Minimum Wages and Rent:
Evidence from U.S. Cities

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Abstract

This project examines the effects of local minimum wages on rent prices. The minimum wage has proven to be an effective policy for improving real earnings of workers at the lower end of the income distribution. However, to fully understand the policy's efficacy, we must understand how minimum wage impacts other economic factors, such as rent. This topic is particularly salient as housing costs absorb a large share of low-wage workers' budgets and have the potential to offset the benefits from wage increases. Existing research on this topic has generally focused on broader policy levels, such as state minimum wages in the U.S., using a two-way fixed effects approach. Using zip code rent data from Zillow, I estimate this effect with a border pairs design on nearby zip codes across city-level minimum wage boundaries. I find that typically observed rent prices raise 0.8-1.1% in response to 10% increases in minimum wage, suggesting that low-income renters may benefit from wage hikes without passing much of their new income through to landlords.

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1 Introduction

The minimum wage has existed in the United States since the 1930's in order to benefit low-wage workers by raising their earnings and thus providing reasonable standards of living. However, a federal minimum wage is often not enough to sustain workers as there is considerable heterogeneity in costs of living across the country. This has led a majority of U.S. states, as well as a handful of counties and dozens of cities, to adopt their own minimum wages which exceed the federal level. A rich body of research has studied various potential impacts of increasing minimum at all of these policy levels, with earnings, employment, and prices being some of the more common topics. This paper, however, examines a relatively unexplored, yet highly salient, externality of minimum wage policy: home rent prices.

The rent effect is important for two reasons. First, housing costs represent a significant portion of low-wage workers' budgets. Several housing studies have shown that the majority of low-income renters spend more than 30% of their income on rent, or are housing cost-burdened, and a significant share are severely cost-burdened, spending over 50% on rent (NLIHC, 2021; JCHS, 2021). So while higher minimum wages provide a way to potentially offset such cost burdening by expanding the income of these renters, landlords also have the opportunity to raise rent prices and soak up a huge share of their tenants' new income, making landlords effective beneficiaries of minimum wages at the expense of worker benefits. This makes understanding the rent effect highly relevant for minimum wage policy decisions.

Second, the effect on rent is unique within the literature as it is primarily a demand-side incident of minimum wage. The majority of research in this field has focused on how *firms* react to increased wages, finding, for example, that businesses employ roughly the same number of people when wages go up. However, firms tend to offset these new costs in other ways that directly affect consumers. Very common is for the costs to be passed through to consumers via increased prices, such as with McDonald's raising the price of Big Macs following wage hikes (Ashenfelter & Jurajda, 2021). Although, while these sorts of effects are generally interpreted in the literature as stemming from higher costs faced by employers, it is unclear in the case of price increases how much of the effect is due to supply cost-push versus increased demand for those goods. This is especially true in industries frequented by low-wage consumers, such as fast food. Conversely, rent changes due to wage increases must come from increased demand from workers to be employed in, and thus live in, areas with higher wages. Therefore, studying the effect on rent gives a better understanding about the demand-side incidents of minimum wages on consumers and workers, which can additionally further the literature's understanding of effects such as price increases.

This paper studies the effect on rent through disparities in minimum wage along local borders, most often at the city level. While some papers have previously examined how minimum wages affect rent (Agarwal, Ambrose, & Diop, 2020; Yamagishi, 2021), I am the first to my knowledge to study it at the city level. I generate monthly zip code-level minimum wage data from historical local wage ordinances and I make use of rent price data from Zillow, an online platform that facilitates housing sales and rents. These rent data provide an index of the typically observed rent price at the zip code level and allow me to construct nearby zip code pairs along local minimum wage boundaries in the methodological style of Dube, Lester, and Reich (2010). Using their border pairs design, I first create pairs of nearby zip codes on opposite sides of city minimum wage borders. I am then able to observe how zip codes within pairs experience rent price changes differently when one is subjected to a higher city wage while the other is not. This strategy allows me to compute the minimum wage's effect on rent in such a way that naturally accounts for spatial heterogeneity in rent trends and potential reverse causality. I find that wage hikes lead to a statistically significant and positive, though relatively small, uptick in typical rents.

The rest of this paper is organized as follows: in Section 2, I review the prior literature related to this topic and how this paper contributes to it; in Section 3, I describe my data; in Section 4, I detail my statistical methodology; in Section 5, I discuss the results from my main estimation and those from several robustness checks; and in Section 6, I conclude this paper with further discussion of the results and suggestions for future research.

2 Related Literature

2.1 Minimum Wages

While minimum wages have long been the focus of economic research, the current abundance of literature can largely be traced back to a series of papers from the late 20$^{\text{th}}$ century. In the early 1980's, Brown, Gilroy, and Kohen (1982) surveyed existing work in the field, focusing on the effects of minimum wages on employment. They find that wage increases lead to small decreases in employment at the lower end of the age distribution. Specifically, 10 percent minimum wage increases resulted in reductions in employment for teenagers (16-19 years) by between 0 and 3 percent across the literature. For young adults (20-24 years), the effect is smaller but still negative, and the effect is deemed "uncertain" for adults.

The results of Brown et al. (1982) were generally accepted until the early 1990's (Schmitt, 2015) and were supported by other studies that used national panels of minimum wage data. For example, Neumark and Wascher (1992) use a nation-wide dataset of state level minimum wages and find disemployment effects when using differences-in-differences between states.

Around this time, though, minimum wage research with a focus on exploiting natural experiments and geographic variation began to flourish. The most influential of this wave of papers is that of Card and Krueger (1994), which looks at fast food restaurants in New Jersey around a 1992 state minimum wage increase and compared their employment levels to similar restaurants in Pennsylvania, where the minimum wage was constant, and to other New Jersey restaurants that had been paying higher wages since before the policy. Their findings are in stark contrast to those of Brown et al. (1982): the rise in wage does not lead to a reduction in employment by the affected restaurants. These studies led to a significant expansion in the literature, referred to as the "new minimum wage" research.

Following papers like Card and Krueger (1994) in the 1990's, studies on employment effects became especially popular within the sphere of minimum wage research. This is true of both those who followed Card and Krueger's example of using case studies and those who preferred the more traditional method of national-level time series analyses of cross-state minimum wage variation. However, both approaches are critiqued by Dube et al. (2010) in what has become one of the most influential minimum wage papers. The authors first show that each approach is subject to unique heterogeneity issues. Namely, traditional studies fail to account for spatial heterogeneity in employment growth trends while case studies are often subject to autocorrelation that over-state the significance of their estimates. They offer an empirical strategy that accounts for both issues while generalizing case study techniques to the national level: the county border pairs design, which compares effects within pairs of contiguous counties that experience different minimum wages. Ultimately, they find that increases in minimum wages lead to increases in real earnings and no changes in employment.

Literature reviews and metastudies generally reach the same conclusion about the vast body of employment effect research: estimates on the effect of minimum wage on employment are heavily concentrated at zero or are very slightly negative, depending on the specific measurement used (Schmitt, 2015; Dube, 2019). This is highly contradictory to traditional intuition about how employers should treat the costs borne from increased wages. However, recent work has focused on other labor factors that could explain this apparent contradiction. Clemens (2021) argues that a zero employment effect should not be surprising once one considers the various non-employment changes that employers can make to offset costs, such as to health insurance benefits and safety measures. Manning (2021) also considers arguments and evidence of other factors that may influence employers' hiring decisions, such as higher wages leading to increased employee productivity and lower costs employee turnover costs. However, because minimum wage jobs are concentrated in non-traded service sectors, such as retail and food service, costs being passed through to consumers via higher prices seems to be the most relevant way for firms to offset these costs (Ashenfelter & Jurajda, 2021; Esposito, Leamer, & Nickelsburg, 2021).

While employment effects have captured a large portion of minimum wage economists' focus, other interesting questions have also been explored. One such question has been on the minimum wage's ability to reduce inequality. Majchrowska and Strawiński (2018) find that an increase in Poland's minimum wage reduced the gender wage gap for young workers. However, these findings don't extend to more experienced or educated groups of workers. Similarly, Derenoncourt and Montialoux (2021) analyze a 1967 reform in the United States that extended the federal minimum wage to a number of industries which employed a disproportionately large number of black workers. They find that the reform reduced the wage gap between white and black workers in America, both by reducing the gap between the treated and non-treated industries and by observing faster wage rises for black workers than for others within the treated industries.

Another topic that has been explored is the effect of wages on local labor supply. On one hand, Giulietti (2014) finds that increased federal minimum wages attracted low-skilled immigrants to the United States. But on the other hand, some studies conclude that low-skilled workers tend to migrate away from regions with minimum wage increases and settle in areas with stagnant minimum wages (Monras, 2019; Cadena, 2014).

2.2 City Level Wages

The vast majority of minimum wage literature has looked at state and federal changes. However, as a growing number of cities adopt their own local minimum wages, the same questions that are asked at broader levels become relevant for cities. For example, one might wonder whether earnings and employment effects follow the same trend at the city level when city boundaries are much more porous than state boundaries and both businesses and workers are able to relocate much easier. While limited, the body of work exploring these questions suggests, in aggregate, that the effects are similar: earnings are improved while employment effects remain muted (Dube & Lindner, 2021).

Recent papers have attempted to answer other important questions as well, such as who bears the cost of a minimum wage increase if employment is not affected. Esposito et al. (2021) find that following minimum wage increases in Los Angeles, restaurants in high-income areas far from the city border tended to raise their prices, passing the costs on to their customers. But restaurants in low-income areas or near the border were more likely to change their menus or simply close.

2.3 Minimum Wages and Rent

There is some literature on how minimum wages impact rent, though it is relatively small and has tended to focus on broader policy levels. In the United States, Agarwal et al. (2020) look primarily at how state wage increases influence tenant default rates using individual lease data from RentBureau. However, the authors are able to use these data to evaluate how landlords respond to those wage hikes in terms of adjusting rent. They find that landlords raise their rent prices following minimum wage increases, and that these upswings in rent soak up, on average, over half of the minimum wage increases they respond to–a huge positive effect.

Yamagishi (2021) provides a more robust theoretical framework and analysis of the topic. He develops a spatial equilibrium model wherein the attractiveness of higher-wage areas drive demand of workers to live there, which in turn influences the prices of rent. If low-wage workers view increased minimum wages as a policy that on balance benefits them, they will want to work and live in areas with higher wages, which leads to higher rent prices to meet the housing demand. After describing this framework, Yamagishi moves into an empirical analysis of prefecture minimum wage hikes in Japan and their effects on the rent prices of low-quality apartment buildings. Like Agarwal et al. (2020), he finds a positive, albeit smaller, impact. Namely, a 10% increase in the wage yields a 2.5-4.5% increase in rent, with 7.5-13.5% of workers' income benefits going to landlords under the assumption that renters spend 30% of their income on rent.

Both Agarwal et al. (2020) and Yamagishi (2021) rely on two-way fixed effects differences-in-differences as their primary method of estimation. In contrast, because my rent data are aggregated at the zip code level, I am able to use nearby untreated zip codes as controls for treated zip codes through a border pairs design (Dube et al., 2010). Thus, I contribute to the literature in two ways. First, I use a methodology that better controls for spatial heterogeneity in rent trends and potential reverse causality. Second, to my knowledge, I am the first to explore how city minimum wages affect rent.

I expect rent prices to follow the theoretical framework laid out in Yamagishi (2021). That is, workers near cities with increasing minimum wages may perceive better conditions in those cities, swelling housing demand and, consequently, rent. However, there is a second channel through which rents could be increased that is independent of worker migration. Specifically, low-wage workers who already live in these cities and receive larger budgets after wage hikes without having to move may seek better housing in the same area if they can now afford it. Thus, higher-quality rental units may raise their prices due to this sort of demand shift while low-quality units see no such increase in demand.

3 Data

3.1 Rent Data

The rent data used in this paper are the Zillow Observed Rent Index (ZORI) data from Zillow, an online service for matching buyers, sellers, and renters of real estate. ZORI is a dollar-denominated, repeat-rent index of the typically observed market rate rent at the zip code level (Zillow, 2021). At the time of initially downloading the data in September, 2021, there was ZORI information for 2,233 U.S. zip codes across 87 metropolitan statistical areas (MSAs). The data went back to January 2014 and extended to August 2021, though this is updated monthly to include the most recent month's data.

However, I am restricting the ZORI observations that I use to a sub sample based on a number of salient factors. First, it is important to note that the COVID-19 pandemic would likely introduce unwanted noise to my estimation as it carried with it a recession and widespread job loss, as well as eviction moratorium and rent cap policies to combat loss of housing for those who found themselves suddenly unemployed. For this reason, I am only considering observations between January 2014 and December 2019.

I also make a number of spatial restrictions for my analysis. First, to comply with my methodology, I need only look at zip codes that are in or around cities with local minimum wages. Zillow has ZORI data for 30 of the cities with minimum wages, although many of these are left out of my analysis because they either did not introduce city minimum wages before 2020 or because there were no nearby untreated zip codes to use in border pairs.

One caveat to these data are that, as mentioned, they measure the *overall* typical rent price in a zip code, similar to a median level, rather than the typical rent price at a lower quantile of the rental distribution. This is important as, in my case, it makes sense to assume that minimum wage policies impact low-cost rental units that are more likely to be rented by low-income workers, but wouldn't have any effect on high-cost units. So it is possible to see small to zero changes near the median level of rent even if lower-cost units are increasing in price. I discuss the potential impacts of this more in Sections 5.4 and 6.

3.1.1 Missing Data

While Zillow's data provide a useful measure of typical rent prices, there are unfortunately a large number of zip codes that either have missing values for ZORI in some months or are absent from the data entirely.[1] The issue of absent zip codes is addressed in Section 4 as it is primarily relevant for producing pairs of contiguous zip codes. However, the missing rent values for zip codes in the Zillow data pose a more general threat as they could bias the results of my estimation if they are not distributed at random.[2] To account for this, I run my main regressions on three separate panels. The first is the unbalanced panel of all of my zip code pairs with some missing values spread across the observations. The second is a balanced panel of all zip

[1] Table A.1 and Figures A.1 - A.4 summarize the patterns of missing values for zip codes that are in the dataset of 706 zip codes that I describe at the end of Section 3.2. Figures A.5 - A.6 show zip code maps around Washington, DC, to provide an example of how many zip codes are missing at least one rent value.

[2] I have reached out to Zillow asking about the missing values but have not received a reply, so I can't say for sure why they are present in the data. However, based on Figures A.3 - A.4, the missing values are evidently most common at the beginning of the rent index's history and, within each year, are less common during the summer months, when more leases are generally signed. So my intuition is that while ZORI is a repeat-rent index that takes into account rental units even when they are not being listed, the missing values are associated with time periods when there are fewer rental listings on Zillow's site with which to construct the index.

code pairs in which rent values are linearly interpolated wherever they were originally missing. The final is a balanced panel of zip code pairs where any zip codes that have at least one missing value are removed before forming the pairs. In the regression tables, I label these as Unbalanced, Interpolated, and ZCs Dropped (Zip Codes Dropped) respectively.

3.2 Minimum Wage Data

For my analysis, I require data on the minimum wage that each zip code in my sample is subject to during each month between 2014 and 2019. There are two reasons for this. First, ZORI data change at the monthly level, so having monthly minimum wage data ensures that I can maintain that level of granularity instead of having to aggregate my rent data. Second, minimum wages policies typically set a target minimum wage and a schedule of increases that take effect annually and during a specific month until that target is reached, rather than simply changing the minimum wage to the desired wage immediately. For example, Palo Alto, CA, has steadily increased its minimum wage every January 1st since 2016, while Minneapolis, MN, first set its minimum wage on January 1st, 2018, and has increased it steadily every July 1st since 2018.

Unfortunately, no data set with this level of detail exists to my knowledge. However, as minimum wage schedules are codified in city, county, and state laws and ordinances, there are official records of the schedules. So I was able to create this data set by researching those various statutes. Additionally, the data used in Dube and Lindner (2021) provide detailed yearly schedules of local and state minimum wages. So when specific ordinances were unclear, I was able to use the Dube and Lindner data to fill in the gaps.

Some further notes should be taken regarding the minimum wage data. First, as alluded to above, minimum wages exist broadly at four levels in the U.S.: federal, state, county, and city. In regards to counties, the following have their own minimum wages: Los Angeles County in California; Cook County in Illinois; Bernalillo County in New Mexico; Montgomery and Prince George's Counties in Maryland; and Nassau, Suffolk, and Westchester Counties in New York. Furthermore, cities and towns in Cook County, IL, are allowed to opt out of the county minimum wage in favor of the state minimum wage (Waltmire, 2017). And while the cities of Los Angeles, Malibu, and Pasadena, which are all inside Los Angeles County, have codified their own minimum wages, they in fact adopted the same schedule as Los Angeles County. For my analysis, while I have to account for these minimum wages in my data, I do not generally consider counties to be "local minimum wage areas." Los Angeles County is the exception to this as it is highly urbanized and contains multiple cities which have adopted the same minimum wage schedule as the county. There is also one local area in the data with its own minimum wage that is not one of the four levels mentioned previously: the Portland Urban Growth Boundary in Oregon. I include this as a treated local area as well since it is by definition the highly urban area in and around the city of Portland.

One issue with zip codes is that they often do not follow political boundaries, so it is not uncommon for only part of a zip code to be within the jurisdiction of a particular city or county. This makes it difficult to assign a single minimum wage schedule to each zip code as businesses in different parts of one zip code can be subject to different minimum wages at the same time. In order to have a "well-defined" sample in which each zip code has one minimum wage schedule, I generally remove any zip codes that straddle wage policy boundaries, with some rare exceptions. Specifically, sometimes part of a zip code will technically be on the other side of a boundary, but that part will clearly contain nothing that would be subject to a minimum wage, such as wilderness. I remove 57 zip codes this way. In all, I am left with a sample of 706 zip codes that are in the MSAs of 20 local minimum wage areas. For my primary regression, I use a sample of 356 of those zip codes that are able to be paired together as described in Section 4, totalling 1,771 zip code pairs, for my Unbalanced and Interpolated panels, and I use 153 zip codes in 757 pairs for my Zip Codes Dropped panel.

3.3 Additional Covariates

In addition to the rent and minimum wage data required for my estimation, I gather zip code data on a variety of demographic and economic covariates that are used in robustness checks in Sections 5.3 and 5.4. I first gather population, age, race and ethnicity, and housing and renting data from the 2010 Census, as the Census provides accurately measured data and the 2010 iteration is the most recent one from before the years in my panel. Next, I get educational attainment, income, and unemployment data from the 2011 American Community Survey (ACS). While not as accurate as the Census, the ACS is still well-measured. And the 2011 iteration is the closest one to the 2010 Census that provides estimates as the zip code level. Finally, I get data on filings of the Earned Income Tax Credit (EITC), a tax credit that assists low- to moderate-income individuals and families, for both 2010 and 2013 from the Internal Revenue Service's Statistics of Income (IRS SOI). I use these two years as 2010 is the same year as my Census data and 2013 is the year just before my sample begins. While the IRS publishes yearly tax filing data, I choose not to use EITC data from any of the years in my sample because it is likely that increasing minimum wages would influence the number of people filing EITCs, making EITC data in these years less appealing in the context of robustness checks.

4 Methodology

For my primary analysis, I borrow the border pairs design methodology from Dube et al. (2010). However, as mentioned in Section 3.1.1, the lack of total zip code representation for each MSA in the Zillow data presents a problem for generating contiguous zip code pairs. Namely, the Zillow data do not contain enough zip codes that lie directly along city boundaries for me to construct a large enough dataset of contiguous pairs. Figure A.5 displays this issue for zip codes in and around Washington, DC. Even though Zillow has data for many zip codes in this area, few of the zip codes that actually border D.C. are present in the data, so I can only form a small number of contiguous pairs along that border. To account for this, I adjust the methodology to form pairs based on distance rather than contiguity, where two zip codes are paired together if they are on opposite sides of a treated area border and their population-weighted centroids are within 10 miles of each other. This creates a much richer sample of zip code pairs while preserving, to a degree, the crucial aspect of close proximity between treated and control observations from the original design. With these data, I estimate the effect of local minimum wages on rent with zip code and pair-time fixed effects:

$$\ln(ZORI_{zpt}) = \alpha + \beta \ln(MW_{zt}) + \phi_z + \tau_{pt} + \varepsilon_{zpt} \tag{1}$$

In Equation 1, $ZORI_{zpt}$ is the ZORI value in zip code z and year-month t, MW_{zt} is the effective minimum wage by zip code and time, ϕ_z and τ_{pt} are zip code and pair-time fixed effects respectively, and ε_{zpt} is the errors. Note that $ZORI$ and ε are counted not just by zip code and year-month but also by zip code pair as individual zip codes can appear in several pairs.

Additionally, I cluster my standard errors at two levels. The first is the border segment level, which accounts for autocorrelation in zip code pairs that are in the same pair of local areas. For example, zip code pairs with one zip code in Washington, DC, and the other in Arlington, VA, would be in one cluster while pairs with one zip code in Washington, DC, and the other in Bethesda, MD, would be in another cluster. This allows for the difference in trends between the treated zip codes and zip codes of a particular untreated city to be correlated over time. However, individual zip codes are likely to be in multiple border segment clusters due to the close proximity of most cities in these MSAs, leading to correlation between the clusters. Thus, the second level of clustering is at the zip code level to account for this correlation. Altogether, I have 227 border segment clusters and 356 zip code clusters in my Unbalanced and Interpolated panels, as well as 126 border segment clusters and 153 zip code clusters in my Zip Codes Dropped Panel, all of which are much higher than the 42 required for asymptotically valid standard errors (Bertrand, Duflo, & Mullainathan, 2004).

The main advantage of this method over traditional two-way fixed effects estimation is the economic similarity we expect between nearby zip codes. Crucially in this case, we expect that nearby zip codes experience very similar rent trends over time. Thus, by using pair-time fixed effects with pairs of nearby zip codes, this design accounts for problems like spatial heterogeneity and reverse causality that can plague two-way fixed effects models. The latter issue is particularly relevant for this topic. While we may expect to see landlords react to minimum wage increases, it is also likely that city lawmakers increase minimum wages in response to higher costs of living. However, within each of my pairs, I have two nearby zip codes experiencing similar rent trends that could plausibly induce city minimum wage legislation, but with only one of the zip codes actually experiencing that legislation. So I am able to draw out just the effect of minimum wage on rent, and not the reverse.

There are some potential drawbacks to this methodology. Primarily, the border pairs methodology can in some cases be subject to spillover effects. This is especially salient for my analysis as zip codes are often quite small and city boundaries are very porous. So it is reasonable to expect that some workers in a particular city may live in a zip code just outside the city, and thus that landlords outside of cities with higher minimum wages may react to those wages rather than the ones in their own zip codes. However, because I create zip code pairs based on distance and not just contiguity, fewer of my pairs would be subject to any spillover across minimum wage boundaries that is present.

5 Results and Discussion

5.1 Primary Results

Table 1 summarizes the results from Equation 1 on my three panels. In this estimation, the coefficient for $lnMW_{zt}$ is the elasticity of typical rent prices with respect to minimum wages. I find that a 10% increase in city minimum wage induces a 0.8-1.1% increase in typical rent price. Even though the coefficients are statistically non-zero, this effect is still quite small as we would expect a coefficient closer to 1 if landlords were increasing their rents proportionally to minimum wage hikes.

Table 1: Minimum Wage Effect on ZORI

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	0.083***	0.081***	0.110***
	(0.029)	(0.029)	(0.029)
Observations	121596	127512	54504
Zip Code FE	Yes	Yes	Yes
Pair-Time FE	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To best understand these results, though, they should be put into more context. A common rule of thumb is to spend about 30% if one's income on housing costs, such as rent. However, according to recent reports by the National Low Income Housing Coalition (NLIHC) and Harvard University's Joint Center for Housing Studies (JCHS), the vast majority of low-income renters pay *more* than 30% of their income on rent, with a substantial portion spending over 50% (NLIHC, 2021; JCHS, 2021). If we assume that a worker at minimum wage spends 50% of their income on rent and sees rent increases that correspond to the highest coefficient in Table 1 following minimum wage hikes, then 5.5% of their new income would be passed through to their landlord. On the other end of this spectrum, we might assume that they spend 30% of their income on rent and see rent increases that follow the lowest coefficient, giving a pass-through rate of 2.43%. The estimated rates in this range are quite small, suggesting that landlords would receive a tiny portion of their tenants' additional income from wage increases, leaving those tenants with the vast majority of the monetary benefits. Though because my estimates are at the median level, it is unclear how accurate they are at the lower end of the housing cost distribution. Comparatively, these pass-through rates are smaller than the 7.5-13.5% pass-through rates found in Yamagishi (2021), and are *much* smaller than the finding in Agarwal et al. (2020) that the average rent increase soaked up 54.6% of the average minimum wage increase in their sample. Additionally, I can reject even the smallest pass-through rate from Yamagishi (2021) at the 95% confidence level given my standard errors, at least at the median level.

5.2 Dynamic Evidence

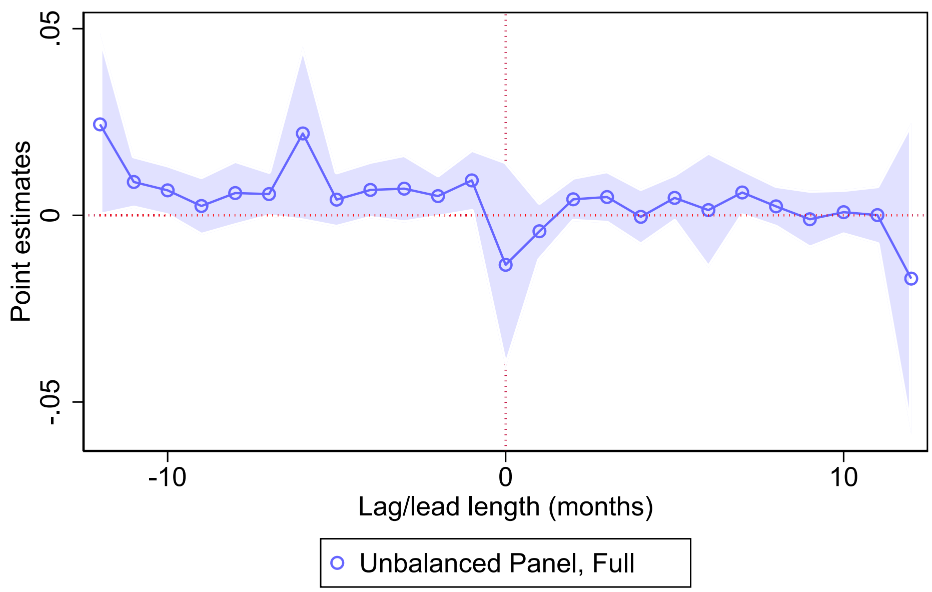
I also consider how a minimum wage change in one time period can affect rent prices in past or future time periods. This is important for two reasons. First, it can illuminate whether there are rent price pre-trends in my data, which would violate one of the basic assumptions required for difference-in-differences estimation. Second, it's possible that rent prices do change in response to minimum wage policy, but not in the same month that the policy takes effect. For example, Agarwal et al. (2020) find that there is no contemporaneous effect of minimum wages on rent, but that rents do increase a few months after wages increase. This is very possible in the context of landlord reactions because landlords generally cannot raise their rent in the middle of a lease term and must raise rents when signing on a new tenant. This limits their ability to react at the time of wage hikes, so we may see rents go up several months before or after wage policies take effect. This is especially true if minimum wages increase during a time of year when relatively few are signing new leases.

To analyze the dynamic effects of minimum wage, I use a model similar to Equation 1 but with 12 months of lags and leads:

$$\ln(ZORI_{zpt}) = \alpha + \sum_{i=-12}^{12} \beta_i \ln(MW_{z,t+i}) + \phi_z + \tau_{pt} + \varepsilon_{zpt} \tag{2}$$

As in Equation 1, I cluster standard errors at the border segment and zip code levels. Figure 1 graphs the point estimate and 95% confidence interval of each lead and lag coefficient, with negative months corresponding to leads and positive months corresponding to lags.[3] There are multiple positive and statistically significant coefficients in the leads, and the sum of lead coefficients are jointly significant, while these effects do not persist into the contemporaneous and lagged time periods, suggesting that there is not a pre-trend present. Rather, this implies that the positive coefficients in Table 1 are entirely explained by an anticipatory effect: rents go up in response to minimum wage changes that are scheduled to take place several months later.

Figure 1: Minimum Wage Effect on ZORI Over Time



Anticipation effects are plausible in this context because laws are often passed well in advance of minimum wage increases, many times years in advance. Nonetheless, it is still curious to see no effects during or after minimum wage changes. Given these results, I investigate whether there are any particular cities driving them. I re-run Equation 2 on my panels several times, each time leaving out zip code pairs in one of the cities in my sample. Only removing observations around New York City has any meaningful effect on the coefficients. Although, leaving out New York observations does remove a sizeable portion of my sample: 974 of the 1,771 zip code pairs in my Unbalanced and Interpolated panels and 584 of the 757 pairs in my Zip

[3]While Figure 1 just graphs the coefficients for the unbalanced panel, Tables A.2 - A.3 provide the numerical coefficients for all three of my panels. There are some differences between them in terms of which specific coefficients are significant, but all three show an anticipatory effect with practically no contemporaneous or lagged significance.

Codes Dropped panel are left out. Additionally, while I am left with 88 border segment and 248 zip code clusters in my larger samples, removing New York leaves only 28 border segment and 77 zip code clusters in my Zip Codes Dropped panel, so the standard errors from that panel's regressions without New York are less reliable (Bertrand et al., 2004). Figure 2 plots the coefficients from running Equation 2 without any New York area observations.[4] Without these pairs, the anticipatory effect goes away, but all of the other coefficients stay statistically insignificant.

Table A.6 provides the coefficients from running Equation 1 on all three panels without New York observations and confirms that the positive effect on rent disappears entirely without them. Interestingly, I find a statistically significant negative coefficient with the smaller balanced panel, though it is still very small in magnitude. It is hard to imagine rents decreasing in response to higher minimum wages, but there are some potential explanations. First, it is possible that there *are* some real spillover effects and that zip codes just outside of cities should be considered treated rather than untreated. If that's the case, then this coefficient may be picking up that these zip codes see rent increases due to the minimum wages while the zip codes they're paired with see smaller or no rent increases. And since most of my zip code pairs around New York contain one zip code in New York and another in New Jersey, spillover effects would naturally occur less often there than in other parts of my sample. So it would be unsurprising to see the spillover effects manifest once those observations have been removed. Second, since my rent index measures the overall typical rent price rather than the typical low-cost unit rent price, there may be small negative effects happening at the median rent level unrelated to minimum wage while low-cost units raise their rents in response to wage policies. Finally, since I lose most of my clusters in the Zip Codes Dropped panel when removing New York observations, it is very possible that the significance of this estimate is simply being overstated by invalid standard errors.

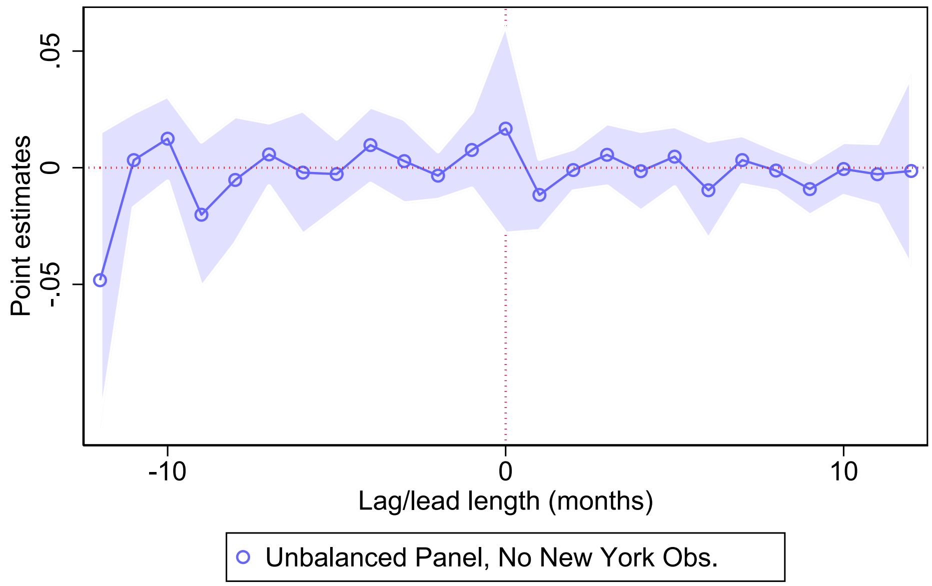
5.3 Covariate Balance

One of the crucial assumptions of my methodology is that paired zip codes are economically and demographically similar enough that I can single out the minimum wage effect on rent by including pair-time fixed effects. To test this assumption, I gather a number of zip code covariates from the the Census, American Community Survey (ACS), and the Internal Revenue Service (IRS). The covariates for which I gathered data are described in Table A.7 and basic summary statistics of ZORI, minimum wages, and the covariates are provided in Table A.8.[5]

Initially, I compute the means of each covariate for treated and untreated zip codes in my sample of 356

[4] Tables A.4 - A.5 provide the numerical coefficients for all three panels without New York observations.
[5] All percentage variables are on a 0-100 scale.

Figure 2: Minimum Wage Effect on ZORI Over Time, w/o New York



zip codes and perform a t-test on the means difference for each one. These results are reported in Table A.9 and give a general sense of whether my treated and untreated zip codes are similar. Most of the p-values are quite low, suggesting that there may be serious imbalance in my sample.[6] However, since I am averaging across all of the cities in my data and then testing for a difference in means, this is not the best way to tell whether my zip code pairs are balanced, which is the actual assumption that needs to be satisfied. To more carefully test this assumption, I run a series of regressions on my zip code pairs that estimate whether the average difference of each covariate between paired zip codes is different from zero using the following equation:

$$Y_p = \alpha + \beta MW_p + \varepsilon_p \tag{3}$$

In Equation 3, Y_p is the difference in values for a given covariate between the treated and untreated zip codes in pair p, MW_p is the average difference in minimum wages between the treated and untreated zip codes in pair p across my panel's time period (in 2010 dollars), and ε_p is the errors. I run this regression for each of my covariates on my large (unbalanced and interpolated) pair sample and my small (zip codes

[6]Tables A.10 - A.12 similarly compare covariate means between in-sample and out-of-sample zip codes, in-sample treated and untreated zip codes in and around New York, and in-sample treated and untreated zip codes outside of the New York area. The results are generally the same: most of the variables seem to have different means between the two groups.

dropped) pair sample, both with and without New York observations.[7] Additionally, I cluster standard errors at the border segment and zip code level as in my previous equations.

Table A.13 reports the coefficients and standard errors on MW_p from each of these regressions. Given the magnitude of coefficients and the number of statistically significant coefficients in this table, far fewer covariates appear to be imbalanced compared to the previous balance tables. With these results, I examine whether the covariates with significance in Table A.13 actually affect my primary results by running the following regression on my three panels:

$$\ln(ZORI_{zpt}) = \alpha + \beta \ln(MW_{zt}) + \boldsymbol{\delta X_z} \times \sum_{i=2}^{72} T_i + \phi_z + \tau_{pt} + \varepsilon_{zpt} \tag{4}$$

This has the same general interpretation of variables as Equation 1, but I now include $\boldsymbol{X_z}$, a vector containing any covariates that are significant at any conventional level in Table A.13 for the given panel, as controls.[8] Since I do not collect dynamic covariate data, I interact each covariate in $\boldsymbol{X_z}$ with time period dummies, T_i, so that they do not get swept out by my zip code fixed effects, ϕ_z. In fact, while zip code fixed effects should naturally sweep out covariate imbalance, there is still the concern that characteristic differences between zip codes may lead to differential trends in rent or differential effects from minimum wages. So it is worthwhile to investigate this possibility even with fixed effects. Again, I cluster standard errors at the border segment and zip code levels. Table 2 reports the coefficients on minimum wage when I include controls. Comparing this table to the primary results in Table 1, it does not seem that adding the controls significantly changes the coefficients on minimum wage. So even though there may be some covariate imbalance in my sample, my results appear robust to the it.

Table 2: Minimum Wage Effect on ZORI, w/ Controls

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	0.073**	0.071**	0.107***
	(0.032)	(0.032)	(0.030)
Observations	120589	126504	53568
Controls	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes
Pair-Time FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In addition, Table A.14 reports the results of this estimation on my panels without New York observations.

[7] The only difference between my unbalanced and interpolated panels is in how I deal with missing values of ZORI. Since that is not relevant here, they are effectively the same panel when using Equation 3.

[8] For example, I would include % 65 or Older, % Black, % Hispanic/Latino, and Vacancy Rate in $\boldsymbol{X_z}$ when using my full unbalanced and interpolated panels.

Similarly, the coefficients on my unbalanced and interpolated panels remain practically unchanged. However, the coefficient on my zip codes dropped panel shrinks in magnitude and becomes statistically insignificant. This suggests that the significant and negative coefficient from Table A.6 may have been due to covariate imbalance in my non-New York zip code pairs rather than spillover effects, as I had previously mentioned.

5.4 Interaction with EITC

One of the downsides of the Zillow rent data mentioned earlier in this paper is that it just measures the typically observed rent price in a given zip code, similar to a measure of the median rent price. While it is generally useful for my analysis to have the data already indexed and aggregated, it means that I cannot easily test whether there are rent shifts happening at the lower end of the rental cost distribution that simply don't appear near the median. This is especially problematic because it would be expected that minimum and low-wage workers would rent relatively low-cost units, so minimum wage hikes are more likely to affect these units in general.

However, I can get a better sense of how low-income renters are affected using Earned Income Tax Credit (EITC) data. According to the IRS website, the EITC is a tax break that "helps low- to moderate-income workers and families," so I can use the percentage of individuals filing an EITC in a given zip code as a proxy for the proportion of low- to moderate-income workers in that zip code. In turn, I expect that zip codes with higher percentages of EITC filing would contain more low-income individuals renting near that zip code's median rent price. If the minimum wage does have a positive effect on rents but is localized on units being rented by low-wage workers, then there should be a larger effect of minimum wage on ZORI in zip codes with more EITC filing. I test this possibility using the following equation:

$$\ln(ZORI_{zpt}) = \alpha + \beta \ln(MW_{zt}) + \gamma \ln(MW_{zt}) \times EITC_p + \phi_z + \tau_{pt} + \varepsilon_{zpt} \tag{5}$$

The variables here have the same interpretation as in Equation 1, but I include an interaction between $\ln(MW_{zt})$ and $EITC_p$, where $EITC_p$ is the average percentage of individuals filing an EITC in 2010 in zip code pair p.[9] As in my previous equations, I cluster standard errors at the border segment and zip code levels. Table 3 reports the results from this equation. The point estimates of the coefficients are not much different than in my primary results in Table 1, although they do lose some significance. However, the interaction term is effectively zero, suggesting that zip codes with higher rates of EITC filing do not

[9] The reason I use an EITC variable at the pair level rather than zip code level is due to how my data are structured. In my dataset, each observation is a zip code pair in a given time period, rather than a zip code in a given time period. So it is easier to interact minimum wage with an average measure of EITC filings within a zip code pair. And because my EITC variables seemed well-balanced in Section 5.3, the average percent of EITC filings within a pair is likely very close to the levels of each of that pair's zip codes.

experience different minimum wage effects on their typical rent prices.[10]

Table 3: Minimum Wage Effect on ZORI, w/ EITC Interaction

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	0.081**	0.074*	0.074
	(0.039)	(0.042)	(0.061)
$lnMW_{zt} \times Avg. \%\ Filing\ EITC\ 2010_p$	0.000	0.000	0.002
	(0.002)	(0.002)	(0.003)
Observations	120589	126504	53568
Zip Code FE	Yes	Yes	Yes
Pair-Time FE	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This is not a perfect method for determining whether there is a differential effect on rent at the lower end of the rental cost distribution. But it does give a better idea of how low-income renters are impacted differently. And from this preliminary evidence, it appears that low-wage renters may see upswings in their rent following minimum wage hikes, but that these rent increases won't be nearly proportional to the renters' income increases.

6 Conclusion

I estimate the impacts of city minimum wages on home rent prices with monthly zip code data for 16 U.S. cities that have local minimum wages. I use a modified border pairs design wherein I pair up zip codes that are nearby and on opposite sides of city boundaries where there is a minimum wage difference. This work contributes to the existing literature in two main ways. First, it adds to the growing body of research on city minimum wages by analyzing its rent effect, something that has yet to be researched at that policy level and is scarcely researched at all in the minimum wage literature. Second, it contributes to the existing studies of wages and rent by utilizing the border pairs design instead of traditional two-way fixed effects methods.

My primary results indicate that a 10% increase in city minimum wage causes the typically observed rent price in that city to increase by 0.8-1.1%. It turns out that this is driven by observations in the New York area where there is an anticipation effect, i.e., rents increase in response to future minimum wage increases. Without New York observations, the effect goes to zero. This contrasts my hypothesis and the conclusions of other papers which find a larger positive effect on rent when minimum wages at higher policy levels increase (Agarwal et al., 2020; Yamagishi, 2021). In particular, the result from Agarwal et al. (2020) that average

[10]Table A.15 reports the results of Equation 5 on my panels without New York observations. The interpretation is very similar: the interaction term is practically zero and the coefficients on minimum wage do not change much from Table A.6.

rent hikes soak up over half of the average minimum wage increase is much larger than my findings, but is dubious to begin with. Even though many low-wage renters spend a large portion of their income on rent, a majority still don't spend more than 50%, so it's hard to imagine this being a real pass-through rate for most renters. One possible reason for their result is that their panel goes from 2000 to 2009, during which the United States experienced a historic housing bubble, which may be driving up their estimates. Regardless, there are several potential reasons for for my estimates being as small as they are.

First, the rent data from Zillow are a measure of the overall typical rent price in a given zip code, similar to a median measure of rent prices. It is very possible that rent prices at the lower end of the rental distribution increase significantly after minimum wage hikes, but rents in the middle and upper quantiles remain relatively unchanged. While I am unable to directly test this hypothesis with the Zillow data, I make a preliminary attempt by regressing ZORI on minimum wages interacted with the percentage of individuals filing an Earned Income Tax Credit (EITC), a tax break meant to help low- to medium-income individuals. This gives an idea of how the typical rent changes differently in zip codes with higher proportions of low-income workers where I would expect to see more low-income individuals renting at or near the typical rate. The results from this indicate that there is no difference, suggesting further that low-wage renters benefit from city minimum wages without having much of their new income soaked up by housing costs.

Additionally, research has found positive effects on income from minimum wages up to around the 25$^{\text{th}}$ percentile of the wage distribution (Autor, Manning, & Smith, 2016). So it is possible that minimum wages often affect the housing decisions of workers who rent close to the median of rent prices, in which case we would expect to see a positive effect even at the typical rent price. However, I am unable to concretely test this or my hypothesis about low-cost rental units, so in the future I would like to use more granular rental data to carefully test these ideas.

Second, there could be spillover effects dampening the coefficients, especially since city borders are naturally porous. If low-wage workers are willing to live just outside a city with higher minimum wages and travel into the city for work, then landlords in those surrounding zip codes may raise their rents as well as landlords inside the city. This would attenuate the coefficient on minimum wage when regressing with pairs of nearby zip codes. However, because zip codes in my data are paired together based on distance rather than contiguity, some pairs contain zip codes that are miles apart. So it's less likely that all of my pairs would be subject to spillover that would greatly attenuate my results.

Third, it is possible that higher city minimum wages affect housing demand very little, if at all. My hypothesis rests on housing demand increasing after wage hikes from two main channels: workers moving into a city with higher wages and low-wage workers who currently live in those cities seeking better housing after seeing their income go up. But it is possible that neither of these happen enough to drive up typical rent prices. Moving can be an expensive endeavor, so low-wage workers may not perceive much benefit in moving into a city just because wages are slightly higher, especially if there are some spillover effects and they can commute into the city at a lower cost. In fact, some papers find evidence that low-skilled workers *don't* tend to migrate to areas with higher minimum wages (Monras, 2019; Cadena, 2014). Further, because rents soak up huge portions of low-wage renters' income, renters who experience minimum wage hikes in the cities in which they currently live may prefer to allocate the extra income to other parts of their budgets, rather than to seek higher quality housing and spend the same proportion of their budgets on rent. So higher minimum wages do not necessarily lead to significantly higher housing demand, at least through these channels.

Finally, since minimum wages are meant to benefit low-income workers, cities that pass minimum wage ordinances may tend to pass concurrent policies that directly affect rent, such as rent control, in an attempt to further improve conditions for those workers. In those cases, even if minimum wage policies increase housing demand, landlords would be constrained in their ability to react to the higher demand by raising rent prices. To better understand how landlords respond to city minimum wage hikes, future research should investigate these possibilities.

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A Appendix

Table A.1: Observations and Missing Values by MSA

MSA	Frequency of Observations	Percent of Observations	Frequency of Missing Values	Percent of Missing Values
Albuquerque, NM	288	0.57	5	0.26
Chicago, IL	5184	10.20	171	8.74
Los Angeles-Long Beach-Anaheim, CA	12456	24.50	600	30.66
Minneapolis-St Paul, MN	1872	3.68	49	2.50
New York, NY	10512	20.68	132	6.75
Portland, OR	1728	3.40	122	6.23
San Diego, CA	3672	7.22	193	9.86
San Francisco, CA	2880	5.67	127	6.49
San Jose, CA	864	1.70	49	2.50
Seattle, WA	3888	7.65	326	16.66
Washington, DC	7488	14.73	183	9.35
Total	50832	100.00	1957	100.00

Notes: An observation is a zip code during a particular year-month.

Figure A.1: Histogram of Number of Missing Values per Zip Code, All Zip Codes

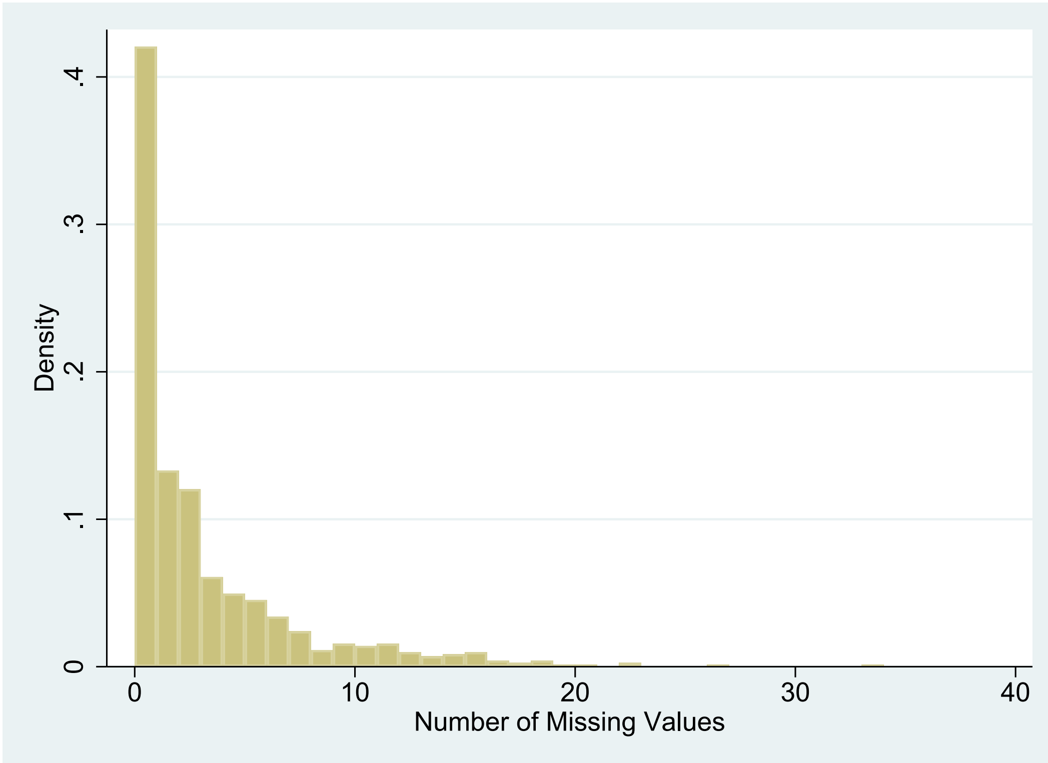

Figure A.2: Histogram of Number of Missing Values per Zip Code, Zip Codes w/ At Least One Missing Value

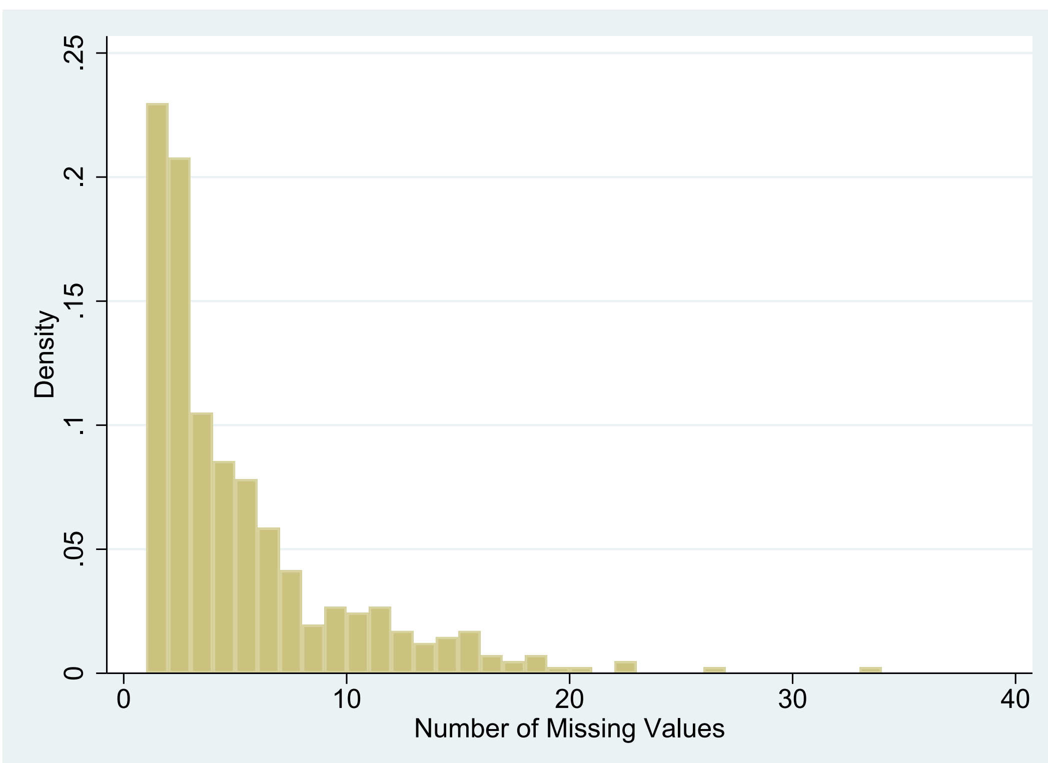

Figure A.3: Missing Values by Year

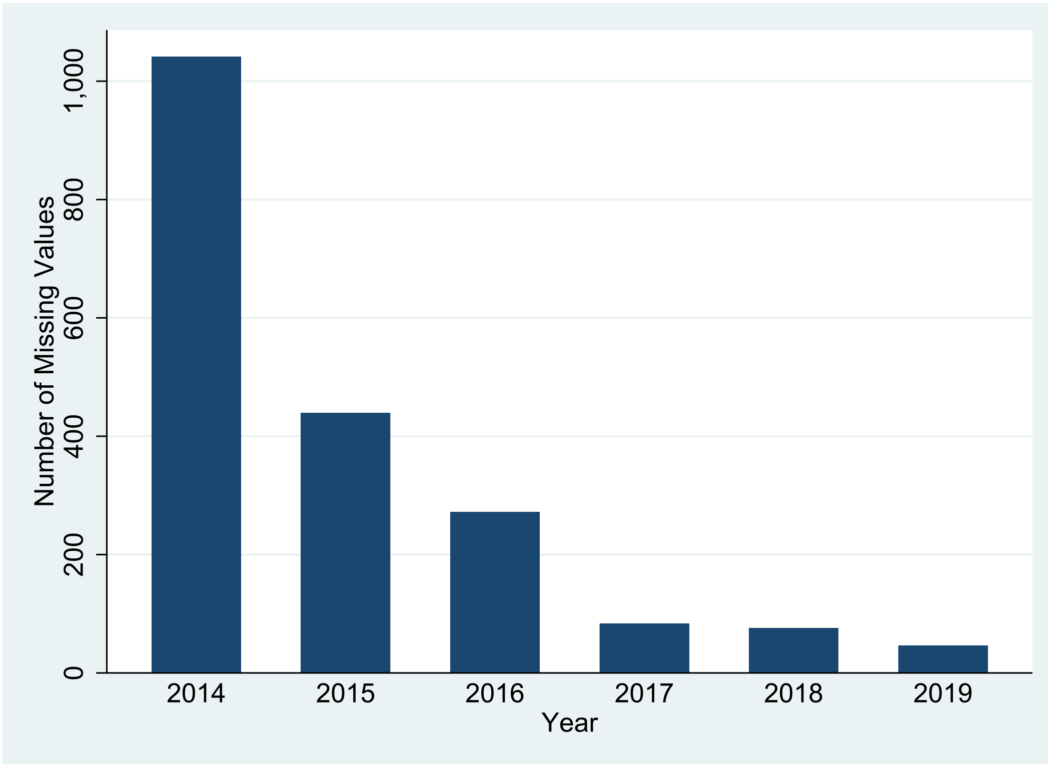

Figure A.4: Missing Values by Month

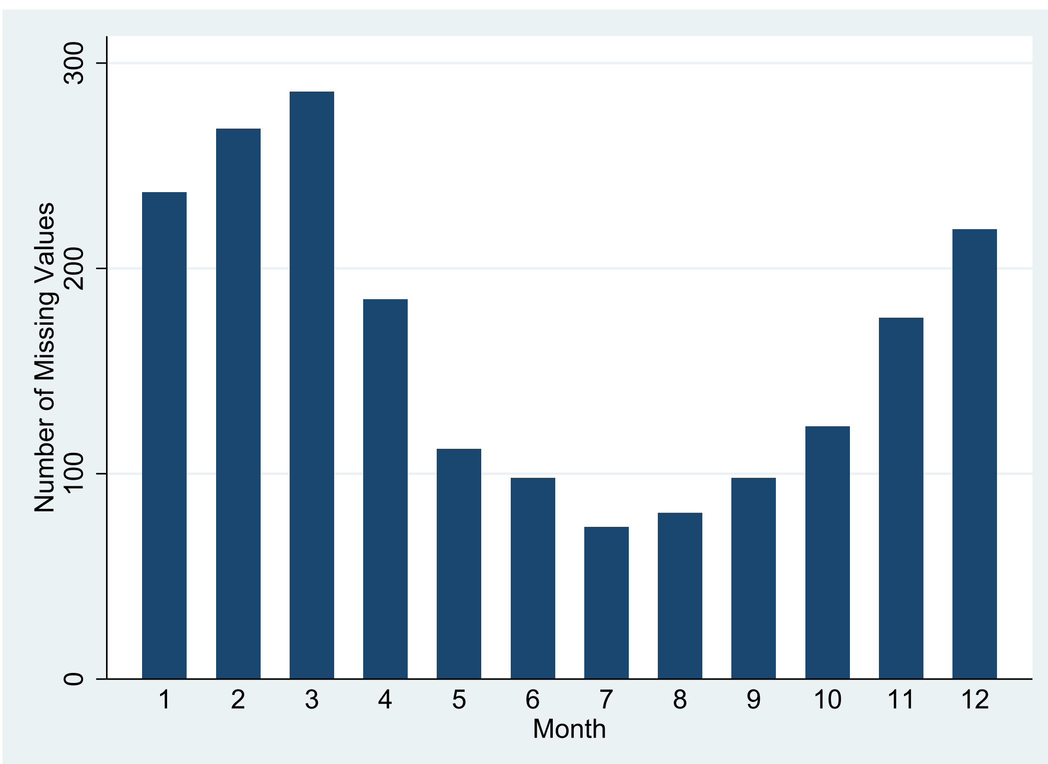

Figure A.5: District of Columbia Area Zip Codes in ZORI Data

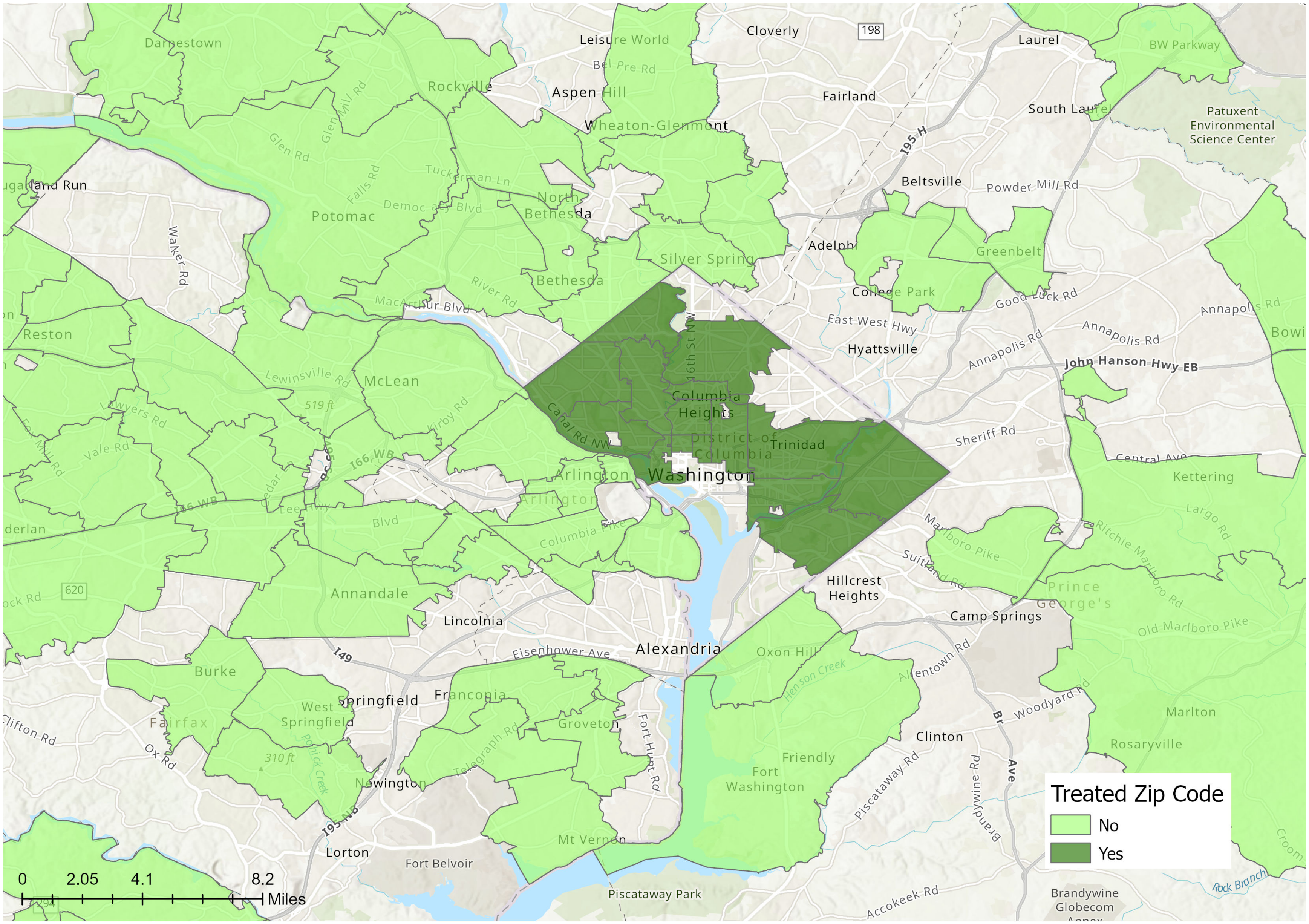

Figure A.6: District of Columbia Area Zip Codes in ZORI Data with No Missing Values

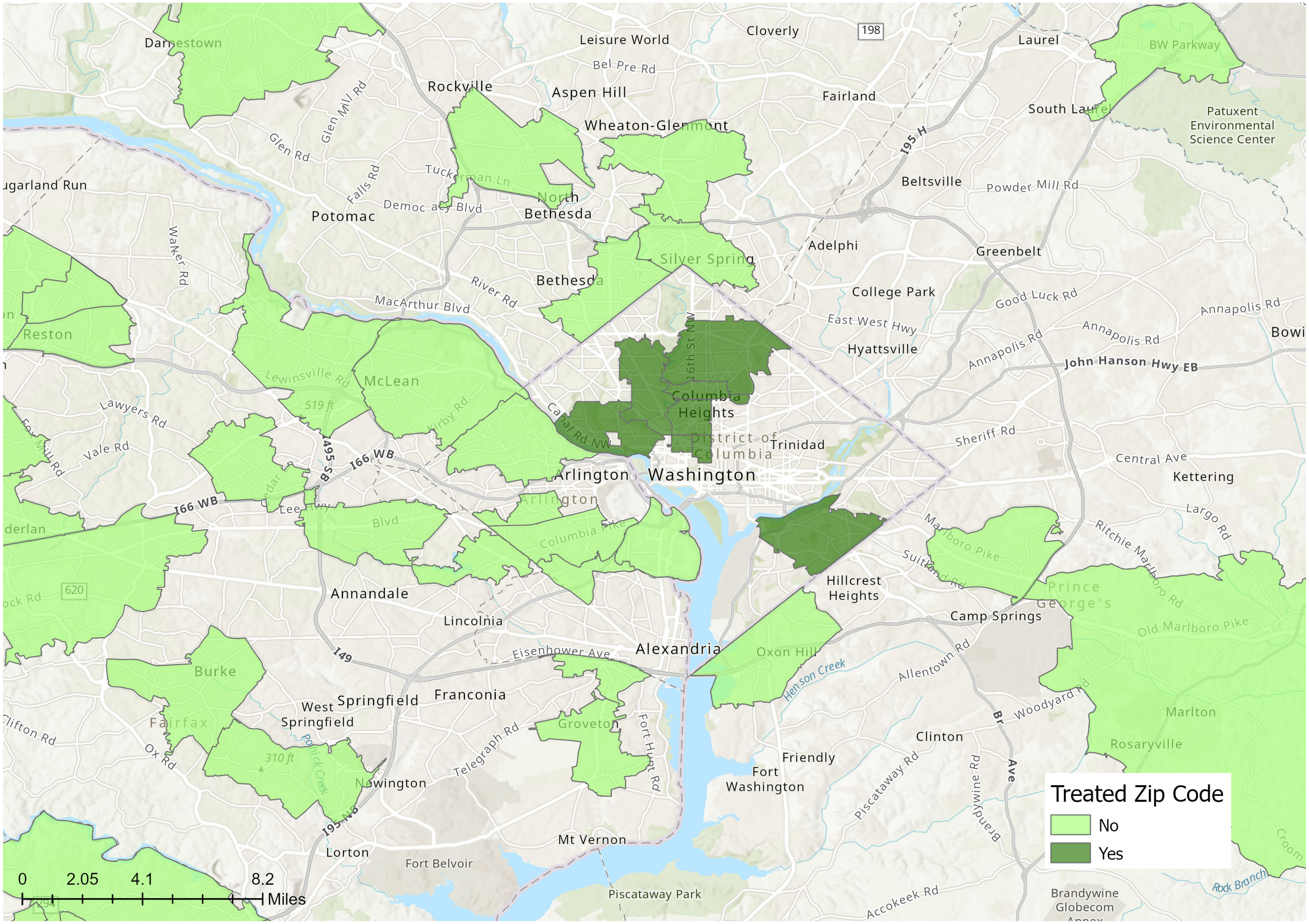

Table A.2: Minimum Wage Effect on ZORI, Leads

	Unbalanced	Interpolated	MVs Dropped
$lnMW_z$ (*Lead* 12)	0.024*	0.031**	0.036**
	(0.012)	(0.013)	(0.014)
$lnMW_z$ (*Lead* 11)	0.009**	0.005**	0.008***
	(0.004)	(0.002)	(0.003)
$lnMW_z$ (*Lead* 10)	0.007*	0.005**	0.008***
	(0.003)	(0.002)	(0.002)
$lnMW_z$ (*Lead* 9)	0.002	0.006*	0.008**
	(0.004)	(0.003)	(0.003)
$lnMW_z$ (*Lead* 8)	0.006	0.004	0.009***
	(0.004)	(0.003)	(0.003)
$lnMW_z$ (*Lead* 7)	0.006*	0.004	0.008**
	(0.003)	(0.003)	(0.003)
$lnMW_z$ (*Lead* 6)	0.022*	0.021*	0.037**
	(0.012)	(0.012)	(0.014)
$lnMW_z$ (*Lead* 5)	0.004	0.005*	0.008**
	(0.004)	(0.003)	(0.003)
$lnMW_z$ (*Lead* 4)	0.007*	0.007*	0.009**
	(0.004)	(0.004)	(0.004)
$lnMW_z$ (*Lead* 3)	0.007	0.009*	0.012**
	(0.005)	(0.005)	(0.005)
$lnMW_z$ (*Lead* 2)	0.005*	0.004**	0.007***
	(0.003)	(0.002)	(0.002)
$lnMW_z$ (*Lead* 1)	0.009**	0.004**	0.007***
	(0.004)	(0.002)	(0.002)
$lnMW_{zt}$	-0.013	-0.017	0.000
	(0.014)	(0.013)	(0.020)
Observations	82433	85008	36336

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Minimum Wage Effect on ZORI, Lags

	Unbalanced	Interpolated	MVs Dropped
$lnMW_{zt}$	-0.013	-0.017	0.000
	(0.014)	(0.013)	(0.020)
$lnMW_z$ $(Lag\ 1)$	-0.004	0.002	0.002
	(0.004)	(0.002)	(0.002)
$lnMW_z$ $(Lag\ 2)$	0.004	0.003	0.001
	(0.003)	(0.002)	(0.002)
$lnMW_z$ $(Lag\ 3)$	0.005	0.002	-0.002
	(0.004)	(0.003)	(0.004)
$lnMW_z$ $(Lag\ 4)$	-0.000	0.002	-0.000
	(0.004)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 5)$	0.005	0.003	0.002
	(0.003)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 6)$	0.001	0.007	0.002
	(0.008)	(0.008)	(0.010)
$lnMW_z$ $(Lag\ 7)$	0.006*	0.002	0.003
	(0.003)	(0.002)	(0.002)
$lnMW_z$ $(Lag\ 8)$	0.002	0.001	0.002
	(0.003)	(0.003)	(0.003)
$lnMW_z$ $(Lag\ 9)$	-0.001	-0.000	-0.002
	(0.004)	(0.003)	(0.004)
$lnMW_z$ $(Lag\ 10)$	0.001	0.002	0.003
	(0.003)	(0.002)	(0.002)
$lnMW_z$ $(Lag\ 11)$	0.000	0.002	0.003
	(0.004)	(0.002)	(0.002)
$lnMW_z$ $(Lag\ 12)$	-0.017	-0.016	-0.059*
	(0.021)	(0.021)	(0.033)
Observations	82433	85008	36336

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Minimum Wage Effect on ZORI, Leads w/o New York

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_z$ $(Lead\,12)$	-0.048	-0.024	-0.042
	(0.032)	(0.035)	(0.057)
$lnMW_z$ $(Lead\,11)$	0.003	-0.003	-0.006*
	(0.010)	(0.003)	(0.003)
$lnMW_z$ $(Lead\,10)$	0.012	-0.004	-0.007*
	(0.009)	(0.003)	(0.004)
$lnMW_z$ $(Lead\,9)$	-0.020	-0.008	-0.043
	(0.016)	(0.014)	(0.027)
$lnMW_z$ $(Lead\,8)$	-0.005	-0.004	-0.001
	(0.014)	(0.003)	(0.005)
$lnMW_z$ $(Lead\,7)$	0.006	-0.003	-0.003
	(0.007)	(0.003)	(0.004)
$lnMW_z$ $(Lead\,6)$	-0.002	0.004	0.036
	(0.014)	(0.014)	(0.032)
$lnMW_z$ $(Lead\,5)$	-0.003	-0.001	-0.003
	(0.008)	(0.003)	(0.003)
$lnMW_z$ $(Lead\,4)$	0.010	0.003	0.005
	(0.008)	(0.008)	(0.015)
$lnMW_z$ $(Lead\,3)$	0.003	0.004	0.013
	(0.009)	(0.008)	(0.012)
$lnMW_z$ $(Lead\,2)$	-0.003	-0.002	-0.006
	(0.005)	(0.002)	(0.004)
$lnMW_z$ $(Lead\,1)$	0.008	-0.002	-0.005
	(0.008)	(0.002)	(0.003)
$lnMW_{zt}$	0.017	0.010	0.044
	(0.023)	(0.020)	(0.033)
Observations	36214	38256	8304

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Minimum Wage Effect on ZORI, Lags w/o New York

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	0.017	0.010	0.044
	(0.023)	(0.020)	(0.033)
$lnMW_z$ $(Lag\ 1)$	-0.012	-0.001	-0.005
	(0.008)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 2)$	-0.001	0.001	-0.004
	(0.005)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 3)$	0.006	0.002	-0.015*
	(0.007)	(0.006)	(0.008)
$lnMW_z$ $(Lag\ 4)$	-0.001	0.000	-0.006
	(0.009)	(0.003)	(0.003)
$lnMW_z$ $(Lag\ 5)$	0.005	0.001	-0.004
	(0.007)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 6)$	-0.010	-0.001	-0.010
	(0.011)	(0.011)	(0.022)
$lnMW_z$ $(Lag\ 7)$	0.003	-0.001	-0.003
	(0.006)	(0.002)	(0.002)
$lnMW_z$ $(Lag\ 8)$	-0.001	-0.003	-0.008
	(0.005)	(0.004)	(0.008)
$lnMW_z$ $(Lag\ 9)$	-0.009	-0.004	-0.016*
	(0.006)	(0.005)	(0.009)
$lnMW_z$ $(Lag\ 10)$	-0.001	-0.001	-0.002
	(0.006)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 11)$	-0.003	-0.001	-0.003
	(0.007)	(0.002)	(0.003)
$lnMW_z$ $(Lag\ 12)$	-0.001	-0.000	-0.038
	(0.021)	(0.021)	(0.032)
Observations	36214	38256	8304

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Minimum Wage Effect on ZORI, w/o New York

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	-0.032	-0.034	-0.107**
	(0.038)	(0.042)	(0.044)
Observations	52937	57384	12456
Zip Code FE	Yes	Yes	Yes
Pair-Time FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Covariate Descriptions

Covariate	Description
Population	*Total population (2010 Census)*
% Under 18	*Percent of population who are under 18 years old (2010 Census)*
% 65 or Older	*Percent of population who are at least 65 years old (2010 Census)*
Median Age	*Median age of population (2010 Census)*
% Nonwhite	*Percent of population who are not white at all (2010 Census)*
% Not White Alone	*Percent of population who are anything except white alone (2010 Census)*
% Black	*Percent of population who are black or African American (2010 Census)*
% Hispanic/Latino	*Percent of population who are Hispanic or Latino (2010 Census)*
% with Less Than HS	*Percent of population who are at least 18 years old having earned less than a high school diploma (2011 ACS 5-year estimate)*
% with Bachelors or Higher	*Percent of population who are at least 18 years old with a bachelors or higher degree (2011 ACS 5-year estimate)*
Median Income	*Median income over previous 12 months in 2011 inflation adjusted dollars (2011 ACS 5-year estimate)*
Unemployment Rate	*Unemployment rate for population who are at least 16 years old (2011 ACS 5-year estimate)*
Average Household Size	*Average number of people per household (2010 Census)*
% of Population Renting	*Percent of population in occupied housing units who are renting (2010 Census)*
% of Units Rented	*Percent of occupied housing units that are occupied by renters (2010 Census)*
Vacancy Rate	*Percent of total housing units that are unoccupied (2010 Census)*
% Filing EITC 2010	*Percent of total 2010 tax filings that included an Earned Income Tax Credit (2010 IRS Statistics of Income)*
% Filing EITC 2013	*Percent of total 2013 tax filings that included an Earned Income Tax Credit (2013 IRS Statistics of Income)*

Table A.8: Summary of Covariates, In-Sample Zip Codes

	Mean	SD	Min	Max	Obs.
Average ZORI	2115.73	1516.36	678.40	26632.09	356.00
Average Minimum Wage	10.43	1.25	7.25	13.23	356.00
Population	40011.57	20572.87	1204.00	1.1e+05	355.00
% Under 18	18.84	6.70	1.82	32.71	355.00
% 65 or Older	11.74	4.20	0.80	38.68	355.00
Median Age	36.33	4.44	23.10	57.50	355.00
% Nonwhite	35.21	21.79	5.24	99.08	355.00
% Not White Alone	37.66	21.72	5.82	99.46	355.00
% Black	14.41	20.52	0.52	98.91	355.00
% Hispanic/Latino	19.01	16.11	0.93	84.64	355.00
% with Less Than HS	11.93	8.60	0.00	43.70	355.00
% with Bachelors or Higher	44.74	20.51	6.15	89.78	355.00
Median Income	71578.39	29858.96	19692.00	2.3e+05	355.00
Unemployment Rate	8.13	3.55	1.90	24.60	355.00
Average Household Size	2.38	0.49	1.32	3.83	355.00
% of Population Renting	52.77	19.75	8.47	94.90	355.00
% of Units Rented	54.56	19.50	7.72	95.40	355.00
Vacancy Rate	8.12	4.38	2.47	30.73	355.00
% Filing EITC 2010	14.99	10.31	2.42	57.92	355.00
% Filing EITC 2013	15.13	10.64	2.10	60.05	355.00

Notes: There is one zip code in New York City that is included in Zillow's data but does not have any covariate data.

Table A.9: Comparison of Covariates, In-Sample Zip Codes

	Treated		Untreated		
	Mean	SD	Mean	SD	p-value
Population	41447.89	22698.60	37464.34	15895.33	0.080
% Under 18	17.07	7.11	21.96	4.47	0.000
% 65 or Older	11.23	4.04	12.65	4.34	0.002
Median Age	35.53	4.06	37.74	4.73	0.000
% Nonwhite	37.55	23.27	31.05	18.24	0.007
% Not White Alone	40.04	23.13	33.43	18.31	0.006
% Black	16.26	22.38	11.13	16.30	0.023
% Hispanic/Latino	18.70	16.21	19.58	15.99	0.621
% with Less Than HS	12.66	9.50	10.62	6.54	0.031
% with Bachelors or Higher	45.68	21.51	43.07	18.56	0.250
Median Income	66441.54	29508.59	80688.26	28368.72	0.000
Unemployment Rate	8.53	3.84	7.42	2.83	0.005
Average Household Size	2.27	0.52	2.56	0.36	0.000
% of Population Renting	59.50	18.10	40.84	16.74	0.000
% of Units Rented	61.45	17.62	42.36	16.51	0.000
Vacancy Rate	8.99	4.76	6.58	3.04	0.000
% Filing EITC 2010	16.49	11.80	12.76	6.88	0.011
% Filing EITC 2013	16.44	12.17	13.26	7.37	0.036

Notes: The p-values correspond to a t-test of the difference in means. Thus a lower p-value corresponds to a higher likelihood that the treated and untreated means are different.
Because EITC regulations can vary across states, the two EITC variables used here are restricted to zip codes that are in same-state pairs.

Table A.10: Comparison of Covariates, All Zip Codes

	In Sample		Out of Sample		
	Mean	SD	Mean	SD	p-value
Population	40011.57	20572.87	9025.43	13188.10	0.000
% Under 18	18.84	6.70	22.77	5.86	0.000
% 65 or Older	11.74	4.20	15.68	6.85	0.000
Median Age	36.33	4.44	41.05	7.53	0.000
% Nonwhite	35.21	21.79	13.90	19.56	0.000
% Not White Alone	37.66	21.72	15.17	19.92	0.000
% Black	14.41	20.52	7.59	15.49	0.000
% Hispanic/Latino	19.01	16.11	8.27	14.87	0.000
% with Less Than HS	11.93	8.60	15.45	10.07	0.000
% with Bachelors or Higher	44.74	20.51	19.53	13.71	0.000
Median Income	71578.39	29858.96	51176.90	21730.15	0.000
Unemployment Rate	8.13	3.55	8.31	7.15	0.638
Average Household Size	2.38	0.49	2.49	0.46	0.000
% of Population Renting	52.77	19.75	25.28	15.47	0.000
% of Units Rented	54.56	19.50	26.14	15.45	0.000
Vacancy Rate	8.12	4.38	16.48	14.24	0.000

Notes: The p-values correspond to a t-test of the difference in means. Thus a lower p-value corresponds to a higher likelihood that the in-sample and out-of-sample means are different.

Table A.11: Comparison of Covariates, In-Sample New York Area Zip Codes

	Treated		Untreated		
	Mean	SD	Mean	SD	p-value
Population	48570.89	26105.95	39625.81	16802.17	0.104
% Under 18	17.35	6.19	20.88	3.76	0.007
% 65 or Older	12.29	4.69	13.73	4.09	0.163
Median Age	36.16	4.25	38.06	4.08	0.048
% Nonwhite	42.49	24.56	33.72	18.60	0.098
% Not White Alone	44.68	24.79	35.82	18.89	0.098
% Black	19.36	24.19	12.29	13.08	0.158
% Hispanic/Latino	22.26	17.29	29.00	20.95	0.104
% with Less Than HS	15.34	10.40	14.41	7.50	0.674
% with Bachelors or Higher	46.19	23.01	38.70	16.37	0.127
Median Income	68843.51	37306.04	72100.46	27734.50	0.683
Unemployment Rate	8.57	3.37	7.96	2.84	0.409
Average Household Size	2.28	0.47	2.51	0.26	0.018
% of Population Renting	71.58	15.95	50.04	20.96	0.000
% of Units Rented	73.13	14.93	52.13	19.98	0.000
Vacancy Rate	9.16	4.43	7.35	2.51	0.051

Notes: The p-values correspond to a t-test of the difference in means. Thus a lower p-value corresponds to a higher likelihood that the treated and untreated means are different.

Table A.12: Comparison of Covariates, In-Sample Non-New York Area Zip Codes

	Treated		Untreated		
	Mean	SD	Mean	SD	p-value
Population	37496.10	19569.46	36913.38	15694.40	0.803
% Under 18	16.92	7.59	22.24	4.61	0.000
% 65 or Older	10.64	3.51	12.38	4.37	0.001
Median Age	35.18	3.92	37.66	4.90	0.000
% Nonwhite	34.81	22.13	30.37	18.17	0.097
% Not White Alone	37.47	21.82	32.82	18.20	0.079
% Black	14.54	21.21	10.83	17.06	0.144
% Hispanic/Latino	16.72	15.29	17.18	13.56	0.809
% with Less Than HS	11.18	8.64	9.65	5.93	0.123
% with Bachelors or Higher	45.41	20.70	44.19	18.99	0.638
Median Income	65108.95	24172.55	82877.30	28244.20	0.000
Unemployment Rate	8.50	4.10	7.28	2.82	0.009
Average Household Size	2.27	0.55	2.57	0.38	0.000
% of Population Renting	52.80	15.60	38.50	14.71	0.000
% of Units Rented	54.97	15.58	39.87	14.59	0.000
Vacancy Rate	8.90	4.95	6.38	3.14	0.000

Notes: The p-values correspond to a t-test of the difference in means. Thus a lower p-value corresponds to a higher likelihood that the treated and untreated means are different.

Table A.13: Covariate Balance Regression Coefficients

	Unbalanced/Interpolated		ZCs Dropped	
Dependent Variable	Full	w/o New York	Full	w/o New York
Population	1207.352	1816.777	591.889	2378.175*
	(1346.817)	(1252.924)	(1637.094)	(1249.799)
% Under 18	-0.180	-0.203	-0.110	0.640
	(0.876)	(1.049)	(0.922)	(0.861)
% 65 or Older	1.094**	0.973*	0.648	0.560
	(0.470)	(0.561)	(0.473)	(0.521)
Median Age	0.309	0.128	0.026	0.113
	(0.508)	(0.650)	(0.533)	(0.656)
% Nonwhite	-0.130	1.313	0.821	3.855*
	(2.593)	(2.158)	(3.033)	(2.148)
% Not White Alone	-0.339	1.169	0.565	3.691
	(2.599)	(2.144)	(3.132)	(2.173)
% Black	4.272**	4.820***	5.437***	6.957***
	(1.777)	(1.666)	(1.965)	(1.708)
% Hispanic/Latino	-4.227*	-2.432*	-3.626	-1.944
	(2.189)	(1.450)	(3.713)	(1.709)
% with Less Than HS	-1.028	-0.444	-1.258	-0.396
	(0.983)	(0.836)	(1.197)	(0.817)
% with Bachelors or Higher	-0.711	-2.173	-0.049	-1.461
	(3.066)	(2.369)	(3.264)	(2.494)
Median Income	-1818.866	-4459.321*	-5767.182	-7097.916**
	(3233.582)	(2293.260)	(4759.106)	(2850.385)
Unemployment Rate	0.355	0.637	0.460	0.929**
	(0.506)	(0.390)	(0.636)	(0.364)
Average Household Size	-0.038	-0.025	-0.050	0.006
	(0.061)	(0.064)	(0.062)	(0.060)
% of Population Renting	-2.105	-1.299	-2.855	-1.343
	(2.077)	(2.227)	(1.915)	(1.804)
% of Units Rented	-1.337	-0.595	-1.573	-0.155
	(2.032)	(2.218)	(1.961)	(1.895)
Vacancy Rate	0.682*	0.677*	0.098	-0.154
	(0.389)	(0.395)	(0.328)	(0.376)
% Filing EITC 2010	-0.348	0.293	0.010	2.427
	(1.046)	(0.981)	(2.737)	(2.142)
% Filing EITC 2013	-0.349	0.370	-0.245	2.470
	(1.129)	(1.054)	(3.057)	(2.385)

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Because EITC regulations can vary across states, the two EITC variables used here are restricted to pairs of same-state zip codes.

Table A.14: Minimum Wage Effect on ZORI, w/ Controls & w/o New York

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	-0.030	-0.037	-0.009
	(0.040)	(0.044)	(0.044)
Observations	52937	57384	12456
Controls	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes
Pair-Time FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.15: Minimum Wage Effect on ZORI, w/ EITC Interaction & w/o New York

	Unbalanced	Interpolated	ZCs Dropped
$lnMW_{zt}$	-0.047	-0.055	-0.126**
	(0.038)	(0.044)	(0.053)
$lnMW_{zt} \times Avg.\,\%\,Filing\,EITC\;2010_p$	0.001	0.002	0.002
	(0.001)	(0.001)	(0.003)
Observations	52937	57384	12456
Zip Code FE	Yes	Yes	Yes
Pair-Time FE	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$