

Do Minimum Wages Increase Rents? Evidence of Spatial Spillovers Using Local High-Frequency Data ^{*}

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Abstract

In this paper, we estimate the effect of minimum wage policies on housing rental prices. We construct a panel data set at the ZIP code and month level using rents data from Zillow and state and local minimum wage changes between 2010 to 2019. Our baseline empirical approach assumes that unobservable determinants of rents are uncorrelated with past and future minimum wage changes after we condition on proxies for the state of the economy, monthly date and ZIP code fixed effects. Results indicate that increasing the minimum wage by 10 percent is associated with an increase of between 0.25 and 0.5 percent in median rents per square foot. Heterogeneity analysis indicates that the effect is driven by ZIP codes where minimum wage workers reside, with a larger impact in ZIP codes with a high proportion of unemployed, African-American, and younger households. We further create a measure of *experienced minimum wage* which keeps track of the difference between residence and workplace location of low-wage workers. Estimated effects using this measure suggest the presence of income redistribution across ZIP codes following a MW increase, with those in the receiving end experiencing a marked increase in rents. We discuss the magnitude of our main estimates by computing an income-to-rent pass-through for minimum wage legislation of approximately one-third.

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1 Introduction

In recent years, many U.S. jurisdictions have introduced minimum wages (hereafter MW) above the federal level of \$7.25.¹ Following the early work of Card and Krueger (2000), most research effort has been devoted to understanding the effects of MW policies on employment (e.g., Neumark and Wascher 2006; Dube, Lester, and Reich 2010; Meer and West 2016; Cengiz et al. 2019) and income inequality (Lee 1999; Autor, Manning, and Smith 2016). This is not surprising, as labor market effects are of first order importance to determine the welfare implications of MW changes on households. However, the *place-based* nature of MW provisions makes it natural to expect that such policies will affect the welfare of households through other channels, such as the housing market. Not accounting for the potential effect of MW changes on rents may lead to inaccurate welfare analysis.

Given these remarks, we pose the question: by what extent (if any) are local rents affected by the minimum wage? Surprisingly, there is little research attempting to estimate the causal effect of MW policies on the housing market. To the best of our knowledge, the only papers aiming at answering this question directly are Tidemann (2018) and Yamagishi (2019; 2021).² Even though they both use the same data at the year county level for the U.S, they find opposing results. Tidemann’s (2018) estimates are negative, whereas the results of Yamagishi (2019) point towards a positive effect.³ In a related paper, Agarwal, Ambrose, and Diop (2019) shows that minimum wages decrease the probability of rental default, suggesting a strengthening of the local labor market. Lastly, in a closely related paper, Hughes (2020) leverages MW changes as a natural experiment to study the relationship between housing demand and policies affecting low-wage households. His approach aims at disentangling the income effect on housing demand from the “general equilibrium” price effect in the long-run, making our work related yet distinct as we focus on prices in the short-run. He shows that a 10 percent increase in MW decreases the rent-to-income ratio by 1.4 percent.

Provided that MW policies have small disemployment effects, theory suggests that the effect on rents will be positive. A canonical version of the Alonso-Muth-Mills model, for example, predicts that general wage increases will be fully capitalized by landlords.⁴ In the same tradition, Yamagishi (2021) shows that minimum wage policies increase rents if disemployment effects are small, and that rents are a sufficient statistic of welfare under free mobility. Our paper is motivated by the view that rents are determined by supply and demand of housing in the local market. MW provisions will increase incomes and thus housing demand in locations where MW workers live and, as a result, rents should be impacted there.

Understanding the effects of MW policies on the housing market is important both from a theoretical and policy perspective. As recent literature has shown, individuals respond to changes in local prices (and amenities) by migrating, and this fact has important implications for welfare and inequality (Diamond 2016; Couture et al. 2019). Several papers make a similar point for the

¹As of January 2020, there were 29 states with a MW larger than the federal one, 52 counties that set a MW above the state, and 15 cities with a minimum above the county.

²Yamagishi (2019) explores this question using data from both the U.S. and Japan. In an updated version of the paper, Yamagishi (2021) excludes the analysis of the U.S. case.

³Yamagishi (2019) attributes this difference to different model specifications, and argues that with proper standard errors clustering the results in Tidemann (2018) are statistically insignificant.

⁴See Brueckner et al. (1987) for a complete treatment of this model.

case of MW policies, arguing that they influence migration decisions and the location of economic activity (Pérez Pérez 2018; Monras 2019). We believe that a reliable estimate of the effect of MW policies on the local housing market will inform this literature and can serve as an important input for policy-makers.

In this paper, we construct a dataset at the U.S. ZIP code and monthly date levels to explore the reduced form effects of MW changes on rents. Our main rent variable comes from Zillow, the largest online real estate platform in the U.S. (PDX 2020; Investopedia 2020), and corresponds to the median rent price per square foot across Zillow listings in the given ZIP code-month cell of the category Single Family, Condominiums and Cooperative Houses (SFCC). This is the most popular housing category in the U.S. (Fernald 2020), and also the most populated series in Zillow. We collect data on minimum wage changes from Vaghul and Zipperer (2016) for the period from 1974 to 2016, which we update until January 2020 using UC Berkeley Labor Center (2020). We use our MW data coupled with origin-destination matrices obtained from the LEHD Origin-Destination Employment Statistics (LODES) database to construct the experienced MW,⁵ defined, for each ZIP code, as a weighted average of the statutory MW in each other ZIP code where its residents work, using as weights the share of population that work in each one of them. We also collect data from the Quarterly Census of Employment and Wages (QCEW) sources to both validate our empirical model and to deploy as controls in our regressions. Finally, we use data from the U.S. Census and from LODES to explore heterogeneity of the effect of interest.

Our main empirical approach uses difference-in-differences panel specifications that condition both on monthly date and ZIP code fixed effects. We begin by estimating a *static* model in which minimum wage changes affect rents in the same period. As has been long established in the literature, this estimate recovers the true causal effect assuming that, within a ZIP code, MW changes are *strictly exogenous* with respect to changes in the error term (conditional on our controls). We argue that this is a plausible assumption. We subsequently estimate a *dynamic* model by extending our static model with leads and lags of MW changes. This allows us to test potential dynamic effects of the MW on rents—ruled out by assumption in the static model—and to assess the underlying parallel trends assumption. Intuitively, assuming no anticipatory effects in the housing market, differential trends across treated and untreated ZIP codes would show up as non-zero pre-trends. Reassuringly, our models show no effects of future MW changes on current rents. They do, however, suggest a short-lived dynamic effect over the first couple months following a MW increase.

We validate our empirical strategy by testing our basic model in several ways. First of all, we check for the presence of unobservables affecting both rents and MW changes in three ways: (i) we show that it is reasonable to think of supply as fixed around a short window of the MW changes; (ii) we include controls that proxy for the state of the economy and are plausibly not affected by the changes in the MW; and (iii) we allow for richer heterogeneity across ZIP codes by controlling for ZIP-code-specific linear and quadratic trends. These specifications should capture a wide range of potential confounders in our main regressions. The fact that our baseline estimates are robust to the inclusion of these controls strengthens the case for the strict exogeneity assumption of MW

⁵LEHD is short for Longitudinal Employer-Household Dynamics, which corresponds to the source of the origin-destination data.

changes. Second, as rental listings may stay on Zillow for more than one month, one may worry about structural auto-correlation in the dependent variable which, if not accounted for, may bias our estimates. For this reason, we estimate alternative models that include the lagged first difference of rents as controls, estimated via instrumental variables following Arellano and Bond (1991) and related literature. The estimates of this model are strikingly similar to our baseline results, rendering credibility to our econometric assumptions.

Finally, a drawback of the Zillow data is that it includes a subsample U.S. ZIP codes. This fact brings about two concerns. First, the composition of ZIP codes changes over time potentially introducing bias in our estimates. We tackle this issue by performing our main analysis with a constant set of units with valid data as of 2015.⁶ This strategy alleviates concerns arising from the changing composition of ZIP codes, but significantly lowers the number of observations used in the estimation. For this reason, we also estimate a model on the full sample of available ZIP codes, obtaining similar results. Secondly, we worry that our estimated effect may be particular to our subsample. In order to approximate the average treatment effect for the typical urban ZIP code, we re-weight our data to match average demographic characteristics of the top 100 Metropolitan Statistical Areas and re-estimate our main models. The effects not only survive this test but are also larger.

The *static* difference-in-differences specification implies that a 10 percent increase in the MW leads to an average 0.26 percent increase in the rental price per square foot. When expanding the model to account for *dynamic* effects, we see that a 10 percent increase in the minimum wage increases rents by 0.58 percent in a 6-months period. Estimates of the long-run effect using the panel specification that includes the lagged first difference of rents as controls returns are very similar, but more precisely estimated.

In an effort to disentangle who are the “winners and losers”, we perform an heterogeneity analysis of the average treatment effect by allowing the coefficients to differ across quartiles of the distribution of ZIP code characteristics. The results suggest that the effect of interest is indeed heterogeneous. ZIP codes with lower shares of college graduates (bottom quartiles), as well as ZIP codes with higher shares of residents below 24 years old and African-American residents (top quartiles) experience a rent increase which is almost twice as large. We then use LODES data to identify, for each ZIP code, the share of young, low-income workers and residents, respectively. We argue that these characteristics proxy for MW workers fairly well. Consistently, we show that ZIP codes with low concentration of MW earners as residents exhibit no significant effects. On the other hand, we find that the effect is roughly constant across ZIP codes with different share of MW workers who work there.

Since it is reasonable to expect workplace and residence locations to differ, the use of statutory changes is probably not an accurate measure of where the income gains of MW changes are located. In an extreme case, it could be that a ZIP code experiencing a MW increase actually has no MW earners as residents, but plenty as workers. In that case, we expect to observe changes in rents in those nearby ZIP codes where the MW workers reside. To accommodate for this, we use a measure

⁶Because these ZIP codes enter the sample at different moments in time before 2015, our estimating panel is still unbalanced. However, no ZIP code has missing values after it enters the panel.

of “experienced minimum wage” that uses the average MW level a ZIP code’s workers are exposed to in their workplace location. Our results show a slightly higher impact of MW changes on rents when we replace our main explanatory variable: a 10 percent MW increase leads to a simultaneous 0.31 percent increase in rents. Interestingly, when we include both the statutory and experienced MW measures in our model the effect of the latter gets even larger, whereas the former slightly decreases rents. We interpret this result as consistent with the view that MW policies redistribute income geographically from ZIP codes where MW workers work to those where MW workers reside. Given these income shocks, the supply and demand of local rentals determine the new equilibrium in the housing market.

In the final part of the paper, we gauge the magnitude of our estimates by computing the pass-through of the policy, which is defined as the ratio between the elasticity of rents to the MW and the elasticity of wages to the MW. To do that, we perform two exercises. First, we compute the elasticity of wages to the MW using county-quarter data from the QCEW. These estimates imply a pass-through of 19 percent. Secondly, we use estimates of the elasticity of wages to the MW taken from the literature and combine it with our results. We compute in this case a 23 percent pass-through. Because the rents elasticity is somewhat larger, estimates of the pass-through using the experienced MW are slightly larger as well.

Our approach has several differences with respect to previous research on the topic. Both Tidemann (2018) and Yamagishi (2019) for the U.S. exploit Fair Markets Rents data from the Department of Housing and Urban Development (HUD), which is available at the yearly level and aggregated at the geographical level of counties.⁷ An important advantage of our approach is that we use the exact timing of the MW change at the monthly level. Another advantage is that we use data at the ZIP code- instead of the county-level.⁸ We illustrate the importance of having smaller units of analysis with the following example. For a given county, suppose that (1) all low-skill jobs are in one particular ZIP code; and (2) low-skill households prefer to live near their jobs. Further assume that, following a MW change, employment effects are near zero.⁹ One should then expect demand for housing in the ZIP code with low-skill jobs to increase and demand for housing in the rest of the ZIP codes to go down. If we focus on the effects of the MW increase on the county we might even find that the rents go down, when in fact the rents in the ZIP codes where the low skill jobs are located are increasing. Indeed, Tidemann (2018) finds that a \$1 increase in the MW decreases the yearly average of the monthly rent by 1.5 percentage points.¹⁰

Using a more detailed geography also aids in the empirical estimation. First of all, it means that we can exploit MW changes at any jurisdictional level, effectively increasing the number of events used for identification. Secondly, it allows us to use more detailed controls, such as ZIP code fixed effects, local employment and wages for sectors likely unaffected by MW changes, and ZIP code-specific polynomial trends. This is important because the dynamics of the rental market

⁷Yamagishi (2019), updated in Yamagishi (2021), also uses data at the year-prefecture level for the 47 Japanese prefectures.

⁸As of 2019 there were 3,142 counties and 39,295 meaningful ZIP codes in the U.S. We exclude military and unique business ZIP codes in this count, since they are irrelevant for the housing market.

⁹This is consistent with findings in the literature (for a comprehensive review see Dube 2019).

¹⁰As pointed out by Tidemann (2018), the sign of this effect implies that the labor demand for low skilled workers is elastic. This is at odds with results of null employment effects in the literature.

plausibly vary across ZIP codes within a county following trends at the very local level (Almagro and Dominguez-Iino 2019). Importantly, these controls make the required identification assumptions more credible. Given that the identifying variation comes from within-ZIP codes, the determinants of these MW changes are unlikely to be related to the particular ZIP code and, thus, are less likely to be correlated to the unobservable determinants of rent dynamics there.

Beyond the contribution to the very recent literature on the effects of MW changes on rents, we contribute to several strands of the literature. First, we contribute to the literature studying the effects of MW policies on the welfare of low-skill households (DiNardo, Fortin, and Lemieux 1995; Lee 1999; Card and Krueger 2000; Neumark and Wascher 2006; Autor, Manning, and Smith 2016; Cengiz et al. 2019, among others). Most of this literature has focused on disemployment effects. We contribute to this strand of literature by exploring the effects of minimum wage policies on the housing market. To the best of our knowledge, we are the first to compute a local measure of experienced MW that takes into account commuting patterns. Related research focused on the direct impact of MW changes on commuting patterns across state and city borders (McKinnish 2017; Pérez Pérez 2018). We, on the other hand, use commuting patterns to keep track in a more precise way of the income redistribution generated by MW policies.

We also contribute to the literature that studies the response of local prices to MW changes. **Leung2018** provides evidence of the MW impact on grocery store prices, suggesting that MW also affects products demand besides raising labor costs. In his analysis, he further shows that the effect is larger in poorer regions. Similarly to our study, this points out novel channels of heterogeneity in the pass-through that may have non-trivial distributional consequences.

Our work also relates to the literature that studies the location decision of agents either based on income (Roback 1982; Kennan and Walker 2011; Desmet and Rossi-Hansberg 2013; Pérez Pérez 2018; Monras 2019) or on spatial rents and amenity differentials (Diamond 2016; Almagro and Dominguez-Iino 2019; Couture et al. 2019). We contribute by adapting this framework to the case of MW changes as a means to rationalize through residential location sorting part of the observed reduced-form effect on rents.

The rest of the paper is organized as follows. In Section ?? we introduce a motivating model of the rental market. In Section 3 we present our data sources and show the characteristics of our estimating panel. In Section 4 we explain our empirical strategy and we discuss our identification assumptions. In 5 we present our main results. Section 6 discusses relevant policy implications, and Section 7 concludes.

2 A partial-equilibrium model of a neighborhood’s rental market

In this section we lay down a motivating demand and supply model of the rental market. We intend to illustrate why one may expect a different impact of workplace and residence MW changes. We also show how, in the model under certain assumptions, changes in log rents can be expressed as a function of the changes in two MW-based measures.

The model is purposefully stylized. Because we study the consequences of MW changes in the very-short run, our model is static. We discuss the addition of the time dimension in Appendix A. We also assume an exogenous distribution of workers across residence and workplace locations. We think of the specification of a spatial model with worker mobility as an avenue for future work.

We emphasize that the model is designed to highlight a possible mechanism through which one may expect residence and workplace MWs to have different impacts. However, our empirical results do not hinge on any of the assumptions made in this section.

2.1 Set-up

We consider the rental market of some ZIP code i embedded in a metropolitan area, which is characterized by a set of ZIP codes \mathcal{Z} . Workers with residence $i \in \mathcal{Z}$ work in some other ZIP code $z \in \mathcal{Z}$. More precisely, we let L_{iz} denote the measure i 's residents who work in z ; $L_i = \sum_{z \in \mathcal{Z}} L_{iz}$ and $L_z = \sum_{i \in \mathcal{Z}} L_{iz}$ the number of residents in i and workers in z , respectively; and $\mathcal{L} = \sum_{z \in \mathcal{Z}} \sum_{i \in \mathcal{Z}} L_{iz}$ the total measure of workers. We assume that the distribution of residence-workplace pairs is fixed.¹¹ Each ZIP code is characterized by a binding minimum wage, which we denote by $\{\underline{w}_z\}_{z \in \mathcal{Z}}$.

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

Assumption 1 (Housing demand). *For all residence-workplace pairs, the housing demand function $h_{iz}(r_i, \underline{w}_i, \underline{w}_z)$ is (i) continuously differentiable in its three arguments; (ii) decreasing in rental prices r_i ; (iii) decreasing in residence MW, \underline{w}_i ; (iv) increasing in workplace MW, \underline{w}_z .*

Points (i) and (ii) simply say that h_{iz} is a “smooth” demand function. Point (iv) follows from the fact that housing is a normal good. Given that, under negligible disemployment effects, workplace MW increases income (at least of the workers who make close to the MW), it should increase housing demand. Point (iii) is less obvious, and we motivate it as follows. Residence MW, while increasing the incomes of people working in the same place, also increases the cost of production of non-tradable goods. The higher cost of non-tradables will translate into a lower demand of housing if the substitution effect of an change in local prices on local demand of housing is smaller than the corresponding income effect. A sufficient condition is that housing and local consumption are complements.¹² Yet another possibility is that the owners of firms tend to live in the same ZIP code where the firm is located and suffer losses in profits after the MW increase (thus, for them, residence MW decreases income even further).

¹¹To simplify we assume that all of i ' residents work, so that the number of residents equals the number of workers.

¹²We can formalize this discussion with a simple worker choice problem. Say a representative (i, z) worker chooses between housing demand h_{iz} , non-tradable consumption c_{iz}^{NT} , and tradable consumption c_{iz}^{T} , by maximizing

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

subject to $r_i h_{iz} + p_i(\underline{w}_i) c_{iz}^{\text{NT}} + c_{iz}^{\text{T}} \leq y_{iz}(\underline{w}_z)$, where $p_i(\underline{w}_i)$ gives the price of local consumption, which is increasing in residence MW; the price of tradable consumption is normalized to one; and $y_{iz}(\underline{w}_z)$ is an income function that depends positively on the workplace MW. Let h_{iz}^* and c_{iz}^* denote marshallian demands, and \tilde{h}_{iz}^* denote the hicksian

We think that the interpretation underlying point (iii) is plausible for several reasons. First, recent evidence by Miyauchi, Nakajima, and Redding (2021) shows that individuals tend to consume close to home. As a result, we expect them to be sensible to prices of local consumption in their same neighborhood. Second, MWs have been shown to increase prices of local consumption (e.g., Allegretto and Reich 2018; Leung forthcoming). These empirical facts suggest that residence MW changes might (conditional on workplace) negatively affect incomes and thus demand for housing.

Housing supply We assume a simple supply side. Denote by $D_i(r_i)$ the supply of square feet in i , which is increasing in r_i . Note that this formulation allows for an upper limit on the number of houses at which point the supply becomes perfectly inelastic.

2.2 Equilibrium and Comparative Statics

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

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

Given that housing demand functions are continuous and decreasing in rents, under a suitable regularity condition there is a unique equilibrium in this market.¹³ We denote equilibrium rents as $r_i^* = f(\{\underline{w}_i\}_{i \in \mathcal{Z}})$.

Note that equilibrium rents are a function of the entire vector of minimum wages. We are interested in two questions. What is the effect of a change in the vector of MWs $(\{d \ln \underline{w}_i\}_{i \in \mathcal{Z}})'$ on equilibrium rents? Under what conditions can one reduce the dimensionality of the rents function? We organize a couple of answers in the upcoming propositions.

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

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

$$\left(\eta_i - \sum_z \pi_{iz} \xi_{iz} \right) d \ln r_i = \sum_z \pi_{iz} (\epsilon_{iz}^i d \ln \underline{w}_i + \epsilon_{iz}^z d \ln \underline{w}_z) \quad (2)$$

where $\pi_{iz} = \frac{L_{iz}}{L_i}$ represents the share of workers from i working in z ; $\xi_{iz} = \frac{dh_{iz}}{dr_i} \frac{r_i}{\sum_i \pi_{iz} h_{iz}}$ is the elasticity of housing demand at the average per-capita demand of ZIP code i ; $\epsilon_{iz}^i = \frac{dh_{iz}}{d\underline{w}_i} \frac{\underline{w}_i}{\sum_i \pi_{iz} h_{iz}}$

housing demand. The slusky equation implies that

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

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

¹³ Assume $D_i(0) - \sum_{z \in \mathcal{Z}} L_{iz} h_{iz}(0, \underline{w}_i, \underline{w}_z) < 0$ and apply the intermediate value theorem.

and $\epsilon_{iz}^z = \frac{dh_{iz}}{dw_z} \frac{w_z}{\sum_i \pi_{iz} h_{iz}}$ are the elasticities of housing demand to workplace and residence MWs also at the average per-capita demand of ZIP code i ; and $\eta_i = \frac{1}{L_i} \frac{dD_i}{dr_i} \frac{r_i}{D_i}$ is the elasticity of housing supply in ZIP code i .

Because $\xi_{iz} < 0$, $\epsilon_{iz}^i < 0$, and $\epsilon_{iz}^z > 0$ for all $z \in Z$, it is apparent from (2) that an increase in workplace MW unambiguously increases rents, whereas a residence MW increase will have an unconditional muted effect and a negative effect conditional on the experienced MW.¹⁴ \square

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

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

Proof. Under the assumption that $\epsilon_{iz}^z = \epsilon_i^z$ for all $z \in Z$ we can manipulate (2) to write

$$d \ln r_i = \beta_i \sum_z \pi_{iz} d \ln w_z + \gamma_i d \ln w_i \quad (3)$$

where $\beta_i = \frac{\epsilon_i^z}{\eta_i - \sum_z \pi_{iz} \xi_{iz}} > 0$ and $\gamma_i = \frac{\sum_z \pi_{iz} \epsilon_{iz}^i}{\eta_i - \sum_z \pi_{iz} \xi_{iz}} < 0$. \square

Proposition 2 shows that, as an approximation to small changes in the profile of MWs, we can write the change in rents in the ZIP code as a function of two MW-based measures. This motivates our empirical strategy, where we further impose that $\beta_i = \beta$ and $\gamma_i = \gamma$ for all $i \in Z$.¹⁵

3 Data and Sample Construction

In this section, we describe the construction of our data set. First, we explain in detail what our sources of data are and the steps we take to put them together in a ZIP code by month panel data set. We focus on describing data on rents coming from Zillow, and our construction of the actual and experienced MW—a new measure of the MW that accounts for the fact that residence and workplace may differ—. Later, we explore how the sample of ZIP codes available in Zillow, our source for rents data, compares to the U.S. sample of ZIP codes. We conduct our main analysis on a balanced panel of ZIP codes which construction we describe as well.

3.1 Rents Data from Zillow

One of the main challenges to estimate the effects of any policy on the housing market is to obtain reliable data. Housing rent data has been particularly scant in the literature. Recent papers

¹⁴The sign of the overall partial effect depends on the sign of $\sum_z \pi_{iz} \epsilon_{iz}^i + \pi_{ii} \epsilon_{ii}^z$.

¹⁵The assumption of constant effects is sufficient, although not necessary, to map our empirical model to (3). A weaker assumption is that, in the context of the empirical model, the heterogeneity in these parameters is uncorrelated to rents.

have used Small Area Fair Market Rents (SAFMRs) series from U.S. Department of Housing and Urban Development (2020), available at the ZIP code and year level (Tidemann 2018; Yamagishi 2019). We, on the other hand, leverage newly available data from Zillow at the ZIP code and month level. The high frequency of the Zillow data is an advantage since it allows us to explore the effects of MW changes on rents exploiting the precise timing of their enactment.

Zillow is the leader online real estate and rental platform in the U.S., hosting more than 110 million homes and 170 million unique monthly users in 2019 (Zillow 2020a). Zillow provides the median rental and sale price (both total and per square foot) among homes listed on the platform in a given period. Time series are provided for different house types and at several geographic and time aggregation levels (Zillow 2020b).¹⁶ We collect the USPS ZIP code level monthly time series. The time span of the data varies at the ZIP code level, and geographical units with a small number of listings are omitted.¹⁷ As explained below, we construct a balanced panel to address the changing composition of the sample.

Clearly, even within a single ZIP code, there could be a great deal of heterogeneity in terms of house sizes and types, threatening the validity of our estimations. To minimize price variation arising from housing units' characteristics, we focus our primary analysis on a single housing category: *single-family* houses, *condominium*, and *cooperative* units (SFCC). This is by far the series with the largest number of non-missing ZIP codes, as it covers the most common U.S. rental house types. In fact, roughly a third of the nation's 47.2 million rental units in 2018 fit the category of single-family homes, with the remaining 43 percent made up from buildings with five or more units (Fernald 2020). Because we want to condition our comparisons on house size we focus on *per square foot* rents. As a result, our main outcome variable represents the median rental price per square foot in the SFCC category among units listed in the platform for a given ZIP code and month.

Zillow data has several limitations. The first one is that we do not observe the underlying number of units listed for rent in a given month. Therefore, changes in the inventory introduce additional variation in the reported median rental price that we are unable to control for. We do observe the number of houses listed *for sale*, which we use as a proxy for this variable in robustness analyses.¹⁸ A second limitation is that Zillow's market penetration dictates the sample of ZIP codes available. As a result, we observe a selected sample of typically urban ZIP codes. We describe our sample in more detail later in this section.

To ensure that our data correctly captures the price evolution of the U.S. rental market, we compare Zillow's median rental price with 5 SAFMRs series for houses with a different number of bedrooms (0, 1, 2, 3, and 4 or more). SAFMRs are calculated for ZIP codes within metropolitan areas at a yearly level, and generally correspond to the 40th percentile of the rent distribution for that ZIP code.¹⁹ The correlation between Zillow's SFCC and SAMFR's ZIP-code-level time series is consistently above 90 percent. Appendix ?? compares the time series variation of the Zillow SFCC

¹⁶The availability of different time series changed over time, so not all series used for the analysis might be still available to download. See Zillow (2020b) for more details on the data shared by Zillow.

¹⁷Two related notes are the following: (i) once a ZIP code enters our panel, it shows a complete time-series; (ii) we do not know the threshold used by Zillow to censor the data.

¹⁸We are not aware of a ZIP code-month dataset that provides counts of houses for rent.

¹⁹For more information on how SAFMRs are calculated, see U.S. Department of Housing and Urban Development (2017, page 41641).

series and a weighted average of the SAFMR series for different number of bedrooms.²⁰ The Zillow rent data is always higher in levels. Part of this difference is intuitively related to the fact that Zillow reports median rent prices while SAFMRs are based on the 40th percentile of the rent distribution. However, the two series show similar trends, confirming that Zillow does a decent job in capturing the overall dynamics of the U.S. rental market in metropolitan areas.

3.2 The Statutory and Experienced Minimum Wage

Our main explanatory variable is the minimum wage. We collect data on federal, state, county, and city-level MWs from Vaghul and Zipperer (2016). We complement their data, which runs up to mid-2016, with MW data for the years 2016 to 2019 from UC Berkeley Labor Center (2020). Because we are interested in studying rental dynamics at the ZIP code level using Zillow, we assign MW levels to ZIP codes by taking the following steps. First, we map each ZIP code to a metropolitan statistical area or rural town using HUD crosswalks (Housing and Development 2020). Given that ZIP codes can cross different administrative borders, we use the number of housing units from the 2010 census and geographic codes to map each ZIP code to a unique county by assigning it to the one with the highest share of houses from that ZIP code. We also map each ZIP code to a county and state analogously. After this process, we are able to assign a unique state and local level MW to each ZIP code. We define the *statutory* MW variable as the maximum between the ones required by the federal, state, county, and city levels.²¹ As a result, we only use MW changes that are binding, meaning that they actually impact that maximum.

When restricting to the sample of ZIP codes available in Zillow, our data reports 18,689 MW changes at the ZIP code-month level. These, in turn, arise from 151 state-level and 182 county- and city-level changes. Figure 1 shows the distribution of positive increases in our statutory MW variable among all ZIP codes available in the Zillow data.²² Panel (a) shows the distribution of intensity of our MW changes. The average percent change among Zillow ZIP codes is 5.5%. However, we observe a decent amount of large increases. Our estimation strategy will exploit the intensity of MW changes. On the other hand, panel (b) shows the timing of those changes between 2010 and 2019. Most changes occur in either January or July, and the majority of them take place later in the panel. This could be problematic since the timing of entry of ZIP codes into the panel is also concentrated in these months. We construct a balanced sample of ZIP codes to tackle this issue.

We construct an alternative measure to capture the effects of MW policies: the *experienced* MW. This measure aims to account for the fact that workplace location often differs from the residence

²⁰ To compute the weighted SAMFR series, we proceed as follows. First, we compute the national yearly average for both the Zillow SFCC and the 5 SAFMR series. Then, for each of the latter, we compute the U.S. share of single family, condo, and cooperative houses with that number of bedrooms using the *American Housing Survey* (AHS). To ensure comparability, we only use the estimated count for rental houses in this step. (Additionally, AHS data is available only for years 2011, 2013, 2015, 2017, and 2019. We therefore fill missing years with the previous year's share.) Finally, we weight SAFMR series using the shares mentioned above.

²¹ 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.

²² There are a few cases of decrease in the MW arising from judicial decisions overthrowing local MW ordinances. For expository reasons, they are not shown in the figure. However, they are used in estimations throughout the paper.

one. The MW that matters for a given local rental market is the one experienced by the people living in it, and so by tracking where people in each ZIP code work we can get a better sense of the relevant MW there. To construct this measure we need to know, for each ZIP code, where workers residing in that ZIP code work. We obtain this information from the 2017 Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES). In particular, we use the origin-destination matrix mapping jobs from residence to workplace locations. The data come at the block group level. We aggregate that to construct a ZIP code residence-workplace matrix where we observe the number of workers for each residence-workplace pair.

We then use the ZIP code residence-workplace matrix to build exposure weights. Denote ZIP codes by i and monthly dates by t . Let \mathbb{Z}_i be the set of ZIP codes in which i 's residents work (including i). We construct the set of weights $\{\omega_{iz}\}_{z \in \mathbb{Z}_i}$ as

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

where N_{iz} is the number of workers who reside in ZIP code i and work in z , and N_i is the total working-age population of ZIP code i .²³ Given that origins present a large number of destinations with extremely low percentages of workers, we trim the number of destination ZIP codes to those making up to 90 percent of the workforce.²⁴ Letting \underline{w}_{it} denote the statutory MW in ZIP code i and month t , we define the experienced minimum wage measure as

$$\underline{w}_{it}^{\text{exp}} = \sum_{z \in \mathbb{Z}_i} \omega_{iz} \underline{w}_{zt} . \quad (4)$$

The experienced MW of a ZIP code is based on the MWs binding in other ZIP codes where its residents work. An increase in a city, for example, may not have an impact on the local rental market if most residents are not MW workers. It will, however, affect neighboring ZIP codes where MW workers reside. We will use this insight in our analysis. See subsection 4.4 for some discussion on how we use this measure, and ?? for further details and estimation results.

3.3 Other Data Sources

We collect socio-demographic information from the 2010 Census and the 5-years 2008-2012 American Community Survey (ACS). The data is initially obtained at the Census tract level and mapped into USPS ZIP codes using HUD crosswalks (Housing and Development 2020). We assign the following characteristics to each ZIP code: population, number of housing units, median income, African-American population, number of unemployed, and number of college students. We use this information to classify ZIP codes into, for example, high or low median income to perform heterogeneity analysis.

To proxy for local economic activity we collect data from the Quarterly Census of Employment

²³The LODES data additionally reports origin-destination matrices for number of workers 29 years old and younger and number of workers earning less than \$1,251 per month. We compute weights based on both these sub-groups as well. However, the resulting experienced MW measures with any set of weights are highly correlated among each other ($\rho > 0.99$ for every pair). Thus, we use working population weights throughout the paper.

²⁴Results based on the full distribution are identical to those presented in the paper.

and Wages (QCEW) at the county-quarter and county-month levels for every main industrial division.²⁵ For each county-quarter-industry cell, we observe the number of establishments and the average weekly wage. For each county-month-industry cell, we additionally observe the number of employed people. We merge this data onto our ZIP code-month panel by county and quarterly date.

Finally, we use the LODES data to proxy for MW workers’ residence and workplace location. Beyond the origin-destination matrices, the LODES data provides block-level information on jobs by residence area (RAC) and workplace area (WAC) characteristics. These include jobs for various types of workers.²⁶ We use RAC and WAC datasets to “locate” workers likely to earn MW by looking at the state-level distribution of such type of workers. We build, for each ZIP code in the sample, the share (out of the state total) of workers under 30 years old earning less than \$1,251 per month that either *live* or *work* there. We take these data as time-invariant characteristics of our ZIP codes.

3.4 The Resulting Panel

Using the data described above, we put together a panel dataset at the ZIP code and monthly date levels from February 2010 to December 2019. Given that ZIP codes enter the Zillow data progressively over time affecting the composition of the sample, we construct our baseline *estimating panel* by keeping in the sample those ZIP codes with valid rents data as of July 2015.²⁷ This panel contains 5,302 MW increases, which arise from 124 state changes and 99 county and local level changes. 4,224 of those changes take place after ZIP codes already entered the panel, and thus are used in estimation.

We stress the fact that our data does not cover the full sample of ZIP codes, but rather a selected one. Appendix ?? maps the full set of available ZIP codes in the Zillow data, together with population density. The Zillow sample seems fairly distributed across urban areas, although some important areas have limited coverage.

Table 1 further compares the Zillow sample to the population of ZIP codes along several critical demographic dimensions. Columns 1 and 2 report data for the whole universe of U.S. ZIP codes and for the top 100 U.S. metropolitan areas, respectively. In column 3 we show the complete set of ZIP codes in the Zillow data. Finally, column 4 shows our baseline estimating sample. Focusing on our preferred variable—median rent per square foot in the SFCC category—, we collect rent data from Zillow for 3,315 unique ZIP codes, which amount to 8.5 percent of the 38,893 total for the entire U.S. and 46.7 percent of the 2010 U.S. population.

²⁵The QCEW covers the following industrial aggregates: “Agriculture, Forestry, and Fishing”, “Mining”, “Construction”, “Manufacturing”, “Transportation and Public Utilities”, “Wholesale Trade”, “Retail Trade”, “Financial activities” (including insurance and real state), “Services”, and “Public Administration”.

²⁶LODES RAC and WAC datasets provide workers’ breakdown for the following characteristics: age (less than 29, 30 to 54, more than 55); workers’ earnings (less than \$1,251/mo., \$1,251/mo. to \$3,333/mo., more than \$3,333/mo.); NAICS(11, 21, 22, 23, 31-33, 42, 44-45, 48-49, 51, 52, 52, 54, 55, 56, 61, 62, 71, 72, 81, 92); race (White alone, Black or African-American alone, American-Indian or Alaskan Native alone, Asian alone, Native Hawaiian alone, two or more race groups, not Hispanic or Latino, Hispanic or Latino); educational attainment (less than high school, high school or equivalent, some college or associate, bachelor’s degree or advanced degree); sex (male, female).

²⁷We note that the resulting panel is still unbalanced, in the sense that the time series for some ZIP codes starts before July 2015. However, from July 2015 onward our data contains no missing values in the main rent variable used in the analysis.

The average median household annual income for those ZIP codes is \$65,475, almost 25 percent higher than the same figure for the average U.S. ZIP code and 5 percent higher than the top 100 metropolitan areas. ZIP codes in the baseline sample are even richer, with an average household income of \$66,920. Furthermore, both Zillow ZIP codes and those in our estimating panel have a higher share of urban population, college students, African-American and Hispanic population, and houses for rent than the average urban ZIP code. In an attempt to capture the treatment effect for the average urban ZIP code we conduct an estimation re-weighting our sample to match characteristics of the top 100 CBSA sample of ZIP codes. Because our ZIP codes are richer than the average (i.e., arguably less influenced by MW changes), we expect to find a larger effect in this exercise.

Finally, Table 2 shows some basic sample statistics of our baseline estimating panel. As suggested in the table, the statutory and experienced MW are quite similar. We compare these measures in more detail in ???. We also show summary statistics of median rents in the SFCC category. The average of monthly median rents is \$1,651 in absolute values and \$1.27 per square foot, although these variables show a great deal of variation. Finally, for illustration, we show average weekly wage, employment and establishment count for the “Financial activities” sector from the QCEW. Appendix Table C.1 additionally shows summary statistics for the experienced MW computed using alternative weights, rents in different categories of the Zillow data, and the full set of QCEW industries we use as controls in our regressions.

4 Empirical Strategy and Identification

In this section, we present the empirical strategy adopted to study the effect of the MW on rents and we discuss the assumptions required for identification. We begin with a *static* difference-in-differences specification that imposes no dynamics in the effects. To ease concerns of contemporaneous shocks systematically affecting both changes in rents and the MW within a ZIP code, we directly control for several county-level time-varying proxies of the health of the local labor market.

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

Our specifications are distinct from classic event-study models used commonly in the literature (discussed in, e.g., Borusyak and Jaravel 2017; Abraham and Sun 2018). Rather, they share features with empirical work estimating the impact of high-frequency and changing-intensity events (see, e.g., Fuest, Peichl, and Siegloch 2018; Suárez Serrato and Zidar 2016). First, our models allow for units treated more than once. Secondly, our models allow for the inclusion of never-treated units, which aid in the identification of fixed effects and other parameters, diminishing concerns of under-

identification (Borusyak and Jaravel 2017). Finally, our specifications identify the treatment effect of minimum wages on median rents exploiting not only the timing of a MW change but also its intensity.

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

4.1 Static Model

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

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

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

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

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

Estimates of the parameter β can be interpreted causally under the assumption of *strict exogeneity*. That is, we require the unobserved shocks to rents to be uncorrelated with past and present values of minimum wage changes (Wooldridge 2010, chapter 10). Formally, we assume that the set of conditions

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

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

We worry that unobserved shocks, such as local business cycles, may systematically affect both rents and minimum wage changes. We deploy two strategies to account for that. First of all, we include economic controls from the QCEW. These data are aggregated at the county level, and represent a second best given the unavailability of controls at the ZIP code level. Specifically, we use average weekly wages, employment and establishment count for the sectors “Professional and business services,” “Information,” and “Financial activities.” We assume that these sectors are not affected by the minimum wage. In fact, according to U.S. Bureau of Labor Statistics (2020, table 5), in 2019 such industries accounted for 3.5, 1, and 1.2 percent of the total number of MW workers, respectively.²⁸ We believe that these controls plausibly capture variation arising from unobserved trends in local markets. Our second strategy to deal with unobserved heterogeneity is to control for ZIP code-specific linear and quadratic trends. These models, presented in the Appendix, should account for time-varying heterogeneity not captured by our economic controls that follows this pattern.

4.2 Dynamic Models

One concern with the static model is that, despite controlling for economic factors or ZIP code-specific trends, preexisting time paths of rents per square foot in anticipation of MW changes may be different in treated and non-treated ZIP codes due to either residual demand shocks or anticipatory effects in the supply of rentals. In order to assess whether pre-trends are present, we extend our static model with leads and lags of our minimum wage variable. In addition, one may believe that the effect of MW changes on rents is not a one-time discrete level jump but that it has persistence. In such cases the estimated coefficient β from equation (5) might only have limited relevance for policy evaluation purposes (Callaway and Sant’Anna 2019). Extending the model with leads and lags of the MW allows us to explore this possibility as well. The *dynamic* difference-in-differences model is

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

where s is the number of months of a symmetric window around the MW change. Our baseline specification sets $s = 5$. We show in the appendix that our results are very similar for $s \in \{3, 6, 9\}$. Results hold for larger values of s as well; however, increasing s implies dropping observations at the start and end of the panel, which decreases our sample size and makes the results less reliable.

Importantly, this model allows us to test whether $\beta_{-s} = \beta_{-s+1} = \dots = \beta_{-1} = 0$. Under the assumption that there is no anticipatory effects in the housing market, we interpret the absence of pre-trends as evidence against the presence of unobserved economic shocks driving our results.²⁹ We think that, given the high frequency of our data and that we focus on short windows around

²⁸In Section B we show suggestive evidence that they are not affected by the MW by using them as dependent variable in our models.

²⁹We can also interpret the absence of pre-trends as a test for anticipatory effects if we are willing to assume that the controls embedded in \mathbf{X}_{ct}' capture all relevant unobserved heterogeneity arising from local business cycles. While we find the interpretation given in the text more palatable, the data does is consistent with both.

MW changes, the assumption of no anticipatory effects is reasonable. We further present evidence in favor of this assumption by showing that MW changes do not predict the number of listings of houses for sale in Zillow.³⁰ Specifically, we can track the number of houses listed for sale in selected ZIP codes during the period 2013-2019 for our preferred house type (SFCC). We use such series to run placebo regressions where we estimate both the static and dynamic models using the change in (log) listings as dependent variable.

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

Finally, we consider a dynamic panel specification that includes the lagged dependent variable as a control. Such a model has two important advantages. First and most important, it accounts for the possibility of structural auto-correlation in the rents variable that would bias our results. This is in fact a possibility, given that some listings likely remain in the Zillow data for more than one month. Secondly, it allows us to relax the strict exogeneity assumption (Arellano and Honoré 2001). The less stringent *sequential exogeneity* assumption required for identification allows for feedback effects of minimum wages on rents.³¹ The model with can be written as

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

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

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

4.3 Heterogeneity by ZIP code Characteristics

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

³⁰Ideally, we would run this regression on the number of rental units. Unfortunately, as described in Section 3, this information is not available.

³¹More precisely, the sequential exogeneity assumption states that, conditional on controls and fixed effects, unobserved shocks to rents are uncorrelated with *past* MW changes.

by ZIP codes expected to have more MW workers. Formally, the model is

$$\Delta \ln y_{ict} = \theta_t + \sum_{q=1}^4 \beta_q \mathbf{1}\{i \in q\} \Delta \ln w_{ict} + \Delta \mathbf{X}_{ct}' \eta + \Delta \varepsilon_{ict} , \quad (8)$$

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

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

4.4 Experienced Minimum Wage

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

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

Identification now requires the timing of within-ZIP code experienced MW changes to be orthogonal with respect to dynamics of rent unobservables. Given that the experienced MW is constructed as a weighted average of the statutory MWs of close-by zipcodes, this restriction is similar to imposing unobserved determinants of rents in one zipcode to be orthogonal to MW changes in other zipcodes where its residents work.³² This restriction seems unquestionable for state-wide changes, since they tend to affect near-by ZIP codes in the same fashion, and thus are unlikely to be correlated with specific ZIP codes' unobservables. For local changes the assumption could be questioned if local policy-makers set MW changes at the same time as rents in places where MW workers reside are rising. Considering the hard-to-predict commuting patterns across zipcodes and the fact that MW changes tend to be set without consideration of housing market dynamics in general, we think this identifying assumption is likely to hold. On the other hand, because the weights are fixed over

³²They are not *exactly* equal because we use the natural logarithm of the experienced MW in our model.

time we are not worried about changes in commuting patterns that hamper identification. However, if those weights are inaccurately measured or change over time significantly following MW changes our estimations will be biased. For example, if MW workers move to places with low rents that are close to cities with regular MW changes the effect of experienced MW on rents will be understated by our model.

5 The Effect of Minimum Wage Changes on the Housing Market

In this section we...

5.1 Baseline Results

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

5.2 Robustness checks

Alternative rents measure Discuss results with SAFMR

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

5.3 The Heterogeneous Impact of MW Changes on Rents

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

6 Discussion

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

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

Discuss policy implications.

7 Conclusions

In this paper, we ask whether minimum wage changes affect housing rental prices. To answer this question we use rental listings from Zillow and MW changes collected from Vaghul and Zipperer (2016) and UC Berkeley Labor Center (2020), to construct a panel at the ZIP code-month level. The

high frequency and resolution of our data allows us to analyze state, county, and city-level changes in the MW to identify the causal impact of raising the MW on the local rental housing market.

To do that, we leverage on a panel difference-in-differences approach that exploits the staggered implementation and the intensity of hundreds of MW increases across thousands of ZIP codes. Our results indicate that minimum wage increases have a positive impact on rents, which we find to be robust to different specifications. Our models suggest that a 10 percent increase in MW causes rents to increase approximately by 0.26 percent in the same month, and 0.5 to 0.6 percent in the long run. We go beyond the average MW effect and we look at the heterogeneity of effects across ZIP codes. We show that rents disproportionately increase in ZIP codes where: (i) it is more likely to find MW workers as residents; (ii) there is a lower share of college graduates; (iii) a larger share of younger residents (15-24 yrs old); and (iv) a larger share of African-American residents.

We then leverage LODES data to create a measure of experienced MW that accounts for the difference between residence and workplace location of MW earners. We are able in this way to better track the changes in MW that residents of different ZIP codes experience, which allows us to (i) estimate an arguably more relevant treatment effect when using the experienced MW instead of the statutory one, and (ii) identify suggestive evidence of income transfers across ZIP codes to locations where MW workers leave.

To assess the magnitude of the effect of MW on housing rents, we perform benchmarking exercises to recover the income-to-rent pass-through associated with the causal estimates. We use both QCEW county-quarter wage data, and results from Cengiz et al. (2019) to first recover the average wage elasticity to the MW used to obtain compute the pass-through. All exercises consistently show that a share approximately between 19 and 28 percent of the additional income generated by MW policies end up captured by landlords via rents.

Our results highlights that place-based policies aimed at the labor market can also have significant impacts on other related markets. In particular, MW provisions are usually thought as a way to guarantee economic means to low income workers. However, they may also be benefiting landlords in ways that are unintended. In this sense, studying how place-based policies affect the housing market becomes an important step to better understand income inequality across U.S. neighborhoods.

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Table 1: Descriptive Statistics of Different Sets of ZIP Codes

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

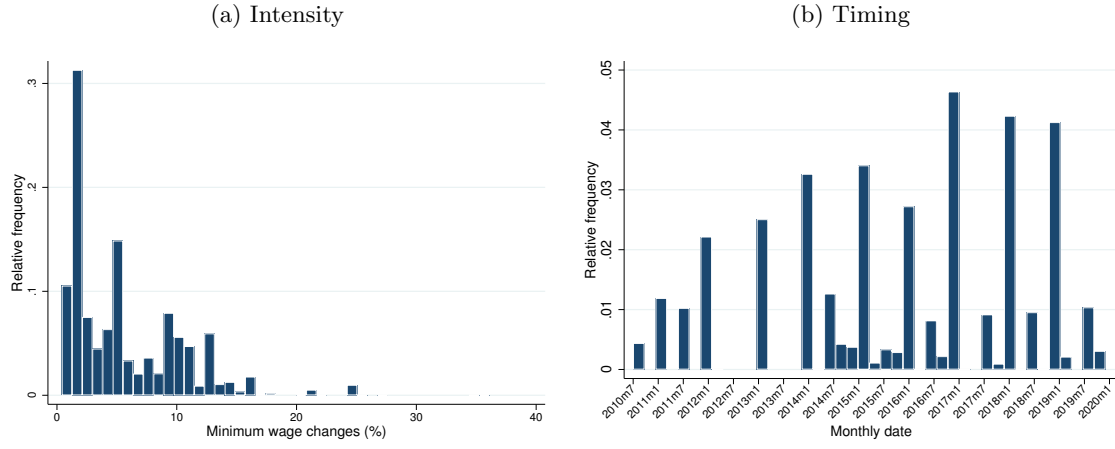
Notes: The table shows characteristics of four sets of U.S. postal service ZIP codes. Column 1 reports demographic statistics for the universe of USPS ZIP code we were able to map. Column 2 shows the same statistics for the top 100 Core-Based Statistical Areas (CBSA). Column 3 shows the characteristics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

Table 2: Descriptive Statistics of Estimating Panel

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

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

Figure 1: Distribution of Minimum Wage Changes



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

Appendix

A A dynamic supply and demand model

The geography is represented by a set of ZIP codes \mathcal{Z} . There is an exogenous given distribution of people with differing residence i and workplace z locations across these ZIP codes which, as in the main body of the paper, we denote by $\{L_{iz}\}_{i,z \in \mathcal{Z} \times \mathcal{Z}}$.

Let H_{it} be the stock of square feet rented in period t , where t is characterized by a month m and year y . This stock is composed of contracts starting at different calendar months. We assume that all contracts last for one year. We impose that $H_{it} \leq D_i$ for all t , where D_i denotes the total number of available square feet in ZIP code i .

We further decompose H_{it} as follows. Let $h_{izt} = h_{iz}(r_{it}, \underline{w}_{it}, \underline{w}_{zt})$ be the per-person demand of housing of group (i, z) in period t , which depends on the prevailing MW at the time of contract sign-up. Let λ_{it} denote the share of i 's residents who started their contracts in period t .³³ Then, we can write

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

where r_{it} represents rents *per square foot* in period t . We assume that all square feet are homogeneous and so they have the same price in the market.

Within-period equilibrium

Within this simple model, we assume the following timing: (1) A share λ_{it} of people's contracts expire (the ones who started 12 month ago); (2) The square feet from expiring contracts are added to the pool of available rental space; (3) Renters in t and a flow supply of rental space in t determine equilibrium rents r_{it} . Let's go by these steps more formally.

In every period, $\lambda_{it} L_{iz} h_{iz,t-12}$ 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

$$\lambda_{it} \sum_z L_{iz} h_{iz,t-12} + \mathbb{1}\{H_{it} < D_i\}(D_i - H_{it})$$

where $\mathbb{1}\{\cdot\}$ is the indicator function. We denote by $V_{it}(r_{it}, \lambda_t)$ the supply of housing, increasing in r_{it} . A feasibility constraint is that

$$V_{it}(r_{it}, \lambda_t) \leq \lambda_t \sum_z L_{iz} h_{iz,t-12} + \mathbb{1}\{H_{it} < D_i\}(D_i - H_{it}) \quad (9)$$

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

$$\lambda_{it} \sum_z L_{iz} h_{izt}(r_{it}, \underline{w}_{it}, \underline{w}_{zt})$$

³³For simplicity, we assume that these shares don't vary by workplace.

This demand arises because a share of the ZIP code's contracts expired. Those people go to the market and may desire to rent more square feet given changes in their income.

The market in period t clears if $\lambda_{it} \sum_z L_{iz} h_{izt} = V_{it}$. Dividing by the number of workers in the ZIP code L_i we can write this condition in per-capita terms:

$$\lambda_t \sum_z \pi_{iz} h_{iz} (r_{it}, \underline{w}_{it}, \underline{w}_{zt}) = v_{it}(r_{it}, \lambda_t) \quad (10)$$

where $v_{it} = \frac{V_{it}}{L_i}$.

Given minimum wages $\{\underline{w}_{it}\}_{i \in \mathcal{Z}}$, share of workers looking to rent in period t , λ_t , and a number of vacancies that satisfies (9), equation (10) determines equilibrium rents in period t . This condition has the same properties as equation (1) in the main. Thus, an analogue of Propositions 1 and 2 hold.

B The Rationale for Selecting Economic Controls

Throughout the paper we use wages, employment, and establishment-count from the QCEW to control for the local business cycle. However, those variables may themselves be impacted directly by changes in the MW. As a result, incorporating those controls raises the concern of a “bad control” problem (Angrist and Pischke 2009). In the scenario that the MW affects the controls used in the regression, this approach will not unveil the average treatment effect of the MW on rents even if the policy is known to be *randomly assigned*. The reason is that conditioning on these variables opens a potential alternative channel through which the MW affects rents, introducing bias in our estimates.

To avoid the “bad controls” problem, while at the same time include control variables that proxy for the local economic conditions, we select QCEW county-level time-series for the sectors that we think are unlikely to be affected by MW legislation: “Professional and Business Services”, “Information”, and “Finance”. Our interpretation of the effects of the MW on rents as causal relies on the assumption that these controls are not influenced by the MW. In this appendix we provide evidence in favor of this assumption.

We start by noting that these sectors employ a rather small portion of MW labor. According to U.S. Bureau of Labor Statistics (2020, table 5), in 2019 such industries accounted for 3.5, 1, and 1.2 percent of the total number of MW workers, respectively. These low percentages make direct impacts unlikely. However, these sectors may be influenced by MW legislation indirectly. We test this possibility by using a version of the dynamic model to analyze whether MW affects either wages, employment, or establishment-count in these sectors. The presence of significant pre-treatment trends would suggest that these sectors react to MW, and cast doubt on the identification strategy discussed in the paper.

For each industry, we observe employment at the county and month levels, and average weekly wages and establishment-count at the county and quarter levels. As a result, estimation of the dynamic model defined at the zipcode and month level is not straightforward. To be able to estimate our model, we aggregate MW zipcode-month information at the county-month level by taking, for each period, the weighted average of MW levels in each zipcode associated with a given county, using the number of housing units as weights. We call this our MW variable \underline{w}_{ct} , where (c, t) is a county and monthly date cell.³⁴

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

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

where y_{ct} is the outcome variable (employment) in county c and month t , and s defines the number of leads and lags in the model.

Because the QCEW provides average weekly wages and establishment-count at the county-quarter level, we estimate the model for these variables in quarterly averages of monthly obser-

³⁴For counties without city level ordinances this procedure would simply reflect the state- or county-level MW. For places with a MW at the city level our MW variable corresponds to a weighted average between places affected by the city MW and places not affected by it.

uations. We compute quarterly average of QCEW measures as $\overline{\Delta y_{cq}} = \frac{1}{3}\Delta y_{cq}$. As for MW data, we define \underline{w}_{cq} as the third month in each quarter q . We are then able to compute the quarterly average for MW changes, $\overline{\Delta \ln \underline{w}_{cq}} = \frac{1}{3}\Delta \ln \underline{w}_{cq}$. The estimating model then becomes

$$\overline{\Delta \ln y_{cq}} = \bar{\delta}_q + \sum_{r=\tau}^{\tau} \rho_r \overline{\Delta \ln \underline{w}_{c,q+r}} + \overline{\Delta \nu_{ct}}, \quad (12)$$

where τ defines the quarterly window. We interpret the ρ_r coefficients in Equation 12 as averages of the monthly coefficients β_r in Equation 11. The reason is that the average change over a quarter is a linear combination of monthly changes.³⁵

?? shows the results of our estimation for the three industries selected as controls. Even though some of the coefficients are significant, we interpret the results as suggestive of a noisy zero effect. The most worrisome sector is “Professional and Business Services”, which shows a significant same quarter coefficient of average weekly wages, and a slight negative pre-trend in employment. While we decided to keep this sector as control in our main models, we stress that our results are virtually identical when dropping it.

³⁵To see this, let's define q_1, q_2, q_3 as the first, second, and third months in a quarter q , respectively. Then, for any variable x ,

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

C Appendix Tables

Table C.1: Extended Descriptive Statistics of Estimating Panel

Placeholder

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

D Appendix Figures