Do Minimum Wages Increase Rents? Evidence from U.S. Zipcodes using High Frequency Data *

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Abstract

In this paper, we estimate the effect of minimum wage policies on housing rental prices. To do so, we construct a panel data set at the zipcode-month level using data from Zillow and state and local minimum wage changes between 2010 to 2019. Our baseline empirical approach assumes that, conditional on monthly date and zipcode fixed effects, unobservable determinants of median rents are mean independent of minimum wage changes. Results indicate that increasing the minimum wage 10 percent is associated with an increase of between 0.25 and 0.5 percent in median rents per square foot. We use several alternative empirical approaches and construct falsification tests that support a causal interpretation of this estimate. We show evidence that indicates the effect is driven by zipcodes where minimum wage workers reside. Further heterogeneity analysis indicates that the effect is larger in zipcodes with a high proportion of unemployed, African-American, and low-income households.

*We thank ...

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1 Introduction

In recent years, many US 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 employment effects are of first order importance to determine the welfare implications of MW changes on households, whereas income inequality proxies for an important dimension of the welfare implications of these policies. However, the *place-based* nature of MW provisions (accentuated by the fact that most recent legislation arises from local jurisdictions) 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 is tantamount to omitting from the analysis one of the main channels trough which these policies may affect welfare and inequality.

Given these remarks, we pose the question: by what extent (if any) are local rents affected by minimum wage policies? Surprisingly, there is very little research attempting to estimate the causal effect of minimum wages on the housing market. To the best of our knowledge, the only papers aiming at answering this question directly are Yamagishi (2019), Yamagishi (2020), and Tidemann (2018).[2] Even though they use the same data at the year-county for the U.S, these papers find opposing results. Tidemann's (2018) estimates are negative, whereas the estimation of Yamagishi (2019) indicates 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.

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 (2020) shows that minimum wage policies increase rents if disemployment effects are small, and that rents are a sufficient statistic of welfare under free mobility. We being our paper by constructing a simple model of a zipcode's rental market, and argue that the effect should be positive. We use the model to benchmark the magnitude of our empirical estimates. [UNDER CONSTRUCTION]

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

[1] As 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.

[2] Yamagishi (2019) explores this question using data from both the U.S. and Japan. In an updated version of the paper, Yamagishi (2020) excludes the analysis of the U.S. case.

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

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

In this paper, we construct a dataset at the U.S. zipcode and monthly date levels to explore the reduced form effects of MW changes on rents. Our main rent variable comes from Zillow, the largest real-estate company in the US, and corresponds to the median rent price per square foot across Zillow listings in the given zipcode-month cell of the category Single Family and Condos (SFCC). This is the most popular housing category in the US (CITE NEEDED), and also the most populated series in Zillow. However, a drawback is that only a subsample of all U.S. zipcodes are included in the data. We deal with the issue of representativeness of our sample in the paper. 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 these data, we construct the actual minimum wage in force in each zipcode and month. We also collect data from other sources to both validate our empirical model and to deploy as controls in our regressions, including the Quarterly Census of Employment and Wages (QCEW) and the Building Permits Survey (BPS). Finally, we use data from the U.S. Census and from the LEHD Origin-Destination Employment Statistics (LODES) to explore heterogeneity of the effect of interest.[5]

Estimating the effect of MW policies on rents presents several challenges. First of all, it appears plausible that determinants of local level MW changes might correlate with geographical and time factors also affecting the housing market, invalidating naive OLS regressions. To account for this, we use difference-in-differences (DiD) panel specifications that condition both on monthly date and zipcode fixed effects. We call this two-way fixed-effect model estimated in first differences the *static* model. Identification comes from exploiting the size and fine timing of hundreds of MW changes staggered across different US jurisdictions from 2010 to 2019. As a result, this specification does not suffer from the underidentification problem arising when units are treated only once (Borusyak and Jaravel 2017). As we discuss in the paper, this estimate recovers the true causal effect of MW changes on rents assuming that, within a zipcode, MW changes are *strictly exogenous* with respect to innovations in the error term. We note that this assumption allows for unrestricted auto-correlation of the error term, which we also cluster at the state-level.

Strict exogeneity imposes the restriction that past and future MW changes must be uncorrelated with innovations in unobservables. This assumption may not hold for several reasons. First of all, there might exist dynamic effects of the MW on rents, ruled out by the model. Secondly, the restriction embodies a "parallel trends" assumption in the time-path of treated and untreated zipcodes which may not hold in practice. Intuitively, if effects are being driven by some preexisting time-varying unobserved difference between treated and untreated zipcodes, we should see that future MW changes have an effect in the rental prices. On the other hand, if MW changes can be thought of exogenous with respect to the zipcode rental market (as assumed by our model), we should see no anticipatory effects. Motivated by this, we estimate a *dynamic* model by extending our static DiD model with leads and lags of minimum wage changes. This model allows us to test potential dynamics of the effect of interest, and to assess the parallel trends assumption. 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 of months following a MW increase.

We provide several other tests for the validity of our identification strategy. First of all, we check for the presence of unobservables affecting both rents and MW changes in two ways: (i) we

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

allow for zipcode-specific linear and quadratic time trends; and (ii) we include time-varying controls that proxy for local economic shocks as well as shocks to the housing market which, as we stress in the paper, are unlikely to be influenced by our minimum wage variable. 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 changes. Secondly, our rents variable is constructed as the median rent across available listings in the month, with many of them staying in the sample for more than one month. This introduces auto-correlation in the dependent variable which, if not accounted for, may bias our estimates. Thus, 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. At the cost of imposing a particular auto-correlation structure in the error term, this specification has the advantage of allowing for feedback effects from current shocks to future minimum wage changes (Arellano and Honoré 2001). The estimates of this model are strikingly similar to our baseline results, rendering credibility to our econometric assumptions.

Finally, the fact that we observe a sub-sample of all US zipcodes introduces two concerns. First, the composition of zipcodes 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 zipcodes, 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 zipcodes, obtaining similar results. Secondly, we worry that our estimated effect may be well-identified but also be particular to our sample. In order to approximate the average treatment effect for the typical urban zipcode, we re-estimate our models after reweighting our data to match average demographic characteristics of the most import urban centers. Our effects not only survive this test, but are bigger in magnitude and more precisely estimated.

Our results indicate a small yet robust impact of MW changes on rents. 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 find a statistically significant impact in the first two months following a MW change. As a result, a 10 percent increase in the minimum wage is estimated to rise rents in 0.45 percent.

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 the distribution of zipcode characteristics. The results suggest that the effect of interest is indeed heterogeneous. Those zipcodes which are more likely to have minimum wagers as residents –with a highest highest share of unemployed workers, lower-income households, and a larger share of African-American population– experience a pass-through which is almost twice as large. Consistently, we show that zipcodes with very low probability of having minimum wage workers as residents exhibit no significant effects. On the other hand, we find that the effect is constant across zipcodes with different share of MW workers who work there.

Our approach has several differences with respect to previous research on the topic. Both Tide-

[6]Because these zipcodes enter the sample at different moments in time before 2015, our estimating panel is still unbalanced.

mann (2018) and Yamagishi (2019) for the U.S. use 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. When using variation arising from a yearly frequency some units are "partially treated" which will tend to understate the magnitude of the effect.

Another advantage is that we use we use data at the zipcode- instead of the county-level, which are much finer.[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 zipcode; 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 zipcode with low-skill jobs to increase and demand for housing in the rest of the zipcodes 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 zipcodes were the low skill jobs are located are increasing. Indeed, Tidemann (2018) found 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 zipcode fixed effects and zipcode-specific linear and qudratic trend. This is important because the dynamics of the rental market plausibly vary across zipcodes within a county following trends at the very local level (Almagro and Domınguez-Iino 2019). Importantly, these controls make the required identification assumptions more credible. Given that the identifying variation comes from within-zipcodes, the determinants of these MW changes are unlikely to be related to the particular zipcode and, therefore, 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 minimum wages 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.

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 Domınguez-Iino 2019; Couture et al. 2019). We hope to contribute by adapting this framework to the case of the MW changes as a means to rationalize through residential location sorting part of the observed reduce form effect on rents.

[7] Yamagishi (2019), updated in Yamagishi (2020), uses data at the year-prefecture level for the 47 Japanese prefectures.

[8] As of 2019 there were 3,142 counties and 39,295 meaningful zipcodes in the US. We exclude military and unique business zipcodes as they are irrelevant for the housing market.

[9] This is consistent with the findings of Card and Krueger (2000) and Cengiz et al. (2019), among others.

[10] As 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 the results from Card and Krueger (2000), Cengiz et al. (2019), and many others.

The rest of the paper is organized as follows. Section 2 motivates the paper with a simple 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 section 5, we present our main results. Section 6 discusses relevant policy implications, and section 7 concludes.

2 A simple model of the local rental market

We build a simple partial-equilibrium model of the rental market in a zipcode that illustrates the main mechanism we believe will drive our results. Later, in subsection 5.4 we use the model to benchmark our empirical results.

BRIEFLY DESCRIBE MODEL. BRIEFLY COMPARE WITH MODEL IN Yamagishi (2020).

2.1 Model set-up

We focus on the supply and demand of housing in a given zipcode. Consider an environment with an exogenously given continuum of households in each zipcode divided in two groups: minimum wage and non-minimum wage households (HH). The former are fully affected by the MW, whereas the latter are not affected at all.

On the supply side, we denote by H the continuous measure of housing units available for rent in the zipcode. We assume that units are homogeneous, and can be rented at the a rent of r. The supply of housing $H(r)$ is assumed to be increasing in rents r, so that $H'(r) > 0$.

Let us move to the demand side. Households receive monthly a income, which we denote by $\underline{w}$ and w for MW HH and non-MW households, respectively. Demand for housing is given by $\underline{H}(r, \underline{w})$ and $\overline{H}(r, w)$ for each household type. We make two standard assumptions on these objects: (i) the demand of housing is downward sloping (i.e., $\underline{H}_r(r, \underline{w}) < 0$ and $\overline{H}_r(r, w) < 0$); and (ii) the demand for housing is increasing in income (i.e., $\underline{H}_w(r, \underline{w}) > 0$ and $\overline{H}_w(r, w) > 0$)

2.2 Equilibrium and the elasticity of rents to the minimum wage

Equilibrium rents r^* are such that local housing supply is equated to local housing demand. Formally,

$$H(r) = \underline{H}(r, \underline{w}) + \overline{H}(r, w) \ .$$

We are interested in the elasticity of equilibrium rents r^* to the minimum wage $\underline{w}$, which we denote by ρ. The implicit function theorem applied on the above equation yields

$$\rho := \frac{d \ln r^*}{d \ln \underline{w}} = \frac{\underline{w} \ \underline{H}_w}{r \ H'(r) - r \ \underline{H}_r - r \ \overline{H}_r} \ , \tag{1}$$

where we denote partial derivatives with sub-indexes.

Note that, since $\underline{H}_r < 0$ and $\overline{H}_r < 0$, the above expression is always positive. When the MW increases the local housing market moves to a new equilibrium with higher rents. The magnitude of the elasticity is driven by the relative magnitudes of the earnings of minimum wage workers ($\overline{w}$) and rents (r), and the slopes of the different functions in equilibrium. For instance, a higher response of housing demand to the minimum wage change ($\underline{H}_w$) would result in a higher elasticity.

2.3 Extensions

Above, we assumed above that the measure of each type of households is exogenously given. However, people could move across zipcodes in response to a minimum wage change.[11] Allowing the measure of households to be endogenous would not alter the conclusions as long as the overall demand for housing increases after a MW hike. In this case, however, the overall effect will arise from MW households moving into the zipcode, and potentially some none-MW households leaving (as they face higher rents).

.... **IT WOULD BE NICE TO HAVE AN EXPRESSION FOR THIS CASE (WHICH COULD GO IN APPENDIX)**

Another extension would involve houses of different quality, since it's possible that MW households rent houses of lower quality on average.... **THINK ABOUT THIS**

3 Data and sample selection criteria

Our main data is a panel at the US postal service zipcode-month level from January 2010 to December 2019. This panel comes from five distinct sources.

First of all, our data contains MW changes at the federal, state, county, and city level.[12] Most of these changes come from Vaghul and Zipperer (2016) and **cengiz2019effect**, but we updated this data for the years 2017, 2018, and 2019. For each zipcode we assume that the prevailing MW at a given month is the maximum between the required by the federal, state, county, and city levels. We only use MW changes that are binding, so only changes that actually change the maximum. In our baseline panel, we use 5,301 MW changes at the zipcode-month level. These changes are constructed out of 166 state level changes and 229 county and city level changes.

Second, we use rent and house value data from properties listed in Zillow (Zillow 2020) in our sample period. 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.[13] Zillow provides the median rental and listing price (both total and per square foot) at which homes were listed on the platform. Time series are provided for different house types, and for different geographic aggregation level.[14] We choose to focus on USPS zipcode level monthly time series so to capture the local behavior of

[11] We are not extremely concerned about this possibility because most MW changes arise from the state in our sample. Therefore, they tend to be uniform across connected zipcodes.

[12] Note that federal level MW changes still could induce meaningful variation as it is binding in some zipcodes and not in others, so that identification does not come only from time series variation. However, the last federal MW increase was in 2009 so changes used in our estimates come from state, county, and city level.

[13] `https://www.zillowgroup.com/facts-figures/` (accessed on October 23rd, 2020).

[14] https://www.zillow.com/research/data/ provides more information on the data shared by Zillow. The availability of different time series changed over time, so not all series used for the analysis might be still available to download.

the housing market. Clearly, even within a single zipcode, there could be great heterogeneity in terms of house sizes and types, making it more difficult to assess the impact of local intervention. In an effort to minimize price variation coming from houses' characteristics, such as the number of bedrooms, we focus our main analysis on the single family, condominium and cooperative homes (SFCC) series. This is by far the series with the largest number of non-missing zipcode, as it covers the most common U.S. rental house types. In 2018, roughly a third of the nation's 47.2 million rental units were single-family homes, while another 43 percent was made up from buildings with 5 or more units (JCHS 2020). We then select – for all our analysis – *per square foot* variables: this allows us to reduce confonding variation based on supply-side factors such as land availability. A limitation in the use of Zillow data comes from the fact that we cannot observe the underlying number of houses listed for rent in a given month. Changes in the Zillow inventory therefore introduce additional variation in the reported median rental price.

In Table 1, we compare descriptive statistics for our data and for representative US aggregates from the 2010 Census and the 5 years 2008 ACS. Columns 1 and 2 report data for the whole universe of US zipcodes and for the top 100 US metropolitan areas respectively. In column 3 we show the complete set of Zillow data. Finally, in column 4 we restrict our sample by balancing the panel keeping fixed the number of zipcodes only using zipcodes that have complete SFCC rental data (baseline sample). Focusing on our preferred series, Zillow provides information on rents for 4,604 unique zipcodes accounting for 11.8 percent of the US zipcodes and 46.7 percent of the 2015 US population. The average median household annual income for those zipcodes is $64,289, 22.5 percent higher than the same figure for the average US zipcode, but it is slightly lower than the figure for the average zipcode in the top 100 metropolitan areas. Zipcodes in the baseline sample are more populous and slightly higher income than the average US zipcode. Zillow is a real estate company and as such it is present in more dynamic rental markets. Those markets have a higher share of urban population, a higher share of college students, and a higher share of house for rent that the average US zipcode. For these reasons, and given that we will show that our effects are driven by the lower income zipcodes in our sample, we interpret our estimates as a lower bound for the true average treatment effect.

To ensure that our data correctly captures the price evolution of the US rental market, we compare Zillow's median rental price with 5 Small Area Fair Market Rents (SAFMRs) series for houses with different number of bedrooms (0, 1, 2, 3, and 4 or more) coming from the US Department of Housing and Urban Development (2020). SAFMRs are calculated for zipcodes within metropolitan areas at a yearly level, and generally equal the 40th percentile of the rent distribution for that zipcode.[15] The yearly time series correlation between Zillow SFCC and all of the SAMFRs series is consistently above 90 percent. Single family houses, as well as condos and cooperative houses, are fairly loose categories and are therefore expected to vary in terms of the number of bedrooms they might have. For this reason, in Figure 1 we compare the Zillow SFCC series with a weighted combination of the different SAMFRs series.[16] 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

[15] For more information on how SAFMRs are calculated, see page 41641 of the Federal Register/Vol. 82, No. 169

[16] 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 previous year's share.) Finally, we weight SAFMR series using the aforementioned shares.

SAFMRs are based on the 40th percentile of the rent distribution. The two series however show similar trends, confirming that Zillow rental series indeed captures the dynamics of the U.S. rental prices.

Table 1: Descriptive statistics and comparison with representative zipcodes

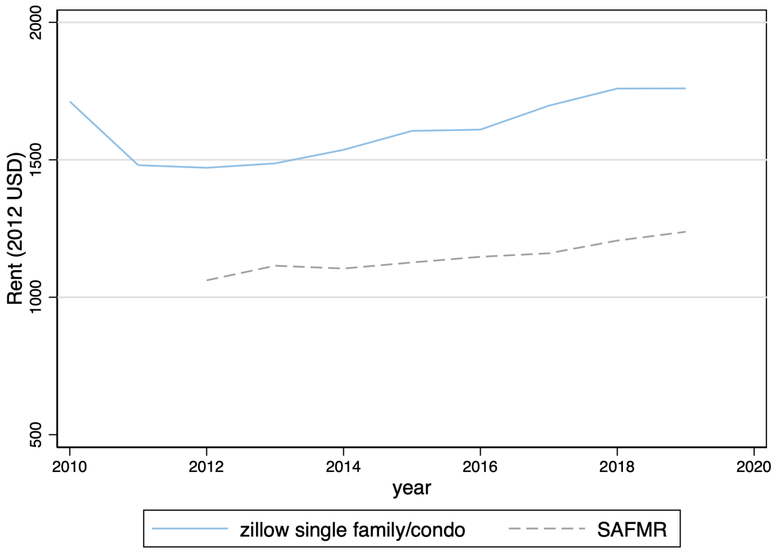
	U.S.	CBSA 100	Full Panel	Rent Panel
Zipcode	38,893	14,293	4,604	1,305
(%)	(1)	(0.367)	(0.118)	(0.034)
Population (Millions)	311.177	189.712	145.379	50.619
(%)	(1)	(0.61)	(0.467)	(0.163)
Housing units (Millions)	132.833	78.738	61.415	21.323
(%)	(1)	(0.593)	(0.462)	(0.161)
Median income (USD)	52,493	62,774	64,289	66,920
Houses for rent (%)	0.295	0.347	0.401	0.383
Urban population (%)	0.464	0.754	0.962	0.972
College Educated (%)	0.314	0.386	0.436	0.445
Black population (%)	0.086	0.124	0.145	0.166
Hispanic population (%)	0.097	0.136	0.17	0.192
Pop. in poverty (%)	0.154	0.143	0.143	0.133
Children 0-5 (%)	0.185	0.186	0.19	0.199
Elders 65+ (%)	0.15	0.13	0.124	0.11
Unemployed(%)	0.089	0.092	0.091	0.092
Work in same County (%)	0.701	0.684	0.755	0.756
State MW event (%)	.	.	0.862	0.875
County MW Event (%)	.	.	0.03	0.035
Local MW Event (%)	.	.	0.052	0.09
Median Rent psqft 2BR (USD)	.	.	1.775	1.975
(N)	.	.	(2,391)	(273)
Median Rent psqft MFR5PLUS (USD)	.	.	1.808	1.973
(N)	.	.	(3,365)	(417)
Median Rent psqft SFCC (USD)	.	.	1.479	1.275
(N)	.	.	(3,316)	(1,143)

Notes: The table shows average values for the full sample (column 3), and the restricted balanced samples, our baseline (column 4). In column 1 we report demographic statistics for the universe of USPS zipcode we were able to map. In column 2 we report demographic statistics for the top 100 CBSA. All demographic information comes from the 2010 Census and the 5-years 2008-2012 ACS.

Third, we add socio-demographic information to each zipcode in our sample using the 2010 Census and the 5-years 2008-2012 ACS. The data is originally obtained at the Census tract level and mapped into USPS zipcodes using HUD crosswalks.[17] We assign to each zipcode the following characteristics: number of inhabitants, the number of houses, the median income, the number of black inhabitants, the number of unemployed, and the number of college students. We use this information to classify zipcodes into, for example, high or low median income to then perform heterogeneity analysis. In addition, given that zipcodes can cross county borders, we use the census data and geographic codes to map each zipcode to a county by assigning it to the one with the highest share of houses from that zipcode. We also map each zipcode to a metropolitan statistical area or a rural town analogously. We use this information to assign the prevailing MW to each zipcode.

[17] Crosswalks are obtained from `https://www.huduser.gov/portal/datasets/usps_crosswalk.html`

Figure 1: National Time Series for Zillow and SAFMR data



Notes: The figure plots the monthly rent annual national average for the main Zillow series used in the analysis (SFCC) and a weighted combination of SAFMR series with different number of bedrooms. Weights are based on the US share of single family, condos and cooperative houses with given number of bedrooms as recorded in the AHS.

Fourth, to proxy for local economic activity we collect data from the Quarterly Census of Employment and Wages (QCEW) at the county-quarter and county-month level for each industry and level of government.[18] 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 zipcode-month panel based on county and quarterly date.

We add data from the *Building Permit Survey* (BPS) at the county-month level to account for time-varying shocks in the housing market. The BPS provides building permit statistics on new privately-owned residential construction disaggregated by house type. Lacking information on condos and cooperative houses, we only add the number of new units and the permits valuation for single family houses to each zipcode-month observation based on the county and month they belong.

Finally, we use data from the 2017 Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics (LODES) to proxy for MW workers' residence and workplace location. The LODES data sets provide block-level information on jobs and are organized in 3 groups: residence area characteristics (RAC), with information about characteristics of jobs for various types of workers (e.g. number of jobs in different sectors, number of job for workers under 30 years old, etc.); workplace area characteristics (WAC) that provide the same information as RAC files but aggregated with respect to workplace location; and a origin-destination matrix mapping jobs from residence to workplace locations. We use RAC and WAC datasets to "locate" workers likely to be MW by looking at the state-level distribution of such type of workers: we build, for each zipcode in the sample, the

[18]The QCEW covers the following industries: goods-producing; natural resources and mining; construction; manufacturing; service-providing; trade, transportation and utilities; information; financial activities; professional and business services; education and health services; leisure and hospitality. The QCEW additionally provides employment data for federal, state, and local government.

share (out of the state total) of workers under 30 years old earning less than \$1251 that either *live* or *work* there.

4 Empirical strategy and identification

In this section, we present the empirical strategy adopted to study the effect of MW on rents, and we discuss the assumptions needed for identification. We begin with a panel DiD model and we build on that following Meer and West (2016). This allows us to estimate the full dynamics of rents around MW changes under various identifying assumptions. Our dynamic specifications are distinct from the usual DiD and event-study ones (Borusyak and Jaravel 2017; Abraham and Sun 2018) for two main reasons: first, our models allow for the use of variation coming from more than one MW change per geographic unit and from geographic units that never experience a MW change. This is desirable because we both avoid under-identification issues with the two-way fixed effects and because we use never-treated zipcodes as control units. Secondly, our specifications not only exploit the timing of a MW change for identification but also its intensity.

4.1 Baseline Specifications

Consider the following panel difference-in-differences model relating rents and the minimum wage:

$$y_{it} = \alpha_i + \alpha_t + \gamma_i t + \beta \underline{w}_{it} + \epsilon_{it} \tag{2}$$

where y_{it} is the log rent per square foot for the Zillow SFCC series, $\underline{w}$ is the log of the minimum wage, α_i is a zipcode fixed effect, α_t is a time fixed effect, and γ_i is a zipcode-specific linear trend.[19] We then re-write equation (2) in first differences:

$$\Delta y_{it} = \theta_t + \gamma_i + \beta \Delta \underline{w}_{it} + \Delta \epsilon_{it} \tag{3}$$

We reference this model as *static DiD*. We spell out the model in first differences because we believe that the unobserved shocks to rental prices are likely to be persistent over time. Both the first differences and the level models are consistent under similar assumption but the first difference model is more efficient if the shocks are serially correlated (Wooldridge 2010).

Identification comes from assuming that within a zipcode the change in the level of the logarithm of the minimum wage is mean independent of the change in the unobserved shock $\Delta\epsilon_{it}$ conditional on the time fixed effects and the zipcode-specific linear trend. This implies that if the true effect is a one-time level change, then β has a causal interpretation and it can be seen as the elasticity of the rent per square foot to the MW.

One potential concern with the static DiD model, is that, despite controlling for a zipcode-specific linear trend, preexisting time-paths of rents per square foot might be different in zipcodes that had a MW change relative to zipcodes that did not experienced a change. To assess if that is the case,

[19] We add a zipcode-specific linear trend to allow for heterogeneity in the time path of zipcodes (Angrist and Pischke 2008). In the next section we additionally present results from models without zipcode-specific linear trends as well as with zipcode-specific quadratic trends.

one can extend the model to include leads of $\Delta \underline{w}_{it}$. In addition, one may be believe that the effect of MW changes on rents is not a one time discrete level jump but that it also affects the growth rate of rental prices. In such cases the estimated coefficient β from equation (3) might only have limited relevance in evaluating the policy of interest (Callaway and Sant'Anna 2019). To allow for dynamics in the effects, we extend the model to also include lags of $\Delta \underline{w}_{it}$. The *dynamic* model is

$$\Delta y_{it} = \theta_t + \gamma_i + \sum_{r=-s}^{s} \beta_r \Delta \underline{w}_{i(t-r)} + \Delta \epsilon_{it} \ , \tag{4}$$

where s is the number of months of a symmetric window around the MW change. Note that this dynamic DiD model still allows for treatment and control groups to have different averages, even though it now requires a more stringent identification:

$$E\left[\Delta \epsilon_{it} \Delta \underline{w}_{it-r} \middle| \theta_t, \gamma_i\right] = 0 \quad \forall r \in \{-s, ..., -1, 0, 1, ..., s\} \ .$$

In this context, a violation of the identification assumption would require a change in MW to be systematically correlated with unobserved shocks to treated zipcode relative to untreated ones. Importantly, this model allows us to test whether $\beta_{-s} = \beta_{-s+1} = ... = \beta_{-1} = 0$, the well known pre-trends test, to establish whether there are significant rent responses preceding a change in MW. Under the assumption of no pre-trends, we can gain efficiency through estimating a model only with distributed lags as follows:

$$\Delta y_{it} = \theta_t + \gamma_i + \sum_{r=0}^{s} \beta_r \Delta \underline{w}_{i(t-r)} + \Delta \epsilon_{it} \ . \tag{5}$$

This model allows us to estimate the dynamics of the logarithm of the rent per square foot around changes in the MW and we can recover the elasticity of rents to MW by summing β_0 to β_s. We present results from this model in the results section. In past settings using yearly data (Tidemann 2018; Yamagishi 2019), MW changes are so common in a given geographic area relative to the timespan of the data that it is very hard to credibly estimate the lags. Intuitively, this is the case because it is hard to distinguish which variation of the rental price is due to the current MW change or to a preceding one. In our estimates that concern is not justified, as given that we have month to month variation, we use short windows (5 months) in which there is no overlap in MW changes within a zipcode. The absence of pre-trends does not exhaust the potential threats to identification. Effects could still be driven by contemporaneous shocks systematically affecting both changes in rents and MW within a zipcode. To ease those concerns, we directly control for several county-level time-varying proxies of the health of the local labor and housing markets.[20]

As mentioned in section 3, part of the variation in the median rental price comes from unobserved changes in the Zillow inventory for a given zipcode through time. This may pose a threat to identification in the case which changes to MW directly affect the composition of rentals posted on the platform in a given zipcode-month period. Such concerns are partly mitigated by directly controlling for county-level time-varying housing market conditions, but we additionally investigate the issue by leveraging on the richer set of information Zillow provides on houses listed for sales. Specifically, we

[20]This amounts to adding a vector ΔX_{ct} on the right-hand side of our models, where c indexes counties, and we map zipcodes to a single county as explained in section 3.

can track the number of houses listed *for sale* in the selected zipcodes during the period 2013-2019 for our preferred house type (SFCC). We use such series to run a placebo regression where we estimate equation (3) and equation (4) using the (log) change in listings as outcome variable. Significant effects of MW changes, or pronounced pre-trends will indicate that policy changes actually affect the Zillow inventory composition and cast doubt on the identifying assumption.

Finally, in our appendix, we consider a dynamic panel specification to allow for full dynamics on the rental prices. The model then becomes

$$\Delta y_{it} = \Delta y_{i(t-1)} + \theta_t + \gamma_i + \sum_{r=0}^{s} \beta_r \Delta \underline{w}_{i(t-r)} + \Delta \epsilon_{it} \ . \tag{6}$$

However, by construction we now have that $\Delta y_{i(t-1)}$ is necessarily correlated with $\Delta\epsilon_{it}$. To address that, we take two separate approaches. First, we follow Arellano and Bond (1991) and, as it is customary in the literature, we instrument $\Delta y_{i(t-1)}$ with $\Delta y_{i(t-2)}$. Second, we follow Meer and West (2016) and instrument $\Delta y_{i(t-1)}$ with an off-window lag of the change in the logarithm of the MW. In particular, as most of our models have a window $s = 5$, we use as an instrument $\Delta \underline{w}_{i(t-6)}$. Intuitively, if there is an effect of MW changes to rents past MW changes should predict future rents and past MW changes should not be correlated with contemporaneous unobserved determinants of rents once we take into account the dynamic effect of MW on rents.

4.2 Heterogeneity by Zipcode Characteristics

In order to allow for heterogeneous effects based on zipcode characteristics, and to make sure that our effects are driven by the zipcodes that are expected to have more MW earners, we extend the baseline panel difference-in-differences model defined in equation (3) by interacting the local MW change with zipcode level characteristics. To minimize the possibility of any characteristic being endogenous to MW changes, we use use socio-demographic data that predate our panel. We take them from the 2010 Census and the 5-years 2008-2012 ACS. Then, the model we take to the data becomes

$$\Delta y_{it} = \theta_t + \gamma_i + \sum_{q=1}^{4} \beta_q \mathbb{1}\{i \in q\} \Delta \underline{w}_{it} + \Delta \epsilon_{it} \ , \tag{7}$$

where q identifies quartiles of some zipcode level characteristic, and $\mathbb{1}\{\cdot\}$ is the indicator function. We report results for these models in subsection 5.5.

5 Results

In this section we present our main results. In all cases standard errors are clustered at the state level so to match the main source of variation of the MW changes. We initially show how the estimated elasticity of rents to MW is approximately 0.025 when adopting the *static DiD* model. We then present estimates for the *dynamic* models that highlight both the absence of pre-trends, and the presence of a 2-months dynamic effect: the cumulative impact of a 10 percent MW change is estimated to be between 0.5 and 0.6 percent over the course of the 5 months after the policy change.

In subsection 5.2 we assess to what extent our results are representative of the true underlying Average Treatment Effect in two ways. First, since Zillow is present in relatively more dynamic markets than the average U.S. zipcode, we reweight observations so as to match population demographics for the top 100 CBSA. We show that the estimated impact slightly increases to 0.035 percent indicating how our estimates can be seen as a lower bound. Second, we expand the panel used for the estimation by including zipcodes "entering" after 2010. We account for changes in zipcode composition by controlling for *entry cohort* $\times$ *year-month* and we show how results are robust. We subsequently check for the presence of unobserved time-varying factors systematically affecting changes in MW and rents that may confound our estimates. We progressively include controls for the local economy, the labor and the housing market and we show how results do not change.

After establishing the robustness of our results, we then investigate how the incidence of the effect may vary across zipcodes. We use LODES data to proxy for MW workers residence and workplace location to show how effects disproportionately affect those zipcode that are more likely to have MW worker residents. We additionally estimate the heterogeneous impact of MW changes across the distribution of several census-based demographics. We show how effects are disproportionately concentrated in poorer, less-educated, and more African American zipcodes.

5.1 Baseline Results

In Table 2, we present results from the model defined in equation (3). In column 1, we show the classic two-way fixed effects (i.e. zipcode and year-month). To alleviate the concern that treated and untreated zipcodes could be on different time paths, in column 2 (our baseline static DiD specification) we introduce linear time trends at the zipcode level. Finally, in column 3, we relax the linearity assumption on the zipcode-specific trends and allow for a quadratic time path for each zipcode. The estimated coefficients for MW changes are stable and significant across all specifications and indicate that a 10 percent increase in MW leads to a 0.26 percent increase in rent.

Table 2: Results from Difference-in-Differences model

	(1)	(2)	(3)
$\Delta \ln(MW)_t$	0.0260**	0.0257**	0.0255**
	(0.0128)	(0.0120)	(0.0117)
Zipcode-specifc linear trend	No	Yes	Yes
Zipcode-specific quadratic trend	No	No	Yes
R-squared	0.022	0.024	0.026
Observations	112,232	112,232	112,232

Notes: The table reports coefficients from versions of equation (3) estimated on the balanced panel of zipcode-months that contains SFCC rental price. Column (1) does not include a zipcode-specific linear trend and results correspond to a two-way fixed effects difference-in-differences. Column (2) includes zipcode-specific linear trends, and column (3) allows for zipcode specific quadratic trends. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In order to test for the presence of pre-trends in rents that may invalidate the causal interpretation of our results, we estimate the model with leads and lags of the MW changes defined in equation (4). We display the results in Table 3 and, again, present the results allowing progressively for more flexible zipcode level rental price heterogeneity over time.

Consistent with a causal interpretation of our results, future MW changes do not have an effect

Table 3: Results from Difference-in-Differences model with leads and lags

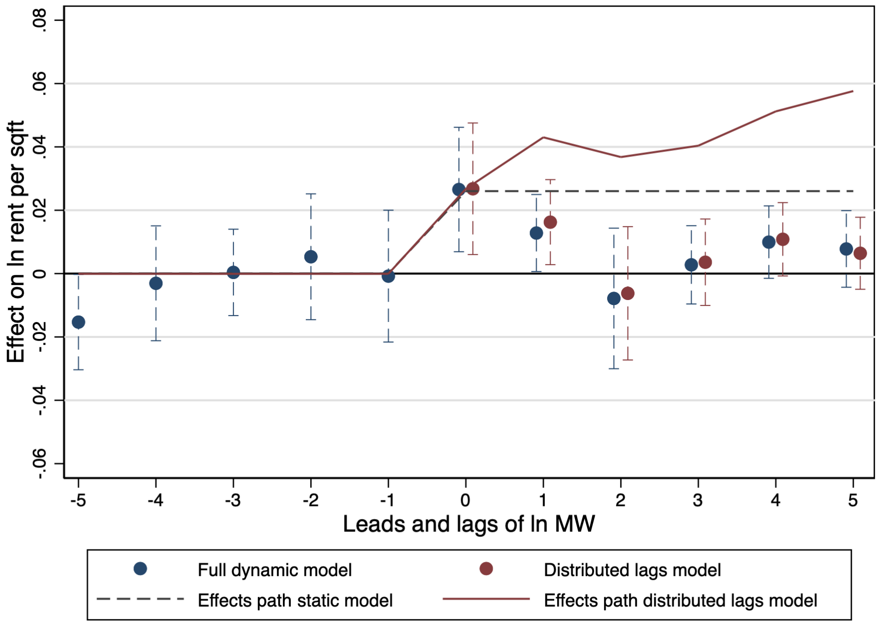
	(1)	(2)	(3)
$\Delta \ln(MW)_{t-5}$	-0.0148	-0.0153	-0.0157
	(0.00903)	(0.00915)	(0.00938)
$\Delta \ln(MW)_{t-4}$	-0.00237	-0.00306	-0.00382
	(0.0116)	(0.0110)	(0.0101)
$\Delta \ln(MW)_{t-3}$	0.00111	0.000380	-0.000214
	(0.00916)	(0.00829)	(0.00831)
$\Delta \ln(MW)_{t-2}$	0.00603	0.00531	0.00477
	(0.0116)	(0.0121)	(0.0115)
$\Delta \ln(MW)_{t-1}$	-0.000222	-0.000798	-0.00143
	(0.0123)	(0.0126)	(0.0142)
$\Delta \ln(MW)_{t}$	0.0271**	0.0265**	0.0260**
	(0.0126)	(0.0119)	(0.0110)
$\Delta \ln(MW)_{t+1}$	0.0136*	0.0128*	0.0118
	(0.00715)	(0.00739)	(0.00805)
$\Delta \ln(MW)_{t+2}$	-0.00702	-0.00785	-0.00884
	(0.0133)	(0.0135)	(0.0124)
$\Delta \ln(MW)_{t+3}$	0.00363	0.00277	0.00191
	(0.00808)	(0.00751)	(0.00812)
$\Delta \ln(MW)_{t+4}$	0.0108	0.00994	0.00918
	(0.00693)	(0.00695)	(0.00724)
$\Delta \ln(MW)_{t+5}$	0.00862	0.00778	0.00736
	(0.00687)	(0.00735)	(0.00717)
P-value no pretrends	0.568	0.581	0.602
Zipcode-specifc linear trend	No	Yes	Yes
Zipcode-specific quadratic trend	No	No	Yes
R-squared	0.022	0.024	0.027
Observations	106,446	106,446	106,446

Notes: The table reports coefficients from versions of equation (4) estimated on the balanced panel of zipcode-months that contains SFCC rental price. Column (1) does not include a zipcode-specific linear trend and results correspond to a two-way fixed-effects difference-in-differences. Column (2) includes zipcode-specific linear trends, and column (3) allows for zipcode specific quadratic trends. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

on rent prices. This suggests how there are no pre-treatment differentials in the evolution of rental prices between treated and untreated zipcodes. Table 3 additionally reports the results of an F-test for all leads to be jointly equal to zero. We comfortably fail to reject that hypothesis in all cases. On the other hand, we detect a significant effect on rents at the period of the MW change. Specifically, we estimate that rents increase by around 0.27 percent following a 10 percent raise in the MW (column 2), and the effect is largely unchanged both by the exclusion of linear trends (column 1) and by the inclusion of more flexible zipcode quadratic level trends (column 3).

A second important result shown by Table 3 is the presence of a mild persistence of the effect of

Figure 2: Estimated Impact of changes in MW on changes in Rents for Different Specifications



Notes: The plot shows the estimated coefficients for the *dynamic DiD* models estimated through equation (4) and (5) alongside 90 percent confidence intervals. The dashed line additionally reports the point estimate from the equation (3). The solid red line show the point estimates for the cumulative effects estimated through the distributed lags model in equation (5). Standard errors for both the *static* and the final period of the distributed lag models are reported in Table 2 and Table A.1 respectively.

MW changes on rents. After a 10 percent change in the MW, rents tend to increase by 0.13 percent in the month *after* the change, while the impact appears to vanish after the first two periods. In column 3 the estimated coefficients in $t+1$ loses statistical significance being slightly lower, but the point estimate remains larger than any of the following post-treatment periods. The results shown implies that - when allowing for dynamic effects of MW changes on rents - the cumulative impact is even larger than the one estimated by the static DiD model. Over the course of a semester, a 10 percent raise in the MW translates to between 0.5 and 0.6 percent increase in the rental price.

We summarize and compare the results from the *static* and the *dynamic* DiD models in Figure 2. The dashed line shows the effect path on rents implied by the point estimates (the standard error is omitted to avoid cluttering the figure) from the static DiD (equation 3). The blue-dot series plots the estimates from equation (4), where we can appreciate the absence of pre-trends and that the bulk of the effect is concentrated in the first two periods. We also report the estimated coefficients from the dynamic model defined in equation (5) (red-dot series), showing how the estimates mimic closely those found with the leads and lags model. Finally, with the continuous red line we show the cumulative effect of MW changes on rents implied by the red dots. As stated before, the effects implied by the dynamic models are larger than the ones implied by the static DiD.

To directly account for the presence of zipcode level rent dynamics, we further test our results by estimating a *dynamic* DiD model that controls for the lagged value of the changes in rents (equation 6). We compare that with our baseline estimates in Table A.2: columns 1, 2, and 3 show coefficients from equations (3), (4), and (5) respectively. In columns 4 and 5, we allow for full blown dynamics in the dependent variable and we recover the coefficients using instrumental variables following the classic **arellano1991some** approach –using deeper lags of the dependent variable to instrument for the lagged dependent variable– for the cases with and without leads. In columns 6 and 7 we show estimates from the model in equation 6 but instrumenting the lagged dependent variable with the sixth MW change lag as in **meer2016effects**. Our effects are robust to all of this stringent tests: the same-month change in rents following a 10 percent increase in MW is consistently estimated between 0.25 and 0.3 percent.

5.2 External Validity and Data Sensitivity

Our results suggest a noticeable impact of MW policies on the rental housing market. However, as explained in section 3, the number of zipcodes included in the final sample is only a small portion of the total U.S., and they come from more urban and richer neighborhoods that likely have a dynamic housing market. This limited sample size might hinder the external validity of the estimated effect. Additionally, the zipcodes included in the final sample are the ones appearing earlier in the Zillow data (i.e. zipcodes whose rent data are available since January 2010), and this might result in unobserved differences affecting sample selection.

We test the sensitivity with respect to our sample restrictions in two ways. First, we extend our panel by including the full set of zipcodes for which there is available rent data. This, on one hand, doubles the sample size (we now use the full $3,316$ zipcode in the Zillow rent data for single family, condos and cooperative houses), but, on the other hand, makes the composition of zipcodes vary over time by including, as they enter the sample, zipcodes whose time series start later than January 2010. Therefore, to fully exploit our data we estimate models using an unbalanced panel but controlling for "cohort $\times$ period" fixed effects. We do this for our main specifications in equations (3), (4), and (5). In this way, we are able to compare treated and untreated zipcodes with the same panel length. In Table A.3 we show that that the estimated effects for the different models remain widely unchanged. In Figure 3, panel (a) we compare *dynamic* DiD estimates obtained using the baseline sample and the unbalanced sample. Using the unbalanced panel, our estimates are slightly lower but they are largely identical to the baseline results.

Secondly, we assess the representativeness of our estimates by re-weighting zipcodes so as to match socio-demographic characteristics of the zipcodes in the top-100 CBSA. We do this by applying the entropy balancing procedure developed by Hainmueller 2012 on the following zipcode level demographics: share of rental houses, share of African-American residents, share of college graduates, and median income. We target averages from Table 1, column 2.[21] We subsequently re-estimate our models with weighted regressions.

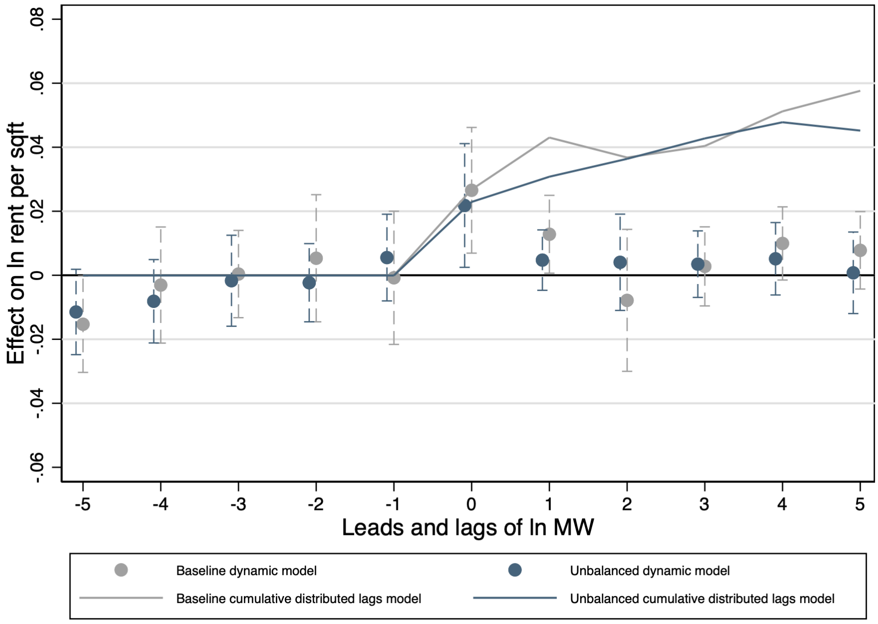
The results shown in Figure 3, panel(b) confirm what we found in our baseline case, although point estimates are somewhat higher. Note that the simultaneous effect from the *dynamic* DiD model presents the only statistically significant post-treatment coefficient. The effect in month $t+1$ becomes indeed smaller and not significant, suggesting how the baseline model might overestimate the persistence of the true average effect. A comparison with the *static* DiD estimate supports this finding: $\hat{\beta}$ from equation (3) and $\hat{\beta}_t$ from equation (4) are almost identical, identifying an elasticity

[21] The entropy balancing procedure consists of a re-weighting scheme that assigns a scalar weight to every unit such that the re-weighted sample matches moments of a target population. We implement this by leveraging the `STATA` package `ebalance` described in Hainmueller and Xu (2013).

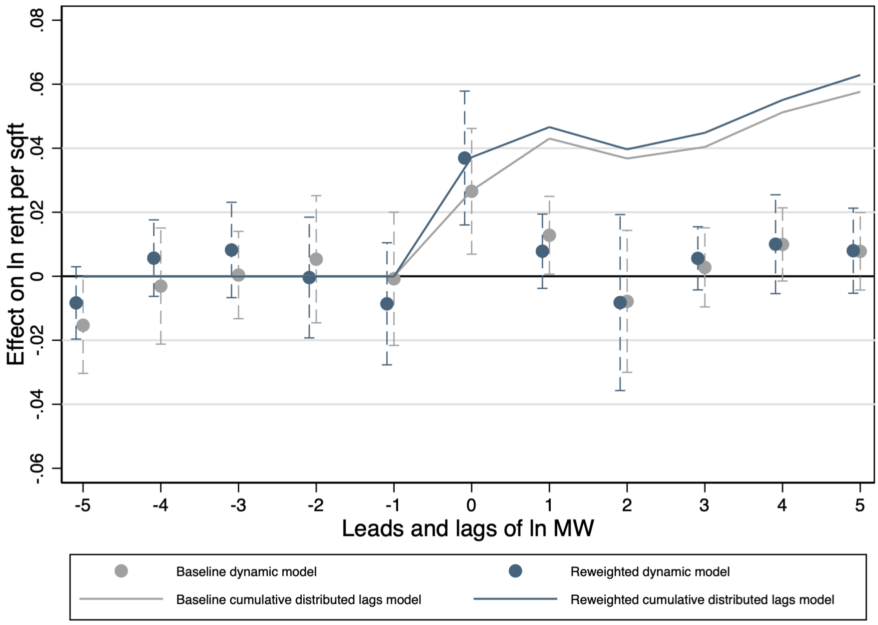
on rents of approximately 0.036 (Table A.4).

Figure 3: Comparison between dynamic DiD models

(a) Unbalanced panel



(b) Reweighted panel



Notes: The plot compares results obtained from the baseline panel summarized in Table 1, column 4 with those obtained from the full unbalanced panel (a), and with the reweighted baseline panel (b). Each subfigure compares estimated coefficients for the *dynamic DiD* models calculated through equation (4), and point estimates for the cumulative effect of MW on rents estimated through equation (5).

5.3 The Role of Unobserved Local Shocks

In subsection 5.1 we used equation (4) to establish the absence of significant pre-trends in rent dynamics. Another potential threat to the identification of the true causal effect might come from unobserved local shocks systematically affecting MW and rent changes. In order to account for that, we directly control for proxies of general economic shocks, as well as shocks related to the labor and housing markets aggregated at either the county-month or county-quarter level, while rents are defined at the zipcode-level. While this prevents us from studying the presence of zipcode-level time-varying confounding factors, it substantially strengthens the robustness of the estimated impact since the treatment is administered at city, county, or state level. In fact, if there are underlying factors affecting MW changes that also affect zipcode-level rents, they would likely arise from this larger geographic units.

Controls included in our regressions are the following. First, to account for local economic shocks, we use the county-quarter number of establishments by industry obtained from the QCEW (see section 3 for more details). We then proxy for local labor market dynamics with two sets of controls: county-quarter weekly average wage, and county-month employment by industry. Since we are using a first-difference specification, we augment each model with their log difference. Second, we proxy for shocks that may stem from the housing market using the county-month number of new permits for residential one-unit buildings and the associated permits' valuation. Since these two series already report changes between periods, we only control for the log levels.

In Table 4 we report the estimated coefficients for equation (4), progressively increasing the set of controls included in the regression. Column 1 replicates our baseline results (Table 3, column 2); columns 2 to 5 show estimated coefficients when adding all the aforementioned covariates. The estimated impact of MW changes remains substantially unchanged regardless of the set of controls used: we consistently observe that a 10 percent increase in MW causes a simultaneous increase in rents of approximately 0.26 percent. Only in column 5 we cannot reject the null hypothesis that $\hat{\beta}_t = 0$, as the points estimate slightly decreases while the smaller sample size leads to higher standard errors, but the coefficient on $t+1$ is also larger and significant. A quick comparison with leads and lags however clearly indicates the unchanged nature of our results. The inclusion of this relevant controls reveals the presence of a very mild pre-trend, however, we note that the joint F-test on all leads still fails to reject that they are all zero.

5.4 Benchmarking the elasticity estimates

In this subsection we use the model introduced in section 2 combined with some auxiliary assumptions to assess whether the magnitude of our estimates is plausible.

Assume that functions that characterize supply and demand of rental units are constant elasticity, so that $\underline{\gamma}$ and $\underline{\beta}$ are the elasticity of MW households demand to rents and income, $\overline{\gamma}$ and $\overline{\beta}$ are analogous parameters for non-MW households, and k is the elasticity of housing supply to rents.[22].

[22] More precisely, we assume that $\underline{H}(r, \underline{w}) = Ae^{\underline{\gamma} \ln r + \underline{\beta} \ln \underline{w}}$, $\overline{H}(r, w) = Be^{\overline{\gamma} \ln r + \overline{\beta} \ln w}$, and $H(r) = Ce^{k \ln r}$. $A, B, C > 0$ are constants.

Table 4: Results from Difference-in-Differences model with leads and lags and controls

	(1)	(2)	(3)	(4)	(5)
$\Delta \ln(MW)_{t-5}$	-0.0153	-0.0145*	-0.0145*	-0.0141*	-0.0161*
	(0.00915)	(0.00801)	(0.00794)	(0.00801)	(0.00858)
$\Delta \ln(MW)_{t-4}$	-0.00306	-0.000518	-0.000556	-0.000404	-0.00354
	(0.0110)	(0.00971)	(0.00971)	(0.00970)	(0.0101)
$\Delta \ln(MW)_{t-3}$	0.000380	-0.000904	-0.00132	-0.000722	-0.000523
	(0.00829)	(0.00869)	(0.00891)	(0.00857)	(0.00859)
$\Delta \ln(MW)_{t-2}$	0.00531	0.00257	0.00284	0.00319	0.00610
	(0.0121)	(0.0121)	(0.0122)	(0.0123)	(0.0138)
$\Delta \ln(MW)_{t-1}$	-0.000798	-0.00332	-0.00332	-0.00331	-0.00608
	(0.0126)	(0.0132)	(0.0132)	(0.0132)	(0.0150)
$\Delta \ln(MW)_{t}$	0.0265**	0.0270**	0.0274**	0.0262**	0.0208
	(0.0119)	(0.0121)	(0.0118)	(0.0114)	(0.0146)
$\Delta \ln(MW)_{t+1}$	0.0128*	0.0118	0.0122	0.0124	0.0154**
	(0.00739)	(0.00792)	(0.00818)	(0.00832)	(0.00646)
$\Delta \ln(MW)_{t+2}$	-0.00785	-0.00461	-0.00470	-0.00461	-0.00369
	(0.0135)	(0.0136)	(0.0137)	(0.0137)	(0.0148)
$\Delta \ln(MW)_{t+3}$	0.00277	0.00398	0.00395	0.00518	0.00469
	(0.00751)	(0.00746)	(0.00742)	(0.00709)	(0.00760)
$\Delta \ln(MW)_{t+4}$	0.00994	0.0112*	0.0115*	0.0104	0.0107
	(0.00695)	(0.00657)	(0.00655)	(0.00657)	(0.00633)
$\Delta \ln(MW)_{t+5}$	0.00778	0.00782	0.00785	0.00781	0.0107
	(0.00735)	(0.00795)	(0.00796)	(0.00789)	(0.00781)
P-value no pretrends	0.581	0.630	0.620	0.646	0.561
county-month industry-level employment	No	Yes	Yes	Yes	Yes
county-quarter industry-level establ. count	No	No	Yes	Yes	Yes
county-quarter industry-level weekly wage	No	No	No	Yes	Yes
county-month new house permits and value	No	No	No	No	Yes
R-squared	0.024	0.024	0.025	0.025	0.026
Observations	106,446	101,448	101,448	101,448	87,298

Notes: The table reports coefficients from equation (4) estimated on the balanced panel of zipcode-months that contains SFCC rental price. All specifications include zipcode linear trends. Column (1) replicates our baseline results from Table 3, column 2. Columns 2 to 5 progressively add sets of time-varying covariates that control for local shocks. Columns 2 to 4 add controls for the following industries: goods-producing; natural resources and mining; construction; manufacturing; service-providing; trade, transportation and utilities; information; financial activities; professional and business services; education and health services; leisure and hospitality. They additionally control for federal, state, and local government. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As a result, it can be shown that equation (1) takes the form

$$\rho = \frac{\underline{\beta}\ \underline{s}}{k - \underline{\gamma}\ \underline{s} - \overline{\gamma s}} \tag{8}$$

where $\underline{s} = \frac{\underline{H}}{H}$ is share of housing occupied by MW households and $\overline{s} = \frac{\overline{H}}{H}$ is the share of housing occupied by non-MW households. Note that $\overline{s} = 1 - \underline{s}$.

The above expression is intuitive, in the sense that factors which increase housing demand make the elasticity higher, whereas factors that increase supply lower it. For instance, a higher $\underline{\beta}$ –elasticity of housing demand to income– implies a higher ρ, whereas a higher k –elasticity of housing demand to rents– implies a lower ρ.

Suppose that the share of MW is $\underline{s} = 0.3$, so that $\overline{s} = 0.7$. Assume that demand elasticities and $(\underline{\gamma}, \overline{\gamma}, \underline{\beta}) = (-0.7, -0.5, 0.1)$, implying that MW households are more sensible to increases in rents, and that demand for housing is price-inelastic and a normal good. Finally, let $k = 0.1$, similar to estimate of **diamond2016determinants**. Substituting these values in (8) results in an elasticity of 0.45. This value turns out to be very close to the cumulative sum of our t and $t-1$ coefficients.

5.5 The Heterogeneity of MW Impacts on Rents

Our baseline results in subsection 5.1 have documented the presence of a causal impact of MW on rents, and the effect appears robust to multiple checks introduced in Sections 5.2 and 5.3. We now investigate the heterogeneity of such effect by characterizing zipcodes based on socio-demographic characteristics. The goal of this exercise is twofold: first, MW is a place-based policy targeted to a specific sub-population that does not necessarily live and work in the same zipcode. The presence of a significant effect in treated zipcodes does not reveal whether MW workers are actually bearing the burden of this increase, or if instead rents increase in those zipcodes where MW jobs are concentrated. We therefore try to answer the following question: do rents increase more where MW workers live, or where they work? second, independently from the incidence on MW workers, who are the winners and losers when rents increase due to new MW provisions? we look at zipcode characteristics to identify which sub-population ends up paying more in rents.

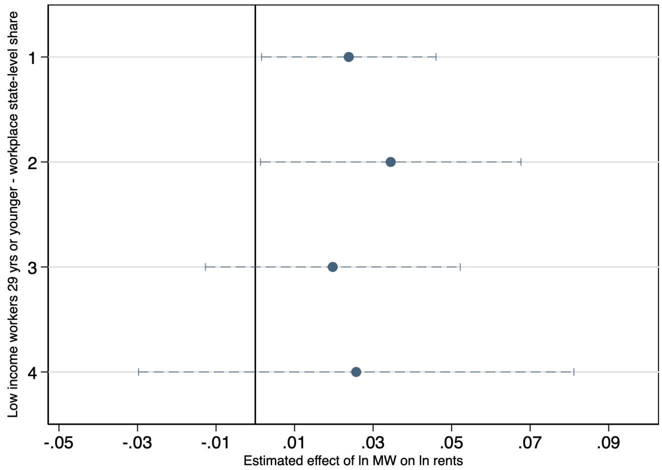
To answer the first question it requires to localize MW workers job and residence locations at the zipcode level. While direct data on this feature of zipcodes is not available, we build proxies based on the LODES data. Specifically, we use the 2017 files to compute the share (out of state totals) of low-income workers under 30 years old that either live or work in any given zipcodes (MW workplace and residence distribution henceforth).[23] Since the majority of the MW changes in our data are at the state-level, we calculate shares over state totals so that we are able to study the impact of this type of policy on the relevant distribution of low-income, young workers. While these proxies by definition include more than MW workers, **dube2016minimum** show how MW changes actually affect a larger part of the income distribution than just workers below MW thresholds (a statistically significant impact on wages is reported up to \$4 above the new MW thresholds). We then bin each state distribution into quartiles and use equation (7) to estimate the differential effect for each group.

In Figure 4 we plot the estimated coefficients for the interaction between changes in (log) MW and each quartile of the two distributions. Panel (a) presents results for MW workplace location. The point estimates are very similar, suggesting that the effect on rents is orthogonal to the geographic distribution of MW workplace. The coefficients for the first 2 quartiles are significant at the 10 percent level, but standard errors for the 3^{rd} and 4^{th} quartiles become very large partly due to a heavily right-skewed distribution of the underlying variable. In panel (b) we re-estimate equation (7) using the MW residence distribution. Here we do observe a different pattern: the point estimate in the lowest quartile is precisely zero, but this increases and becomes statistically significant both in the 2^{nd} and 3^{rd} quartile. Even more, the effect appears larger: a 10 percent increase in MW leads to a 0.5 percent increase in rents. The estimated effect for zipcodes with the highest share of young, low-income workers decreases to 0.3 percent and becomes not significant, but we notice how also

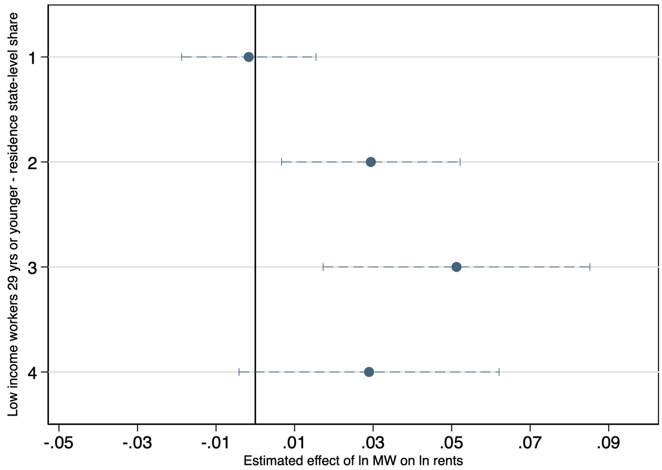
[23]See section 3 for more details on the construction of such variables.

Figure 4: Static DiD model: MW impact by workers job and residence location

(a) Workplace location



(b) Residence location



Notes: The Figure shows the estimated coefficients β_q, $q \in \{1, 2, 3, 4\}$ from equation (7) when differentiating zipcodes with respect to the share of MW workers that either work (a) or live (b) in each zipcode. Shares are taken over state totals. MW workers is used as a loose label for workers below 30 years old earning less than \$1250 month identified using the 2017 LODES datasets (see section 3 for more information). 90 percent confidence intervals reported.

the underlying MW residence distribution is heavily right-skewed, and this higher variance partially justify the lower precision in our estimates. Overall, this exercise shows how MW workers indeed seem to bear most of the impact with relatively higher rents in their place of residence.

The LODES-based proxies for MW workers are approximate by definition. We then turn to investigate how the impact of MW changes on rents differs across the distribution for different census-based zipcode demographics. The Bureau of Labor Statistics reports how MW workers tend to be young and less educated.[24] The study of heterogeneous effects by demographics can therefore help both in confirming what the LODES-based measures indicate, and in uncovering additional

[24]See, for example, BLS Report 1085, *Characteristics of Minimum Wage Workers 2019* at `https://www.bls.gov/opub/reports/minimum-wage/2019/home.htm`

patterns of the effects under study.

Table 5 shows the estimated coefficients for the interaction between changes in (log) MW and quartiles of the distribution for several demographics. In column 1 we show how the effect disproportionately impact zipcodes in the lowest quartile of the median income distribution: the estimated elasticity of rent to MW is 0.039 (s.e. 0.022). The effect on the other quartiles becomes not significant and it shows a non-monotone behavior in the 2^{nd} and 3^{rd} quartiles. When looking at the richest neighborhoods however, we have a markedly smaller and imprecise estimate. In column 2 we focus on the zipcode-level unemployment rate. Not surprisingly, we find that the strongest effect is localized in the 4^{th} quartile of the distribution, 0.045 (s.e. 0.017). Estimates lose significance in the remaining part of the distribution: similarly to column 1, we find not-significant not-monotone estimates in the middle quartiles, while the effect is a clear zero in the bottom quarter. In column 3 we look at the share of college graduates, and the estimates confirm that indeed lower educated neighborhoods bear the bulk of the rent increase: there is a clear divide between above median zipcodes showing zero and not significant effects, and below median ones where a 10 percent increase in MW leads to a 0.47 and a 0.37 percent rent increase for the 2^{nd} and 1^{st} quartiles, respectively. Lastly, in column 4 we show the impact across the distribution over share of African-American residents. Similarly to column 3, we do find a stark contrast between above and below median zipcodes. The effect of MW changes on rents monotonically increases starting from a not significant effect of 0.017 in the 1^{st} quarter to a statistically significant 0.042 in the 4^{th} one.

Table 5: Heterogeneity Results - static DiD model

	(1) Median Income	(2) Unemp. rate (%)	(3) College Grad. (%)	(4) African Am. (%)
$\Delta \ln(MW) \times 1^{st} qtl$	0.0395*	0.00357	0.0373*	0.0178
	(0.0223)	(0.0100)	(0.0196)	(0.0163)
$\Delta \ln(MW) \times 2^{nd} qtl$	0.0202	0.0355	0.0473**	0.0218
	(0.0144)	(0.0228)	(0.0222)	(0.0163)
$\Delta \ln(MW) \times 3^{rd} qtl$	0.0304	0.0269	0.0258	0.0231*
	(0.0252)	(0.0248)	(0.0214)	(0.0133)
$\Delta \ln(MW) \times 4^{th} qtl$	0.0133	0.0452**	-0.000369	0.0419**
	(0.0130)	(0.0173)	(0.0116)	(0.0164)
R-squared	0.024	0.024	0.024	0.024
Observations	112,232	112,232	112,232	112,232

Notes: The table reports estimates for β_q, $q = \{1, 2, 3, 4\}$ from equation (7) when differentiating zipcodes based on several socio-demographics from the 2010 Census and the 5-year 2008-2012 ACS. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6 Discussion

TO ADD

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 **vaghul2016historical**, **cengiz2019effect** and our ourselves, to construct a panel at the zipcode-month level. We exploit state, county, and city-level changes in the MW to identify the causal impact of increasing the MW. 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 zipcodes. Our results indicate that minimum wage increases have a small but significant positive impact on rents that is robust to many alternative explanations. Across most specifications, a 10% percent increase in MW causes on average an increase of 0.03% percent of the rental prices. The effect is largely concentrated in the first two months of the MW change. We go beyond the average MW effect and we look at the heterogeneity of effects across zipcodes. We show that rents disproportionately increase in zipcodes where: (i) it is more likely to find MW workers as residents, (ii) there is higher unemployment rate, and (iii) a larger share of African-American residents. 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 but 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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Online Appendix

A Appendix Tables

Table A.1: Dynamic DiD: cumulative effect over 6 months

	(1)	(2)	(3)
Sum of MW effects	0.0567	0.0520	0.0474
	(0.0346)	(0.0335)	(0.0301)
Zipcode-specifc linear trend	No	Yes	Yes
Zipcode-specific quadratic trend	No	No	Yes
Observations			
N	106,446	106,446	106,446

Notes: The table shows estimates for the cumulative impact of MW (log) changes on (log) rents changes over 6 months. Coefficients are obtained by taking the sum of $\hat{\beta}_r$, estimated via equation (5): $\sum_{r=0}^{5} \hat{\beta}_r$. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: Results from different dynamic models

	(1) DiD	(2) Distributed leads and lags	(3) Distributed Lags	(4) AB Distributed leads and lags	(5) AB Distributed Lags	(6) MW Distributed leads and lags	(7) MW Distributed Lags
$\Delta \ln(MW)_{t-5}$		-0.0153		-0.0131		-0.0187	
		(0.00915)		(0.00982)		(0.0184)	
$\Delta \ln(MW)_{t-4}$		-0.00306		0.00553		-0.0138	
		(0.0110)		(0.0101)		(0.0416)	
$\Delta \ln(MW)_{t-3}$		0.000380		0.00244		-0.00192	
		(0.00829)		(0.00889)		(0.0183)	
$\Delta \ln(MW)_{t-2}$		0.00531		0.00631		0.00476	
		(0.0121)		(0.0141)		(0.0109)	
$\Delta \ln(MW)_{t-1}$		-0.000798		-0.00484		-0.000476	
		(0.0126)		(0.0153)		(0.0156)	
$\Delta \ln(MW)_{t}$	0.0257**	0.0265**	0.0268**	0.0299*	0.0296*	0.0254***	0.0267**
	(0.0120)	(0.0119)	(0.0126)	(0.0152)	(0.0160)	(0.00881)	(0.00991)
$\Delta \ln(MW)_{t+1}$		0.0128*	0.0162*	0.00118	0.00441	0.0314	0.0301
		(0.00739)	(0.00816)	(0.00796)	(0.00823)	(0.0590)	(0.0493)
$\Delta \ln(MW)_{t+2}$		-0.00785	-0.00623	-0.0128	-0.0132	0.000149	0.00192
		(0.0135)	(0.0128)	(0.0128)	(0.0121)	(0.0314)	(0.0332)
$\Delta \ln(MW)_{t+3}$		0.00277	0.00359	0.00691	0.00695	-0.00266	0.000571
		(0.00751)	(0.00830)	(0.00753)	(0.00732)	(0.0190)	(0.0143)
$\Delta \ln(MW)_{t+4}$		0.00994	0.0108	0.00933	0.00948	0.0109	0.0123
		(0.00695)	(0.00704)	(0.00760)	(0.00736)	(0.0108)	(0.0120)
$\Delta \ln(MW)_{t+5}$		0.00778	0.00641	0.00416	0.00200	0.0128	0.0112
		(0.00735)	(0.00691)	(0.00909)	(0.00882)	(0.0168)	(0.0169)
$\Delta \ln(y)_{t-1}$				0.424***	0.439***	-0.663	-0.500
				(0.0236)	(0.0230)	(1.913)	(1.542)
Observations	112232	106446	112161	104208	109923	105303	111018

Notes: The table presents baseline estimates obtained from equation (3), (4), and (5) in columns (1), (2), and (3) respectively. Column (4) allows for rental price dynamics by adding the lagged change in (log) rents, $\Delta y_{i(t-1)}$, and it is estimated instrumenting it with a deeper lag $\Delta y_{i(t-2)}$ (**arellano1991some**). Similarly, column (5) solves for equation (6) where only lags of MW changes are allowed. Finally, columns (6) and (7) instrument $\Delta y_{i(t-1)}$ with an off-window lag of the MW (log) change, $\Delta MW_{i(t-6)}$ for the leads and lags, and lags only versions of the model. All specifications additionally control for a zipcode-level linear trend. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3: Comparison between unbalanced and baseline panel model estimation

	Unbalanced Panel			Baseline Panel		
	(1) DiD	(2) Distributed leads and lags	(3) Distributed Lags	(4) DiD	(5) Distributed leads and lags	(6) Distributed Lags
$\Delta \ln(MW)_{t-5}$		-0.0115			-0.0153	
		(0.00810)			(0.00915)	
$\Delta \ln(MW)_{t-4}$		-0.00812			-0.00306	
		(0.00792)			(0.0110)	
$\Delta \ln(MW)_{t-3}$		-0.00171			0.000380	
		(0.00864)			(0.00829)	
$\Delta \ln(MW)_{t-2}$		-0.00233			0.00531	
		(0.00743)			(0.0121)	
$\Delta \ln(MW)_{t-1}$		0.00552			-0.000798	
		(0.00823)			(0.0126)	
$\Delta \ln(MW)_{t}$	0.0220*	0.0218*	0.0229*	0.0257**	0.0265**	0.0268**
	(0.0111)	(0.0117)	(0.0115)	(0.0120)	(0.0119)	(0.0126)
$\Delta \ln(MW)_{t+1}$		0.00473	0.00788		0.0128*	0.0162*
		(0.00574)	(0.00586)		(0.00739)	(0.00816)
$\Delta \ln(MW)_{t+2}$		0.00405	0.00558		-0.00785	-0.00623
		(0.00915)	(0.00772)		(0.0135)	(0.0128)
$\Delta \ln(MW)_{t+3}$		0.00347	0.00638		0.00277	0.00359
		(0.00632)	(0.00636)		(0.00751)	(0.00830)
$\Delta \ln(MW)_{t+4}$		0.00515	0.00505		0.00994	0.0108
		(0.00688)	(0.00684)		(0.00695)	(0.00704)
$\Delta \ln(MW)_{t+5}$		0.000767	-0.00261		0.00778	0.00641
		(0.00774)	(0.00800)		(0.00735)	(0.00691)
Observations	194295	177659	194209	112232	106446	112161

Notes: The table compares estimates from our main specifications (*static DiD*, *distributed leads and lags DiD*, and *distributed lags DiD*) obtained using the baseline sample with estimates obtained using the unbalanced, full sample of zipcodes. Columns (1), (2), and (3) show results from from equation (3), (4), and (5) respectively, using the unbalanced sample. All three columns additionally control for "cohort × period" FE to account for differences in the each zipcodes time series. Columns (4), (5), and (6) replicates our main results obtained with the baseline sample and presented in **??**, column (2), **??**, column (2) and Table A.2. All specifications control for zipcode-level linear trends. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

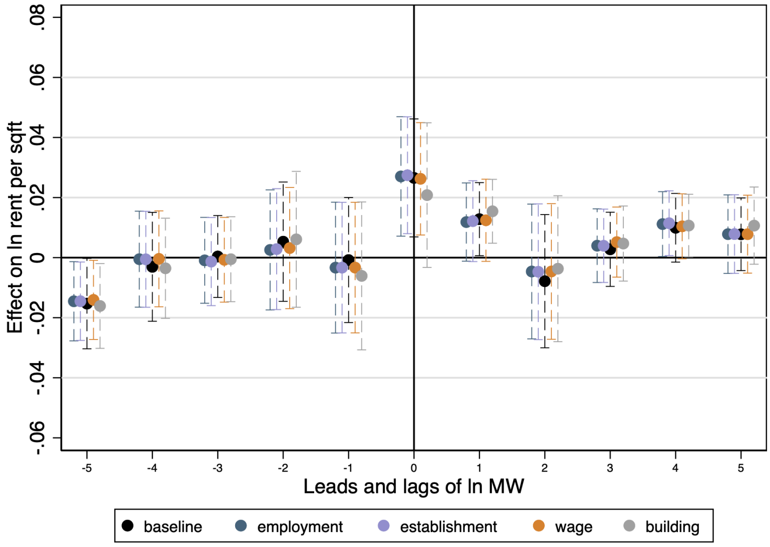
Table A.4: Comparison between baseline and re-weighted panel model estimation

	Reweighted Panel			Baseline Panel		
	(1) DiD	(2) Distributed leads and lags	(3) Distributed Lags	(4) DiD	(5) Distributed leads and lags	(6) Distributed Lags
$\Delta \ln(MW)_{t-5}$		-0.00832			-0.0153	
		(0.00687)			(0.00915)	
$\Delta \ln(MW)_{t-4}$		0.00566			-0.00306	
		(0.00726)			(0.0110)	
$\Delta \ln(MW)_{t-3}$		0.00821			0.000380	
		(0.00905)			(0.00829)	
$\Delta \ln(MW)_{t-2}$		-0.000403			0.00531	
		(0.0115)			(0.0121)	
$\Delta \ln(MW)_{t-1}$		-0.00860			-0.000798	
		(0.0116)			(0.0126)	
$\Delta \ln(MW)_{t}$	0.0365***	0.0369***	0.0372***	0.0257**	0.0265**	0.0268**
	(0.0124)	(0.0127)	(0.0132)	(0.0120)	(0.0119)	(0.0126)
$\Delta \ln(MW)_{t+1}$		0.00782	0.00942		0.0128*	0.0162*
		(0.00706)	(0.00730)		(0.00739)	(0.00816)
$\Delta \ln(MW)_{t+2}$		-0.00822	-0.00694		-0.00785	-0.00623
		(0.0167)	(0.0160)		(0.0135)	(0.0128)
$\Delta \ln(MW)_{t+3}$		0.00560	0.00516		0.00277	0.00359
		(0.00600)	(0.00693)		(0.00751)	(0.00830)
$\Delta \ln(MW)_{t+4}$		0.0100	0.0103		0.00994	0.0108
		(0.00939)	(0.00935)		(0.00695)	(0.00704)
$\Delta \ln(MW)_{t+5}$		0.00798	0.00781		0.00778	0.00641
		(0.00808)	(0.00870)		(0.00735)	(0.00691)
Observations	112,232	106,446	112,161	112,232	106,446	112,161

Notes: The table compares estimates from our main specifications (*static DiD*, *distributed leads and lags DiD*, and *distributed lags DiD*) obtained using the baseline sample with estimates obtained using the reweighted sample (see subsection 5.2 for more details on how the weights are built). Columns (1), (2), and (3) show results from from equation (3), (4), and (5) respectively, using the unbalanced sample. All three columns additionally control for "cohort × period" FE to account for differences in the each zipcodes time series. Columns (4), (5), and (6) replicates our main results obtained with the baseline sample and presented in **??**, column (2), **??**, column (2) and Table A.2. All specifications control for zipcode-level linear trends. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Appendix Figures

Figure B.1: Dynamic DiD model comparison - local shocks



Notes: The figure show estimates for $\hat{\beta}_r$ obtained from equation (4) when progressively adding time-varying controls for local shocks. The *baseline* series plots coefficients taken from Table 4, column (1). The *employment*, *establishment*, *wage*, and *building* series plot coefficients from Table 4, columns (2) to (5) respectively. 90 percent confidence intervals reported.