naperville-Elgin Core-Based Statistical Area (herefater CBSA) in July 2019, when the city of Chicago increased the MW from $12 to $13 and Cook County from $11 to $12. We observe that, even though the statutory MW only changed in some locations in the metropolitan area, the increase affected the workplace MW of most locations.

Estimating the effects of MW policies on rents is challenging for several reasons. First, as opposed to assessing effects on pure labor market outcomes where jobs and wages are tied to the workplace, when evaluating the housing market it is crucial to account for the fact that people may reside and work under different MW levels.[1] This is challenging because accounting for changes in the MW where residents of a location work requires data on commuting patterns at the local level. Second, successful identification of MW effects at the local level requires spatially disaggregated,

[1]However, several papers have highlighted the importance that studies on the effect of the MW on employment account for potential spillovers that may "contaminate" the control group (Kuehn 2016; Jardim et al. 2022b).

high-frequency data on rents. Using large geographies might result in null or even negative effects on average, even if no one commutes outside this region and the actual effect (of the workplace MW) on some local housing markets is positive.[2] Even if the effects in such geographies may be of interest, they may mask substantial heterogeneity and therefore miss the fact that some people may be paying higher rents due to the policy change. Similarly, MW policies change at the monthly level, so using variation at a lower frequency (such as yearly) will not allow a clean identification using the exact month of the MW change. Finally, the effects of the MW on rents may operate through different channels, such as prices of consumption, income, or changes in migration and commuting. Studying the contribution of each channel separately is important to evaluate the incidence of the policy over different locations and time horizons.

We introduce several innovations to tackle these challenges. First, we develop a tractable model that allows the MW to spill over across local housing markets through commuting. According to the model, rents in each local housing market are affected by two MW-based measures: the residence MW and the workplace MW. The model maps these measures to the effect of the MW via (i) consumption prices in the same location and (ii) income generated across locations. Second, we use a novel panel dataset on rents at the ZIP code level and with a monthly frequency from Zillow, the largest online rental marketplace in the US. We couple those data with an original panel dataset of statutory MWs at the ZIP code level, and commuting origin-destination matrices constructed from administrative records. As a result, we are able to estimate the effect of MW policies on rents using hundreds of policy changes staggered across jurisdictions and months that generate plausibly exogenous variation of workplace and residence MW levels. We use our estimated model to evaluate the impact of two MW policies: a federal MW increase from \$7.25 to \$9 and a MW increase from \$13 to \$14 in the city of Chicago. Coupling our estimates with ZIP code-level income data, we estimate the share of each dollar of extra income (generated by the MW) that accrues to landlords both summing all affected areas and in each particular location. We discuss the implications of our results for assessing the distributional impact of MW policies.

We start by laying out a partial equilibrium model of a ZIP code's rental market, which is embedded in a larger geography. The model allows residents of this ZIP code to commute to other ZIP codes to work, potentially under a different MW policy. In the model, workers demand square feet of housing as a function of non-tradable prices and income which, in turn, depend on the MW levels workers face at residence and workplace locations, respectively. The model imposes fixed commuting patterns alongside fully flexible prices.[3] This assumption, which is motivated by our empirical setting, is also consistent with the literature.[4] However, we note that validity of our results in the long-run will depend on the degree of migration as a result of the policy. The model illustrates that, if housing is a normal good and is complementary with non-tradable consumption, the effect of a change in MW legislation would be heterogeneous across ZIP codes depending on whether it

[2]Rents in neighborhoods where low-wage workers live are likely to increase, whereas elsewhere they are likely not to change or decrease, as those residents "pay" for the higher MW through higher prices and lower profits.

[3]We also assume fixed housing quality.

[4]Our data varies at the monthly level. Thus, we are assuming that the first order effects of MW changes do not affect where agents live and work in a window of a few months around MW changes. Relatedly, Pérez Pérez (2021) finds small effects of the MW on commuting in a time horizon of several years.

mostly changes the MW of its residents at their residence or at their workplace locations.[5] The model implies that the effect of changes in the MW at workplaces on log rents can be summarized in a single measure: a ZIP code's workplace MW. This measure is defined as the weighted average of log minimum wage levels across a ZIP code's workplaces, using commuting shares as weights.[6] The effect of changes in the MW at the residence can be summarized by a single measure as well, the log of the statutory MW in the location. We use this result to motivate our empirical model.

We construct a panel at the ZIP code and monthly levels with rental prices, statutory MW levels, and our MW-based measures from January 2015 to December 2019. The main rent variable comes from Zillow, and corresponds to the median rental price per square foot across Zillow listings in the given ZIP code-month cell of the category Single Family Houses and Condominiums and Cooperative units (SFCC). We show that low-wage households are more likely to rent, tend to reside in this type of housing units, and that rents per square foot are surprisingly constant across the household income distribution. These facts suggest that any effects of the MW can plausibly be captured in the Zillow data. The MW data is collected from Vaghul and Zipperer (2016) and UC Berkeley Labor Center (2020). We use our MW data coupled with commuting origin-destination matrices obtained from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES; US Census Bureau 2021) database. These data provide workplace locations for the residents of all the US census blocks, and we use it—along with a novel correspondence table between census blocks and USPS ZIP codes—to construct our workplace MW measure.

Guided by the theoretical model, we pose an empirical model where log rents in a location depend linearly on the residence MW, the workplace MW, ZIP code and time period fixed effects, and time-varying controls. This model recovers the true causal effect of the MW assuming that, within a ZIP code, changes in each of our MW variables are strictly exogenous with respect to changes in the error term, conditional on the other MW measure and the controls. To mitigate concerns of changes in the composition of our sample of ZIP codes, in our baseline analysis we use a balanced panel.[7] In an appendix we discuss a general potential outcomes framework following Callaway et al. (2021). We show that, under the assumptions of *parallel trends* and *no selection on gains*, the effects of the residence and workplace MW are identified from the conditional slope of log rents with respect to each MW measure. We discuss evidence in favor of these assumptions, both of which are satisfied by the linear functional form used as baseline.

Our preferred specification implies that a 10 percent increase in the workplace MW (holding constant the residence MW) *increases* rents by 0.69 percent (SE=0.29). A 10 percent increase in the residence MW (holding constant the workplace MW) *decreases* rents by 0.22 percent (SE=0.18). As a result, if both measures increase simultaneously by 10 percent then rents would increase by 0.47 percent (SE=0.16). These results imply that, holding fixed the commuting shares, MW changes spill over spatially through commuting, affecting local housing markets in places beyond the boundary of the jurisdiction that instituted the policy. We find that a naive model estimated only on the same-location MW would yield a coefficient similar to the sum of the coefficients on our workplace

[5]In particular, MW increases in workplace locations will cause rents to go up, whereas increases at residence will (conditional on a constant workplace MW) will lower rents.

[6]We discuss the plausibility of the required constant-elasticity assumptions in the body of the paper.

[7]We use all ZIP codes with valid rents data as of January 2015.

and residence MW measures. However, this model would not account for MW spillovers to other locations. We explore heterogeneity of our results based on ZIP code characteristics and find that results are stronger and more significant where MW workers are located. We show that using panels at the county by month and ZIP code by year levels one fails to detect effects, highlighting the importance of the granularity of our ZIP code by month data.

We conduct several robustness checks to assess the validity of our results. First, we provide support for our identification assumptions by (1) testing for "pre-trends" by estimating an extended model that includes leads and lags of the MW measures, and (2) developing 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 that the conditional relationship of log rents with respect to each MW measure is nearly linear. Second, we show robustness of our results to different sets of controls, alternative definitions of commuting shares, alternative samples, and reweighing observations to match demographics of the population of urban ZIP codes. Third, we estimate models for different categories of housing rentals and find overall consistent results. Finally, in the appendix we estimate two alternative models. We construct a "stacked" regression model, similar to Cengiz et al. (2019), that compares ZIP codes within metropolitan areas where some but not all experienced a change in the statutory MW. This should 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). The second alternative model includes the lagged first difference of rents as a control, and is estimated via instrumental variables following Arellano and Bond (1991). We find consistent results in both exercises.

In the final part of the paper, we construct a counterfactual exercise to capture the incidence of MW policies on landlords. We compute the share pocketed by landlords in each ZIP code, and also compute the total incidence summing across locations. We posit 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 exercise, we posit an increase 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.

Our results imply that a share of the extra income that low-wage workers receive due to the policy is actually captured by landlords. Viewed through the lens of our theoretical model, the mechanism behind this is a rise in housing demand in a scenario of a finite housing supply elasticity. In the context of a general equilibrium model, Kline and Moretti (2014) argue that this mechanism causes place-based policies to be welfare inefficient. While studying the full welfare effects of MW policies is beyond the scope of the paper, our results imply that ignoring rent changes will lead to an overstatement of the gains of low-wage workers following a MW increase.

Our analysis has some important limitations. A first limitation is that our derivation of the residence and workplace MW measures as reflecting changes in non-tradable prices and income relies on several constant-elasticity assumptions. We discuss the plausibility of the required assumptions in the body of the paper. A second limitation is that we do not account for changes in migration and commuting. While we maintain that this is a plausible assumption to obtain our empirical estimates, the long-run incidence of the policy on landlords may differ from our computations if the residence and workplace locations of low-wage households responds strongly to the MW. A final limitation is that our exercises do not capture the full welfare effect of MW policies. A full account of the long-run welfare effect of the sub-state MW policies in the 2010s requires specifying a general equilibrium model that accounts for changes in consumption prices, changes in workplace and residence locations of workers, and potential employment effects. However, as low-wage households are more likely to rent and thus to be negatively affected by rent changes, our analysis suggest that such computation should take into account 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 that estimate the effect of the MW on rents in the same location are Tidemann (2018) and Yamagishi (2019, 2021).[8] Agarwal et al. (2021) show that MW increases lower the probability of rental default, and 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 main difference of our paper with this work is that we differentiate between residence and workplace MW levels, incorporating spillovers across regions. 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. Hsieh and Moretti (2019) quantify the costs of housing constraints in the US. In line with this insight, we show that landlords may benefit from a place-based MW policy. 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.

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).[9] 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.

[8]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.

[9]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 (Cadena 2014; Monras 2019; Pérez Pérez 2021, e.g.,), and prices of consumption goods (Allegretto and Reich 2018; Leung 2021, e.g.,).

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. Section 5 presents 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 illustrate why we expect a different impact of MW changes on rents at workplace and residence locations. Our model is static and assumes a fixed distribution of workers across residence and workplace locations. The addition of a time dimension is discussed in Appendix A.

2.1 Setup

We consider the rental market of some ZIP code i embedded in a larger geography composed of a finite number of ZIP codes $\mathcal{Z}$. Workers with residence i work in a ZIP code $z \in \mathcal{Z}(i)$, where $\mathcal{Z}(i) \subseteq \mathcal{Z}$. More precisely, we let L_{iz} denote the number of i's residents who work in z; and $L_i = \sum_{z\in\mathcal{Z}(i)} L_{iz}$ and $L_z = \sum_{i\in\mathcal{Z}(i)} L_{iz}$ the number of residents in i and workers in z, respectively.[10] We assume that $\{L_{iz}\}$ is fixed. This assumption is intended as an approximation to our empirical setting where we look at the effects of MW changes at a monthly frequency.[11] This assumption is consistent with estimates of small effects of the MW on employment, as in Cengiz et al. (2019) and Dustmann et al. (2022), and on migration, as in Pérez Pérez (2021), in a time frame of several years.

Each ZIP code has a binding minimum wage. The set of binding MWs 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 $u_{iz} = u\left(H_{iz}, C_{iz}^{\mathrm{NT}}, C_{iz}^{\mathrm{T}}\right)$ subject to a budget constraint

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

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

Assumption 1 (Effect of Minimum Wages)**.** *We assume that (i) the 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* $z \in \mathcal{Z}(i)$*.*

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

[11]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.

The structure of the problem and Assumption 1 are in line with the literature. First, evidence by Miyauchi et al. (2021) shows that individuals tend to consume close to home. As a result, we expect them to be sensitive to prices of local consumption in their same neighborhood, justifying the inclusion of C_{iz}^{NT} in the utility function.[12] Second, MW hikes have been shown to increase prices of local consumption (e.g., Allegretto and Reich 2018; Leung 2021), and also to increase wage income even for wages above the MW level (e.g., Cengiz et al. 2019; Dube 2019b).[13]

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 $\{h_{iz}(R_i, P_i, Y_z)\}_{z \in \mathcal{Z}(i)}$. We summarize the properties of these functions below.

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.[14] While direct empirical evidence on this particular channel is lacking, we view the evidence of workers sorting towards locations with high housing costs and high-quality and more expensive amenities as consistent with it (e.g., Couture et al. 2019).

Note that, given our assumptions, an increase in a group (i, z)'s workplace MW will tend to increase housing demand in i, and an increase in residence MW will have a negative effect—conditional on its effect via the workplace MW of the group (i, i).

Housing supply

We assume that absentee landlords supply square feet in i according to the function $S_i(R_i)$, 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 houses at which point the supply becomes perfectly inelastic.

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

[13] 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.

[14] 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.

2.2 Equilibrium and Comparative Statics

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

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

Given that the per-capita housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.[15] 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 $\{L_{iz}\}_{z\in\mathcal{Z}(i)}$ and Assumptions 1 and 2, we have that*

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

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

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

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

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

where $\pi_{iz} = \frac{L_{iz}}{L_i}$ represents the share of i's residents working in z, $\xi_{iz}^x = \frac{dh_{iz}}{dx_i}\frac{x_i}{\sum_z \pi_{iz}h_{iz}}$ for $x\in\{R,P,Y\}$ is the elasticity of the per-capita housing demand evaluated at the average per-capita demand of ZIP code i, $\epsilon_i^P = \frac{dP_i}{d\underline{W}_i}\frac{\underline{W}_i}{P_i}$ 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 $\eta_i > 0$ and $\xi_{iz}^R < 0$ for all $z\in\mathcal{Z}(i)$, the first factor is positive. From Assumptions 1 and 2,

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

$\epsilon^Y_{iz} \geq 0$ and $\xi^Y_{iz} > 0$. Therefore, the effect is positive if for z' we have $\epsilon^Y_{iz'} > 0$, and the effect is zero otherwise.

For ZIP code i the partial effect is given by

$$\frac{d \ln R_i}{d \ln \underline{W}_{z'}} = \left(\eta_i - \sum_z \pi_{iz} \xi^R_{iz} \right)^{-1} \left(\epsilon^P_i \sum \pi_{iz} \xi^P_{iz} + \pi_{ii} \xi^Y_{ii} \epsilon^Y_{ii} \right).$$

By Assumption 1 we have that $\epsilon^P_i > 0$ and that $\epsilon^Y_{ii} \geq 0$. By Assumption 2 we have that $\xi^Y_{ii} > 0$ and that, for all $z \in \mathcal{Z}(i)$, $\xi^P_{iz} < 0$. Then, the second parenthesis has an ambiguous sign. The third statement of the Proposition follows directly. $\square$

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. The second part of Proposition 1 establishes that a decreasing effect on rents may follow if the minimum wage also increases in ZIP code i. As a result, the sign of the overall effect of the policy 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 $\xi^Y_{iz} = \frac{dh_{iz}}{dY_z} \frac{Y_z}{\sum_z \pi_{iz} h_{iz}}$ and on the elasticities of income to minimum wages $\epsilon^Y_{iz} = \frac{dY_z}{d\underline{W}_z} \frac{\underline{W}_z}{Y_z}$. These (i,z)-specific terms weigh the change in MW levels at workplace locations, and their sum over z impacts the change in rents. The following proposition establishes conditions under which we can reduce the dimensionality of the rent gradient to two MW-based measures.

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

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

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

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

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

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

Proposition 2 shows that, under a homogeneity assumption on the elasticities of housing demand to income and of income to the MW,[16] 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

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

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.

How likely are these assumptions 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 flow L_{iz} varies strongly by workplace z. The assumption that the elasticity of housing demand to income is constant will hold trivially for all preferences with $h_{iz} = g(R_i, P_i) Y_i$ for some $g(\cdot)$, such as those embedded 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 Proposition 2 requires some strong homogeneity assumptions that will likely not hold in practice. 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 $\beta_i d\underline{w}_i^{\text{wkp}}$ will be close to value of the elasticity-weighted changes in workplace MW levels that, according to the model, determine rents.[17] 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,[18] and obtain qualitatively similar (and even larger in absolute value) estimates of the elasticities.

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. 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 collect data on federal-, state-, county-, and city-level statutory MW levels from Vaghul and Zipperer (2016). We supplement their data, available up to 2016, with data from UC Berkeley

[17] 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:

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

The second term on the right-hand side is equivalent to $\beta_i \underline{w}_i^{\text{wkp}}$ in Proposition 2. The third term reflects the heterogeneity. If, for example, 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.

[18] This exercise can be mapped to the model by assuming that $\xi^Y_{iz}\epsilon^Y_{iz}$ is a linear function of the share of MW workers in i.

Labor Center (2020) and from official sub-national government offices for the years 2016–2020.[19] 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 census blocks and ZIP codes detailed in 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 Appendix B.2.

Appendix Figure 1 shows the different levels of the binding MW policies over time in our data. Panel A focuses on state-level MW policies. There are 30 states with MW policies in 2010–2019, all of which started prior to January 2010. Panel B shows sub-state MW policies. In total, there are 36 counties and cities with a MW policy in the decade. 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 December 2019 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.

3.2 Relationship Between Income and Housing

We explore the joint-distribution of income levels and housing choices in the US using American Housing Survey data for 2011 and 2013 (US Department of Housing and Urban Development 2020a). We compare households within metropolitan areas. Figure 3 shows that low-income households are much more likely to rent. While approximately 60 percent of households in the bottom quintile are renters, only around 13 percent of households in the top quintile are. Appendix Figure 2 shows that, among households that rent, rents per square foot are surprisingly constant across household income levels. Appendix Figure 3 shows the type of building households live in by household income decile.

We explore variations over space in housing expenditure. To do so, for the period 2010–2019 we collected Individual Income Tax Statistics aggregated at the ZIP code level from the IRS (Internal Revenue System 2022b),[20] and Small Area Fair Market Rents (SAFMRs hereafter) data from the HUD (US Department of Housing and Urban Development 2020b).[21] For each ZIP code in 2018, we

[19]Some states and cities issue different MW levels for small businesses (usually identified by having less than 25 employees). In these cases, we select the general MW level as the prevalent one. In addition, there may be different (lower) MW levels for tipped employees. We do not account for them because employers are typically required to make up for the difference between tipped MW plus tips and actual MW.

[20]For each ZIP code and year we observe the number of households, population, adjusted gross income, total wage bill, total business income, number of households that receive a wage, number of households that have business income, and the number of households with farm income.

[21]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). The data 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;

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.[22,23] Appendix Figure 4 maps our estimates for the metropolitan area of Chicago. There is considerable variation in housing expenditure over space, with poorer areas generally spending a higher share of their income in housing.

We would like to know the share of workers earning at or below the MW in each ZIP code. Unfortunately, this variable is not readily available in public data.[24] Thus, 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 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).

3.3 Estimation Data and Samples

3.3.1 Rents Data

One of the main challenges to estimate the effects of any policy on the rental housing market is to obtain adequate data. We leverage data from Zillow at the ZIP code and month levels. The high frequency and resolution of the Zillow data is an advantage since it allows us to explore the effects of MW changes on rents exploiting their precise timing and geographic scope.

Zillow is the leading online real estate and rental platform in the US, hosting more than 110 million homes and 170 million unique monthly users in 2019 (Zillow 2020a). Zillow provides, starting on February 2010, the median rental and sales price among homes listed on the platform for different house types and at different geographic and time aggregation levels (Zillow 2020b).[25] We collect the ZIP code level monthly time series from February 2010 to December 2019. There is variation in the entry of a ZIP code to the data, and units with a small number of listings are omitted.[26] As we will explain below, for our main analysis we will use the subset of ZIP codes with valid rents data as of January 2015.

We focus our primary analysis on a single housing category: single-family houses and condominium and cooperative units (SFCC). This is the series with the largest number of non-missing ZIP codes, as it covers the most common US rental house types (Fernald 2020). We focus on rents *per square foot* to account for systematic differences in housing size. In fact, as shown in Appendix Figure 2, this variable does not seem to vary much by income levels. Our main outcome variable

Yamagishi 2019).

[22]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 Appendix B.3 for details.

[23]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.

[24]The share of MW workers cannot be constructed from usual sources. For instance, for the Current Population Survey (CPS) or the 5% Census samples available in IPUMS the smaller geographical unit is the Public Use Microdata Area (PUMA).

[25]As of the release of this article, the data used in this paper is not available for download. See Internet Archive (2021) for a snapshot of the website as of February 2020. We downloaded the data in January 2020, a month before Zillow removed it from its website.

[26]Two related notes: (i) once a ZIP code enters the Zillow data it never drops out, and (ii) the threshold used by Zillow to censor the data is not made public.

represents the median rental price per square foot in the SFCC category among units listed in the platform for a given ZIP code and month. Appendix Figure 5 shows that this series follows a similar trend over time when compared to SAFMR. We show results using median rents per square foot in other rental categories available in the data as well.

Zillow data has several limitations. First, Zillow's market penetration dictates the sample of ZIP codes available. Appendix Figure 6 shows that the sample of ZIP codes with valid SFCC rents data typically coincides with areas of high population density. Second, we only observe the median per-square-foot rental value among listings. We do not observe actual rents paid by tenants in a given period, the distribution of rents among listings in the given ZIP code and month, nor the number of units listed for rent in a given month.

3.3.2 The residence and workplace minimum wage measures

In this subsection we define the MW variables we use in our analysis, which follow the intuition in Proposition 2. With the panel described in Section 3.1 at hand, computing the residence MW is straightforward. We define it as $\underline{w}_{it}^{\text{res}} = \ln \underline{W}_{it}$.

We also construct the workplace MW, which captures the spillover effects of MW policies across locations. To construct this measure we need to know, for each ZIP code, where workers residing in that location work. We obtain this information from the Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES; US Census Bureau 2021) for the years 2009 through 2018. We collected the datasets for "All Jobs." The raw data is originally aggregated at the census block level. We further aggregate it to ZIP codes using the original correspondence between census blocks and USPS ZIP codes described in Appendix B.1. This results in ZIP code residence-workplace matrices that, for each location and year, indicate the number of jobs of residents in every other location.

We then 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 $\{\omega_{iz}\}_{z\in\mathcal{Z}(i)}$ as $\omega_{iz} = N_{iz}/N_i$, where N_{iz} is the number of jobs who reside in i and work in z, and N_i is the total number of jobs originating in i. The workplace MW measure is defined as

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

The workplace MW has a shift-share structure, where the log of the MW level in workplaces can be interpreted as the exogenous shock. Borusyak et al. (2021) discuss the interpretation and validity of shift-share regressions.

While our baseline estimates use 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.[27]

Figure 1, already discussed in the introduction, illustrates the difference in these measures by

[27]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).

plotting the change in the residence and workplace MW measures in the Chicago-Naperville-Elgin CBSA in July 2019. For completeness, Appendix Figure 7 shows the changes in our main median rents variable around the same date.

3.3.3 Other data sources

Time-varying data 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.[28] 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. We use these data as controls for the state of the local economy in our regression models.

ZIP code characteristics While our MW assignment recognizes that many of 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 empirical models.

In order to describe our sample of ZIP codes we collect data 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 it to ZIP codes using the correspondence table described in Appendix B.1.

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-level changes and 121 county- and city-level changes. Appendix Figure 8 shows the distribution of positive increases in our statutory MW variable among all ZIP codes available in the Zillow data. Given that ZIP codes enter the Zillow data progressively over time affecting the composition of the sample, we construct our *baseline panel* keeping ZIP codes that enter at most in January 2015. The resulting fully-balanced panel runs from January 2015 to December 2019 and contains 2,782 MW changes at the ZIP code level.[29]

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, i.e., those that have some non-missing value of rents per square foot in the SFCC category between February 2010 and December 2019. Finally, the fourth column shows descriptive statistics for our estimation sample, which we refer to as the "baseline sample."

[28]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."

[29]We include six leads and lags of the MW measures in this panel, so the dataset actually runs from July 2014 to June 2020.

While our baseline sample contains only 11.8 percent of all urban ZIP codes, it covers 25.0 percent of their population and 25.8 percent of their households. With respect to demographic characteristics, 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 the case because Zillow is present in almost every large urban market, but it does not operate as often in small urban or rural areas. In an attempt to capture the treatment effect for the average urban ZIP code we conduct an estimation exercise where we re-weight our sample to match the average of a handful of characteristics of those.

Finally, Appendix Table 1 shows some sample 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.

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

4 Empirical Strategy

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 consists of rents, the residence and workplace MW measures, and economic controls. The residence and workplace MW measures are our two continuous treatment variables, and we use a difference-in-differences strategy to estimate their effects on rents. We estimate the effect of the residence MW from the slope of the relationship between the residence MW and rents using places with a similar level of the workplace MW. Intuitively, we condition on the workplace MW and compare places with slightly different residence MW levels. Likewise, we estimate the effect of the workplace MW from the slope of the relationship between the workplace MW and rents conditioning on locations with a similar level of the residence MW.

For these slopes to correspond to the causal effect of each MW measure on rents we need to make two assumptions. The first assumption is a form of *parallel trends*: among ZIP codes with the same workplace MW, ZIP codes with higher and lower residence MW levels would have had parallel trends in rents if not for the change in the residence MW. The second assumption is *no selection on gains*: ZIP codes that receive different levels of the residence MW must experience a similar treatment effect, conditional again on the workplace MW. We need these assumptions to hold for the workplace MW as well. Appendix C formalizes these assumptions in a potential outcomes framework following Callaway et al. (2021). We discuss the plausibility of these assumptions in the next subsection.

4.2 Parametric Model

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

$$r_{it} = \alpha_i + \tilde{\delta}_t + \gamma \underline{w}_{it}^{\text{res}} + \beta \underline{w}_{it}^{\text{wkp}} + \mathbf{X}_{it}^{'} \eta + \varepsilon_{it}, \tag{4}$$

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 $(\overline{T})$. 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 and workplace MW, respectively.

By taking first differences in equation (4) we obtain

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

where $\delta_t = \tilde{\delta}_t - \tilde{\delta}_{t-1}$. We estimate the model in first differences because we expect unobserved shocks to rental prices to be serially autocorrelated over time, making the levels model less efficient. Appendix Table 2 shows strong evidence of serial auto-correlation in the error term of the model in levels. 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 (5) to be estimable is a rank condition, which implies that the MW measures must have independent conditional variation. 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 MW measures experience independent conditional variation.

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

Identification

The model in (4) imposes a linear functional form. Assuming that the true data generating process has 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 Appendix C. We view this as a reasonable assumption. For it not to 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 next 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 to have a causal interpretation we need another assumption: the error term $\Delta\varepsilon_{it}$ must be *strictly exogenous* with respect to the MW measures. In other words, the 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 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 either one of the MW measures at a time in (5).[30] We do so because Appendix C suggests that we only need to condition on the current level of one of the MW measures for parallel trends of the other measure to hold (see Assumption 3). A second reason is that the residence MW and workplace MW are strongly correlated. Including leads and lags of both measures results in standard errors that are two to four times larger, diminishing the power of the pre-trends test.

A second implication of the strict exogenity assumption is that it rules out feedback effects from current values of rents on our MW variables, i.e., MW changes are assumed not to be influenced by past values of rents. While we think that this is a reasonable assumption—MW policies are rarely set taking into account their effects on housing markets—, we allow for this type of feedback effects in a specification described in the following subsection. Finally, we note that the strict exogeneity assumption allows for arbitrary correlation between α_i and both MW variables (e.g., 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 rents and MW changes. 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 jurisdictions. To control for variation arising from unobserved trends in local labor markets we include economic controls from the QCEW $\mathbf{X}_{it}$.[31] Specifically, we control for average weekly wage and establishment counts at the county-quarter level, and for employment counts at the county-month level, for the sectors "Professional and business services," "Information," and "Financial activities."[32] We also try models where we control for ZIP code-specific linear trends, which should account for time-varying heterogeneity not controlled for by our economic controls that follows a linear pattern. Under the

[30] For instance, for the workplace MW we estimate

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

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

[31] 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.

[32] 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 earning at or below the MW in those industries was 0.8, 1.5, and 0.2, respectively.

assumption that there are no anticipatory effects in the housing market, we interpret the absence of pre-trends as evidence against the presence of unobserved economic shocks driving our results. Given the high frequency of our data and the focus on short windows around MW changes, the assumption of no anticipatory effects seems plausible.[33]

4.3 Alternative Strategies

Recent literature has shown that usual estimators in a difference-in-differences setting do not correspond to well-define 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.[34] To try to ease these concerns, in an appendix we construct a "stacked" implementation of equation (5) in which we take 6 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, where we propose a model that includes the lagged rents variable as control. In such a model, β and γ have a causal interpretation under a weaker *sequential exogeneity* assumption (Arellano and Bond 1991; Arellano and Honoré 2001). This alternative assumption 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.

4.4 Sample Selection Concerns and Heterogeneity

As explained in Section 3.3.4, the model in equation (5) 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.

As a separate exercise, we explore heterogeneity of our results with respect to pre-determined variables. If the mechanism proposed in Section 2 is correct, then we expect the effect of the residence MW to be stronger in locations with many residents earning the MW. The reason is that the production of non-tradable goods presumably uses more low-wage work, and thus the increase

[33]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.

[34]Callaway et al. (2021, Section 3.4) discusses the properties of the TWFE estimator in the context of a single continuous treatment.

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:

$$\Delta r_{it} = \Xi_t + \tilde{\gamma}_0 \Delta \underline{w}_{it}^{\text{res}} + \tilde{\gamma}_1 \iota_i \Delta \underline{w}_{it}^{\text{res}} + \tilde{\beta}_0 \Delta \underline{w}_{it}^{\text{wkp}} + \tilde{\beta}_1 \iota_i \Delta \underline{w}_{it}^{\text{wkp}} + \Delta \mathbf{X}_{it}' \tilde{\eta} + \Delta \tilde{\varepsilon}_{it}, \tag{6}$$

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.[35] We conduct a similar exercise using median household income and the share of public housing units.

5 Estimation Results

In this section we present our main 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 Main Results

Table 2 displays our estimates using the baseline sample described in Section 3.3.4 under the parametric model in equation (5). Column (1) shows the results of a regression of the workplace MW on the residence MW, economic controls, and monthly date 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 that there is independent variation to estimate the effect of both variables on rents.

Columns (2) through (4) of Table 2 show estimates of equation (5), varying the set of included MW measures. Column (2) shows the results of estimating a model that does not include the workplace MW. In this model, only locations with a statutory MW change are assumed to experience effects, similar to much of the MW literature (e.g., Dube et al. 2010; Meer and West 2016; Yamagishi 2021). In this case, we estimate the elasticity of median rents to the MW to be 0.0372 (t = 2.57). Column (3) shows the results of a model that does not include the residence 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 proxy of the changes in income generated by MW policies. Column (4) estimates the model using both MW measures. Consistent with the theoretical model in Section 2, the coefficient on the residence MW (γ) now turns negative and equals -0.0219, although it is not statistically significant (t = -1.25). The coefficient on the workplace MW (β) increases to 0.0685 and is statistically significant (t = 2.38). 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 similar in magnitude to the

[35]We discuss our estimates of the share of MW workers who reside in each location in Section 3.2.

coefficient in column (3), and strongly significant ($t = 2.95$). Thus, our results imply that a 10 percent increase in both MW measures will increase rents by 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 be larger.

Identifying assumptions

A central concern with these results is whether our identifying assumptions are likely to hold. Figure 4 shows estimates of the parametric model including leads and lags of either MW measure. Panel A adds leads and lags of the workplace MW measure only, so that the coefficients are $\{\{\beta_s\}_{s=-6}^{-1}, \beta, \{\beta_s\}_{s=1}^{6}, \gamma\}$. We cannot reject the hypothesis that $\beta_{-6} = ... = \beta_{-1} = 0$ ($p = 0.661$). Post-event coefficients $\{\beta_s\}_{s=1}^{6}$ are also estimated to be statistically zero. The only significant estimates are those of β and γ, and they imply similar effects relative to the model without dynamic effects. The estimate of γ is -0.0236 ($t =$ -1.3) and of β is 0.07 ($t = 2.47$). We can now reject the hypothesis of equality of coefficients more precisely ($p = 0.043$). Our estimate of $\gamma + \beta$ is now 0.0464. It is significant ($t = 2.91$) and almost identical to our baseline. Panel B of Figure 4 shows that a similar story obtains when we add leads and lags of the residence MW only.[36] We interpret these results as evidence in favor of the parallel trends assumption.

Appendix Figure 9 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 (4). Furthermore, the slopes in these figures are in line with our baseline estimates of γ and β. Appendix Figure 10 illustrates the identifying variation we use by mapping the residualized change in workplace MW and the residualized change in log rents.[37] Panel A of Appendix Figure 10, 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 Appendix Figure 10 shows residualized rents.

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

[36] Including leads and lags of both MW measures results in confidence intervals for pre- and post-event coefficients between two and four times wider. As discussed in Section 4, we exclude these estimates because the pre-trends test based on this model is statistically weak.

[37] 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.

(d) show that interacting our time fixed effects with indicators for county or CBSA yields similar conclusions. In all these cases our baseline 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 different and 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. We think that within-state comparisons are not appropriate because they fully identify coefficients off of local MW changes which, in turn, are more likely to be passed by cities or counties that have more dynamic rental markets. For instance, comparisons within the state of Illinois between ZIP codes in Cook County (the main jurisdiction with a local MW level) and the average ZIP code in the state are likely to yield biased results, as both MW levels and rents tend to increase at the same time of the year in Cook County. On the other hand, within-CBSA and within-county comparisons use ZIP codes that are likely to experience similar trends in rental markets. Row (f) includes ZIP code fixed effects in the first-differenced model, which is equivalent to allowing for a 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. Rows (j) and (k) use 2017 commuting shares for workers that earn less than $1,251 per month and workers that are less than 29, respectively. If anything, the results seem to be stronger and more significant in this case, consistent with the idea that these workers are more likely to earn close to the minimum wage.

Other geographies and time frames

In this subsection, we compare our results with estimates obtained from alternative panels where the unit of observation is either the county by month or the ZIP code by year. The reason to show these results is twofold. First, it allows us to emphasize the importance of the high resolution of our data for identification. Second, it permits comparing our results with previous literature. Because none of the previous papers distinguish between workplace and residence MW levels, we compare them to our short model that excludes the residence MW. The results are summarized in Appendix Table 3, where Panel A repeats the results in Table 2 for convenience.

Panel B of Appendix Table 3 shows our results based on a county-by-month panel. Overall, the results are similar in magnitude to our baseline but are not statistically significant. In Appendix Figure 11 we extend the model that includes both MW measures adding leads and lags of the workplace MW, as in Panel A of Figure 4. We observe 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.

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

5.2 Alternative Strategies

Appendix Table 4 estimates our main models using a "stacked" sample, as discussed in Section 4.3. Our sample contains 618 "events," that is, CBSA-month pairs that had some strict subset of ZIP codes increasing the residence MW. These estimates interact the year-month fixed effects with event ID indicators, and thus compare ZIP codes within the event and time window. 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. Appendix Figure 12 shows the results of a similar model that 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.

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.

5.3 Sample Selection Concerns and Heterogeneity

Table 4 explores the sensitivity of our estimates to the sample of ZIP codes used in estimation. Columns (1) and (3) use our baseline sample and an unbalanced sample of ZIP codes controlling for quarter-year-of-entry by year-month fixed effects, respectively. While the coefficient on the residence MW is very stable across specifications, the one on the workplace MW seems to decrease when using the unbalanced sample. This suggests that effects may be smaller for earlier periods.

We also worry that our ZIP codes might be a selected sample in ways that affect the estimated average causal response parameters γ and β. In columns (2) and (4) we estimate the same models but re-weighting observations to match relevant pre-treatment characteristics of the sample of urban ZIP codes. Our weights follow Hainmueller (2012) and are designed to match the averages of three variables: the share of renter occupied households according to the 2010 US Census, and the shares of residents and workers that earn less than \$1,251, according to 2014 LODES. Effects appear stronger in column (2), although we cannot reject that they are equal to the estimates in column (1).

Table 5 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 residence MW is -0.062 (SE = 0.024) and on the workplace MW is 0.111 (SE = 0.034). For a ZIP code that is one standard deviation above the average share of MW workers, the effect of the residence MW is of -0.150 (SE = 0.064) and, for the workplace MW, the effect is 0.203 (SE= 0.076). Hosting 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.

Column (3) of Table 5 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. We find that the effects for ZIP codes that have more public housing are 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 the high presence of low-wage residents and workers where, per our previous discussion, we would expect stronger effects.

5.4 Alternative rental categories

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." (The categories SF and CC are the components of our SFCC variable.) Appendix Figure 3 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" where the sign of the coefficients is flipped relative to baseline, although these estimates are not statistically significant.

5.5 Summary and Discussion

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

We compare our estimates to those in Yamagishi (2019) and Agarwal et al. (2021). Using Fair Market Rents data at the county by year level, Yamagishi (2019, Tables 1 and 2) uses a long-differences specification and obtains null results using all counties and statistically significant results using densely populated counties. In the latter case, he reports 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 using only the workplace MW imply a one-time increase in rents of a similar magnitude as Yamagishi's (2019) one-year estimates. While our results are consistent in this sense, Yamagishi (2019, Table 3) detects significant pre-trends, questioning the validity of the longer-run results.[38]

Our results are consistent with Agarwal et al. (2021) as well. While the main goal of this paper is to estimate the effect of the MW on eviction risk, the authors provide estimates of the effect of the MW on rents using individual-level transactions from 2000 to 2009. Agarwal et al. (2021, Figure 4) suggest that a 10 percent hike in the MW (at residence) increases rents paid by individuals by 0.5 percent. The authors find an increasing effect over time that fully materializes after 6 months. This result is consistent with our estimates that show how rents of housing units in the rental market (which we observe in the Zillow data) jump discretely on the month of the MW change.

6 Counterfactual Analysis

We use our empirical results to explore the consequences of counterfactual MW policies. We present two scenarios. First, we study the consequences of an increase in the federal MW from \$7.25 to \$9. Second, we explore the incidence of increasing the local MW of Chicago City from \$13 to \$14. For each policy, we estimate the share of each dollar generated by the new policy that is absorbed by rents in each location across affected areas, and the share of each dollar that accrues to landlords overall. These exercises illustrate the consequences of different MW policies in the housing market.

6.1 Empirical Approach

An increase in the MW will shift income spatially, and therefore affect housing demand across places. The extent of such shift will depend on the nature of the new MW policy. A federal increase will affect all jurisdictions whose previous MW levels was surpassed, potentially influencing most regions in the country indirectly through commuting. The impact of a local increase will be contained to nearby ZIP codes where affected workers reside. In this section, we derive formulas to estimate the incidence of these policies across ZIP codes.

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

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, $H_i R_i = \sum_{iz} H_{iz} R_{iz}$ denotes total housing expenditure in i, and $Y_i = \sum_{iz} Y_{iz}$ 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

$$\rho_i = \frac{H_i^{\text{Post}} R_i^{\text{Post}} - H_i^{\text{Pre}} R_i^{\text{Pre}}}{\Delta Y_i} = H_i \frac{\Delta R_i}{\Delta Y_i}. \tag{7}$$

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

We predict rent changes for all ZIP codes using the model in equation (5). 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,

$$\Delta r_i = \beta \Delta \underline{w}_i^{\text{wkp}} + \gamma \Delta \underline{w}_i^{\text{res}}. \tag{8}$$

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

$$\Delta y_i = \varepsilon \Delta \underline{w}_i^{\text{wkp}}, \tag{9}$$

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.

While gauging the spillover effects of the MW on wages across ZIP codes is not the main goal of the paper, estimates of ε are not readily available in the literature. Appendix D discusses the details of our estimation strategy, and compares the results with estimates of the effect of the MW on income of workers in the same jurisdiction. We also show that our results are heterogeneous depending on the share of MW workers residing in a location, although for ease of interpretation of the results we use the simpler model in (9) in the counterfactual exercises.

Assuming that we know the value of ε, we can substitute (8) and (9) into equation (7) 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:

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

Hence, 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%.[39] In the local case, we focus on all 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.[40] 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 Appendix Figure 13. 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.

Panel A of Appendix Figure 14 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.

[39] 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.

[40] 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 Appendix B.1. We do so to account for the fact that the new MW policy may be partially binding in some ZIP codes.

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 Appendix Figure 14 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, γ, and ε. Following the results in Table 2, we take $\beta = 0.0685$ and $\gamma = -0.0219$. Based on the results discussed in Appendix D, we take $\varepsilon = 0.1013$. 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 equals 0.101, which implies that at the median 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 Appendix Figure 15 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 (as reflected in Appendix Figure 4).

The top rows of Panel A in Table 6 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 have both MW measures affected by the policy, and as a result rent increases are moderated by the negative effect of the residence MW. The median incidence for this group is 9.6 cents of each dollar. Locations in the second group are only affected through changes in the workplace MW, and as a result rent changes are relatively larger. The median incidence for this group is 15.7 cents of each dollar. The bottom row of Panel A in Table 6 shows our estimate of total incidence of the policy. The share accruing to landlords in this sample of ZIP codes is given by 9.2 cents of each dollar. 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 nearly monotonic and positive 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 6 displays median values for ZIP codes in 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 ZIP code.

Panel B of Figure 6 maps the shares. Panel B of Appendix Figure 15 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 income increases and lower price increases, implying a large share pocketed.[41]

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 these patters is the increase in non-tradable consumption in the same location that moderates rent increases.

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.

7 Conclusions

We explore whether minimum wage changes affect housing rental prices. To answer this question we develop a theoretical approach that accounts for the fact that MW workers typically reside and work in different locations. Our model suggests two summary statistics that should capture the effect of statutory MW changes on rents in a particular location, which we call the residence MW and the workplace MW.

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 and residence MW have opposing effects on rents, and thus MW changes appear to spill over spatially through commuting. Our two-parameter model is able to capture rich heterogeneity in the effect of the MW on rents depending on the prevailing commuting structure.

[41] It is worth emphasizing that we estimate large increases in wage income inside the city due to the fact that our model in (9) 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.

To gauge the distributional consequences of the effect of the MW on housing markets we explore two counterfactual MW policies. We compute the incidence of these policies on landlords. Our results suggest that landlords pocket a non-negligible portion of the newly generated wage income. 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 differences in the MW within metropolitan areas. However, one would expect general equilibrium adjustments to large differences 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 appears as a fruitful avenue for future work.

References

Agarwal, Sumit, Brent W. Ambrose, and Moussa Diop (2021). “Minimum Wage Increases and Eviction Risk”. In: *Journal of Urban Economics*, p. 103421.

Ahlfeldt, Gabriel M, Duncan Roth, and Tobias Seidel (2022). *Optimal minimum wages*. CEPR Discussion Paper DP16913. Center for Economic and Policy Research.

Allegretto, Sylvia and Michael Reich (2018). “Are Local Minimum Wages Absorbed by Price Increases? Estimates From Internet-Based Restaurant Menus”. In: *ILR Review* 71.1, pp. 35–63.

Allen, Treb, Simon Fuchs, Sharat Ganapati, Alberto Graziano, Rocio Madera, and Judit Montoriol-Garriga (2020). “Is Tourism Good for Locals? Evidence from Barcelona”. In: *Unpublished manuscript*.

Angrist, Joshua D and Guido W Imbens (1995). “Two-stage least squares estimation of average causal effects in models with variable treatment intensity”. In: *Journal of the American Statistical Association* 90.430, pp. 431–442.

Arellano, Manuel and Stephen Bond (1991). “Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations”. In: *Review of Economic Studies* 58.2, pp. 277–297.

Arellano, Manuel and Bo Honoré (2001). “Panel Data Models: Some Recent Developments”. In: *Handbook of Econometrics*. Vol. 5. Elsevier, pp. 3229–3296.

Autor, David, Alan Manning, and Christopher L. Smith (2016). “The Contribution of the Minimum Wage to US Wage Inequality Over Three Decades: a Reassessment”. In: *American Economic Journal: Applied Economics* 8.1, pp. 58–99.

Berger, David W, Kyle F Herkenhoff, and Simon Mongey (2022). *Minimum wages, efficiency and welfare*. NBER Working Papers 29662. National Bureau of Economic Research.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel (2021). “Quasi-Experimental Shift-Share Research Designs”. In: *Review of Economic Studies* 89.1, pp. 181–213.

Butts, Kyle (2021). “Difference-in-Differences Estimation with Spatial Spillovers”. In: *arXiv preprint arXiv:2105.03737*.

Cadena, Brian C. (2014). “Recent Immigrants as Labor Market Arbitrageurs: Evidence From the Minimum Wage”. In: *Journal of Urban Economics* 80, pp. 1–12.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant’Anna (2021). “Difference-in-Differences With a Continuous Treatment”. In: *arXiv preprint arXiv:2107.02637*.

Card, David and Alan B. Krueger (1994). “Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania”. In: *American Economic Review* 84.4, pp. 772–93.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer (2019). “The Effect of Minimum Wages on Low-Wage Jobs”. In: *Quarterly Journal of Economics* 134.3, pp. 1405–1454.

Couture, Victor, Cecile Gaubert, Jessie Handbury, and Erik Hurst (2019). *Income Growth and the Distributional Effects of Urban Spatial Sorting*. NBER Working Papers 26142. National Bureau of Economic Research.

de Chaisemartin, Clément and Xavier D’Haultfoeuille (2022). *Two-way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey*. Tech. rep.

Delgado, Michael S. and Raymond JGM Florax (2015). “Difference-in-Differences Techniques for Spatial Data: Local Autocorrelation and Spatial Interaction”. In: *Economics Letters* 137, pp. 123–126.

Draca, Mirko, Stephen Machin, and John Van Reenen (2011). “Minimum Wages and Firm Profitability”. In: *American Economic Journal: Applied Economics* 3.1, pp. 129–51.

Dube, Arindrajit (2019a). “Impacts of Minimum Wages: Review of the International Evidence”. In: *Independent Report to the UK Government*.

— (2019b). “Minimum Wages and the Distribution of Family Incomes”. In: *American Economic Journal: Applied Economics* 11.4, pp. 268–304.

Dube, Arindrajit, T. William Lester, and Michael Reich (2010). “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties”. In: *Review of Economics and Statistics* 92.4, pp. 945–964.

Dube, Arindrajit and Attila Lindner (2021). “City Limits: What Do Local-Area Minimum Wages Do?” In: *Journal of Economic Perspectives* 35.1, pp. 27–50.

Dustmann, Christian, Attila Lindner, Uta Schönberg, Matthias Umkehrer, and Philipp Vom Berge (2022). “Reallocation Effects of the Minimum Wage”. In: *Quarterly Journal of Economics* 137.1, pp. 267–328.

ESRI (2020). *Shapefile Layer Package USA ZIP Code Areas 2020*. https://www.arcgis.com/home/item.html?id=8d2012a2016e484dafaac0451f9aea24. Accessed: 2021-01-09.

Fernald, M. (2020). “Americas Rental Housing 2020”. In: *Joint Center for Housing Studies of Harvard University, Cambridge, MA*.

Giroud, Xavier and Holger M Mueller (2019). “Firms' internal networks and local economic shocks”. In: *American Economic Review* 109.10, pp. 3617–49.

Hainmueller, Jens (2012). “Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies”. In: *Political Analysis*, pp. 25–46.

Harasztosi, Péter and Attila Lindner (2019). “Who Pays for the Minimum Wage?” In: *American Economic Review* 109.8, pp. 2693–2727.

Hsieh, Chang-Tai and Enrico Moretti (2019). “Housing Constraints and Spatial Misallocation”. In: *American Economic Journal: Macroeconomics* 11.2, pp. 1–39.

Hughes, Samuel (2020). “Housing Demand and Affordability for Low-Wage Households: Evidence from Minimum Wage Changes”. In: *Available at SSRN 3541813*.

Internal Revenue System (2022a). *Identifying Full-time Employees*. https://www.irs.gov/affordable-care-act/employers/identifying-full-time-employees. Accessed: 2022-03-01.

— (2022b). *SOI Tax Stats - Individual Income Tax Statistics - ZIP Code Data (SOI)*. https://www.irs.gov/statistics/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi. Accessed: 2022-02-03.

Internet Archive (2021). *Zillow Research Data, February 2020 snapshot*. https://web.archive.org/web/20200222220950/https://www.zillow.com/research/data/. Accessed: 2022-01-09.

Jardim, Ekaterina, Mark C. Long, Robert Plotnick, Emma van Inwegen, Jacob Vigdor, and Hilary Wething (2022a). “Minimum-Wage Increases and Low-Wage Employment: Evidence from

Seattle”. In: *American Economic Journal: Economic Policy* 14.2, pp. 263–314. URL: https://www.aeaweb.org/articles?id=10.1257/pol.20180578.

Jardim, Ekaterina S, Mark C Long, Robert Plotnick, Emma van Inwegen, Jacob L Vigdor, and Hilary Wething (2022b). *Boundary Discontinuity Methods and Policy Spillovers*. NBER Working Papers 30075. National Bureau of Economic Research.

Kline, Patrick and Enrico Moretti (2014). “People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs”. In: *Annual Review of Economics* 6.1, pp. 629–662.

Kuehn, Daniel (2016). “Spillover Bias in Cross-Border Minimum Wage Studies: Evidence From a Gravity Model”. In: *Journal of Labor Research* 37.4, pp. 441–459.

Lee, David S. (1999). “Wage Inequality in the United States During the 1980s: Rising Dispersion or Falling Minimum Wage?” In: *Quarterly Journal of Economics* 114.3, pp. 977–1023.

Leung, Justin H. (2021). “Minimum Wage and Real Wage Inequality: Evidence From Pass-Through to Retail Prices”. In: *Review of Economics and Statistics* 103.4, pp. 754–769.

MaCurdy, Thomas (2015). “How Effective Is the Minimum Wage at Supporting the Poor?” In: *Journal of Political Economy* 123.2, pp. 497–545.

Meer, Jonathan and Jeremy West (2016). “Effects of the Minimum Wage on Employment Dynamics”. In: *Journal of Human Resources* 51.2, pp. 500–522.

Miyauchi, Yuhei, Kentaro Nakajima, and Stephen J. Redding (2021). *Consumption Access and Agglomeration: Evidence from Smartphone Data*. NBER Working Papers 28497. National Bureau of Economic Research.

Monras, Joan (2019). “Minimum Wages and Spatial Equilibrium: Theory and Evidence”. In: *Journal of Labor Economics* 37.3, pp. 853–904.

Neumark, David and Peter Shirley (2021). *Myth or Measurement: What Does the New Minimum Wage Research Say About Minimum Wages and Job Loss in the United States?* NBER Working Papers 28388. National Bureau of Economic Research.

Pérez Pérez, Jorge (2021). “City Minimum Wages and Spatial Equilibrium Effects”. In: *SocArXiv preprint*. URL: https://osf.io/preprints/socarxiv/fpx9e/.

Renkin, Tobias, Claire Montialoux, and Michael Siegenthaler (2020). “The Pass-Through of Minimum Wages Into US Retail Prices: Evidence From Supermarket Scanner Data”. In: *Review of Economics and Statistics*, pp. 1–99.

Roth, Jonathan, Pedro HC Sant’Anna, Alyssa Bilinski, and John Poe (2022). “What’s Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature”. In: *arXiv preprint arXiv:2201.01194*.

Tidemann, Krieg (2018). “Minimum Wages, Spatial Equilibrium, and Housing Rents”. In: *Job Market Paper*.

UC Berkeley Labor Center (2020). *Inventory of US City and County Minimum Wage Ordinances*. https://laborcenter.berkeley.edu/inventory-of-us-city-and-county-minimum-wage-ordinances/. Accessed: 2020-04-05.

US Bureau of Labor Statistics (2020a). *Characteristics of minimum wage workers, 2019*. https://www.bls.gov/opub/reports/minimum-wage/2019/home.htm. Accessed: 2020-11-01.

US Bureau of Labor Statistics (2020b). *Quarterly Census of Employment and Wages*. https://www.bls.gov/cew/downloadable-data-files.htm. Accessed: 2020-05-10.

US Census Bureau (2012). *Special Release - Census Blocks with Population and Housing Counts*. https://www.census.gov/geographies/mapping-files/2010/geo/tiger-line-file.html. Accessed: 2022-02-10.

— (2021). *LEHD Origin-Destination Employment Statistics Data (2009-2018) [version 7]*. US Census Bureau, Longitudinal-Employer Household Dynamics Program. https://lehd.ces.census.gov/data/. Accessed: 2021-10-03.

— (2022a). *American Community Survey 5-year Data 2010-2014*. https://www.census.gov/data/developers/data-sets/acs-5year.html. Accessed: 2022-04-01.

— (2022b). *Decennial Census Data 2010*. https://www.census.gov/data/developers/data-sets/decennial-census.html. Accessed: 2022-03-01.

US Department of Housing and Urban Development (2017). *Federal Register*. https://www.huduser.gov/portal/datasets/fmr/fmr2018/FY2018-FMR-Preamble.pdf.

— (2018). *Small Area Fair Market Rent Demonstration Evaluation*. https://www.huduser.gov/portal/sites/default/files/pdf/SAFMR-Evaluation-Final-Report.pdf. Accessed: 2022-06-06.

— (2020a). *American Housing Survey 2011 and 2013*. https://www.census.gov/programs-surveys/ahs.html. Accessed: 2022-06-01.

— (2020b). *Small Area Fair Market Rents*. https://www.huduser.gov/portal/datasets/fmr.html. Accessed: 2020-03-10.

— (2022a). *Assisted Housing: National and Local - ZIP code level data based on Census 2010 geographies*. https://www.huduser.gov/portal/datasets/assthsg.html. Accessed: 2022-02-10.

— (2022b). *USPS ZIP code crosswalk files*. https://www.huduser.gov/portal/datasets/usps_crosswalk.html. Accessed: 2022-02-10.

Vaghul, Kavya and Ben Zipperer (2016). "Historical State and Sub-State Minimum Wage Data". In: *Washington Center for Equitable Growth Working Paper* 90716.

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

Yamagishi, Atsushi (2019). *Minimum Wages and Housing Rents: Theory and Evidence from Two Countries*. Tech. rep. Accessed at https://mpra.ub.uni-muenchen.de/94238/1/MPRA_paper_94238.pdf.

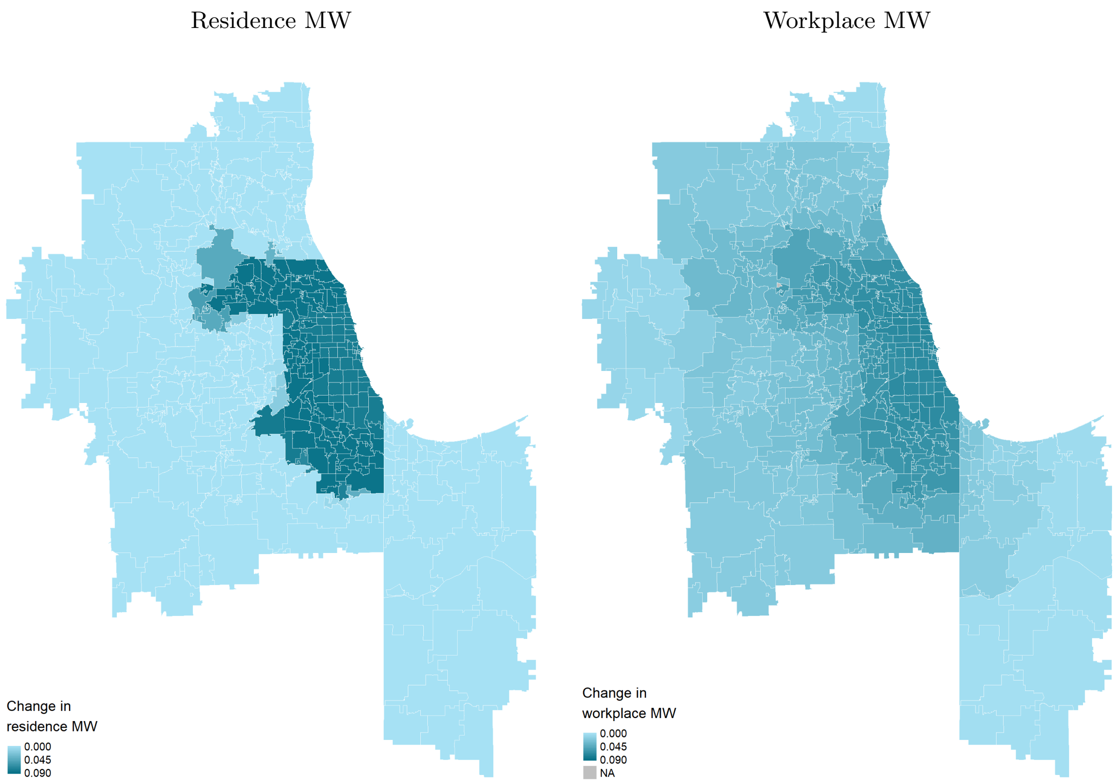
— (2021). "Minimum Wages and Housing Rents: Theory and Evidence". In: *Regional Science and Urban Economics* 87, p. 103649.

Zillow (2020a). *Zillow Facts and Figures*. https://www.zillowgroup.com/facts-figures/. Accessed: 2020-10-23.

— (2020b). *Zillow Research Data*. https://www.zillow.com/research/data/. Accessed: 2020-02-15.

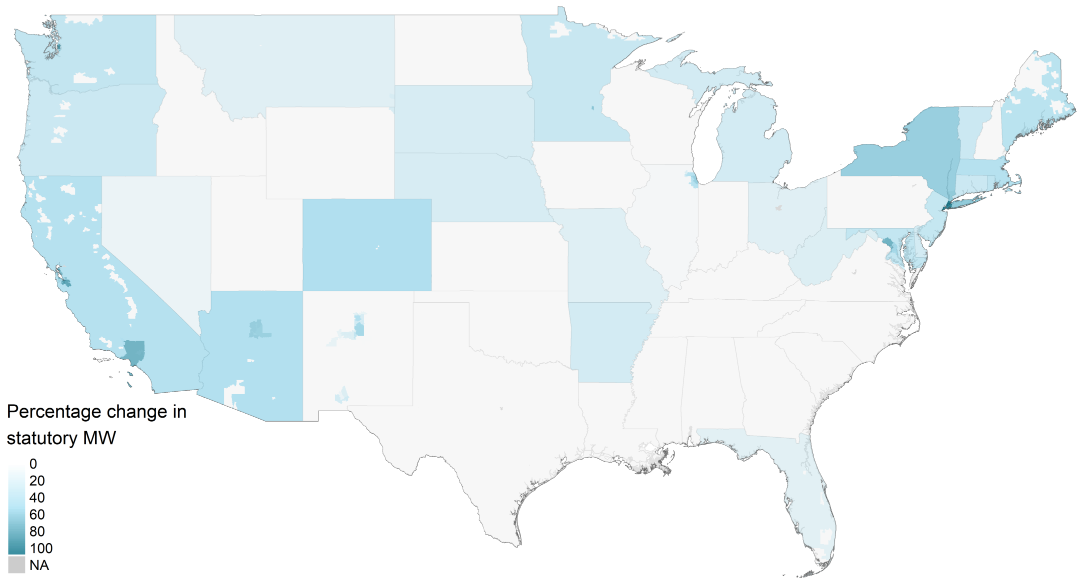
Figures and Tables

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



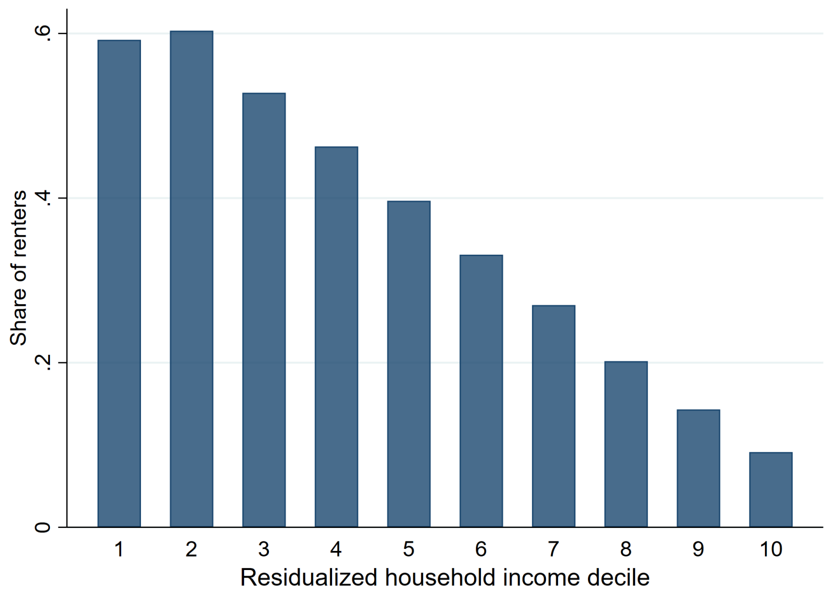
Notes: Data are from the MW panel described in Section 3.1 and from LODES. The figures show changes in the MW measures in July 2019 in the metropolitan area of Chicago. The figure on the left shows the change in the residence MW. The figure on the right shows the change in the workplace MW. The residence 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.

Figure 2: Spatial distribution of minimum wage changes, 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 December 2019.

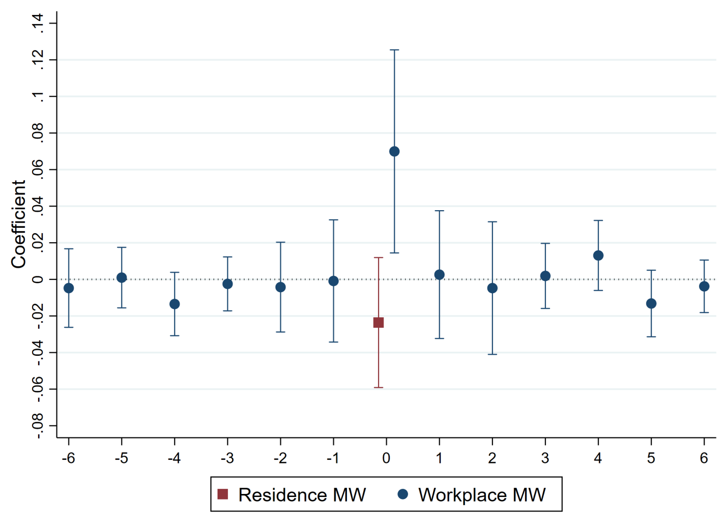
Figure 3: Probability of being a renter by household income decile, full sample



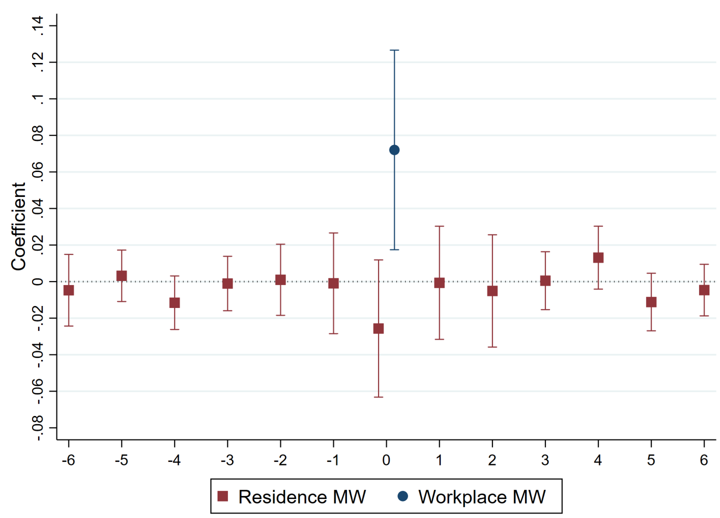
Notes: Data are from the 2011 and 2013 American Housing Surveys. The figure shows the probability of a household living in a rented unit by household income. We construct the figure as follows. First, we residualize an indicator for being a renter and household income by SMSA indicators, the closest 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: Leads and lags of the workplace MW



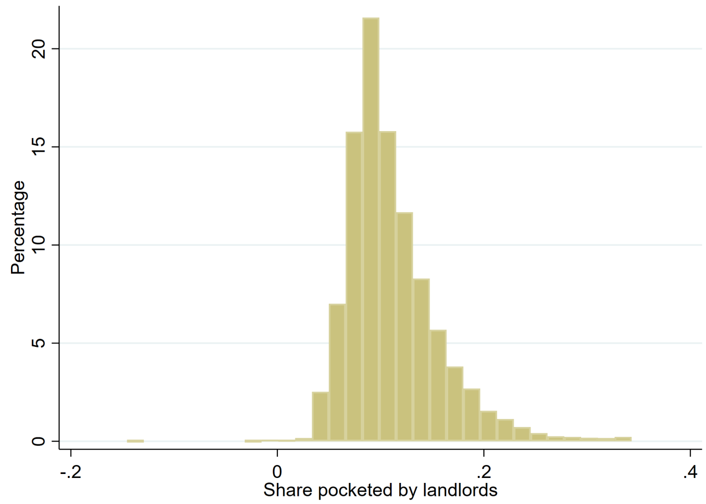
Panel B: Leads and lags of the residence MW



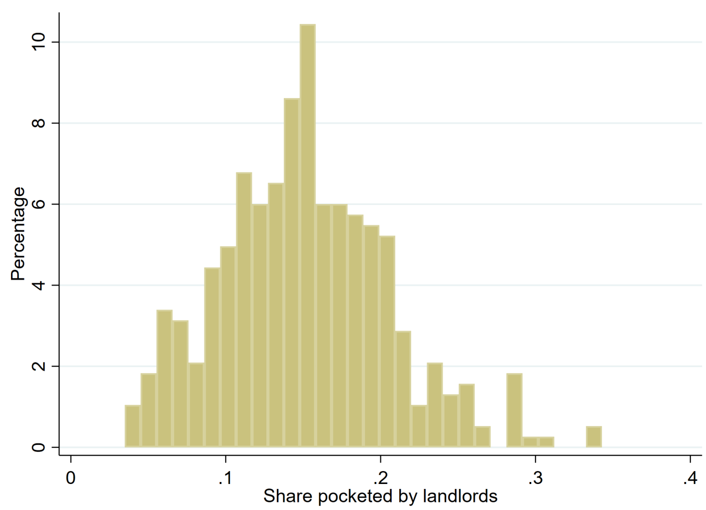
Notes: Data are from the baseline estimation sample described in Section 3.3.4. All panels plot coefficients from regressions of the log of rents per square foot on the residence and workplace MW measures, varying the number of leads and lags of each MW variable included. Panel A includes six leads and lags of the workplace MW measure. Panel B includes six leads and lags of the residence MW measure. 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.

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

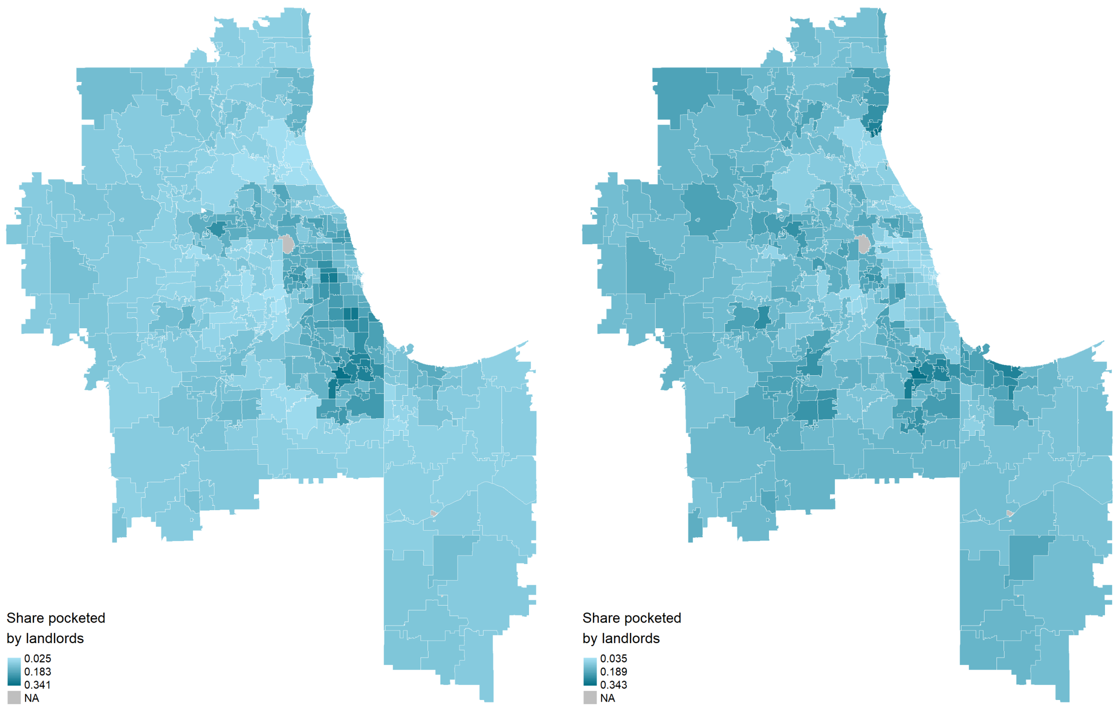


Notes: Data are from the MW panel described in section 3.1 and from LODES. The figures show the distribution of the estimated ZIP-code specific shares of additional income pocketed by landlords ("share pocketed") under different counterfactual policies. Panel A is based on a counterfactual increase to $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 $\varepsilon = 0.1013$.

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

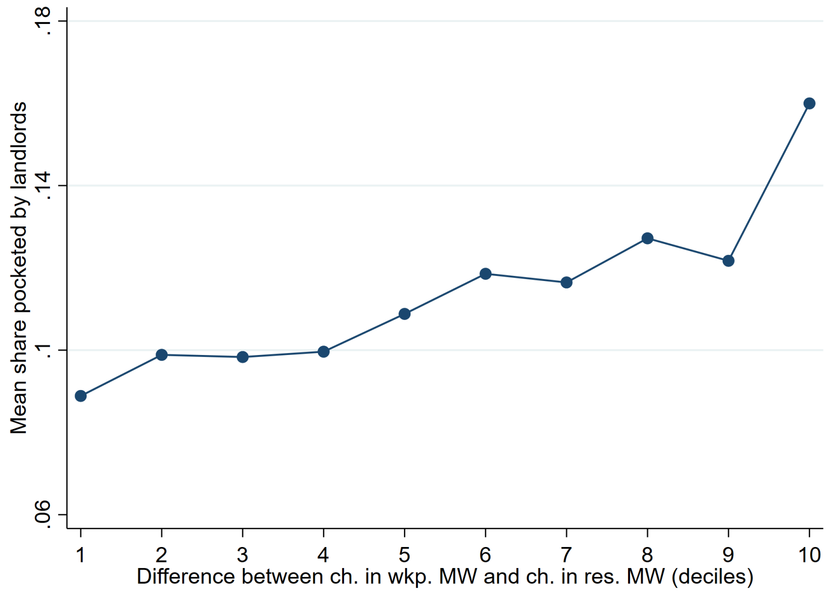
Panel A: Increase in federal MW from \$7.25 to \$9

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 $\varepsilon = 0.1013$.

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 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 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: Estimates of the effect of the minimum wage on rents, different samples

	Change log rents Δr_{it}			
	Baseline (1)	Baseline Reweighted (2)	Unbalanced (3)	Unbalanced Reweighted (4)
Change residence MW $\Delta \underline{w}_{it}^{\text{res}}$	-0.0219	-0.0289	-0.0274	-0.0300
	(0.0175)	(0.0303)	(0.0237)	(0.0232)
Change workplace MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0685	0.0895	0.0528	0.0565
	(0.0288)	(0.0385)	(0.0299)	(0.0321)
P-value equality	0.0514	0.0835	0.1325	0.1131
R-squared	0.0213	0.0171	0.0309	0.0338
Observations	80,241	79,701	193,239	192,124

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) and (4) use the unbalanced sample of all ZIP codes with Zillow rents data at any point in time, and controls for year-quarter of entry to the panel by year-month fixed effects. Even numbered columns re-weight observations so that the sample of ZIP codes in the data matches the averages of the set of ZIP codes located in urban CBSAs in the following variables: share of renter-occupied households (US Census Bureau 2022b), and share of workers and share of residents that earn less than \$1251 in the 2014 LODES (US Census Bureau 2021). Weights for each sample are computed following Hainmueller (2012). We define urban CBSAs as in the second column of Table 1. Standard errors in parentheses are clustered at the state level.

Table 5: 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 $\Delta \underline{w}_{it}^{\text{res}}$	-0.0199	-0.0449	-0.0357	-0.0174
	(0.0195)	(0.0204)	(0.0263)	(0.0175)
Change res. MW $\times$ Std. share of MW workers		-0.0789		
		(0.0404)		
Change res. MW $\times$ Std. median household income			0.0542	
			(0.0282)	
Change res. MW $\times$ Std. share of public housing				-0.0299
				(0.0336)
Change wkp. MW $\Delta \underline{w}_{it}^{\text{wkp}}$	0.0687	0.0950	0.0871	0.0678
	(0.0298)	(0.0307)	(0.0352)	(0.0276)
Change wkp. MW $\times$ Std. share of MW workers		0.0820		
		(0.0461)		
Change wkp. MW $\times$ Std. median household income			-0.0671	
			(0.0350)	
Change wkp. MW $\times$ Std. share of public housing				0.0263
				(0.0377)
Economic controls	Yes	Yes	Yes	Yes
R-squared	0.0216	0.0214	0.0214	0.0216
Observations	78,912	74,082	75,919	78,617

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 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 6: 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	0.096
previous MW $>$ \$9	1,043	0.000	0.013	0.232	0.157
Total incidence	6,784				0.092

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	0.091
previous MW $<$ \$13	323	0.000	0.009	0.231	0.156
Total incidence	385				0.110

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 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 $\varepsilon = 0.1013$. 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.

Appendix

A A Dynamic Supply and Demand Model

The geography is represented by a set of ZIP codes $\mathcal{Z}$. There is an exogenously given distribution of 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.[42] 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 $V_{it}(R_{it}, \lambda_t)$

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

the supply of housing, increasing in R_{it}. A feasibility constraint is that

$$V_{it}(R_{it}, \lambda_t) \leq S_i - \tilde{H}_{i,t-1}. \tag{10}$$

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

$$\lambda_t \sum_z L_{iz} h_{iz} \left(R_{it}, \underline{W}_{it}, \underline{W}_{zt}\right) = V_{it}(R_{it}, \lambda_t). \tag{11}$$

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 (10), equation (11) 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 (11) 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.

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).[43] 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 Appendix B.1, we assign a ZIP code to each block. Finally, we define *the statutory MW* at ZIP code i and month t, $\underline{W}_{it}$, as the weighted average of the statutory MW levels in its constituent blocks, where the weights are given by the number of housing units.[44] For ZIP codes that have no housing units in them, such as those corresponding to universities or airports, we use a simple average instead.

[43] 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).

[44] 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 $\underline{YW}$.[45] 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.[46] 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 same approach to use the workplace MW 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 start by collecting the following variables. 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.[47] 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.

[45] 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.

[46] 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 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.

[47] 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.

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

$$r_{it} = r_{it}(\underline{w}_{it}^{\text{res}}, \underline{w}_{it}^{\text{wkp}}). \tag{12}$$

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*

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

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

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

Assumption 3 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 4 (No selection on gains)**.** *We assume that*

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

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 3 (Identification)**.** *Under Assumption 3 we have that*

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

Furthermore, if Assumption 4 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 3 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 4 imposes that those selection bias terms are zero.[48] We discuss the plausibility of these assumptions in Section 4.

Consider now a functional form for (12) 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 3 and 4 hold under this linear functional form with constant effects. Furthermore, in

[48]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 The Effect of the Minimum Wage on Income Across Space

Our main data source to estimate the effect of the MW on wage income are the yearly ZIP code aggregates from the Internal Revenue System (2022b). Thus, we construct a ZIP code by month panel where we collect, for each ZIP code and year between 2010 and 2019, IRS income aggregates, the yearly average of our MW measures, and the yearly average of our QCEW variables. See Section 3.3.4 for details on the construction of these data.

We estimate versions of

$$y_{it} = \gamma_i + \psi_t + \varepsilon \overline{\underline{w}^{\text{wkp}}}_{it} + \overline{\mathbf{X}}_{it}^{'} \eta + \nu_{it}, \tag{13}$$

where $y_{it} = \log Y_{it}$ is the log of total wages at ZIP code i in year t, γ_i and ψ_t are ZIP code and year fixed effects, $\overline{\underline{w}^{\text{wkp}}}_{it}$ is the yearly average of the workplace MW, $\overline{\mathbf{X}}_{it}^{'}$ represents the yearly average of economic controls, and ν_{it} is an error term. Sometimes we interact the year fixed effects with indicators for different geographies. We use a model in levels because it is not feasible to take monthly first differences with yearly data. A yearly model estimated with monthly averaged variables is identified under the same assumptions as the corresponding monthly model.

Appendix Table 7 shows estimates of ε for different specifications of the model given in (13). Columns (1) through (3) estimate the effect of the workplace MW on log total wages under different specifications. The point estimates of ε fluctuate between 0.0909 and 0.1275. Our preferred specification in column (3), which includes economic controls and CBSA by year fixed effects, suggests that a 10 percent increase in the workplace MW generates a 1.01 percent increase in total wages. These estimates are consistent with those in Cengiz et al. (2019). Cengiz et al. (2019, Table I) estimates that a MW event increases wages by 6.8 percent, and in their data the average MW event represents an increase in the statutory MW of 10.1 percent. For illustration, assume that 15 percent of workers in a location earn the minimum wage. Then, Cengiz et al.'s (2019) estimates imply that a 10 percent increase in the MW will increase total wages by $(6.8/10.1) \times 10 \times 0.15 \approx 1.01$ percent.

Column (4) of Appendix Table 7 replicates column (3) but interacts the workplace MW measure with the standarized share of MW workers estimated as explained in Appendix B.2. As expected, we find that a higher share of MW workers makes the effect of workplace MW increases larger. Column (5) of Appendix Table 7 shows, as a falsification test, estimates of the same model as in column (3) but using the log of total dividends as dependent variable. We obtain a positive but much lower effect that is statistically indistinguishable from zero, suggesting that dividends do not respond to MW changes as wages do.

E Additional Tables and Figures

Appendix Table 1: Summary statistics of baseline panel

	N	Mean	St. Dev.	Min	Max
Minimum wage variables					
Statutory MW $\underline{W}_{it}$	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.

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

	Log rents	
	Levels	First Differences
	(1)	(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.

Appendix Table 3: Comparison of estimates of the effect of the minimum wage on rents across geographies and time frames

	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
Panel A: Baseline (ZIP code by Month)					
(i) Residence MW only		0.0393			78,912
		(0.0150)			
(ii) Workplace MW only			0.0473		78,912
			(0.0161)		
(iii) Both residence and workplace MW	0.8617	-0.0199	0.0687	0.0488	78,912
	(0.0382)	(0.0195)	(0.0298)	(0.0162)	
Panel B: County by Month					
(i) Residence MW only		0.0057			27,267
		(0.0188)			
(ii) Workplace MW only			0.0102		27,267
			(0.0219)		
(iii) Both residence and workplace MW	0.8768	-0.0509	0.0646	0.0137	27,267
	(0.0199)	(0.0387)	(0.0506)	(0.0227)	
Panel C: ZIP code by Year					
(i) Residence MW only		0.0072			6,696
		(0.0637)			
(ii) Workplace MW only			0.0092		6,696
			(0.0701)		
(iii) Both residence and workplace MW	0.8993	-0.0205	0.0308	0.0103	6,696
	(0.0263)	(0.0891)	(0.1139)	(0.0698)	

Notes: Data are from the baseline estimation samples described in Section 3.3.4. The first column and rows labeled (iii) show the results of a regression of the change in the workplace MW measure on the change in the residence MW measure. The second through fourth columns show the results of regressions of the change in log rents on either the change in the residence MW—rows (i)— or the workplace MW—rows (ii)— or both—rows (iii)—, with the fifth column showing the sum of the coefficients on the MW measures. The last column shows the number of observations, fixed within each row. All regressions include economic controls from the QCEW, as defined in Table 2. Regressions estimated at a yearly frequency use the yearly average of the change in the MW measures and the change in the economic controls. Panel A repeats our baseline results from Table 2, where the unit of observation is the ZIP code by month. Panel B shows results for a panel where the unit of observation is the county by month. Panel C shows results for a panel where the unit of observation is the ZIP code by year. In all panels, (i) displays the results of a regression of the change in log rents on the residence MW only, (ii) displays the results of a regression of the change in log rents on the workplace MW only, and (iii) displays the results of a regression of the change in workplace MW on the change in residence MW (column 1), and of the change in log rents on both MW measures (columns 2–5). Standard errors in parentheses are clustered at the state level.

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.

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.

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 4, 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.

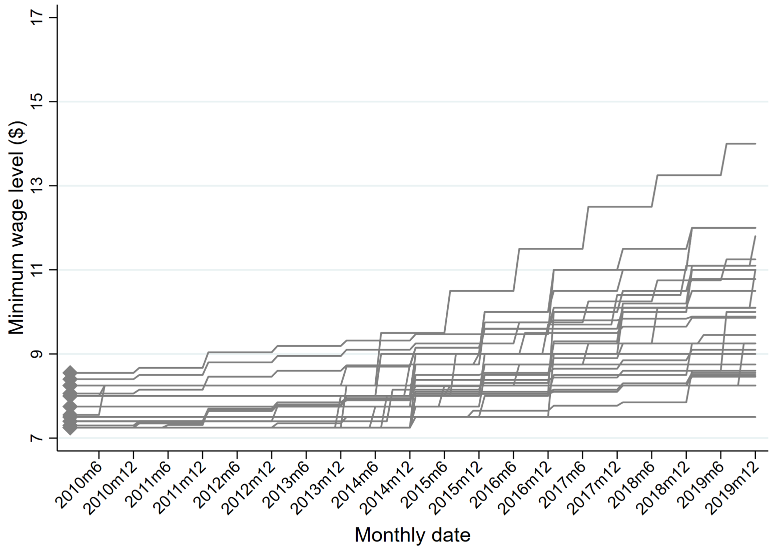
Appendix Table 7: Estimates of the effect of the minimum wage on income, full sample

	Log wage income				Log div.
	(1)	(2)	(3)	(4)	(5)
Wkp. MW	0.1275	0.0909	0.1013	0.1013	0.0169
	(0.0522)	(0.0336)	(0.0274)	(0.0272)	(0.0653)
Wkp. MW × Std. sh. of MW workers				0.0216	
				(0.0090)	
Economic controls	No	Yes	Yes	Yes	Yes
CBSA × year FE	No	No	Yes	Yes	Yes
Within R-squared	0.0158	0.0953	0.0165	0.0170	0.0016
Observations	163,417	146,824	146,759	143,752	138,286

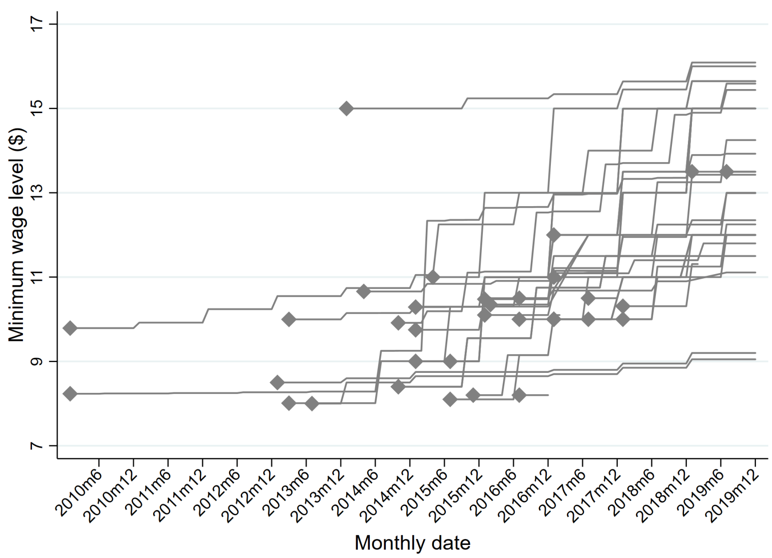
Notes: Income data are from the IRS, commuting data are from LODES, and MW data are from the panel described in Section 3.1. The table shows different estimations of the effect of the workplace MW on income measures using a regression model that includes ZIP code and year fixed effects. The sample includes all ZIP codes with valid income data for the years 2014–2019. The workplace MW and the economic controls are defined as the yearly average of the respective variables used in our baseline estimates of Section 5.1. Columns (1) through (3) show estimates of a regression of log total wage income on the workplace MW and ZIP code and year fixed effects. Column (2) adds time-varying economic controls from the QCEW. Column (3) interacts the year fixed effects with indicators for each Core-Based Statistical Area (CBSA). Column (4) interacts the workplace MW with the standardized share of MW workers ("Std. sh. of MW workers") discussed in Section 3.2. Column (5) repeats the estimation in column (3) but using the log of total dividends ("Log div.") as dependent variable. Standard errors in parentheses are clustered at the state level.

Appendix Figure 1: Minimum wage levels in the US by jurisdiction, 2010–2019

Panel A: State policies

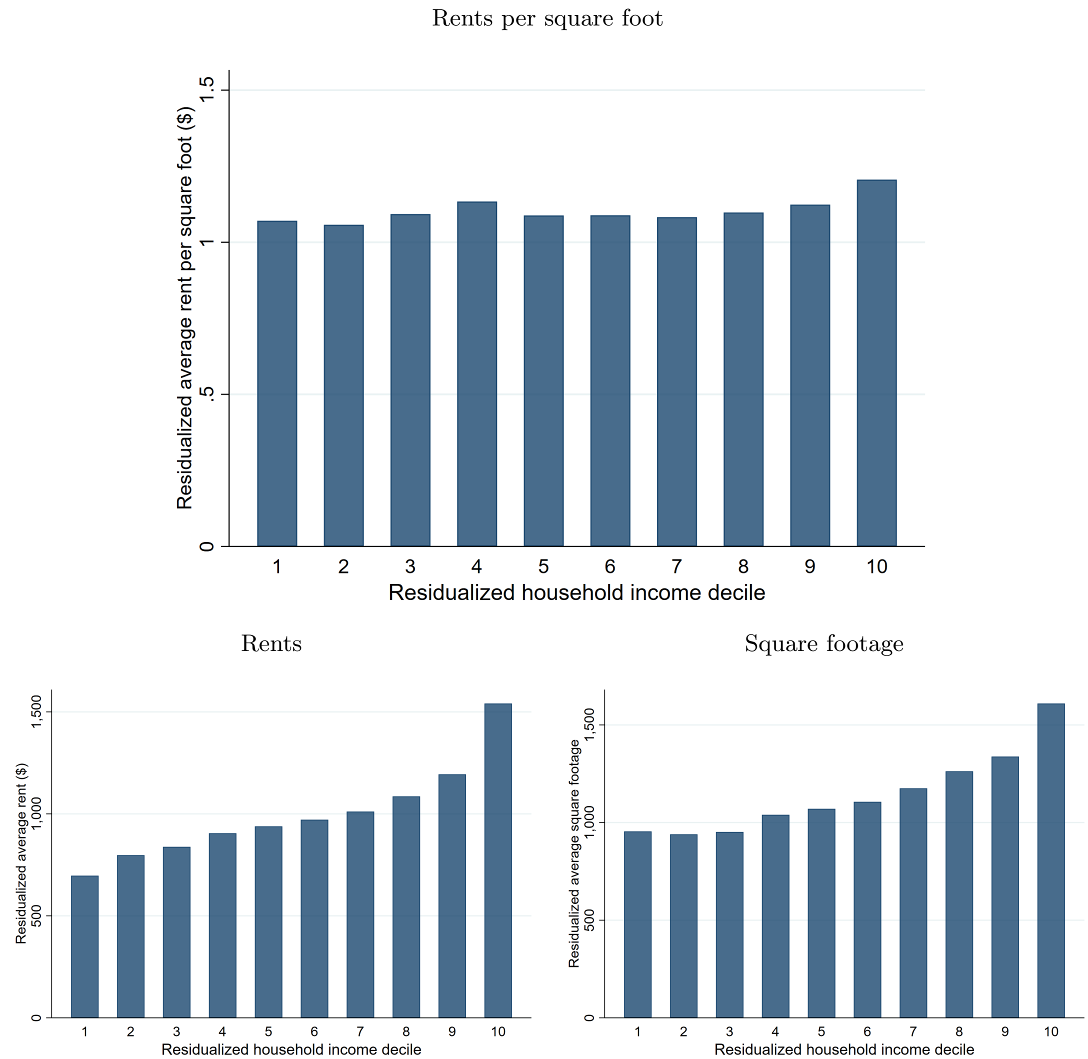


Panel B: Sub-state policies



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 December 2019. 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.

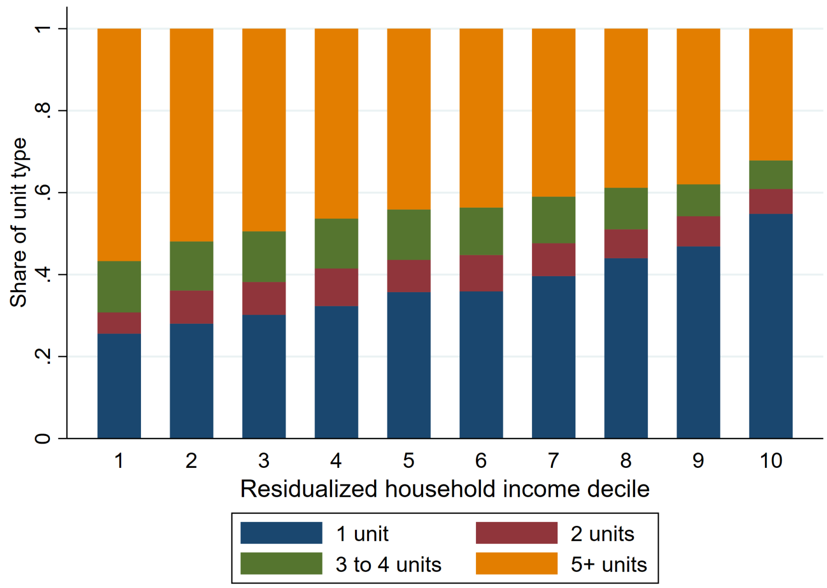
Appendix Figure 2: Average rent, square footage, and rent per square foot by household income decile, renters sample



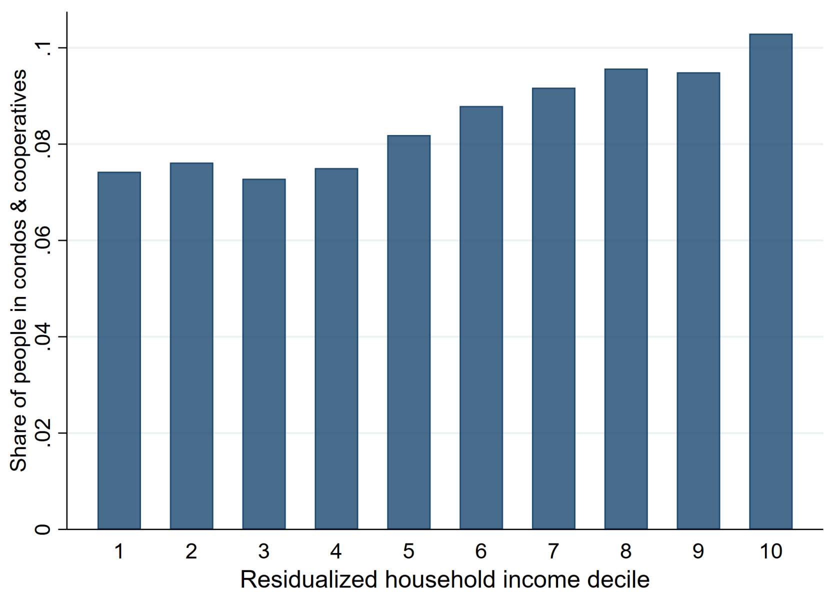
Notes: Data are from the 2011 and 2013 American Housing Surveys. The top figure shows average rents per square foot by household income. The bottom right figure shows average rents by household income. The bottom right figure shows average square feet by household income. The variable rent per square 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.

Appendix Figure 3: Properties of building where household unit is located by household income decile, full sample

Distribution of the number of units in building

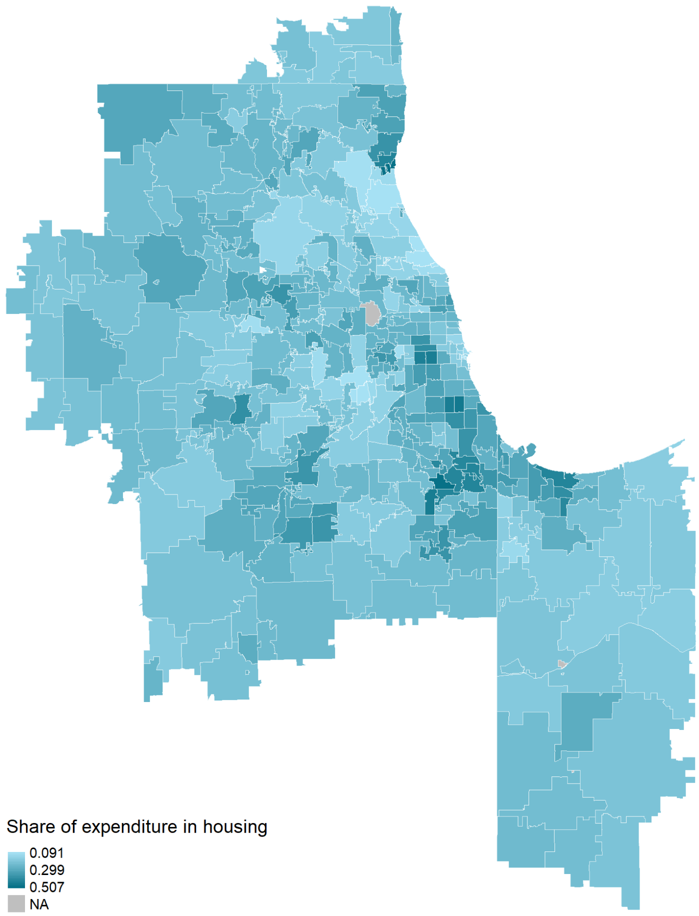


Probability building is a condominium or cooperative housing



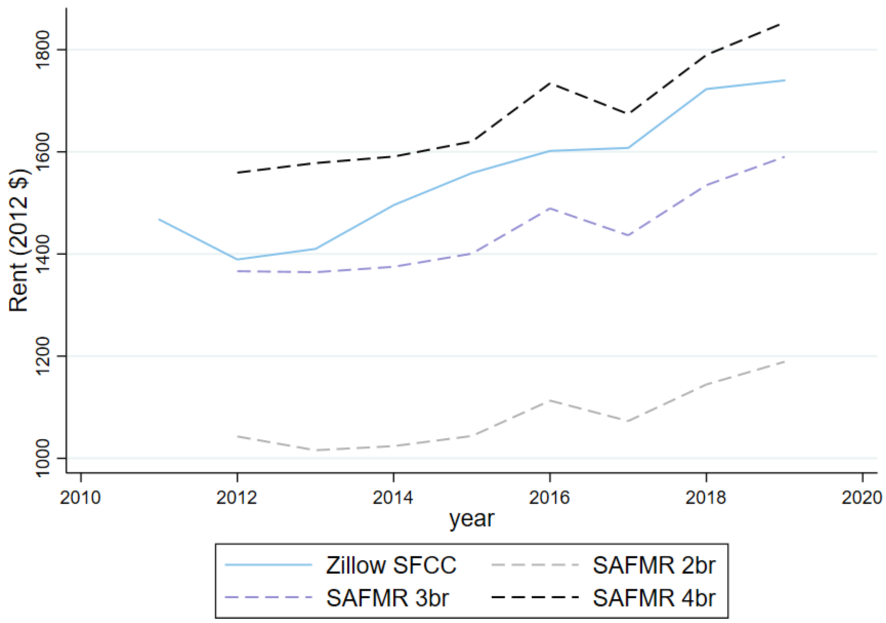
Notes: Data are from the 2011 and 2013 American Housing Surveys. The top figure shows the number of housing units in the building where the household is located, and the bottom figure shows the share of housing units located in condominiums or cooperative housing, both by household income. We construct 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.

Appendix Figure 4: 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 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.

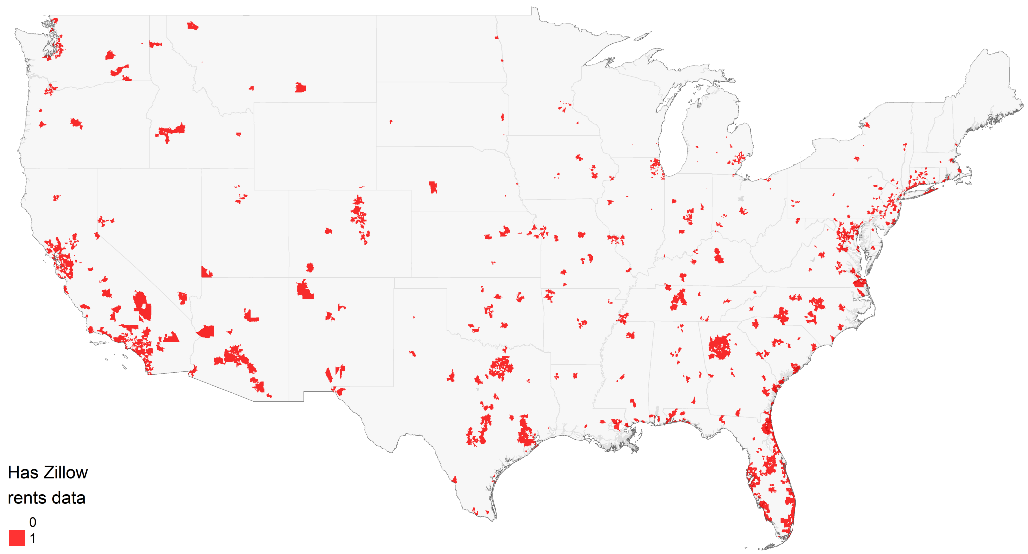
Appendix Figure 5: Time trends in rents according to Zillow and SAFMR



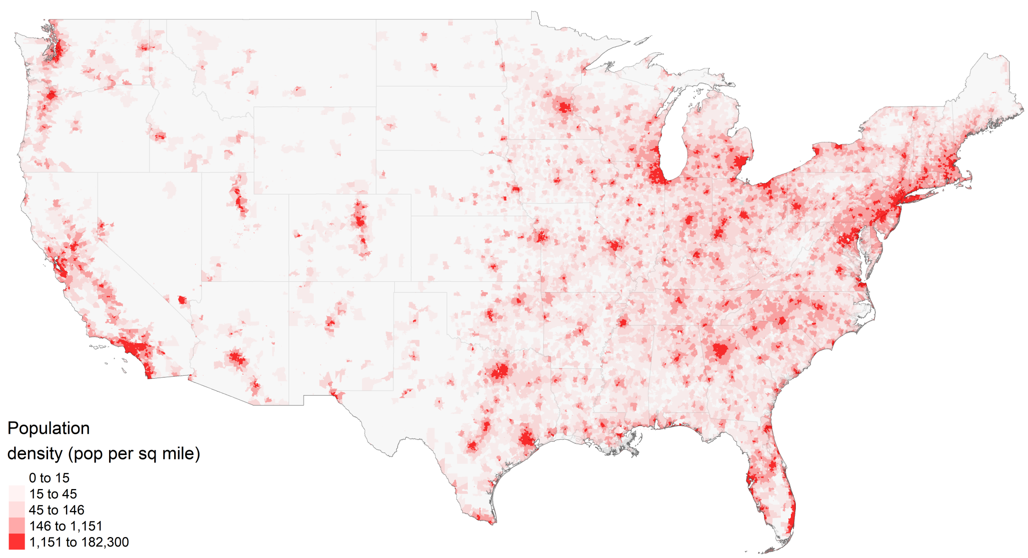
Notes: Data are from Zillow (2020b) and Small Area Fair Market Rents (2020). The figure compares the evolution of the median rental value in Zillow to three SAFMRs series, for 2, 3, and 4 or more bedrooms. SAFMR data generally corresponds to the 40th percentile of the distribution of paid rents in a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

Appendix Figure 6: Sample of ZIP codes in Zillow data and population density, mainland US

Zillow ZIP codes

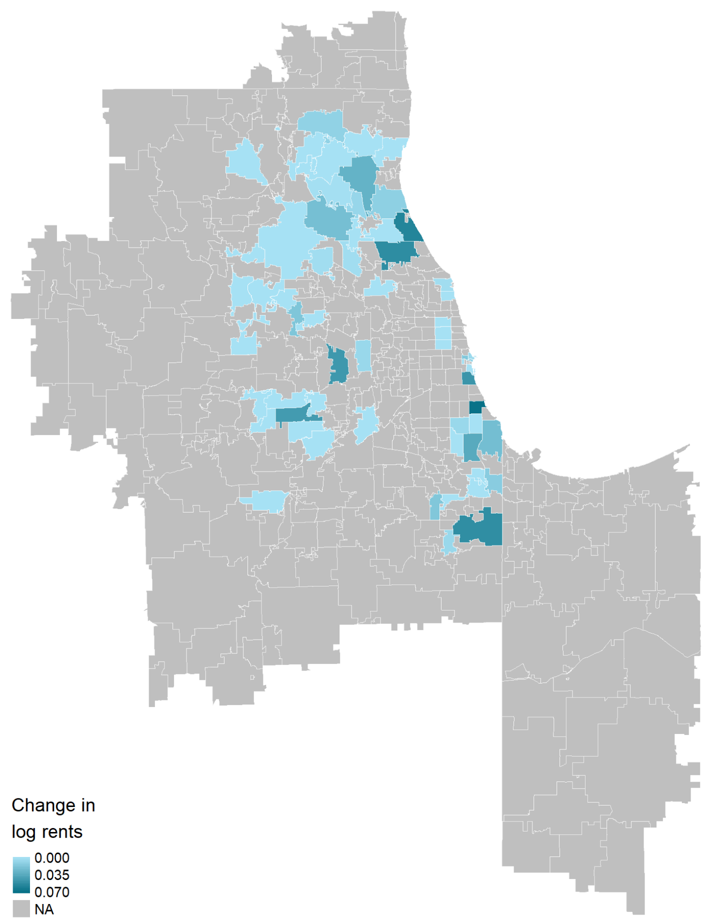


Population Density



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.

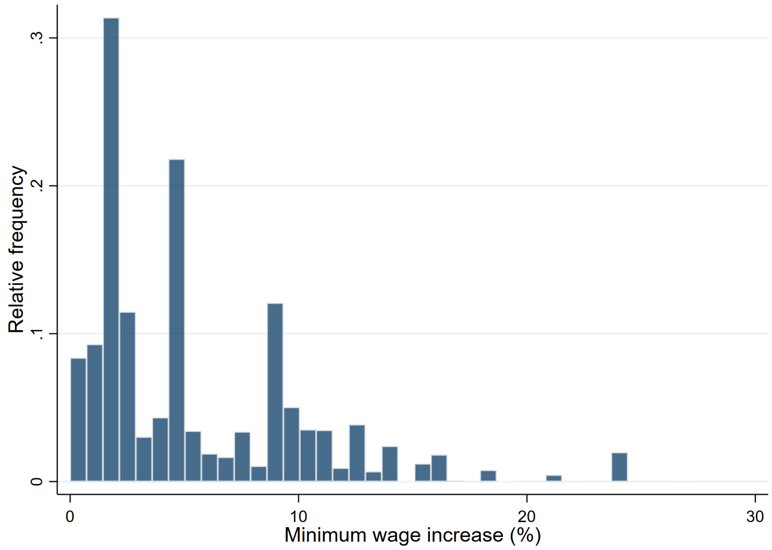
Appendix Figure 7: Changes in log rents in the Chicago-Naperville-Elgin CBSA, July 2019



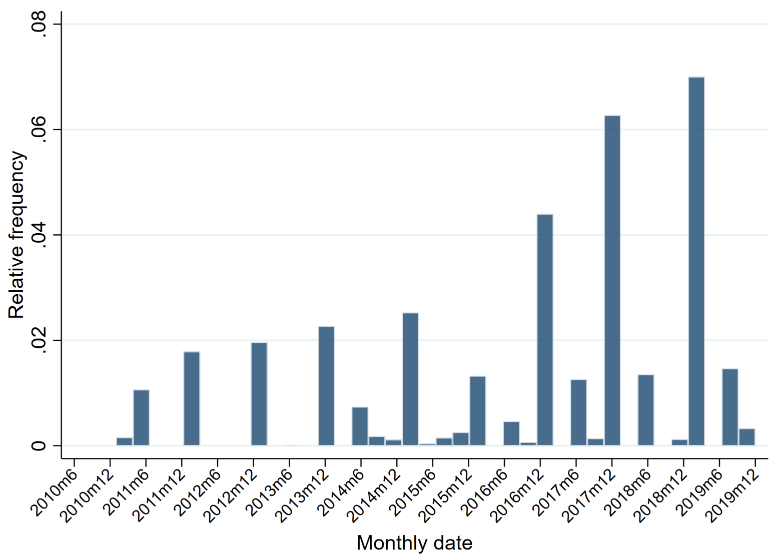
Notes: Data are from Zillow (2020b). The figure shows the change in the log of median rents per square foot in the SFCC category in the month of June 2019 in ZIP codes located in the Chicago-Naperville-Elgin CBSA.

Appendix Figure 8: Distribution of statutory minimum wage changes, Zillow sample

Intensity



Timing

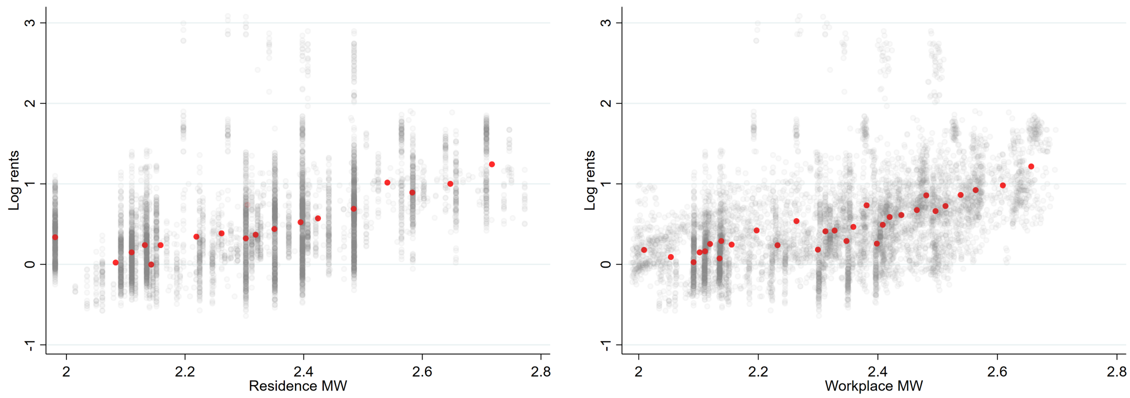


Notes: Data are from the MW panel described in Section 3.1. The histograms show the distribution of positive MW changes in the sample of ZIP codes available in the Zillow data. We exclude a few negative changes for expository purposes. The top figure ("Intensity") reports the intensity of the changes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

Appendix Figure 9: Relationship between log rents and the minimum wage measures, sample of affected ZIP code-months

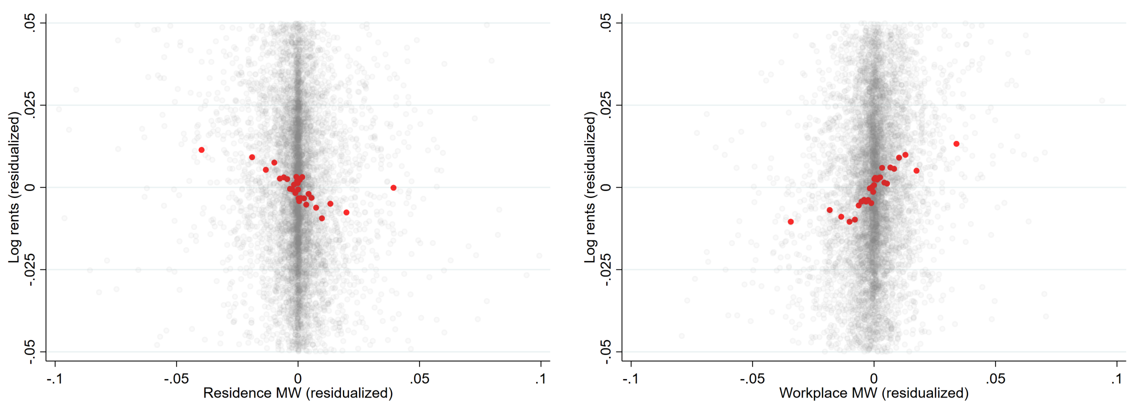
Panel A: Raw data

Residence MW | Workplace MW



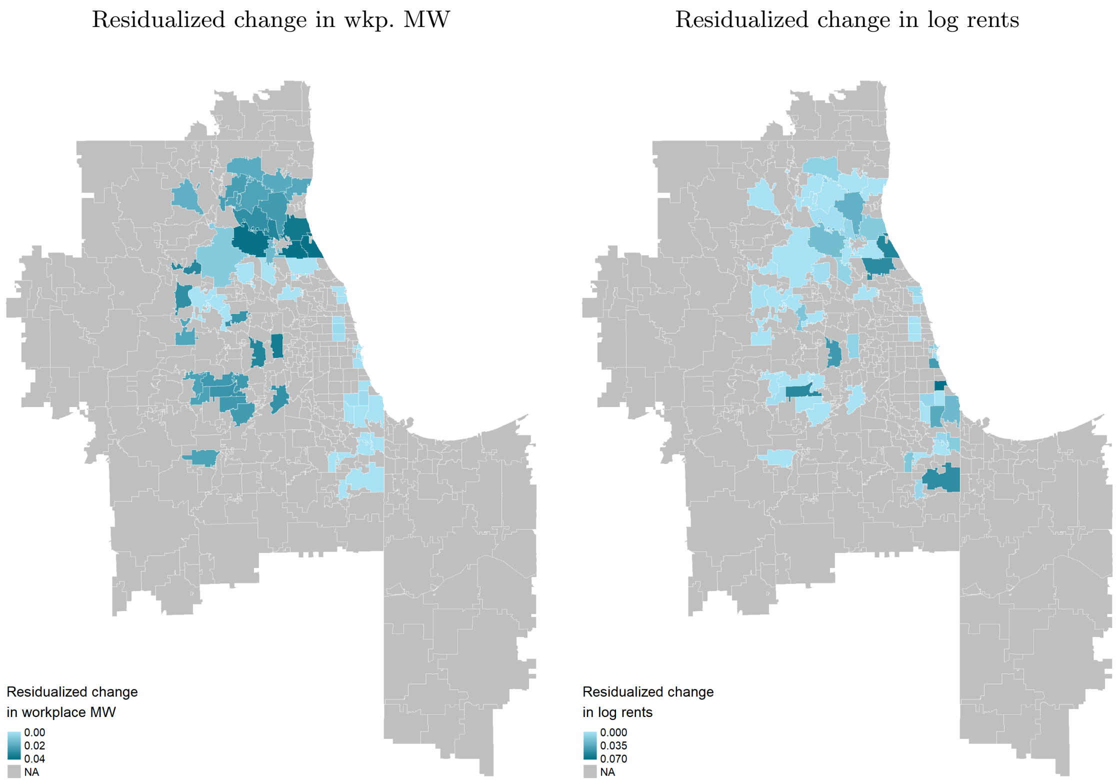
Panel B: Conditional on ZIP code FE and the other MW measure

Residence MW | Workplace MW



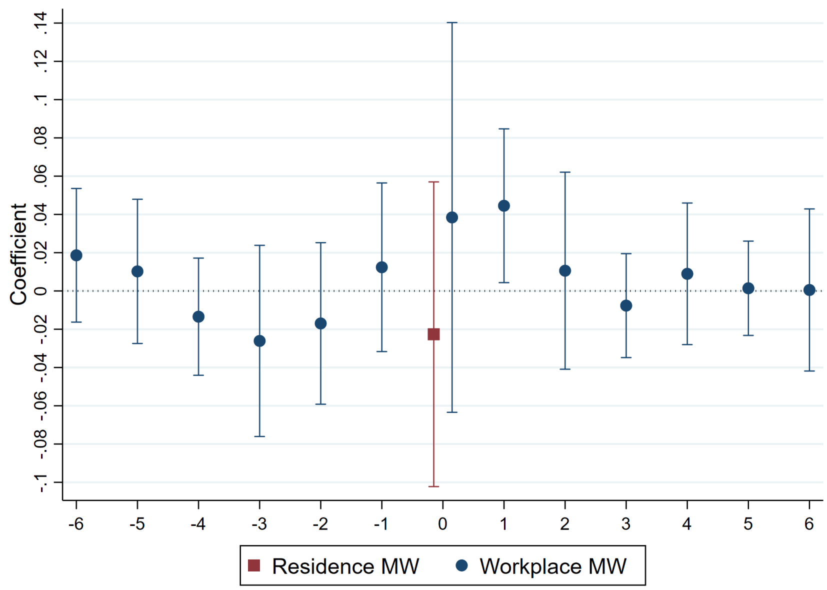
Notes: Data are from Zillow and LODES. The plot shows the unconditional and conditional relationship between log rents and the MW measures. The sample is composed of ZIP code-month observations located in CBSAs where there was some statutory MW increase in the month of interest. The rents variable correspond 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.

Appendix Figure 10: Residualized changes in the workplace minimum wage and log rents, Chicago-Naperville-Elgin CBSA on July 2019



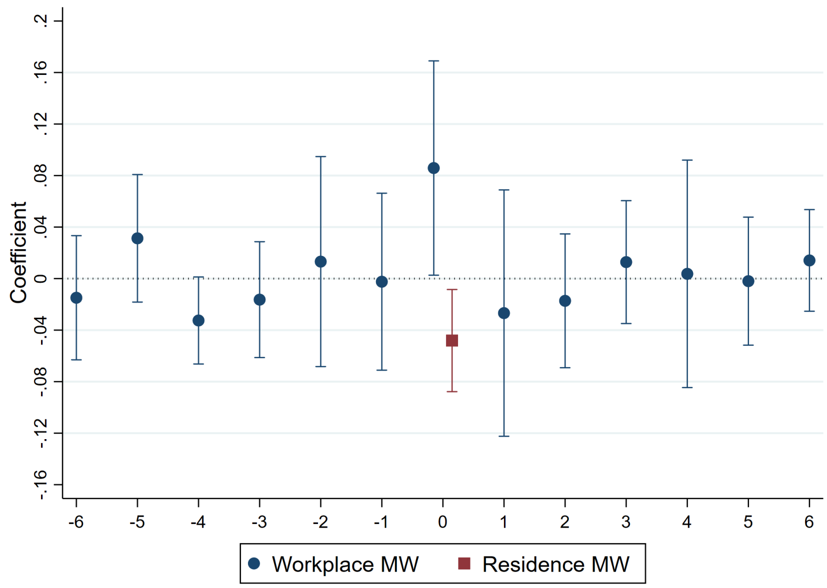
Notes: Data are from the unbalanced estimation panel described in Section 3.3.4. The left figure maps the residuals of a regression of the change in the workplace MW measure on the change in the residence MW measure, including economic controls and year-month fixed effects. The right figure maps the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

Appendix Figure 11: Estimates of the effect of the minimum wage on rents, county by month data



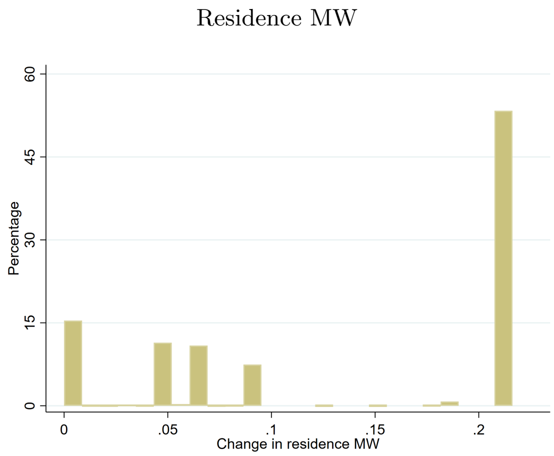
Notes: Data are from the county-by-month panel described in Section 3.3.4. We plot coefficients from regressions of the log of rents per square foot on the residence MW and workplace MW, including six leads and lags of the workplace MW measure. All regressions are estimated in first differences and include 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.

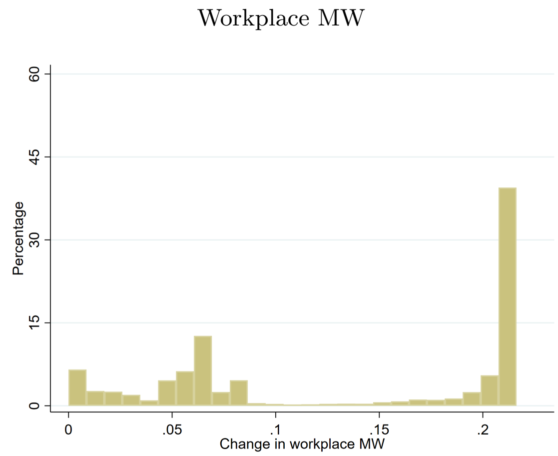
Appendix Figure 12: Estimates of the effect of the minimum wage on rents, stacked sample including leads and lags



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 4 using a "stacked" sample. We construct the sample as explained in Appendix Table 4. 95% pointwise confidence intervals are obtained from standard errors clustered at the state level.

Appendix Figure 13: Distribution of changes in minimum wage measures under a counterfactual federal minimum wage of $9, urban ZIP codes



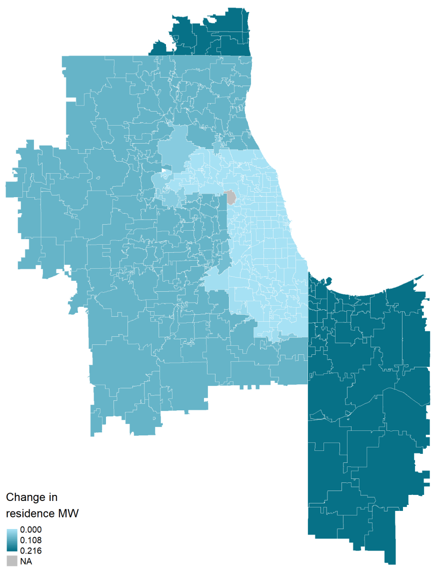


Notes: Data are from LODES and the MW panel described in Section 3.1. The figures show the distribution of changes in the residence and workplace MW measures generated by a counterfactual increase to $9 in the federal MW in January 2020, holding constant other MW policies in their December 2019 levels. 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.

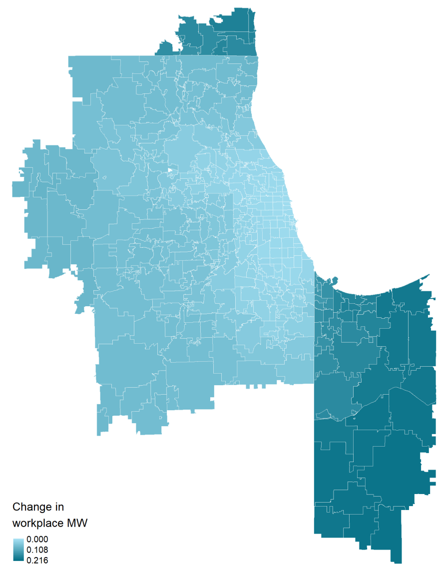
Appendix Figure 14: Changes in the minimum wage measures under counterfactual minimum wage policies, Chicago-Naperville-Elgin CBSA

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

Changes in residence MW

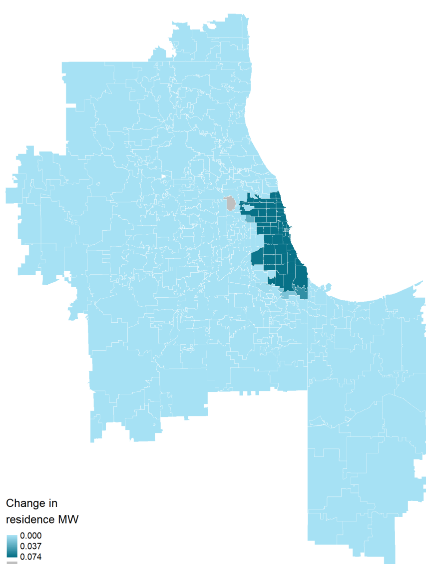


Changes in workplace MW

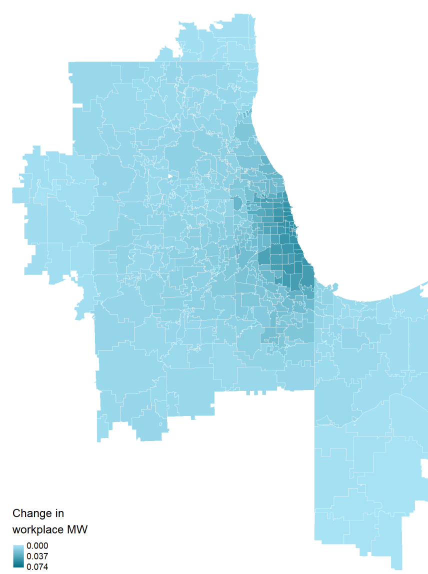


Panel B: Increase in Chicago MW from $13 to $14

Changes in residence MW



Changes in workplace MW

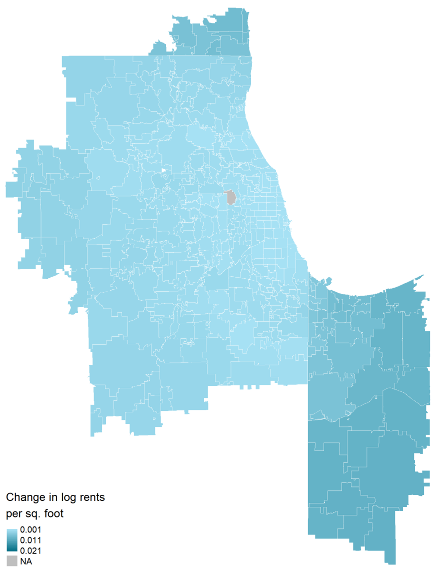


Notes: Data are from the MW panel described in Section 3.1 and from LODES. The figures map changes in the residence and workplace MW measures by counterfactual MW policies in the Chicago-Naperville-Elgin CBSA. Panel A shows a policy where the federal MW increases from $7.25 to $9 in January 2020, holding 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.

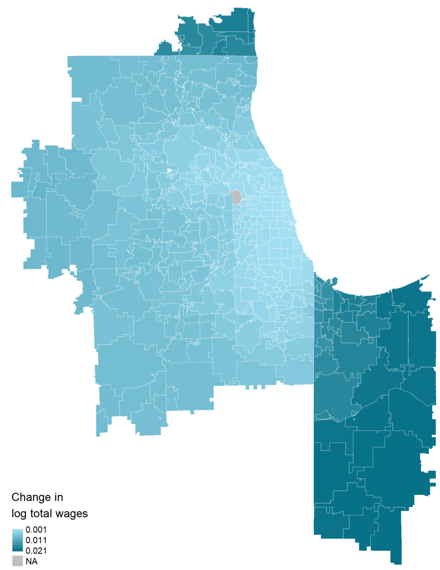
Appendix Figure 15: Changes in log rents and log total wages under counterfactual minimum wage policies, Chicago-Naperville-Elgin CBSA

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

Changes in log rents

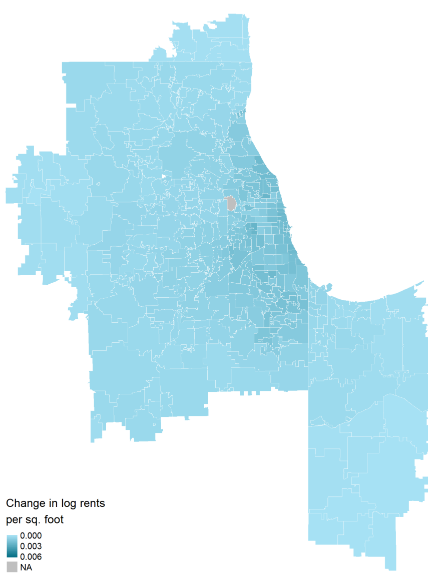


Changes in log total wages

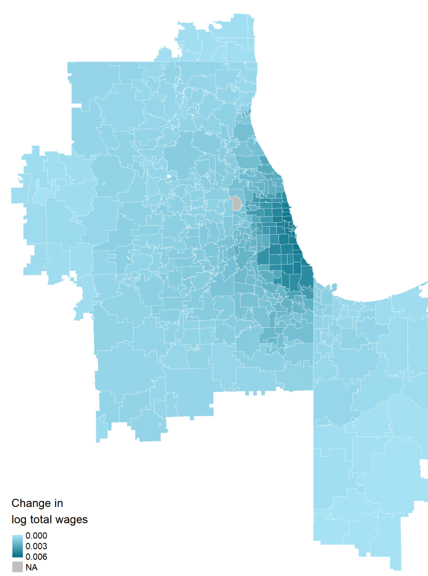


Panel B: Increase in Chicago MW from \$13 to \$14

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, 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 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 $\varepsilon = 0.1013$.