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 uncorrelated with past and future 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.

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 the MW may affect welfare and inequality.

Given these remarks, we pose the question: by what extent (if any) are local rents affected by the minimum wage? Surprisingly, there is very 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), Yamagishi (2019), and Yamagishi (2020).[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 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.

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 begin our paper by constructing a simple model of a zipcode's rental and labor markets, 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 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 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.

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[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 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 zipcode-month cell of the category Single Family, Condominiums and Cooperative Houses (SFCC). This is the most popular housing category in the US (JCHS 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 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. A priori, it appears plausible that determinants of local level MW changes might correlate with geographical- and time-varying 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 term this two-way fixed-effects model estimated in first differences the *static DiD* 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 under-identification 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 changes 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, there might exist dynamic effects of the MW on rents, ruled out by assumption in the static DiD model. Second, strict exogeneity amounts to a "parallel trends" assumption in the time-path of treated and untreated zipcodes which may not hold in practice. Intuitively, if effects are driven by some preexisting time-varying unobserved difference between treated and untreated zipcodes, we should see that future MW changes have an effect on rental prices. On the other hand, if MW changes can be thought as 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 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 months following a MW increase.

We validate our empirical strategy by putting our basic model through several tests. First of all, we check for the presence of unobservables affecting both rents and MW changes in two ways: (i) we allow for zipcode-specific linear and quadratic time trends; and (ii) we include controls that

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

proxy for local economic shocks as well as shocks to the housing market that 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. Second, 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. 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. 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, a drawback of the Zillow data is that it includes a subsample of all U.S. zipcodes only. This fact brings about 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 particular to our subsample. In order to approximate the average treatment effect for the typical urban zipcode, we reweight 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 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 –i.e., zipcodes with a relatively high shares of unemployed workers, low-income households, and 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-mann (2018) and Yamagishi (2019) for the U.S. exploit Fair Markets Rents data from the Department

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

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 data at the zipcode- 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 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 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 zipcode fixed effects and zipcode-specific polynomial trends. 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.

The rest of the paper is organized as follows. Section C motivates the paper with a simple model of the rental market. In section 2, we present our data sources and show the characteristics of

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[7]Yamagishi (2019), updated in Yamagishi (2020), also 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 results of null employment effects in the literature.

our estimating panel. In section 3, we explain our empirical strategy and we discuss our identification assumptions. In section 4, we present our main results. Section 6 discusses relevant policy implications, and section 7 concludes.

2 Data and sample selection criteria

We put together a panel dataset at the US postal service zipcode and monthly date level from January 2010 to December 2019 which contains data on minimum wages, rents and several other variables we use in the analysis. More precisely, our data comes from five distinct sources.

First of all, our data contains MW changes at the federal, state, county, and city level.[11] The main source of data for these changes is Vaghul and Zipperer (2016) up to mid-2016. We extend these data for the years 2017, 2018, and 2019 mainly from Center (n.d.). 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.[12] We only use MW changes that are binding, meaning that they actually impact that maximum. Our baseline estimating panel collects 5,301 MW changes at the zipcode-month level, which arise from 166 state-level changes and 229 county- and city-level changes (Figure 1).

Figure 1: Minimum Wage Changes Distribution

(a) Intensity (b) Timing
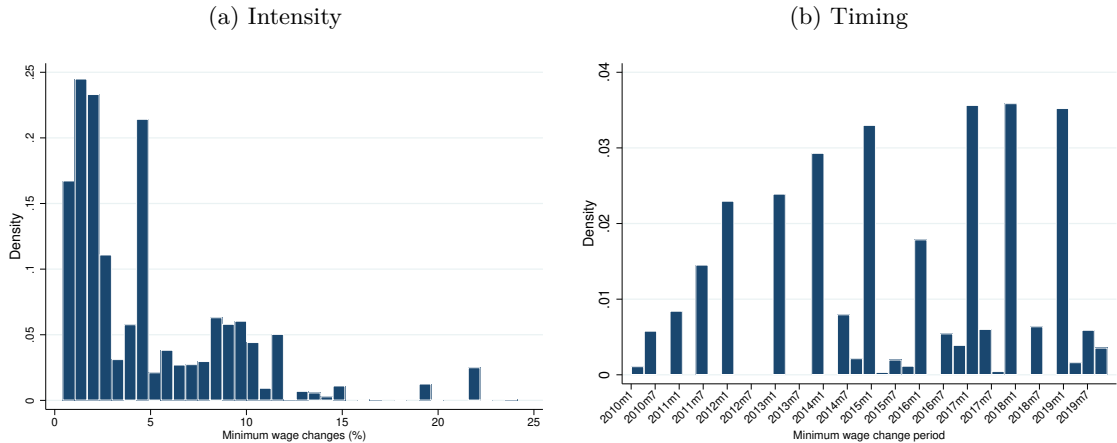
Notes: The histograms show the distribution of binding MW changes used in the analysis. In panel A, we report the intensity of the changes in percentage terms. The average percentage change is 4.85 %. In panel B, we show the distribution in time of such changes.

Second, we use rent and house value data from properties listed in Zillow for our sample period (Zillow 2020). 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) among homes listed on the platform in a given period. Time series are provided for different house types and at different geographic and date

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[11]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.

[12]We abstract from the fact that occasionally minimum wages vary by industry.

[13]Source: https://www.zillowgroup.com/facts-figures/. Accessed on October 23rd, 2020.

aggregation level.[14] Given that we are interested in the behavior of the housing market in the short period following a MW change, we focus on USPS zipcode level monthly time series. 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 zipcodes, 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 single-family homes, with the other 43 percent made up from buildings with 5 or more units (JCHS 2020). Finally, we select for our analysis *per square foot* variables: this allows us to reduce confounding 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.[15]

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 (2020). We assign to each zipcode the following characteristics: population, number of housing units, median income, black population, number of unemployed, and 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.

Table 1 compares 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 zipcodes in the Zillow data. Finally, column 4 restricts the sample by keeping the set of zipcodes that have valid SFCC rental data as of July 2015. We call this our *baseline sample*, since it is the one we use for most of our analysis. 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. In an attempt to capture the average treatment effect we conduct an estimation re-weighting our sample to match characteristics of the US sample of zipcodes.

To ensure that our data correctly captures the price evolution of the US rental market, we

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[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.

[15]We do observe the number of houses listed for sale, which we use as a proxy for this variable.

Table 1: Descriptive statistics of different sets of zipcodes

	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)	0.46	0.75	0.96	0.97
African-American Share (2010)	0.46	0.75	0.96	0.97
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)	0.46	0.75	0.96	0.97
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
Work in same county share (2010)	0.70	0.68	0.76	0.76
Unique zipcodes	$38,893$	$14,583$	$3,315$	$1,305$
Share of state events			0.03	0.03
Share of county events			0.001	0.001
Share of localevents			0.003	0.0005
Mean SFCC psqft rent			1.30	1.27
Unique zipcodes SFCC psqft rent			$3,316$	$1,143$

Notes: The table shows characteristics of four sets of US postal service zipcodes. Column 1 reports demographic statistics for the universe of USPS zipcode we were able to map. Column 2 shows the same statistics for for the top 100 Core-Based Statistical Areas (CBSA). Column 3 shows the characteristics of the set of zipcodes 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.

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.[16] 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 B.1 we compare the Zillow SFCC series with a weighted combination of the different SAMFRs series.[17] 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. The two series however show similar trends, confirming that Zillow rental series indeed captures the dynamics of the U.S. rental prices.

Fourth, to proxy for local economic activity we collect data from the Quarterly Census of Em-

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

[17] 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.

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

As we mentioned above, our baseline estimating sample is constructed by fixing the composition of zipcodes to those that had valid SFCC rent data as of July 2015. Table 2 shows some statistics of those zipcodes.

Table 2: Descriptive statistics and comparison with representative zipcodes

Statistic	N	Mean	St. Dev.	Min	Max
Median rent price per sqft. 2BR	24,789	1.96	0.97	0.53	6.51
Median rent price per sqft. MFR5plus	37,588	1.96	1.05	0.55	6.69
Median rent price per sqft.SFCC	113,375	1.27	0.83	0.47	7.25
Median rent price SFCC	125,644	1,651.10	702.99	595.00	6,595.00
Establishment count Services	152,334	44,916.45	55,846.62	321.00	474,767.00
Average wage Services	152,334	1,000.93	364.54	375.00	3,356.00
Employment Services	152,334	595,498.80	612,701.90	2,751.00	3,449,324.00

Notes: The table shows characteristics of four sets of US postal service zipcodes. Column 1 reports demographic statistics for the universe of USPS zipcode we were able to map. Column 2 shows the same statistics for for the top 100 Core-Based Statistical Areas (CBSA). Column 3 shows the characteristics of the set of zipcodes 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.

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[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.

3 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 required for identification of the effect of interest. We begin with a *static* difference-in-differences (DiD) specification that imposes no dynamics in the effects. To ease concerns of contemporaneous shocks systematically affecting both changes in rents and MW within a zipcode, we directly control for several county-level time-varying proxies of the health of the local labor and housing markets.[19] This model has several shortcomings that motivate the inclusion of leads and lags of MW changes. This *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 following Arellano and Bond (1991) and related literature. We test the robustness of our results by adding zipcode-level linear and quadratic trends, so to nonparametrically account for local dynamics.

Our specifications are distinct from the usual event-study models used commonly in the literature (discussed in, e.g., Borusyak and Jaravel 2017; Abraham and Sun 2018). First, while traditional event-study models struggle to allow for units treated more than once, our models allow for this possibility. Secondly, our model allows for the inclusion of never-treated units, which aid in the identification of time-period effects diminishing concerns of under-identification (Borusyak and Jaravel 2017). Finally, our specifications identify the treatment effect of minimum wages on rents not only exploit out of the timing of a MW change but also its intensity.

3.1 Baseline Specifications

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

$$y_{it} = \alpha_i + \alpha_t + \beta \underline{w}_{it} + X'_{ct}\eta + \epsilon_{it} \tag{1}$$

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 X_{ct} is a vector of county-level time-varying controls.

We then re-write equation (1) in first differences:

$$\Delta y_{it} = \theta_t + \beta \Delta \underline{w}_{it} + \Delta X'_{ct}\eta + \Delta \epsilon_{it} \tag{2}$$

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

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[19] While ideally we would like controls to be at the same geographical- and time-level of the main variation exploited, we do not have such information. For this reason, we replace that with the closest approximation possible by leveraging on QCEW data varying at both county-month and county-quarter levels. This amounts to adding a vector ΔX_{ct} on the right-hand side of our models, where c indexes counties. We map zipcodes to a single county as explained in section 2.

model is more efficient if the shocks are serially correlated (Wooldridge 2010).

Identification comes from assuming that within a zipcode the level change 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 changes in controls. 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. A limitation of using county-level time-varying controls is that there could still be unobserved local dynamics that correlate both with rents and MW, hence biasing the estimates of β. To allow for heterogeneity in the time path of zipcodes, we additionally present results from models with zipcode-specific linear and quadratic trends (Angrist and Pischke 2008).

One potential concern with the static DiD model, is that, despite controlling for local factors or zipcode-specific trends, 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 (anticipatory effect). To assess if that is the case, 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 (2) 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

$$\Delta y_{it} = \theta_t + \sum_{r=-s}^{s} \beta_r \Delta\underline{w}_{i(t-r)} + \Delta X_{ct}'\eta + \Delta\epsilon_{it} \ , \tag{3}$$

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:

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

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.

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.

As mentioned in section 2, 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 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 (2) and equation (3) 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

$$\Delta y_{it} = \Delta y_{i(t-1)} + \theta_t + \gamma_i + \sum_{r=0}^{s} \beta_r \Delta \underline{w}_{i(t-r)} + \Delta \epsilon_{it} \ . \tag{4}$$

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.

3.2 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 (2) 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

$$\Delta y_{it} = \theta_t + \gamma_i + \sum_{q=1}^{4} \beta_q \mathbb{1}\{i \in q\} \Delta \underline{w}_{it} + \Delta \epsilon_{it} \ , \tag{5}$$

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 4.4.

4 Main 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 0.026 when adopting the *static DiD* model.

We 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 and the labor market, and we show how results do not change. We further show how the introduction of zipcode-level linear and quadratic trend leaves our results unchanged. We then present estimates for the *dynamic* models (equation (3)) that highlight both the absence of pre-trends, and the presence of a 2-months dynamic effect: we estimate the cumulative impact of a 10 percent MW change to be between 0.5 and 0.6 percent over the course of the 5 months after the policy change. Since we don't observe changes in the underlying composition of rental listings on Zillow, identification may be threatened by a systematically different impact of MW on the number of listings on the platform for treated and control groups. We partially mitigate such concern by running a placebo regression where we replace our main dependent variable with the number of listings *for sale* (available on Zillow) in equation (3), where we don't find any significant effect.[20]

In subsection 4.2 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 × year-month* and we show how results are robust.

After establishing the robustness of our results, we then investigate how the incidence of the effect may vary across zipcodes. First, 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. Secondly, Using changes in the statutory MW to study the effect of a workplace-related place-based policy on the housing market may introduce measurement error: if MW workers commute to a different zipcode than the one the live in, there may be *treated* units even outside regions covered by MW ordinances. We use a workplace-residence matrix based on LODES data to build a measure of *experienced MW* that account for MW workers commuting outside the zipcode they live in. While results are very similar, we show how this exercise indeed provides slightly a slightly higher estimated impact of MW on rents.

We estimate the heterogeneous impact of MW changes across the distribution of several census-based demographics using equation (5). We show how effects are disproportionately concentrated in poorer, less-educated, and more African American zipcodes.

Lastly, we provide back-of-the-envelope calculations to help assessing the welfare implications of having landlords capturing part of the additional income generated by MW changes. We first perform three different exercises that allow us to quantify the average impact of MW changes on wages. Equipped with these measures, we are then able to obtain a ballpark estimates of the implied pass-through by comparing them to the our main results. We initially compute the average impact of MW on wages with a simple formula based on ACS data, and we get a 57.5 percent pass-through. Then, we alternatively use county-quarter QCEW data to run a DiD regression of wage changes

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[20]This exercises implicitly assumes the presence of of a correlation between the number of listings *for rent* and *for sale* on Zillow.

on the average MW change for this level of geographical and time aggregation. [21] We use to such estimates to obtain a 57.7 percent pass-through. This figure decreases to 53 percent when replacing the statutory MW with the LODES-based experienced MW measure. While these computations are likely to overestimate the real magnitude of the pass-through as they rely on several simplifying assumptions, they nevertheless return strikingly similar numbers. To reduce the margin of error, we finally use estimates of the impact of MW on wages taken from the literature (Cengiz et al. 2019). We obtain an implied pass-through of 27.7 percent.

4.1 Baseline Results

In Table 3, we present results from the model defined in equation (2). In column 1, we show the classic two-way fixed effects (i.e. zipcode and year-month). To alleviate the concern that unobserved local shocks might systematically affect MW and rents, in columns 2, 3, and 4 we add controls for wages, employment and number of establishment respectively.[22] In column 5, we jointly account for all sets of controls. All specifications return consistent estimates: a 10 percent increase in the MW leads to a 0.26 percent increase in rents.

Table 3: The static model

	(1)	(2)	(3)	(4)	(5)
$\Delta \ln \underline{w}_{ict}$	0.0260**	0.0246**	0.0257**	0.0268**	0.0258**
	(0.0128)	(0.0115)	(0.0124)	(0.0126)	(0.0124)
Wage controls	No	Yes	No	No	Yes
Employment controls	No	No	Yes	No	Yes
Establishment-count controls	No	No	No	Yes	Yes
R-squared	0.022	0.024	0.022	0.022	0.022
Observations	112,232	107,814	107,814	108,511	107,814

Notes: The table reports coefficients from versions of equation (2) estimated on the balanced panel of zipcode-months described in section 2. The dependent variable is the difference in the natural logarithm of median rents per square fott in the Single Family, Condos and Condominiums category in Zillow. All columns control for monthly date fixed effects. In addition, columns (2) to (5) include economic controls from the industries "Professional and business services", "Information", and "Financial activities" from the QCEW. Wage controls are the difference in the natural logarithm of average weekly wages, employment controls are the difference in the natural logarithm of employment, and establishment count controls refer to the difference in the natural logarithm of number of establishments. Wages and employment vary at the county-month level, whereas establishment count varies at the country-quarter level. Standard errors are clustered at the state level. Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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 (3). We display the results in Table A.2 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 on rent prices. This suggests how there are no pre-treatment differentials in the evolution of rental prices between treated and untreated zipcodes. Table A.2 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

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[21] see section XX for more details on the aggregation methodology and identification assumptions.

[22] Wages, as well as employment might be directly affected by MW policies, thus generating a *bad control* problem (Angrist and Pischke 2008). To avoid that, we only include controls from sectors that are unlikely to be affected by MW policies: Professional and Business Services, Information, and Finance.

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 A.2 is the presence of a mild persistence of the effect of 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 2). The blue-dot series plots the estimates from equation (3), 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 **??** (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 4). We compare that with our baseline estimates in **??**: columns 1, 2, and 3 show coefficients from equations (2), (3), and (**??**) 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 4 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.

4.2 External Validity and Data Sensitivity

Our results suggest a noticeable impact of MW policies on the rental housing market. However, as explained in section 2, 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.
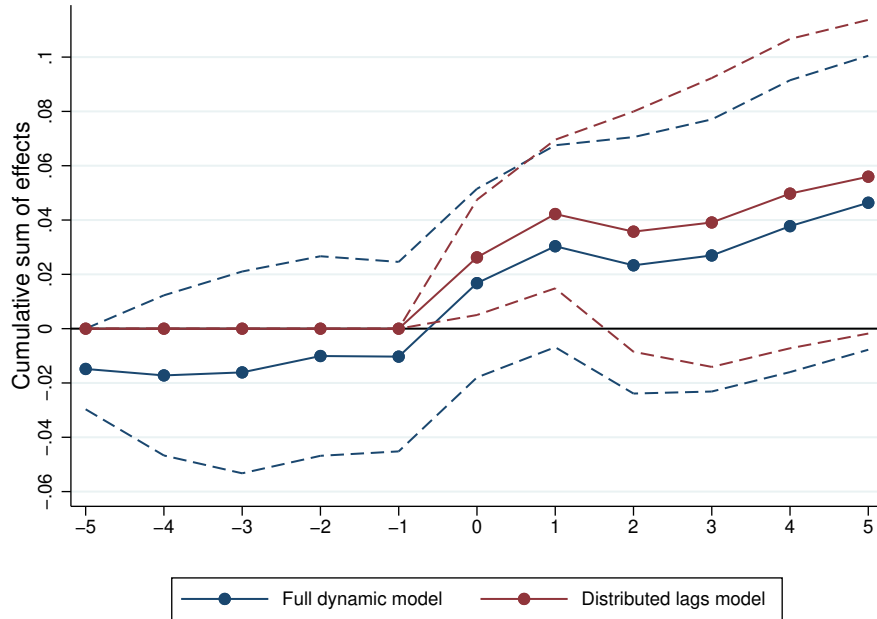
We test the sensitivity with respect to our sample restrictions in two ways. First, we extend

Figure 2: Results of main dynamic model

(a) Coefficients
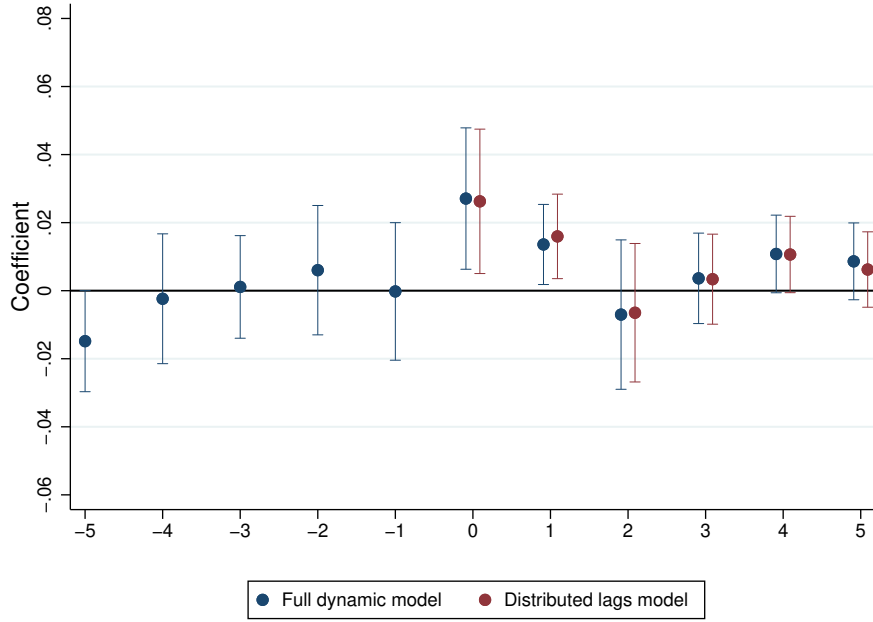
(b) Cumulative sum

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 × period" fixed effects. We do this for our main specifications in equations (2), (3), and (**??**). In this way, we are able to compare treated and untreated zipcodes with the same panel length. In **??** 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.[23] 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 (2) and $\hat{\beta}_t$ from equation (3) are almost identical, identifying an elasticity on rents of approximately 0.036 (**??**).

4.3 The Role of Unobserved Local Shocks

In subsection 4.1 we used equation (3) 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 2 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.

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[23]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).

Figure 3: Comparison between dynamic DiD models

(a) Unbalanced panel
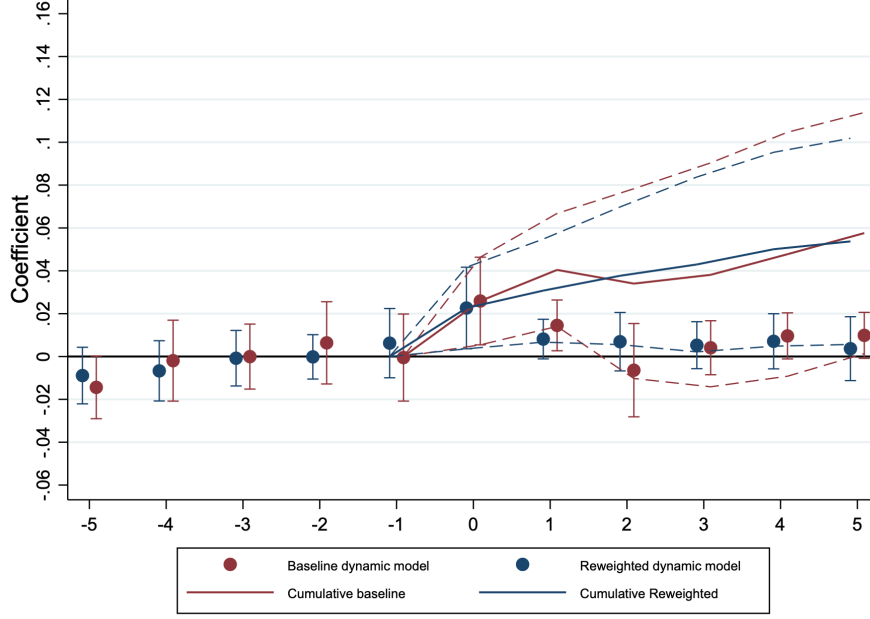
(b) Reweighted panel
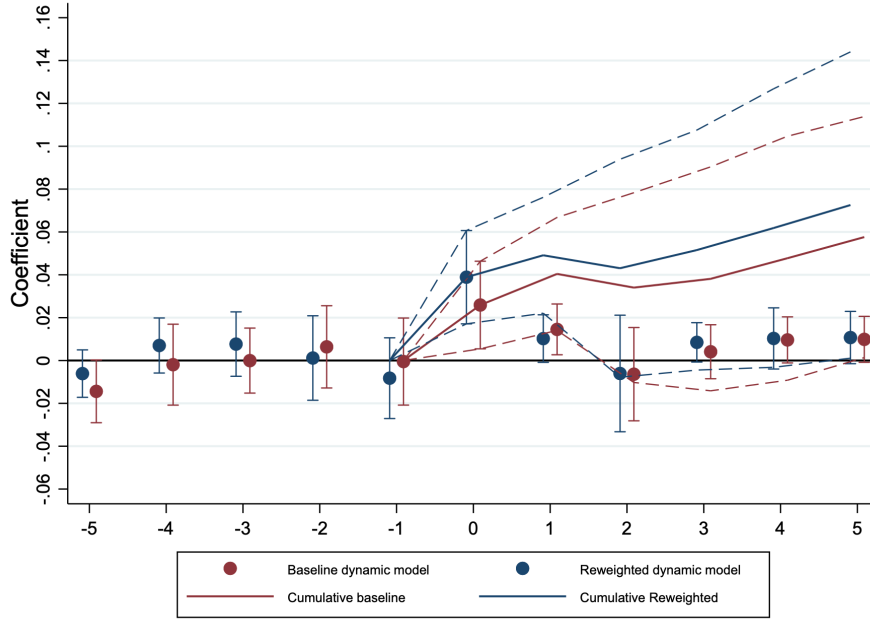
Notes: The plot compares results obtained from the baseline panel summarized in Table 1, column 4 with those obtained from the full unbalanced panel (a), and with the reweighted baseline panel (b). Each subfigure compares estimated coefficients for the *dynamic DiD* models calculated through equation (3), and point estimates for the cumulative effect of MW on rents estimated through **??**.

In **??** we report the estimated coefficients for equation (3), progressively increasing the set of controls included in the regression. Column 1 replicates our baseline results (Table A.2, 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.

## 4.4 The Heterogeneity of MW Impacts on Rents

Our baseline results in subsection 4.1 have documented the presence of a causal impact of MW on rents, and the effect appears robust to multiple checks introduced in Sections 4.2 and 4.3. 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).[24] 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 (5) 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 3rd and 4th quartiles become very large partly due to a heavily right-skewed distribution of the underlying variable. In panel (b) we re-estimate equation (5) 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

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[24]See section 2 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
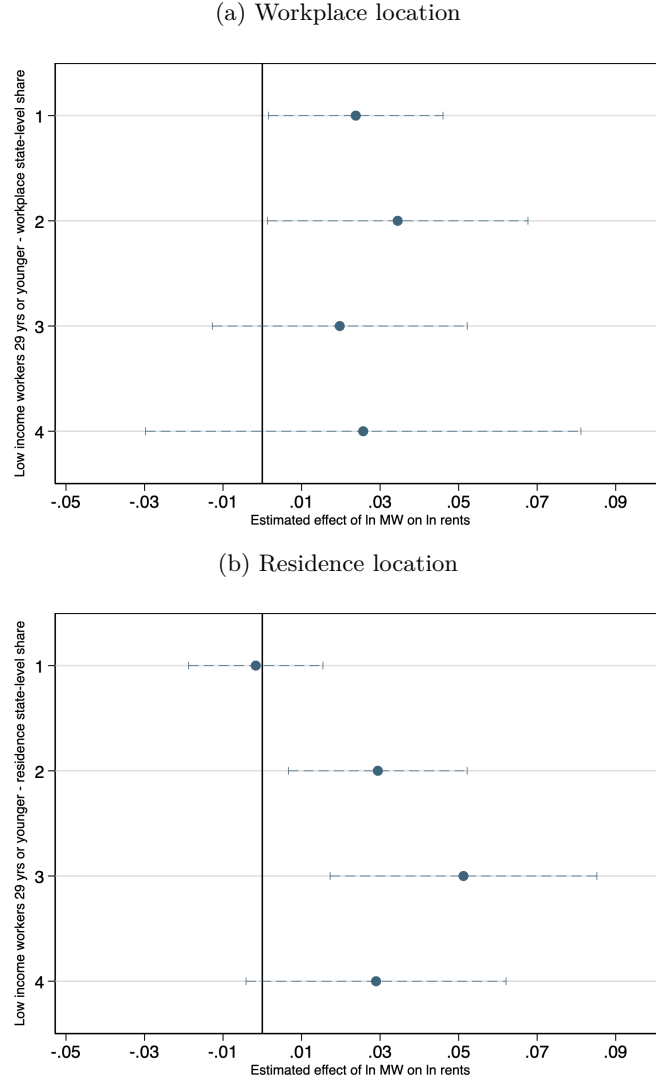
Notes: The Figure shows the estimated coefficients β_q, $q \in \{1, 2, 3, 4\}$ from equation (5) when differentiating zipcodes with respect to the share of MW workers that either work (a) or live (b) in each zipcode. Shares are taken over state totals. MW workers is used as a loose label for workers below 30 years old earning less than $1250 month identified using the 2017 LODES datasets (see section 2 for more information). 90 percent confidence intervals reported.

the 2nd and 3rd 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 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.[25] The study of heterogeneous effects by demographics can therefore help both in confirming what the LODES-based measures indicate, and in uncovering additional patterns of the effects under study.

Table 4 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 2nd and 3rd 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 4th 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 2nd and 1st 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 1st quarter to a statistically significant 0.042 in the 4th one.

Table 4: Heterogeneity Results - static DiD model

| | (1) | (2) | (3) | (4) |
	Median Income	Unemp. rate (%)	College Grad. (%)	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 (5) 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5 Tracking MW workers: the experienced minimum wage

Thus far we have used the statutory minimum wage as our variable of interest. More precisely, we took the maximum across federal, state and local minimum wage as our treatment of interest. However, what matters for the a zipcode's housing market is the wage of the people residing there,

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[25]See, for example, BLS Report 1085, *Characteristics of Minimum Wage Workers 2019.*

which may not coincide with the statutory MW if workers actually work in different zipcodes.

Therefore, we construct a new measure of the "effective" minimum wage in each zipcode that takes into account the fact that MW workers residence may differ from workplace location.

5.1 A new minimum wage measure

Following notation in section section 3, we denote zipcodes by i and monthly dates by t. Furthermore, for each zipcode i define: (i) the set $\mathbb{Z}_i$ of zipcodes in which residents of i work (including i), and (ii) 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 minimum wage workers who reside in zipcode i and work in z, and N_i is the total population of zipcode i. We define the experienced minimum wage measure as follows:

$$\underline{w}_{it}^{\exp} = \sum_{z\in\mathbb{Z}_i} \omega_{iz}\underline{w}_{zt} \ , \ \forall(i,t). \tag{6}$$

The experienced MW of a zipcode is based on the minimum wages binding in other zipcodes where its residents work. An increase in a city, for example, may not have an impact in the local rental market if most residents are not minimum wage workers. It will, however, affect neighboring zipcodes where MW workers reside. If what drives the effect is actually the supply and demand of rental housing, then we would expect it to be larger. In practice, however, most MW changes arise from the state and thus our measures are highly correlated.

As an illustration, figure 5 plots the statutory versus experienced MW variables following the California increase in the minimum wage on January, 2019. As of December 2018 the MW in San Diego city was \$11.50, whereas the state's MW binding outside the city was \$11.[26] The increase in the state level MW to \$12 in January 2020 appears as a discontinuity in the city border. However, when we account for the fact that MW workers commute, we observe a gradient in the intensity of the policy.

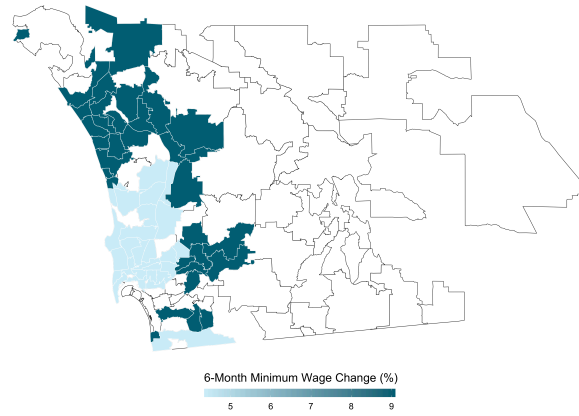## 5.2 Estimation results

Something

6 Discussion

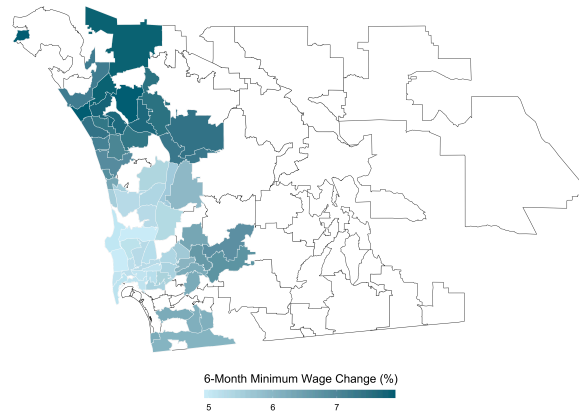In this discussion we discuss the implications of our results.

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[26]For employers larger than 26 employees. For those below 26 employees the level was \$10.50.

Figure 5: The California MW increase of January 2019 in San Diego

(a) Statutory MW change

(b) Experienced MW change

Notes: The figure maps the percent increase in our minimum wage and experienced minimum wage measures following the state increase in California on January 2019. The map colors only zipcodes for which we have non-missing rents data from Zillow.

6.1 The magnitude of the estimates

In this subsection we use the model introduced in Appendix C 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.[27]. As a result, it can be shown that equation (8) takes the form

$$\rho = \frac{\underline{\beta}\,\underline{s}}{k - \underline{\gamma}\,\underline{s} - \overline{\gamma s}} \tag{7}$$

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 Diamond (2016, Table 5). Substituting these values in (7) 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.

6.2 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 **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

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[27]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.

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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27

Appendix

A Appendix Tables

Table A.1: Results from static model controlling for parametric trends

	(1)	(2)	(3)
$\Delta \ln \underline{w}_{ict}$	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 (2) estimated on the balanced panel of zipcode-months described in section 2. The dependent variable is the difference in the natural logarithm of median rents per square foot in the Single Family, Condos and Condominiums category in Zillow. All columns include controls for monthly date fixed effects. In addition, column (2) controls for zipcode-specific linear trend (which in a differenced model amounts to including a zipcode fixed effect), and column (3) controls for zipcode-specific linear and quadratic trends (which amount to including a zipcode fixed effect and a zipcode-specific trend). Standard errors are clustered at the state level. Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: Complete results of dynamic model with leads and lags

	(1)	(2)	(3)	(4)	(5)
$\Delta \ln \underline{w}_{ic,t-5}$	-0.0148	-0.0144	-0.0144	-0.0146	-0.0144
	(0.0090)	(0.0089)	(0.0089)	(0.0090)	(0.0089)
$\Delta \ln \underline{w}_{ic,t-4}$	-0.0024	-0.0019	-0.0020	-0.0022	-0.0019
	(0.0116)	(0.0116)	(0.0115)	(0.0116)	(0.0115)
$\Delta \ln \underline{w}_{ic,t-3}$	0.0011	0.0005	0.0007	0.0004	-0.0002
	(0.0092)	(0.0094)	(0.0092)	(0.0091)	(0.0092)
$\Delta \ln \underline{w}_{ic,t-2}$	0.0060	0.0063	0.0062	0.0060	0.0064
	(0.0116)	(0.0118)	(0.0116)	(0.0115)	(0.0117)
$\Delta \ln \underline{w}_{ic,t-1}$	-0.0002	-0.0004	-0.0005	0.0000	-0.0005
	(0.0123)	(0.0123)	(0.0124)	(0.0122)	(0.0123)
$\Delta \ln \underline{w}_{ic,t}$	0.0271**	0.0257**	0.0259**	0.0269**	0.0259**
	(0.0126)	(0.0123)	(0.0124)	(0.0126)	(0.0124)
$\Delta \ln \underline{w}_{ic,t+1}$	0.0136*	0.0146**	0.0142*	0.0135*	0.0146*
	(0.0072)	(0.0072)	(0.0072)	(0.0072)	(0.0072)
$\Delta \ln \underline{w}_{ic,t+2}$	-0.0070	-0.0066	-0.0064	-0.0068	-0.0064
	(0.0133)	(0.0133)	(0.0132)	(0.0133)	(0.0132)
$\Delta \ln \underline{w}_{ic,t+3}$	0.0036	0.0045	0.0047	0.0031	0.0040
	(0.0081)	(0.0078)	(0.0078)	(0.0079)	(0.0077)
$\Delta \ln \underline{w}_{ic,t+4}$	0.0108	0.0093	0.0104	0.0107	0.0096
	(0.0069)	(0.0066)	(0.0064)	(0.0069)	(0.0065)
$\Delta \ln \underline{w}_{ic,t+5}$	0.0086	0.0095	0.0099	0.0088	0.0099
	(0.0069)	(0.0065)	(0.0065)	(0.0067)	(0.0065)
Cumulative effect	0.057	0.057*	0.059*	0.056	0.058*
	(0.035)	(0.034)	(0.034)	(0.034)	(0.034)
P-value no pretrends	0.568	0.612	0.599	0.594	0.629
Wage controls	No	Yes	No	No	Yes
Employment controls	No	No	Yes	No	Yes
Establishment-count controls	No	No	No	Yes	Yes
R-squared	0.022	0.022	0.022	0.022	0.022
Observations	106,446	105,463	105,463	106,160	105,463

Notes: The table reports coefficients from versions of equation (3) estimated on the balanced panel of zipcode-months described in section 2. The dependent variable is the difference in the natural logarithm of median rents in the Single Family, Condos and Condominiums category in Zillow. We report coefficients of five leads and lags of the difference in the logarithm of the statutory minimum wage. All columns control for monthly date fixed effects. In addition, columns (2) to (5) include economic controls from the industries "Professional and business services", "Information", and "Financial activities" from the QCEW. Wage controls are the difference in the natural logarithm of average weekly wages, employment controls are the difference in the natural logarithm of employment, and establishment count controls refer to the difference in the natural logarithm of number of establishments. Wages and employment vary at the county-month level, whereas establishment count varies at the country-quarter level. The row "Cumulative Effect" shows the cumulative sum of the sum of all coefficients. Standard errors are clustered at the state level. Significance codes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Appendix Figures

Figure B.1: National Time Series for Zillow and SAFMR data
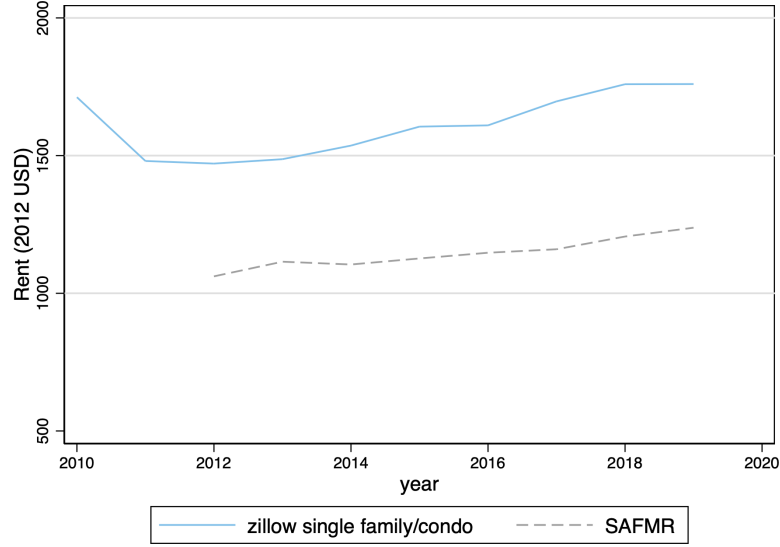
Notes: The figure plots the monthly rent annual national average for the main Zillow series used in the analysis (SFCC) and a weighted combination of SAFMR series with different number of bedrooms. Weights are based on the US share of single family, condos and cooperative houses with given number of bedrooms as recorded in the AHS. See footnote 17 for details on the construction of this time series.

Figure B.2: Dynamic DiD model comparison - local shocks
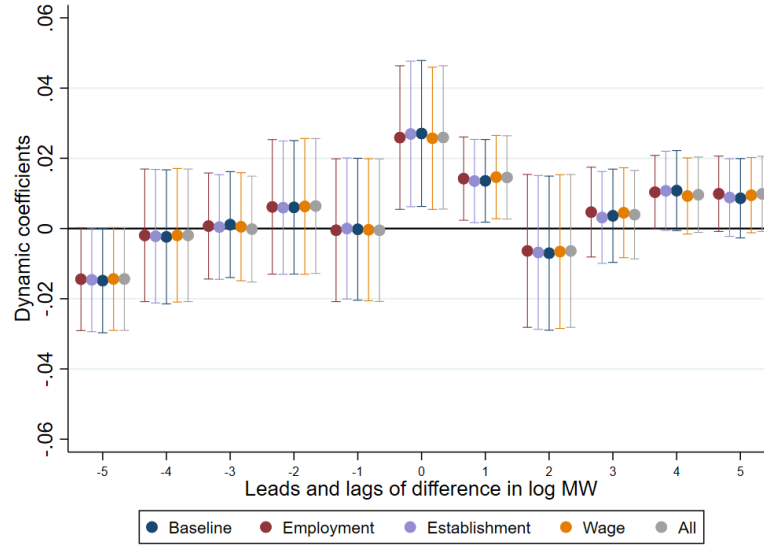
Notes: The figure show estimates for $\hat{\beta}_r$ obtained from equation (3) when progressively adding time-varying controls for local shocks. The *baseline* series plots coefficients taken from **??**, column (1). The *employment*, *establishment*, *wage*, and *building* series plot coefficients from **??**, columns (2) to (5) respectively. 90 percent confidence intervals reported.

C A toy model of the local housing market

We build a simple model that is used to build a benchmark estimate of the effect of MW policies on rents.

C.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 $\overline{H}_w(r, w) > 0$)

C.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

$$\rho := \frac{d \ln r^*}{d \ln \underline{w}} = \frac{\underline{w} \ \underline{H}_w}{r \ H'(r) - r \ \underline{H}_r - r \ \overline{H}_r} \ , \tag{8}$$

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.