Income Transfers, Housing Rents, and Landlord Capture*

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Abstract

This paper explores the impact of income transfers on housing market outcomes using minimum wage hikes as a natural experiment that raises the earnings for low-income households. I find that minimum wage hikes increase both household earnings and housing rents, where the increase in rent accounts for about 30 percent of the earnings gain. I then show that this change in housing rents stems from increases in housing demand from local residents but is not associated with material increases in housing consumption. Thus, I find evidence that the housing market acts as a tax on income transfers, benefitting landlords.

Key Words: minimum wages, housing rental markets, housing consumption
JEL Codes: J38, R21, R31

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1 Introduction

It is well-accepted that geographic-specific productivity shocks impact local housing prices, altering the incidence of who benefits and who is harmed by a change in productivity (Rosen, 1979; Roback, 1982; Glaeser and Gyourko, 2005; Ganong and Shoag, 2017). The main mechanism by which productivity shocks alter housing prices is via migration – with workers migrating from less to more productive areas and increasing the demand for (and prices of) housing in newly productive areas (Glaeser and Gottlieb, 2009; Moretti, 2011). Thus, some of the productivity shock is passed along to owners of housing. Importantly, however, housing costs could also increase in newly productive areas without migration. If housing is a differentiated and normal good (Polinsky and Ellwood, 1979; Notowidigdo, 2020), then a productivity shock that increases earnings should also increase housing demand as local residents seek out hedonically more-desirable housing. While there is less evidence that this local demand channel matters for analyzing the economic incidence of productivity shocks (Hornbeck and Moretti, 2024), it could be quite important when thinking about the economic incidence of means-tested economic transfers.

Low-income households spend a much higher proportion of their earnings on housing (Quigley and Raphael, 2004) and frequently reside in substandard units in less-desirable neighborhoods (Weeden, 2023). Thus, a rational response to income transfers that ease a low-income household's budget constraint might be an increase in housing demand – either in terms of housing characteristics or neighborhood amenities.[1] Like in the canonical Rosen–Roback model, the price effects of this demand-based response from local residents would be particularly acute if housing supply is highly inelastic. Indeed, if housing is perfectly inelastic, then the increased local demand will increase housing prices but not increase housing consumption. Moreover, since most low-income households rent their housing, landlords will capture some portion of the income transfer due to the increase in rent. In this way, the housing market may act as a tax on economic transfers.[2]

[1] Indeed, the benefits to moving to better housing in nicer neighborhoods are substantial, especially for children (Goux and Maurin, 2005; Chetty, Hendren and Katz, 2016; Chyn, Collinson and Sandler, 2023).

[2] Notably, landlords should not be able to absorb all of the income transfer as the increase in rent will be function of the income elasticity of housing.

In this paper, I analyze the effect of minimum wages on household earnings and housing rents using data from the 2012-2019 American Community Survey. This analysis allows me to assess whether minimum wages constitute an economic transfer (i.e. that they increase earnings) and then assess the extent to which higher incomes translate into higher rents. I then analyze whether this change in housing rents comes from changes in housing demand from local residents (as opposed to inflows of new households) and explore the extent to which higher rents appear to be associated with increased housing consumption (or not). This last point is particularly important. If higher rents are associated with substantial increases in housing consumption, then the rent increase does not represent a tax but rather highlights ways in which the transfer increases consumption as in Aaronson, Agarwal and French (2012). On the other hand, if rents increase but housing consumption does not, then the higher rents represent the ability of landlords to capture some portion of the transfer. Thus, it is important to disentangle the mechanisms that bring about this change in housing rents and the extent to which higher rents actually lead to higher consumption.

This paper fits into the small but growing literature exploring the impact and incidence of transfer programs on housing consumption and costs. Not surprisingly, this literature has largely focused on the impact of housing vouchers as they have the most direct effect on housing demand. There is mixed evidence as to whether increasing the number of housing vouchers affects housing rents (Eriksen and Ross, 2015; Susin, 2002).[3] However, there is strong evidence that uniformly increasing the value of a housing voucher increases rents – with landlords taking between 46 percent (Collinson and Ganong, 2018) and 66 percent (Gibbons and Manning, 2006) of the increased voucher value without materially altering housing consumption.[4]

Large cash transfers to low-income households could also impact housing costs and consumption via their impact on housing demand. For example, Pilkauskas and Michelmore (2019) find that the Earned Income Tax Credit increases housing consumption and improves living arrangements. In this paper, I use minimum wage as a natural experiment to examine

[3]Susin (2002) finds that increasing the number of vouchers does increase rents while Eriksen and Ross (2015) finds that it does not, except for units close to the rent cap.

[4]Other studies examine the supply side of housing policy and its impact on housing rents. See, for example: Blanco (2023).

the impacts of cash transfers on housing rents, housing demand, and ultimately housing consumption. A few other studies have previously explored the impact of minimum wages on housing rents (Agarwal, Ambrose and Diop, 2022; Borg, Passaro and Hermo, 2022; Bowdle, 2025; Hughes, 2020; Tidemann, 2018; Yamagishi, 2021).[5] While most of these past studies find that minimum wage hikes are associated with rising rents,[6] none of the existing literature empirically explores the impact of local demand channels and the extent to which rent increases following minimum wage hikes are associated with increased housing consumption or not. In some instances, these past studies did not explicitly explore the underlying causes of the effect on housing rents (Agarwal, Ambrose and Diop, 2022; Bowdle, 2025; Hughes, 2020). In other instances, the impact is examined theoretically through the lens of a spatial equilibrium model, where migration is the primary mechanism by which minimum wage hikes could impact housing rents (Borg, Passaro and Hermo, 2022; Tidemann, 2018; Yamagishi, 2021).[7] Documenting and understanding the underlying forces driving this relationship between minimum wages and rent is important because it speaks to the incidence of minimum wage hikes and the broader question about extent to which landlords may be able to capture some portion of an income transfer.

Using both a staggered and stacked difference-in-difference research design, I find that minimum wage hikes increase household earnings and housing rents for low-income households. The estimates imply that a $1 increase in the minimum wage is associated with about a $70 increase in monthly household earnings but also a $20 increase in housing rents, implying that about 30 percent of the earnings gain from a minimum wage hike is passed along to landlords. These effects are quite persistent and especially evident following large minimum wage hikes. I then find that the increase in housing rents appears to come from increases in housing demand from local residents as the rising minimum wage is associated with a small (half percentage point) increase in intra-area moves, but not increases moves

[5]MaCurdy (2015) and Ahlfeldt, Roth and Seidel (2019) also raise the issue that minimum wages may impact housing rents, but estimating the direct effect is not the main focus of their analyses.

[6]Bowdle (2025) and Tidemann (2018) are the lone exception.

[7]This distinction matters because if its the place-based nature of minimum wages that drives higher rents, then more universal transfers, such as a Universal Basic Income, are unlikely to have the same effect on housing rents. Moreover, there is little evidence that minimum wages actually cause workers to migrate to an area with some studies finding the opposite effect (Cadena, 2014; Monras, 2019).

from outside of the geographic boundary affected by the hike. As the same time, I find that minimum wage hikes are associated with falling housing vacancy rates and the rent effects increase as housing is more inelastically supplied and as neighborhood characteristics improve. All of these effects imply a shift in local housing demand. At the same time, I find only limited evidence that higher rents are associated with greater housing consumption, with some evidence that movers reside in newer buildings and have fewer people in their residence following hikes. However, I find no evidence that housing units change in other hedonically desirable ways or that move rates disproportionately increase (or vacancy rates disproportionately decline) in nicer neighborhoods. On top of this, the impacts on housing rents are almost identical when I condition on housing characteristics or if I limit the data to non-movers. Thus, I find little evidence that increased housing rents are associated with increased housing consumption.

These results imply that minimum wage hikes increase housing rents via their impact on housing demand – where the unique characteristics of the housing market, which make it difficult to quickly increase supply, imply that the equilibrium change will be most evident in prices. Since most low-income households rent housing, landlords are then able to capture a significant portion of the income transfer without meaningful increases in housing consumption. In this way, I find evidence that housing acts as a tax on rising earnings from the minimum wage. These results, however, have implications beyond the minimum wage. Indeed, there is little reason to believe that other income support programs would have different effects as the impact comes from increased local demand for better housing and not migration. Thus, the spillover into housing markets is not because minimum wages are a place-based policy, but because minimum wages are an income transfer.

This paper makes important contributions to several literatures. In addition to its contribution to the literature on the incidence of transfer programs and their spillovers into the housing market, it also contributes to the the broader literature on the efficacy of transfer programs (Ben-Shalom, Moffitt and Scholz, 2011; Haveman et al., 2015); the literature on the distributional consequences of the minimum wage (Autor, Manning and Smith, 2016; Dube, 2019; Phelan, 2019; Vergara, 2022); and the literature on the price effects of the minimum wage (Aaronson, 2001; Harasztosi and Lindner, 2019; Leung, 2021), suggesting

that housing rents may be a particularly important price adjustment. Lastly, the paper also contributes to the literature on the local labor market effects of economics shocks (Moretti, 2011; Glaeser and Gottlieb, 2009), highlighting that the local demand channel can have important effects on housing costs as well.

2 Data

2.1 American Community Survey

The primary data I use in the empirical analysis are from the 2012-2019 American Community Survey (ACS). The ACS is a cross-sectional national survey of about 1 million households in the U.S. that has been undertaken annually by the U.S. Census Bureau since 2005. The survey asks respondents about their demographics, geographic location, labor market experience, migration history, and numerous questions about their housing unit such as the rent paid (if renters) and several characteristics about their residence including the number of bedrooms, bathrooms, age of the unit, and type of unit. The sample size of the ACS combined with the housing characteristics data make it an excellent data source for this analysis. Another benefit of the ACS is that it provides detailed substate geographic information on households, which are called Public Use Micro Areas (PUMAs) and reflect non-overlapping geographic areas that contains at least 100,000 people. Thus, large metropolitan areas are broken up into several unique PUMAs.[8] This detailed geographic information allows me to perform the empirical analysis at the substate level, allowing me to account for the numerous city and county-level minimum wage changes that have taken place over the last decade. It also allows me to analyze housing market outcomes at more detailed geographic levels, which is appropriate for housing markets.

The empirical analysis begins in 2012 because the ACS made fairly substantial changes to the geographic boundaries of PUMAs between the 2011 and 2012 survey, making an analysis at the PUMA-level impossible if we add earlier years. The analysis ends in 2019 to avoid any complications in the housing and labor market associated with the Covid-19

[8]PUMAs are made up of Census tracts and Census blocks that tend to have similar neighborhood characteristics.

pandemic. This time frame is also an active period in terms of minimum wage changes – with several areas imposing large and sustained increases in the minimum wage while many others experienced no changes at all. I discuss this policy variation in more detail below.

Summary statistics from the ACS sample are presented in Table 1, where I initially limit the data to prime working age households (i.e. households where the household head is aged 20-60) that reside in states that did not implement automatic inflation-based minimum wage adjustments over the entire 2012-2019 period.[9] I then further limit the sample (using both household and neighborhood characteristics) to focus-in on a sample of households most-likely to be affected by minimum wage hikes. First, I remove all "high wage households," which I define as households with average household wages greater than the 75th percentile in their state or household earnings greater than the 95th percentile in their state.[10] I then remove households that do not rent their housing; higher educated households (i.e. households where the household head has completed at least some college); and households that reside in more-prosperous PUMAs, which I define as PUMAs where less than 25 percent of households are low-wage (i.e. paid less than twice the minimum) and less than 10 percent of households receive food stamps.

As shown in Table 1, each of these restrictions gives me a sample of household more-and-more likely to be affected by minimum wage hikes. This is evidenced by the changing average wage-to-minimum wage ratio which falls from 3.47 for the overall sample in column (a) to 1.56 once all of these restrictions have been put into place in column (e) – with average monthly household earnings showing a similar decline from $6,600 per month to $2,231 per month.[11] The sample also becomes less white, less-likely to be married, and more-likely to have moved over the past 12 months as each additional restriction is placed on the sample. All in all, the sample restrictions, which have me focusing on a sample of

[9]The nine states with automatic inflation-based adjustments over each year of the sample period include: Arizona, Colorado, Florida, Missouri, Montana, Ohio, Oregon, Vermont, and Washington. These observations are excluded from the analysis because these changes are unlikely to have the same impact as large and unexpected changes.

[10]This household wage restriction is intentionally inclusive, so as to not exclude any affected households. A household wage is simply the sum of all household earnings dividing the sum of total household hours worked. If earnings are zero, household wages are zero.

[11]In the rare instance that household earnings are zero, wages are assumed to be zero and thus, less than the 75th percentile in any state.

lower-wage and less-educated renters in poorer PUMAs, leave me a sample of more than 380,000 households. In the analysis that follows, I refer to the sample described in column (e) as the main analytic sample.

Housing costs also increase (in terms of rent to income ratios) and housing consumption falls with each of the sample restriction. For example, the rent to income ratio more than doubles from 0.15 to 0.31 as we move from columna (a)-(e). At the same time, housing consumption in terms of bedrooms per person, rooms per person, and the proportion of households residing in a new residence all fall. The size of residences (in term of bedrooms and rooms per person fall by about 25-30 percent from the overall sample to analytic sample. At the same time, the proportion of households residing in a new residence falls by about 50 percent. Thus, the analytic sample also tends to pay a higher proportion of their earnings for less housing than the overall sample.

I also use these same sample restrictions to construct a placebo sample, i.e. a sample unlikely to be affected by minimum wage hikes. This sample includes higher-wage and higher-educated households that reside in more-prosperous PUMAs, where each of these restrictions are as described above. One exception, however, is that I lower the threshold to define "higher wage" in an effort to be intentionally inclusive.[12] Summary statistics of the placebo sample are presented in column (f) of Table 1. As expected the placebo sample has higher wages, higher earnings, is more likely to be white and married, and less-likely to rent housing than the overall sample. In the analysis that follows, we use the placebo sample to confirm that the results we estimate are not due to trends affecting all households in areas that increased their minimum wage. That said, if rent increases stem from increased costs of operating an apartment building, which may employ several workers impacted by the hike, then we would expect to find similar results for higher wage renters in more prosperous PUMAs.

[12] "Higher-wage" households to include households where the average wage is above median in the state. This helps increase sample size, but I again restrict to those with total household earnings below the 95th percentile in the state. Moreover, the sample is not limited to renters, but we'll need to restrict it accordingly when we examine the impact on housing rents.

2.2 Minimum Wage Data

The minimum wage data that I use are from Vaghul and Zipperer (2016) and its subsequent updates, where I use the data on both state and substate minimum wage hikes to construct a monthly PUMA-level minimum wage. I then compute the average minimum wage level over the calendar year and use this as the annual minimum wage level. This approach of annualizing the monthly minimum wage matches the ACS data collection, which is equally spread across the calendar year but is then provided to users solely with the year of collection but no information about the month of collection.

In Table 2, I present summary statistics on the minimum wage activity among treatment and control locations, where I follow the recent literature on treatment timing (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021) and organize the treatment sample by the year of their first hike (from the year prior).[13] Interestingly, many geographic areas implemented large minimum wage hikes over this sample period that were adopted over several years. For example, New York City increased it's minimum wage by \$0.75 in both 2014 and 2015, by \$0.25 in 2016, and then by \$2.00 in each of 2017, 2018, and 2019. Indeed, 2014 was a very popular year to first begin increasing one's minimum wages, with 32 percent of the analytic sample experiencing a hike in that year. Moreover, 90 percent of those areas that first increased their minimum wage in 2014, increased it every year between 2014 and 2019.[14] The average of those changes is included in column (c) of Table 2. Areas that first increased their minimum wage in other years followed a similar pattern with regular increases in each subsequent year. However, there are fewer areas that began increasing their minimum wage in other years – especially those that first increased in 2012, 2013, 2016, and 2017. Indeed, areas that first increased in 2014 account for 78 percent of the treatment sample and those that first increased in either 2014 or 2015 account for 95 percent of the treatment sample.[15] Importantly, another common trend over this period is that there are a significant number of places that did not increase their minimum wage at all over 2012-2019 period, with more

[13] Table 2 also includes minimum wage changes in 2011 to give a better sense of hikes before the sample period.

[14] The 90 percent statistic is a sample weighted average (based on observations in the analytic sample) across PUMAs.

[15] 78 percent is $123,755$ observations divided by the $159,494$ total treatment observations. 95 percent is the $(123,755 + 28,250) = 152,005$ dividued by the $159,494$ total treatment observations.

than 58 percent of the analytic sample experiencing no increase following the 2009 hike in the federal minimum wage. These areas constitute the control group in the analysis that follows.

This pattern of minimum wage hikes, which begin in different years, naturally lends itself to a stacked difference-in-differences (DID) research design, where I limit my treatment treatment sample to areas that first increased their minimum wage in 2014 or 2015. I exclude the sub-experiments of treatment states that first hiked their minimum wage in 2012, 2013, 2016, and 2017 because they constitute so few treatment observations and would severely limit the common treatment window I could analyze. Indeed, it is instructive to first plot out average earnings and average rent for the 2014 treatment sample and the control group. These are plotted in Figure 1, where I again follow the literature and limit the treatment sample to those areas with larger average increases in the minimum wage, i.e. larger than \$0.50 per year (Cengiz et al., 2019; Clemens and Strain, 2021; Fone, Sabia and Cesur, 2023; Liu et al., 2024). As shown therein, while treatment and control samples follow similar trends in 2012 and 2013, the treatment sample experiences larger increases in both household earnings and housing rents beginning in 2014 when the treatment sample first increased its minimum wage. The differential trends in treatment locations really take off in 2016. By the end of the sample period, relative earnings for the average household in treatment states had grown by an additional \$450 but housing rents had increased by an additional \$130.[16] I see a similar pattern of effects when all treatment states (i.e. even those with small hikes) are included in the treatment sample (see Appendix Figure A1). Thus, even before estimating an regressions, the summary statistics suggest that minimum wage hikes increased both the average household earnings but also the housing rents of analytic sample in treatment areas – with the increase in housing rents representing about 30 percent of the increase in household earnings.

[16]Note that while the earnings and rent levels differ between treatment and control samples (control samples use the left scale and treatment samples use the right scale), the changes in earnings and rent are identical, making the two plots directly comparable.

2.3 Housing Supply and Housing Amenities

In the empirical analysis that follows, I also use geographic-specific data on the housing supply elasticity and neighborhood amenities. I obtain estimates of MSA-level housing supply elasticities from Baum-Snow and Han (2024);[17] estimates of Census tract school quality data from Reardon et al. (2021); and estimates of Census tract crime data from Krivo, Lyons and Vélez (2022). None of these data sources cover all geographic areas in the ACS – even when Census tracts are aggregated up to PUMAs. Thus, in the analyses that use these additional data sources, the analytic sample is necessarily compressed.[18]

3 Empirical Specifications

The empirical analysis examines how minimum wage hikes affect a range of household level outcomes including household earnings and housing rents. I then go on to explore whether the change in rents I observe appears to be due to changes in housing demand and the extent to which this seems to come from local residents as opposed to in-migration. I explore these questions by examining how minimum wage hikes affect individual-level move probabilities, neighborhood-level vacancy rates, and whether the rent effects I estimate differ by housing supply elasticity (implying a demand shift) and neighborhood amenities. Lastly, I explore whether minimum wages are associated with increased housing consumption using the housing characteristics data in the ACS and how housing rents change as we hold the housing unit fixed (both in terms of measurable housing characteristics and whether an individual remained in the same housing unit or not). For all of these analyses, I regress different housing and labor market outcomes on measures of the minimum wage. This section describes these empirical expressions and the underlying identifying assumptions.

[17] While Baum-Snow and Han (2024) also provide estimates of housing supply elasticites at the Census tract-level, I opt to use MSA-level estimates because the relevant geographic housing market (for a given household) is likely larger than at the tract level.

[18] The geographic scope of the crime data is particularly limited.

3.1 Staggered DID

The primary empirical expressions follow the more-traditional staggered difference-in-differences (DID) empirical expression:

$$Y_{ipt} = \alpha_p + \alpha_t + \beta_1 \Delta MW_{p,t+2} + \beta_2 \Delta MW_{p,t+1} + \beta_3 MW_{pt} + X_i\gamma + \epsilon_{ipt} \tag{1}$$

where Y_{ipt} is an outcome for household i in PUMA p in time t; α_p and α_t are PUMA and time fixed effects; X_i is a vector of household head characteristics including a quadratic in age and indicators for education, sex, ethnicity, immigrant status, and race; and the minimum wage variables are the change in the minimum wage one ($\Delta MW_{p,t+1}$) and two years ($\Delta MW_{p,t+2}$) in the future and the minimum wage level this year (MW_{pt}).[19] This empirical approach, which includes changes in the lead of the minimum wages and levels of the existing minimum wage is very similar to that used in Dube, Lester and Reich (2010) and Jha, Neumark and Rodriguez-Lopez (2022). It has two main benefits.[20] First, it eases interpretation of the β coefficients, which all now represent cumulative/total effects at each point in time (as opposed to marginal effects). Second, it eases identification as changes in minimum wages (ΔMW's) will be less correlated with each other than the minimum wage levels.

In equation (1), β_1, β_2, and β_3 are the coefficients of interest as they each describe the impact of a \$1.00 minimum wage hike on the outcome. β_1 and β_2 describe whether the outcome was trending differently in treatment areas prior to the minimum wage change and help assess the parallel trends assumption. β_3 describes the average effect of a \$1.00 increase in the minimum wage on the outcome over the years following the hike (at least over the average period of time before it is again increased). β_3 identifies the causal effects of these wage shocks on labor and housing market outcomes if minimum wage hikes are not correlated with other factors affecting labor market and housing market outcomes. To help assess the reasonableness of this assumption, I use β_1 and β_2 to assess if there were differential trends in the outcome variable prior to the minimum wage change. Second, I explore how minimum

[19] All minimum wage change variables are from one year prior.

[20] The empirical approach, with it's emphasis on cumulative effects, is also similar to the approach in Aaronson and Phelan (2019, 2024).

wage changes affect labor and housing market outcomes for workers unlikely to be affected by minimum wage hikes, namely higher-wage, higher-educated individuals that tend to live in nicer neighborhoods. If β_3 identifies the effect of the minimum wage, then there should be no effect on workers paid well above the minimum.

I extend equation (1) and estimate an event study version of the staggered DID expression to better understand the dynamic response of minimum wages to outcomes in the labor and housing market. Specifically, I estimate:

$$\begin{aligned} Y_{ipt} = \alpha_p + \alpha_t + \beta_1 \Delta MW_{p,t+3} + \beta_2 \Delta MW_{p,t+2} + \beta_3 \Delta MW_{p,t+1} \\ + \beta_4 \Delta MW_{pt} + \beta_5 \Delta MW_{p,t-1} + \beta_6 MW_{p,t-2} + X_i \gamma + \epsilon_{ipt} \end{aligned} \tag{2}$$

where everything is identical to equation (1), except we now include an additional lead change and two additional lagged changes in the minimum wage. Like equation (1), the β-coefficients in this expression represent the cumulative effect of a \$1.00 change in the minimum wage at each point in time. Thus, while $\beta_1 - \beta_3$ describe the trends in outcomes prior to adoption, β_4 describes the effect the year of the hike, β_5 describes the cumulative effect one year after the hike, and β_6 describes the average cumulative effect two or more year after the hike. This alternate specification provides an additional test of the parallel trends assumption and better models the dynamics of the labor and housing market adjustments following a minimum wage hike. Indeed, I also estimate a version of equation (2) that includes up to four lags to further explore the dynamics of the price adjustment.[21]

In all of these staggered difference-in-differences models, I present estimates using the minimum wage level as opposed to the natural log of the minimum wage in the empirical expression because it eases interpretation. Additionally, I cluster standard errors at the PUMA level and weight observations using the household weights from the ACS.

[21] That said, given the timing of the minimum wage hikes in the sample (see Table 2) and the relatively short period I analyze (2012-2019), the expression with four lags is only weakly identified (i.e. several of the coefficients are identified off of relatively few observations).

3.2 Stacked DID

The staggered DID research design in equation (1) and equation (2) has come under increased scrutiny in recent years because the different treatment timing can bias estimates either if treatment effects phase in over time (and "already treated" areas are used as a part of the control group) or treatment effects vary by calendar year (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). One solution to this is to estimate a stacked difference-in-differences expression, which treats each year of adoption as a separate experiment with a separate control group and then stacks these different treatment and control samples on top of each other and estimates treatment effects with proper controls for the different sub-experiments (Cengiz et al., 2019; Deshpande and Li, 2019).

The baseline stacked DID is:

$$Y_{iptd} = \alpha_{pd} + \alpha_{td} + \beta_1(Treat_{sd} * Post_{td}) + X_{id}\gamma + \epsilon_{iptd} \tag{3}$$

where everything is similar to equation (1), except that the treatment sample is now broken out by the different sub-experiments (d) based on the year of their first minimum wage hike. Along with this change, the specification includes controls for sub-experiment specific PUMA affects (α_{pd}), sub-experiment specific time effects (α_{pd}), and I continue to include the same individual demographic controls (X_{id}). In this set-up, the average impact of increasing the minimum wage on outcome Y is β_1, which is the coefficient on the interaction term with being a PUMA that increased it's minimum wage ($Treat_{sd}$) and an indicator for it being a time period after the hike ($Post_{td}$).

I estimate equation (3) using a balanced panel from two-years prior to the first minimum wage change to four years after the hike using the 2014 and 2015 sub-experiments, i.e. the states that first increased their minimum wage in 2014 and first increased in 2015, as my treatment sample. Thus, the 2014 sub-experiment analyzes data from 2012-2018 while the 2015 sub-experiment analyzes data from 2013-2019. While it is possible to expand the analysis to include other sub-experiments, there are few benefits (and many costs) of doing this. Since the 2014 and 2015 sub-experiments already account for 95 percent of all the hikes in the analytic sample, the increase in additional treatment observations from additional

sub-experiments (such as 2013 or 2016) are quite small. At the same time, adding these sub-experiments would narrow the window of time available to estimate the dynamics effects of the hikes.[22] Thus, the stacked DID estimates solely use the 2014 and 2015 sub-experiments. The control sample for both sub-experiments is the sample of "never hike" PUMAs over the sub-experiments' common time window.

I then extend the stacked DID framework to an event study framework and estimate:

$$Y_{iptd} = \alpha_{pd} + \alpha_{td} + \beta_{-2}(Treat_{sd} * Pre^2_{td}) + \sum_{j=0}^{4} \beta_j (Treat_{sd} * Post^j_{td}) + X_{id}\gamma + \epsilon_{iptd} \qquad (4)$$

This specification is identical to equation (3) except that I now estimate separate coefficients (of the impact of minimum wage changes) for each period relative to the year prior to the first hike. Thus, β_{-2} tests the parallel trends assumption and β_0-β_4 describe how the outcome changes in the years since first adoption. One downside of the stacked DID approach (compared to the staggered DID) is that it does not allow one to differentiate between lagged effects of minimum wage hikes and the effects of new minimum wage hikes. Since the average household in our treatment sample experiences a hike in each year after the first hike was adopted, the β_1-β_4 coefficients in equation (4) include both the impact of new hikes and the lagged effect of previous hikes. This, combined with changing average hikes in each year, make it more difficuly to interpret the magnitudes of these coefficients. Thus, in the analysis that follows, we compare the predicted effects implied by the estimates of equation (2) with the predicted effects implied by the estimates of equation (4).

4 Results

The empirical results are broken up into three sections based on the different labor and housing market outcomes I analyze. First, I present results on how minimum wages affect household earnings and housing rents. This analysis of labor market outcomes is essential to establish minimum wages as an economic transfer. I then assess how the impact on earnings

[22] If we added the 2013 sub-experiemnt, we would only have one pre-period. If we added the 2016 experiment, we would lose one post-period.

translates into housing rents. Second, I present estimates that explore whether this change in housing market outcomes is due to changes in housing demand. Third, I assess the extent to which minimum wage hikes are associated with changes in housing consumption.

4.1 Impact of Minimum Wages on Earnings and Rent

4.1.1 Staggered DID Estimates

Table 3 presents estimates of equation (1) on labor market outcomes including monthly household hours worked (Panel 1), monthly household earnings (Panel 2), and monthly household earnings conditional on hours worked (Panel 3).[23] It includes these results for the main analytic sample, i.e. lower-wage, less-educated renters residing in less-prosperous neighborhoods, both with and without PUMA-specific time trends. I also present the results for the placebo sample (higher-wage, higher-educated individuals in more-prosperous neighborhoods). Appendix Table A1 presents estimates of equation (1) for these same outcomes using less-restrictive criteria for the analytic sample, notably samples 1-3 in Table 1.

The estimates imply that minimum wage hikes over the 2012-2019 period did not materially impact household hours worked. If anything, the estimates suggest a small increase of about 1.5 hours for every one dollar increase in the minimum wage, or about one percent as the average household in the analytic sample works about 182 hours per month. Thus, the average hours worked did not decline from the hike. At the same time, minimum wage hikes did increase household earnings with earnings increasing by about $70 for every one dollar increase in the minimum wage (when hours worked are not accounted for) and about $50 when I condition on hours worked. Holding hours fixed, these estimates imply that a one dollar increase in the minimum wage leads to an average wage increase of about $0.27

[23]Monthly household hours worked is a created variable from the ACS data. The ACS includes variables for usual weekly hours worked over the past year (WKHP) and an indicator for variable for weeks worked over the past year (WKW), where WKW takes one of 6 values, which is indicative of a range of weeks. Missing values for WKW imply that the individual did not work over the past year. I use the midpoint of the range for weeks worked and multiple this by WKHP. This gives me an estimate of the individual's hours worked. I then sum these up for all household members (another variable in the ACS, SERIALNO) and divide this by 12 to get an estimate of the monthly hours worked. Individual earnings over the past 12 months is directly collected by the ACS (WAGP). This is then converted to "monthly household earnings" by summing up WAGP for all individuals within a household and dividing this by 12.

($50 divided by 182 hours). This less than one-for-one change is not surprising since the analytic sample is representative of households "likely to be affected" by the minimum wage and not necessarily individuals earning exactly the minimum wage. Thus, one reason it is smaller than one is that some households may not have been affected by the minimum wage hike. A second reason is that some members of the household may have been affected by the hike while other members were not. Finally, a third reason is that some members may have been paid just above the new minimum and solely impacted via spillovers. In this regard, the spillover effects of minimum wage hikes on higher wage workers tend to be less than full pass through (Phelan, 2019; Gregory and Zierahn, 2022).

In examining the pre-trends in hours worked and earnings (i.e. the coefficients on a change in the minimum wage in two years and next year), there is some evidence that both hours worked and earnings were slightly elevated prior to the hike. That said, none of the coefficients are statistically different than zero and the earnings impact following a hike (i.e. the coefficient on the minimum wage level) is statistically different than the pre-hike coefficients. Moreover, the estimated impacts of a minimum wage hike on earnings (conditional on hours) shows much less of a pre-trend, suggesting that hours may have been slightly elevated but not wages, and the earnings results are robust to the inclusion PUMA-specific time trends. Lastly, the placebo sample (of higher-wage/higher-educated workers) does not experience the same increase in earnings following a minimum wage hike, with clear pre-trends and estimated effects that entirely disappear when PUMA-specific time trends are included in the specification. For all of these reasons, pre-trends do not explain the rising earnings that I estimate for the analytic sample when minimum wages increase. Thus, minimum wage hikes (at least over this period of analysis) amount to an income transfer to low-income households.[24]

Table 3 also presents the impact of minimum wage hikes on housing rents (Panel 4), which show that minimum wage hikes do not preemptively affect housing rents, but housing rents increase following a minimum wage hike. The estimates suggest that a one dollar

[24]Additionally, the sample restrictions we impose to focus-in on a sample of workers likely to be affected by minimum wage hikes appear to be necessary to highlight the impact of minimum wage hikes on household earnings. As I show in Appendix Table A1, the impact of minimum wages on earnings only becomes evident in Sample 3, but grows as I move to Sample 4.

increase in the minimum wage raises housing rents by an average of about $20. Thus, the increase in housing rents accounts for a 29.4 percent (20.4/69.4) of the increase in earnings following the hike. While the impact of a one dollar minimum wage hike on housing rents decreases when the empirical specification includes PUMA-specific time trends, the impact remains highly statistically significant with no pre-trend.[25] Moreover, this impact is unique to those individuals most-likely to be affected by minimum wage hikes (see both the placebo estimates in Table 3 and the estimates using less restrictive samples in Appendix Table A1). Therefore, minimum wage hikes are associated with rising housing rents.

4.1.2 Stacked DID and Event Study Estimates

The stacked research design results are presented in Table 4 and include both the DID and event study estimates. Panel 1 presents the results on earnings while Panel 2 presents the results on housing rents.[26] Columns (a) and (b) show the results solely using the 2014 sub-experiment; columns (c) and (d) present the stacked estimates using the 2014 and 2015 sub-experiments; and columns (e) and (f) present the stacked estimates (using the 2014 and 2015 sub-experiments) when small hike areas are excluded from the treatment sample (like in Figure 1).[27]

All of the results in Table 4 confirm the staggered DID estimates in Table 3 – showing that minimum wage hikes both increased household earnings but also increased housing rents. The stacked DID event study results from column (d) are also depicted in Figure 2. Importantly, the estimates continue to show no pre-trend in earnings or housing rents with

[25]Since most of the treatment sample hiked their minimum wage each year after the initial hike, it is not immediately clear whether PUMA-specific time trends make sense in this situation as they may "eat up" much of the variation I use for identification. Thus, I lean more heavily on the estimates that do not include PUMA-specific time trends.

[26]The stacked DID (and event study) estimates on hours worked and earnings, conditional on hours worked, are presented in Appendix Table A2.

[27]Small minimum wage hike areas are defined to be areas where the average hike (over the window of time after the first hike) is less and $0.50. This includes all PUMAs in Alaska, Arkansas, Connecticut, Delaware, Nebraska, New Jersey, Michigan, South Dakota, and West Virginia. It also includes the PUMAs associated with Iowa City, IA and Louisville, KY.

both earnings and housing rents increasing after the minimum wage increases.[28] Moreover, the results show that the impact of the hikes has grown over time with noticable jumps in both household earnings and housing rents beginning in 2016. That said, as I described earlier, these coefficients are somewhat difficult to interpret because the minimum wage increases each year in the post-period, the average size of the hike changes in each of those years, and there could be both current and lagged effects of these hikes confounding the estimates. One way to interpret the coefficients, however, is to simply assess the relative magnitudes of the rent and earnings effects. In this regard, the increase in housing rents in the stacked estimates continue to represent about 30 percent of the earnings gain. This is definitely true for the DID estimates, but also for the event study estimates. Thus, both the staggered and stacked estimates imply that 30 percent of the earnings gains from a minimum wage hike is offset by rising housing costs.

4.1.3 Staggered Event Study Results

I also present event study estimates from the staggered DID research design (i.e. equation 2). These estimates, which are presented in Table 5, allow me to better understand the dynamics of the labor and housing market adjustments to minimum wage hikes as well as the magnitudes of the individual hikes. The estimates imply that minimum wage hikes have an immediate impact on household earnings and housing rents that generally grows over time. The estimates imply that a one dollar increase in the minimum wage immediately increases household earnings and housing rent by about $50 and $14, respectively, but this impact grows to about an $80 increase in earnings and $25 increase in rents about three years after the hike (all from the same one dollar increase in the minimum wage). While it is difficult to tell from simply looking at these point estimates from the staggered event study, the estimates imply a sequence of earnings and rent changes that are almost identical to the estimates implied by the stacked event study. To demonstrate this, I use the actual average change in the minimum wage in each year from the staggered treatment sample

[28] Interestingly, the event study estimates show that the increase in earnings was a little slower to materialize than the increase in housing rents. This may be due to the backward looking natures of the earnings questions in the ACS, but it could also reflect real differences in dynamics of the labor and housing market adjustment process. This is discussed in more detail later.

(both leading and lagged changes) and compute the predicted overall change in household earnings and housing rents using these values and the coefficients in Table 5. All values are computed relative to the predicted effect in 2013 and then these predicted effects (along with their standard error bars) are plotted relative to the estimated effects from the staggered event study. These predicted effects and stacked estimates are presented in Figure 3.

As I show in Figure 3, the predicted effects on earnings and rent implied by the staggered event study coefficients (and the actual minimum wage changes for the stacked sample) are almost identical to the stacked event study estimates. Thus, given the similarities of these estimates, I focus on the staggered DID and event study empirical approach as it produces coefficients that are easier to interpret.

4.1.4 Robustness

Lastly, I explore the robustness of these estimated effects on household earnings and housing rents to alternative specifications. These results are presented in Appendix Table A3. As I show therein, the estimated effects are highly robust. First, the increase in rent does not appear to be due to any mechanical increase in public housing costs or voucher housing costs, which require recipients to pay 30 percent of their earnings towards rent. While I do not observe participation in these specific programs, I do observe whether households participated in SNAP or if they received any public assistance income.[29] Since transfer program participation is positively correlated with participation in other programs (Macartney and Ghertner, 2023),[30] I present estimates where I exclude SNAP participants in column (b) and exclude public income assistance recipients in column (c). The estimated effects on earnings and housing rents persist (and arguable grow) when I exclude these households.

I also explore the geographic scope of the results. Over the period of analysis, many of the areas that experienced large minimum wage hikes were large metropolitan areas that are relatively unique to the rest of the country (namely in New York and California). Thus,

[29]The specific ACS variable that describes household food stamp participation is "FS" and the individual-level variable showing public assistance income over the past 12 months is "PAP," which is aggregated to the household level.

[30]For example, 85% of housing assistance recipients participate in at least one other transfer program (Macartney and Ghertner, 2023)

to test the generalizability of the results, I exclude difference subgroups of the data. In column (d), I exclude all metropolitan areas where the minimum wage hike was due to a city or MSA-specific minimum wage change since these changes are more likely to be endogenous to local labor and housing market conditions. In column (e), I exclude New York City and Los Angeles due to their uniqueness over this period of analysis and that they experienced several large city-level hikes during the period of analysis. Lastly, in column (f), I exclude all of California given its unique housing market. The results are robust to all of these exclusions. That said, the estimated effects in all of these specification become a little noisier and the magnitudes of the changes (in both earnings and housing rents) are somewhat smaller.

4.2 Do Rising Rents Result from Increased Housing Demand?

The previous analysis demonstrates that minimum wage hikes over the 2012-2019 period amounted to an economic transfer to low-income households with each one dollar increase in the minimum wage increasing monthly household income by about $70 but also increased housing rents by about $20 per month. These estimates imply an income elasticity of demand for housing of 0.93 since the average household in the analytic sample earns $2,231 per month and pays $682 in rent.[31] In this section, I explore whether this increase in rent likely resulted from an increase in housing demand (following the income transfer) and whether the change in demand comes from local residents or new migrants that come into the area.

4.2.1 Moving Rates

Table 6 presents the estimated effects of minimum wage hikes on the probability that a household moves, where moving (at least for renters) is the primary way to change housing consumption. Thus, changes in moving rates is one way to capture changes in housing demand. The table presents staggered DID estimates using different measures of moving. Columns (a)-(c) present estimates on the impact of moving over different time lags, from

[31]The income elasticity of demand computed here is simply the percent change in rent (from $682 to $702, or 2.93%)divided by the percent change in income (from $2,231 to $2,301, or 3.14%).

over the past year to over the past five years. Columns (d)-(f) then decompose the moves over the past year using information about where they moved from. In this way, I examine where the increased demand is coming from: local residents in the PUMA or individuals outside the PUMA (both within a state but also across state lines).[32] These estimates are presented using the full sample of households in Panel 1 and for younger households (where the household head is under age 40) in Panel 2.

The estimates suggest that moving rates appear to decrease in the year before a minimum wage hike, but they then return to earlier levels once the minimum wage increases. This effect is evident whether we measure move rates in terms of moves over the past year (column a), moves over the past two years (column b), or moves over the past five years (column c). Interestingly, the magnitude of the decline in moving prior to the minimum wage hike (i.e. the coefficient on the change in the minimum wage the year before its change) increases as we use a longer windows of time to measure moving rates. This suggests that these coefficients are unlikely to reflect anticipatory effects of the hike (because they have been evident for some time) and instead reflect that moving rates were depressed for many years before the hike. Looked at in this way, the estimates imply that the depressed moving rates disappear after the minimum wage increases, suggesting that the hike does indeed increase moving rates. This effect is even more evident if we look at younger households where moving rates were depressed to a greater extent prior to the hike but the effect still disappeared once minimum wages increase. Indeed, many of the changes in the coefficients from one year

[32]The move variables used in this analysis come from two questions in the ACS: "MV" and "MIG." MV asks respondents how long they have resided in their current residence, where the possible replies include: one year or less, one to two years, two to five years, and several replies associated with five or more years. I use MV to create the dependent variables in columns (a)-(c). The ACS also asks households MIG, whether they moved in the last year. For those that report moving in the last year in MIG, they then ask what state and PUMA the individual moved from ("MIGPUMA" and "MIGSP"). We use this information, combined with the information from MV to determine whether moves over the past year were within PUMAs (column d), across PUMAs but within states (column e), or across states (column f). Note that not all people that report that they moved to their residence in the past year (from MV) also report having moved over the last year (in MIG and vice versa). Thus, the shares in columns (d)-(f) do not add to the share reported in column (a).

prior to the hike to the year of the hike are statistically different than zero.[33] In Appendix Table A4, I present event study estimates using the staggered DID approach. Therein, the estimate suggest that moving rates actually increase in the year of the hike and one year after the hike before returning to their previously depressed levels two years after the hike. Thus, I find some evidence that minimum wage hikes do increase moving rates.

Table 6 also presents results exploring whether the increased move rates into a PUMA come from households within the area affected by the minimum wage (i.e. within PUMA) or across broader geographic areas. For this analysis, I limit the outcome to moves over the past year because I only know where indivduals moved from if they moved in the last year (see fn **). The results show that the depressed moving rates prior to the hike are entirely associated with intra-PUMA moves, i.e. moves within a similar neighborhood (or metropolitan area, if its a small metro area), and this effect entirely disappears when the minimum wage increases (see column d). On the other hand, minimum wage hikes appear to have no effect on within-state but across-PUMA moves (column e) and do not increase interstate moves into treatment PUMAs (and may actually *decrease* interstate moves. This affect is again especially evident on younger households, that have higher move rates in general. These estimates suggest that the increase in housing demand (associated with elevated move rates) appears to come from increases in local demand for housing, i.e. increased demand from households already residing in the area experiencing the hike, and not because the hikes induce new households to move into the area. These findings are largely consistent with the literature of minimum wages on interstate migration, which tends to find that hikes decrease migration into areas that increase their minimum wage (Cadena, 2014; Monras, 2019).

[33]The changes in the coefficients from prior to the hike to once the hike is implemented are: 0.005 (0.003) for moves in last year, 0.005 (0.004) for moves last two years, and 0.006 (0.004) for moves in the past give years. When we limit the sample to household heads under age 40, the change in the coefficients increases to 0.012 (0.005) for moves in the last year, 0.011 (0.006) for moves in the last two years, and 0.008 (0.005) for moves in the past five years.

4.2.2 Housing Vacancies

Next I explore how minimum wage hikes affect housing vacancies, which are easily computed using the ACS.[34] Indeed, if minimum wages increase housing demand, one would expect the number of vacant housing units to fall, but this affect should be relative to the number of households likely to be affected by the minimum wage. Thus, I define vacancy rates not relative to the total number of units in an area, but relative to the number of households in our analytic sample. Defined in this way, I find that minimum wage hikes are associated with falling housing vacancy rates in PUMAs most-likely to be affected by minimum wage hikes. These results are presented in Table 7 using all vacancies, vacancies that are "for rent," and "other vacancies" not currently seeking occupancy. Note that this analysis is done at the PUMA level (and not the household level), where I solely examine vacancy rates in PUMAs that house the analytic sample of lower-wage/less-educated renters residing in less-prosperous PUMAs.

The estimates suggest that a one dollar increase in the minimum wage lowers vacancy rates by 0.05 (off a base of 2.06) or 2.4 percent.[35] Some of this comes from declines in "for rent" vacancies (0.01 off a base of 0.31 or 2.6 percent) but most of it comes from a decline in "other vacancies" not currently seeking occupations (0.04 off a base of 0.77 or 5.7 percent). I find no evidence that other types of vacancies decline. Columns (d)-(f) present the event study estimates. These results show that the decline in vacancies occurs as soon as the minimum wage increases and then persists for up to two years after the hike. Taken together, the estimates show that minimum wages decrease the number of vacant units in affected PUMAs relative to the number of affected households. These results are consistent with the earnings shock from minimum wages increasing housing demand in the PUMA.

[34] A vacant unit in the ACS is a housing unit where the number of people residing in the unit is zero (i.e. the variable "NP" in the ACS is zero). The PUMA-level vacancy rate is then the total number of vacancies in the PUMA divided by the total number of households in the analytic sample. The ACS also provides information about why a unit is vacant with its "VACS" variable or "vacancy status." The possible explanations include: for rent, rent but not occupied, for sale, sold but not occupied, for seasonal/recreational/occasional use, for migrant workers, and other vacant. This then allows me to compute vacancy rates for subsets of vacancies, such as the "vacant and for rent" rate, which is total number of vacanct units that are for rent properties divided by the total number of households in the PUMA in the analytic sample.

[35] Vacancy rates can be above one because they are defined as the total number of vacancies in the PUMA p divided by the number of number of households in the ACS analytic sample (and not the number of housing units) in PUMA p.

4.2.3 Heterogenous Rent Effects

My final approach to examine whether this change in housing rents appear to be due to an increase in housing demand is to explore heterogeneity in the rent effects of minimum wage hikes. First, I explore heterogeneity by supply constraints. If the change in rent is due to a shift in demand, then the state of housing supply should dictate the magnitude of the change in rent, where areas with scarce housing supply (low vacancies) and inelastic housing supply should be associated with larger rent increases. Second, I explore heterogeneity by neighborhood characteristics. If rent increases are due to increases in housing demand, then I would expect this demand to be especially pronounced in more-desirable neighborhoods. Thus, I explore heterogeneity in the rent effects by the neighborhood poverty level, the neighborhood crime rates, and the neighborhood school quality. In all of these empirical expressions, which include interactions between the minimum wage variables and these different neighborhood characteristics (which are fixed and based on characteristics from 2012 or earlier), I exclude the change in the minimum wage two years in the future. This term is only weakly identified and adding the interaction term makes identification even more challenging.[36]

The results, presented in Table 8, show the minimum wage hikes translate into larger increases in housing rents when housing supply is more limited. Using the housing supply elasticities from Baum-Snow and Han (2024), I find that the rent increases are larger when housing is more inelastic. Indeed, the estimates imply that one dollar increase in the minimum wage increases rents by $46 if housing is perfectly inelastic but does not increase rents *at all* if the housing supply elasticity is around one. At the same time, household earnings are not affected by the housing supply elasticity (see Appendix Table A5). The estimates using vacancy rates computed from the ACS show similar results, with higher vacancy rates translating into smaller increases in apartment rents. Thus, the results are consisitent with the minimum wage affecting housing rents through their effect on earnings and housing

[36]Since a vast majority of the geographic areas in my analysis first increased their minimum wage in 2014, there are only two pre-periods. Thus, the change in the minimum wage from two years prior is identified off of areas that first increased their minimum wage in 2015 or later. As discussed earlier, there are much fewer of these observations, and adding an interaction terms further complicates the identification of these coefficients. Indeed, when they are included in the empirical specification, the coefficients are often large and very noisily estimated.

demand as the shape of the supply curve dictates the equilibrium price adjustment. This relationship between housing supply elasticity and the rent effects is also plotted out in Figure 4, which presents the predicted change in housing rents from a $1.00 hike by changes in deciles of the housing supply elasticity, using both the linear interaction term model in Table 8 and a quadratic interaction term model in Appendix Table A5.

This increase in housing demand is unlikely to be equal across all areas but rather, increase more in more-desirable (yet still affordable) areas. I test this hypothsis in columns (d)-(g) of Table 8. Therein, I find that rent increases tend to be larger in areas with smaller concentrations of low-income households,[37] which tend to have better neighborhood amenities (Lee and Lin, 2018). I also find larger rent increases in areas with lower crime rates.[38] That said, I find no effects by school test scores from Reardon et al. (2021). Thus, the housing demand effects tend to be larger in low-income neighborhoods with more-desirable characteristics, at least on some dimensions.

4.3 Are Rising Rents associated with Increased Housing Consumption?

Lastly, I examine whether the increase in apartment rents I observe is associated with increased housing consumption. If consumption also increases then changes in the housing market reflect an important consumption effect of minimum wages (Aaronson, Agarwal and French, 2012). On the other hand, if there is little to no increase in housing consumption following the increase in housing rents, then the rising rents reflect the ability of landlords to capture some portion of the economic transfer to low-income households simply because of the unique characteristics of the housing market where housing supply is generally quite slow to adjust to increases in demand.

[37] I measure the proportion of low-income households in a PUMA for this analysis using the SNAP participation rate and the share of single-parent households. These metircs are computed from the ACS and are as of 2012.

[38] As described above, the property crime rates are from Krivo, Lyons and Vélez (2022), which provide crime rates at the Census Tract level for a limited number of areas. The violent crime rates are even more limited and thus, I focus on property crime.

4.3.1 Impact on Housing Characteristics

The housing consumption variables in the ACS include: number of bedrooms, number of rooms, age of unit, type of unit (house, apartment, etc), a series of very basic housing plumbing questions (including running water, a bathroom, and a full kitchen with stove and refrigerator), and whether the rent includes basic utilities (gas, electricity, and water). Estimates of the impact of minimum wages on these different characteristics is presented in Table 9 and Appendix Table A6. Panel A in each table presents the estimates for the full analytic sample of households while Panel B presents estimates solely for households that moved over the previous two years (since moving is really the only way to increase housing consumption for renters).

Looking across the full sample, we see that minimum wage hikes are not associated with any improvements in housing characteristics – with no evidence that the average household is living in a bigger or newer residence with fewer people (Table 9). There is also no evidence that they are more-likely to live in a house (or less-likely to live in a mobile home) or more-likely to have basic housing plumbing. The one characteristic that does change, is that landlords are now *less-likely* to cover basic utilities with rent (Table A5). This is not really a decline in housing consumption, so much as it is another component of rents that increase with minimum wage hikes – with a one dollar increase lowering the probability that a landlord covers the electricity bill by 10 percent (-0.006 on an average of 0.06), covers gas bill by 27 percent (-0.017 on an average of 0.06), and covers a water bill by nine percent (-0.027 on an average of 0.30). That said, when we limit the sample to movers, I find some evidence that minimum wages are causing households to move to younger/newer buildings and reside with fewer people. The estimates suggest that a one dollar increase in the minimum wage lowers the average age of a building by 0.6 percent (-0.27 years on an average of 42.9), increases the probability that a renter is living in a new building by 6 percent (0.003 on a average of 0.06), and decreases the number of people living in the unit by 0.6 percent (0.018 on an average of 2.83). On top of this, move rates are also elevated following hikes, where we previously showed (see Table 6) that a one dollar increases in the minimum wage increased move rates over the past two years by about 1.4% overall (0.005

on an average of 0.37) but 2.3 percent for younger households (0.011 on an average of 0.47). Thus, while minimum wages are not associated with large increases in housing consumption, housing consumption does increase a little.

4.3.2 Impact on Rents Conditional of Housing Characteristics

Another way to test the extent to which housing consumption explains the rise in housing rents is to estimate the impact of minimum wage hikes on housing rents, holding housing characteristics fixed (i.e. include housing characteristics as explanatory variables in the regression). It is also instructive to estimate the impact of minimum wages on rents separately for movers and non-movers. Since only movers can increase housing consumption in meaningful ways, the impact on non-movers reflects the market adjustment while the difference in the impact for movers and non-movers reflects increased consumption.

These estimates are included in Table 10. Columns (a) and (b) present the estimates on the overall sample (overall and conditioning on housing characteristics). Columns (c) and (d) present the same estimates on households that moved over the previous two years and columns (e) and (f) present the estimates for non-movers. Overall, these results show that most of the change in rent is due to a market-based adjustment in housing rents and not increases in housing consumption. First, the estimates imply that conditioning on housing characteristics has almost no impact on the overall estimates. While the estimated effects decline in all three instances (full sample, movers, non-movers), the decline in all three is only by about 1-2 percent. This implies that changes in measurable housing characteristics (following minimum wage hikes) have almost no overall effect on rent. Second, the rent increases for non-movers ($19 for a one dollar increase in the minimum wage) are only five percent below the overall effect. Thus, 95 percent of the overall increase appears to be due to a market-based adjustment. Movers, however, do increase housing consumption by $10 (on top of the $19 market-based increase in rents) for every one dollar increase in the minimum wage, but there are relatively few of them and thus, there impact on the overall effect of minimum wages on apartment rents is relatively small.

Taken together, these results imply that housing consumption does not explain much of the increase in housing rents. Instead, increases in housing demand (resulting from the

earnings shock) tend to lead to higher rents without large increases in housing consumption. In this way, landlords are able to capture almost 30 percent of the earnings shock without material changes to the housing stock.

5 Conclusion

It is well-accepted that productivity-induced wage growth can impact the costs of local housing as workers migrate to newly productive areas seeking out higher earnings (Rosen, 1979; Roback, 1982; Moretti, 2011). Housing costs could also increase (following a productivity shock) due to rising local demand for housing as existing residents seek out more (and better) housing. In both instances, if housing supply is inelastic, housing prices will capture a sizeable portion of the productivity shock. In this paper, I explore the impact of income transfers to low-income households on housing costs and housing consumption. Since low-income households tend to consume substandard housing (Weeden, 2023), the demand response to increased income (from the transfer) could be particularly large. Like productivity shocks, if housing is inelastically supplied, this increased demand will increase housing costs. Moreover, since low-income households frequently rent their housing, higher housing costs may amount to a tax on the transfer with landlords capturing a significant portion of the new income.

Using the minimum wage as a natural experiment that increases household earnings, I examine the impact of minimum wage hikes on household earnings, housing rents, housing demand and housing consumption using data from the 2012-2019 ACS. I find that minimum wages (over this period of analysis) increase household earnings without harming a household's total hours worked. A one dollar increase in the minimum wage tends to increase household earnings by about $70 per month. I then explore the housing market response to the increase in earnings. A one dollar increase in minimum wages tend to increase monthly rents by about $20. This increase in rent appears to come from increases in local demand, as the move rates of local residents (but not residents from farther away areas) increase by about two percent following a one dollar hike. Moreover, I observe declining housing vacancy rates and the rent increases are heterogeneous: rising more when housing is in-

elastically provided and in neighborhoods with better amenities. All of these results are consistent with rent increases coming from a housing demand response. Lastly, I show that there is little evidence that affected households increase their housing consumption – with some evidence that movers tend to relocate to newer housing units with fewer people living in them following minimum wage hikes. That said, households experience housing rent increases (following minimum wage hikes) whether they move or not. Moreover, the rent increases for non-movers represent 95 percent of the overall effect. Thus, a vast majority of the rent increases I document, appear to reflect a market-based adjustment and not an increase in housing consumption.

Taken together, these results show that housing is indeed acting as a tax on economic transfers – with landlords capturing upwards of 30 percent of the transfer.[39] Moreover, the place-based nature of minimum wage policies are not fueling this phenomenon. Instead, rising housing costs come from increased local demand and the nature of local housing markets. This means that national policies are likely to have similar effects. For example, a universal basic income might be particularly ripe for landlord capture. This is a new and important finding – documenting that landlords are important beneficiaries of cash transfers. It suggests that governments should consider the extent to which landlords can capture income transfers when designing future policies. Moreover, policymakers may want to consider additional policies, which they adopt alongside income transfers, that limit the rate of housing rent increases, at least for a period of time.

[39]The income elasticity of housing implied by this change is 0.93.

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Table 1: Summary Statistics on Households from American Community Survey, 2012-2019

		Analytic Sample				
		Sample 1:	Sample 2:	Sample 3:	Sample 4:	
	All HH Heads Aged 20-60	Remove High Wage HHs	Remove High Wage HHs & Non-Renters	Remove High Wage HHs Non-Renters, & High-Edu HHs	Remove High Wage HHs, Non-Renters, High-Edu HHs, & HHs in Richer PUMAs	Placebo Sample
	(a)	(b)	(c)	(d)	(e)	(f)
Age	43.6	43.0	38.5	41.1	40.9	43.7
Years Education	14.1	13.6	13.3	10.6	10.5	16.0
Female	0.49	0.51	0.55	0.53	0.54	0.47
Latino	0.13	0.15	0.21	0.31	0.34	0.06
Immigrant	0.15	0.15	0.20	0.27	0.27	0.15
Asian American	0.06	0.05	0.06	0.04	0.03	0.11
Black	0.12	0.13	0.20	0.22	0.24	0.07
White Non-Hispanic	0.68	0.65	0.52	0.42	0.38	0.76
Married	0.68	0.65	0.52	0.42	0.38	0.76
Ave. Household Monthly Earnings	$6,666	$4,340	$3,248	$2,466	$2,231	$9,808
Ave. Household Wage	$29.37	$18.47	$16.65	$14.30	$13.58	$36.49
Average Household Wage-to-MW	3.47	2.19	1.91	1.63	1.56	4.39
Share that Rent Housing	0.35	0.39	0.39	0.43	0.45	0.23
Housing Rent (if Renter)	$996	$928	$928	$765	$682	$1,552
Move Last 12 Months	0.16	0.17	0.31	0.25	0.26	0.14
Rent-to-Earnings Ratio	0.15	0.21	0.29	0.31	0.31	0.16
Bedrooms per Person	1.01	0.97	0.81	0.75	0.74	1.09
Rooms per Person	2.15	2.05	1.74	1.57	1.54	2.36
Reside in New Residence	0.08	0.07	0.07	0.05	0.04	0.11
Observations	4,915,057	3,805,269	1,502,285	597,579	383,107	988,417

Notes: The sample is limited to household heads aged 20 to 60 that reside in a state that did not experience continuous inflation-based minimum wage adjustments over the sample period. Thus, households in Arizona, Colorado, Florida, Missouri, Montana, Ohio, Oregon, Vermont, and Washington are excluded from the analysis. Additional adjustments to the analytic sample are described in columns (b)-(e), where each subsequent column removes more-and-more of the sample to focus in on a sample of households most-likely to be affected by minimum wage hikes. "High-Wage Households" are households where the average household wage was greater than the 75th percentile of wages in the state or whose earnings are greater than the 95th percentile in the state. A "Non-renter" household is a household that owns its residence or lives in its residence without rent. "High-educated" households are households where the household head has some college or a college degree. "Richer PUMAs" are PUMAs where low-wage households comprise less than 25 percent of households (where a low-wage household has an average wage less than twice the minimum wage) and fewer than 10 percent of households receive food stamps. The Placebo Sample is the inverse of the most restrictive sample, Sample 4. It includes high-wage, high-educated households in richer PUMAs. Herein, high-wage is expanded to include households where the average wage is greater than the median wage in the state. Summary statistics reflect the characteristics of the household head unless otherwise stated. A "new residence" is defined as one that was built in the last ten years.

Table 2: Average Minimum Wage Hikes by Sample, 2011-2019

	Treatment Sample by Year of First Hike						Control
Year	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**	**Sample**
	(a)	(b)	(c)	(d)	(e)	(f)	(g)
2011	$0.00	$0.00	$0.00	$0.00	$0.00	$0.00	$0.00
2012	$0.49	$0.00	$0.00	$0.00	$0.00	$0.00	$0.00
2013	$0.31	$0.77	$0.00	$0.00	$0.00	$0.00	$0.00
2014	$0.18	$0.23	$0.55	$0.00	$0.00	$0.00	$0.00
2015	$0.76	$0.56	$0.59	$0.71	$0.00	$0.00	$0.00
2016	$0.45	$0.32	$0.71	$0.78	$1.18	$0.00	$0.00
2017	$0.56	$0.12	$0.84	$0.49	$0.01	$1.19	$0.00
2018	$0.59	$0.54	$0.87	$0.20	$0.03	$1.21	$0.00
2019	$0.62	$0.49	$1.01	$0.70	$0.07	$1.00	$0.00
Total Increase	$3.96	$3.03	$4.57	$2.89	$1.29	$3.40	$0.00
Observations	650	2,983	123,755	28,250	583	3,273	223,613

Notes: A minimum wage change in a given year reflects the average change in the minimum wage from the year prior. Over the period 2011-2019, San Francisco and Santa Fe first increased their minimum wage in 2012; Albuquerque, Providence, and San Jose first increased their minimum wage in 2013; California, Connecticut, Delaware, Michigan, Minnesota, Montgomery County (MD), New Jersey, New York, Prince George County (MD), and Washington, DC first increased their minimum wage in 2014; Alaska, Arkansas, Chicago, Hawaii, Iowa City (IA), Las Cruces (NM), Louisville (KY), the rest of Maryland, Massachusetts, Nebraska, South Dakota, and West Virginia first increased their minimum wage in 2015; Lexington (KY) and Portland, ME first increased their minimum wage in 2016; and Cedar Rapids (IA), Cook County (IL), and Maine first increased their minimum wage in 2017. No geographic areas first increased their minimum wage in 2018 or 2019. Alabama, Georgia, Idaho, Indiana, Kansas, Louisiana, Mississippi, Nevada, New Hampshire, North Carolina, North Dakota, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Virginia, Wisconsin, Wyoming, the rest of Illinois, the rest of Iowa, the rest of Kentucky, and the rest of New Mexico did not increase their minimum wage at all over the 2011-2019 period

Table 3: Effects of Minimum Wage Hikes on Hours Worked, Earnings, and Housing Rents

	Analytic Sample		**Placebo Sample**	
	(a)	(b)	(c)	(d)
Panel 1: Impact on Household Hours Worked				
ΔMW in Two Years	1.44	1.62	0.29	-0.66
	(1.02)	(1.36)	(0.42)	(0.60)
ΔMW Next Year	0.93	1.12	1.06**	0.29
	(1.02)	(1.40)	(0.45)	(0.65)
Minimum Wage This Year	1.53***	2.60	0.59***	-0.45
	(0.49)	(1.79)	(0.20)	(0.82)
Panel 2: Impact on Household Earnings				
ΔMW in Two Years	26.76	38.44	55.82***	-28.60
	(21.17)	(25.72)	(17.24)	(22.81)
ΔMW Next Year	16.33	33.29	66.49***	6.50
	(18.06)	(23.07)	(16.89)	(22.01)
Minimum Wage This Year	69.45***	111.16***	78.64***	-30.93
	(8.43)	(26.78)	(8.05)	(28.63)
Panel 3: Impact on Household Earnings (Conditional on Hours Worked)				
ΔMW in Two Years	9.00	18.44	48.20***	-10.95
	(14.92)	(17.19)	(11.28)	(14.46)
ΔMW Next Year	4.91	19.53	38.46***	-1.33
	(11.56)	(15.43)	(11.63)	(14.11)
Minimum Wage This Year	50.61***	79.13***	62.93***	-18.96
	(5.27)	(16.79)	(5.61)	(18.28)
Panel 4: Impact on Housing Rents				
ΔMW in Two Years	1.15	1.08	27.64***	14.32***
	(3.08)	(3.25)	(4.64)	(5.49)
ΔMW Next Year	3.33	0.78	20.59***	7.09
	(2.83)	(3.72)	(5.19)	(6.53)
Minimum Wage This Year	20.43***	12.29***	34.36***	6.37
	(1.60)	(4.72)	(3.57)	(7.57)
Observations	383,107		988,417	
PUMA-Spec. Time Trends	No	Yes	No	Yes

Notes: The Analytic Sample (Sample 4 in Table 1) includes lower wage, less-educated, renters in poorer PUMAs. The Placebo sample includes higher wage, higher-educated, households in more-prosperous PUMAs. The complete sample definitions are described in Table 1. In addition to PUMA and year dummy variables, the empirical specifications also includes controls for the demographic characteristics of the household head including: a quadratic in age, dummy variables for years of education, and dummy variables for race, sex, ethnicity, and immigration status. The regressions in columns (d) and (d) also include PUMA-specific time trends. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table 4: Stacked Estimates of Minimum Wage Hikes on Earnings and Housing Rents

	2014 Experiment		Stacked Estimates			
			All Treatment States		Excl. Small Hike States	
	DID	Event Study	DID	Event Study	DID	Event Study
	(a)	(b)	(c)	(d)	(e)	(f)
Panel 1: Impact on Earnings						
Treat*Post	150.6***		88.1***		126.2***	
	(26.9)		(23.4)		(26.9)	
Treat*Pre2		14.3		4.9		5.5
		(36.3)		(29.1)		(34.2)
Treat*Pre1		**Excl.**		**Excl.**		**Excl.**
Treat*Post0		35.7		15.2		41.3
		(35.7)		(29.6)		(34.4)
Treat*Post1		5.0		9.7		39.4
		(38.9)		(33.9)		(37.9)
Treat*Post2		124.7***		89.5**		117.5***
		(40.9)		(35.8)		(41.2)
Treat*Post3		205.6***		149.2***		206.2***
		(44.5)		(37.3)		(43.7)
Treat*Post4		244.7***		199.8***		253.2***
		(47.0)		(41.9)		(50.3)
Treat*Post5		351.9***				
		(49.8)				
Panel 2: Impact on Housing Rents						
Treat*Post	46.7***		29.3***		43.5***	
	(5.2)		(4.4)		(5.2)	
Treat*Pre2		1.7		-1.5		-1.9
		(5.0)		(4.2)		(4.9)
Treat*Pre1		**Excl.**		**Excl.**		**Excl.**
Treat*Post0		19.2***		13.6***		15.4***
		(5.0)		(4.6)		(5.5)
Treat*Post1		16.6***		12.2**		17.7***
		(5.6)		(5.0)		(5.9)
Treat*Post2		31.9***		25.2***		38.5***
		(6.4)		(5.9)		(7.1)
Treat*Post3		57.7***		41.7***		62.9***
		(7.3)		(6.6)		(7.7)
Treat*Post4		69.7***		52.3***		82.2***
		(8.0)		(7.7)		(8.9)
Treat*Post5		95.0***				
		(9.7)				
Observations	347,368		525,722		491,625	

Notes: The treatment samples differ across the estimates. Columns (a) and (b) are limited to geographic areas that first increased their minimum wage in 2014. Columns (c) and (d) present stacked estimates using both the 2014 and 2015 experimental sample. Columns (e) and (f) are identical to (c) and (d), except that geographic areas with small minimum wage hikes (average annual hike less than $0.50) are excluded, including Alaska, Arkansas, Connecticut, Delaware, Iowa City (IA), Louisville (KY), Nebraska, New Jersey, Michigan, South Dakota, and West Virginia. The empirical specifications also include PUMA dummy variables, year dummy variables, and controls for the demographic characteristics of the household head (as described in Table 3). Standard errors are clustered at the PUMA level. *p≤0.1; **p<0.05; and *** p<0.01.

Table 5: Event Study Estimates using a Staggered DID Research Design

	Two Lags			**Four Lags**		
	Earnings	**Earnings Cond. on Hours**	**Rent**	**Earnings**	**Earnings Cond. on Hours**	**Rent**
	(a)	(b)	(c)	(d)	(e)	(f)
ΔMW in Three Years	-6.4	-8.2	-0.8	-4.9	-4.8	-0.8
	(15.2)	(9.2)	(2.7)	(15.2)	(9.3)	(2.7)
ΔMW in Two Years	24.5	7.5	0.6	28.0	16.2	1.0
	(21.1)	(14.8)	(2.8)	(21.4)	(14.6)	(2.9)
ΔMW Next Year	21.4	9.4	4.8*	25.4	17.1	4.3
	(18.3)	(11.7)	(2.9)	(19.7)	(12.2)	(3.1)
ΔMW This Year	46.5**	29.5*	13.9***	49.7**	36.9**	14.0***
	(23.7)	(15.1)	(4.3)	(24.8)	(15.7)	(4.4)
ΔMW Last Year	45.3*	25.4*	17.2**	49.9*	38.4**	18.3***
	(25.1)	(15.4)	(4.0)	(25.5)	(16.1)	(4.1)
MW Two Years Ago	89.4***	69.9***	24.6***			
	(13.0)	(9.2)	(3.3)			
ΔMW Two Years Ago				82.6***	44.0***	20.0***
				(24.1)	(16.8)	(4.3)
ΔMW Three Years Ago				81.2***	59.5***	28.0***
				(28.3)	(17.9)	(5.9)
MW Four Years Ago				109.2***	118.3***	26.6***
				(26.0)	(15.3)	(3.9)

Notes: Each regression uses solely the sample of household heads described in Table 1 as Sample 4, which is our main analytic samples and includes less-educated, lower-wage renters residing in poorer PUMAs. Standard errors are clustered at the PUMA level. N=383,107 for all specifications. *p<0.1; **p<0.05; and *** p<0.01.

Table 6: Effects of Minimum Wage Hikes on Moving Rates

	Move Past Year					
	Move Past Year	**Move Past 2 Years**	**Move Past 5 Years**	**Move within PUMA**	**Move within State but across PUMA**	**Move across States**
	(a)	(b)	(c)	(d)	(e)	(f)
Panel 1: Impact on All Households						
ΔMW in Two Years	-0.001	-0.003	-0.001	-0.003	0.000	0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)
ΔMW Next Year	-0.005*	-0.008**	-0.014***	-0.006**	0.001	0.002**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)
Minimum Wage This Year	0.000	-0.003**	-0.008***	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	0.000	0.000
Average	0.27	0.37	0.69	0.16	0.03	0.02
Panel 2: Impact on Younger Households						
ΔMW in Two Years	0.005	0.001	0.004	0.000	-0.001	0.001
	(0.005)	(0.006)	(0.004)	(0.004)	(0.002)	(0.002)
ΔMW Next Year	-0.012**	-0.015***	-0.016***	-0.015***	0.001	0.003*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)
Minimum Wage This Year	0.000	-0.004**	-0.008***	0.001	-0.001	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Average	0.34	0.47	0.80	0.20	0.04	0.03

Notes: The move variables used in this analysis come from two questions in the ACS: "mv" and "mig." "mv" asks respondents how long they have resided in their current residence, where the possible replies include: one year or less, one to two years, two to five years, and several replies associated with five or more years. We use "mv" to create the dependent variables in columns (a)-(c). The ACS also asks households "mig," whether they moved in the last year. For those that report moving in the last year in "mig," they then ask what state and PUMA the individual moved from. We use this information, combined with the information from "mv" to determine whether moves over the past year were within PUMAs (column d), across PUMAs but within states (column e), or across states (column f). Note that not all people that report that they moved to their residence in the past year (from "mv") also report having moved over the last year (in "mig" and vice versa). Thus, the shares in columns (d)-(f) do not add to the share reported in column (a). The household level analysis also includes PUMA and year fixed effects as well as basic demographic controls for the household head, which are described in detail in Table 3. $N = 383,101$ in Panel 1 when all households in the analytic sample are included. $N = 176,858$ in Panel 2 when the sample is limited to household heads under age 40 in the analytic sample. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table 7: Effects of Minimum Wage Hikes on Housing Vacancy Rates

	All Vacant	**Vacant & For Rent**	**Vacant & Not Seeking Occupancy**	**All Vacant**	**Vacant & For Rent**	**Vacant & Not Seeking Occupancy**
	(a)	(b)	(c)	(d)	(e)	(f)
ΔMW in Three Years				-0.03	-0.01	-0.03
				(0.03)	(0.01)	(0.02)
ΔMW in Two Years	0.02	-0.01	0.00	0.03	-0.01	0.01
	(0.04)	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)
ΔMW Next Year	0.00	0.00	-0.01	0.00	0.00	-0.02
	(0.03)	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
Minimum Wage This Year	-0.05***	-0.01**	-0.04***			
	(0.01)	(0.00)	(0.01)			
ΔMW This Year				-0.08*	-0.01*	-0.04***
				(0.04)	(0.01)	(0.01)
ΔMW Last Year				-0.05	-0.01	-0.03
				(0.04)	(0.01)	(0.02)
MW Two Years Ago				-0.04*	-0.01	-0.06***
				(0.02)	(0.01)	(0.01)
Sample Mean	2.06	0.31	0.77	2.06	0.31	0.77

Notes: The vacancy rate is defined as the total number of vacancies within a PUMA/year divided by the number of renters in the analytic sample. Thus, the analysis is performed at the PUMA/year level. The total number of vacancies within a PUMA is computed using the ACS, which includes data on vacant housing units. Therein, unoccupied housing units are classified as for rent, rent but not occupied, for sale, sold but not occupied, for seasonal/recreational/occasional use, for migrant workers, or other vacant. The dependent variable in columns (a) and (d) use all vacancies, whereas the dependent variable in columns (b) and (e) and then (c) and (f) solely use vacancies actively for rent and other vacancies, respectively, the latter which I refer to as "not currently seeking occupancy." The regression also includes puma and year fixed effects and averages of the demographic variables included in the household-level analysis (described in Table 3) at the PUMA-year level. $N = 7,000$ for all specifications and solely includes PUMAs in the analytic sample. Standard errors are clustered at the PUMA level. * p<0.1; ** p<0.05; and *** p<0.01.

Table 8: Heterogeneity in Effect of Minimum Wage Hikes on Housing Rents by PUMA Characteristics

		Housing Supply		**Poverty**		**Amenities**	
	Baseline Model	**Housing Supply Elasticity**	**Vacancy Rate**	**Share Receiving Food Stamps**	**Share Single Parent**	**Property Crime per 1000**	**8th Grade Test Scores**
	(a)	(b)	(c)	(d)	(e)	(f)	(g)
ΔMW Next Year	3.74	5.31	4.71	6.11	9.35*	-16.44	1.04
	(2.90)	(10.81)	(3.32)	(5.91)	(5.64)	(14.63)	(4.91)
MW This Year	20.32***	46.14***	24.46***	35.25***	29.21***	38.92***	17.50***
	(1.57)	(5.15)	(1.75)	(3.65)	(2.82)	(5.30)	(3.13)
ΔMW Next Year X PUMA Characteristic		-3.83	-1.39	-13.80	-102.47	0.05	-6.42
		(18.43)	(1.71)	(19.16)	(72.77)	(0.64)	(5.59)
MW This Year X PUMA Characteristic		-56.88***	-7.75***	-60.38***	-153.01***	-1.21***	1.20
		(8.92)	(1.24)	(13.79)	(42.20)	(0.33)	(3.42)
Observations	383,107	237,996	383,107	383,107	383,107	71,536	322,093

Notes: The estimated coefficients describe how the rent effects of minimum wage hikes change as the characteristics of the PUMAs change. Housing supply elasticities come from Baum-Snow and Han (2024) and vary at the metropolitan areas level. The vacancy rate, the share receiving food stamps, and the single parent share are computed using the ACS and are at the PUMA level as of 2012. The property crime rates are from 2010 wave of the National Neighborhood Crime Study, described in Krivo, Lyons and Vélez (2022), which includes Census tract level information on property crime incidents. These are aggregated to the PUMA level, but data are not available on all areas. Lastly, test score data are from Reardon et al. (2021), which provide school district level test scores as of the 2008-2009 academic year. We use the Census block aggregates of these scores from Baum-Snow, Hartley and Lee (2019), which we further aggregate to PUMAs. All analyses use the main analytic sample, but sample sizes differ across some specifications because the geographic coverage of other (non-ACS) data sources are not comprehensive. The change in the minimum wage two years in the future is excluded from the regression specifications because it is weakly identified and highly correlated with the interaction term. The specifications are otherwise identical to these presented in Table 3. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table 9: Effects of Minimum Wage Hikes on Housing Consumption

	Number of Bedrooms	Number of Rooms	Reside in House?	Age of Building	New Building?	Number of People
	(a)	(b)	(c)	(d)	(e)	(f)
Panel 1: Full Sample						
ΔMW in Two Years	0.002	0.005	-0.001	-0.093	-0.001	-0.006
	(0.007)	(0.011)	(0.003)	(0.181)	(0.001)	(0.010)
ΔMW Next Year	-0.003	0.011	-0.001	-0.260	0.002	0.010
	(0.008)	(0.011)	(0.003)	(0.160)	(0.001)	(0.012)
Minimum Wage This Year	-0.002	-0.006	-0.001	-0.113	0.001	0.002
	(0.003)	(0.005)	(0.001)	(0.077)	(0.001)	(0.004)
Sample Mean	2.17	4.45	0.37	45.7	0.05	2.97
Panel 2: Movers Only						
ΔMW in Two Years	0.016	0.028	-0.001	-0.155	-0.002	-0.006
	(0.014)	(0.021)	(0.006)	(0.320)	(0.003)	(0.023)
ΔMW Next Year	0.008	0.025	-0.001	-0.081	0.001	0.018
	(0.015)	(0.021)	(0.006)	(0.319)	(0.003)	(0.023)
Minimum Wage This Year	-0.004	-0.008	0.000	-0.273**	0.003**	-0.018**
	(0.006)	(0.008)	(0.002)	(0.118)	(0.001)	(0.008)
Sample Mean	2.12	4.38	0.35	42.9	0.06	2.83

Notes: A "new building" is defined as a building that is less than 10 years old. The analytic sample for each of the specifications is identical to the sample described in Table 3. Panel 1 uses all observations ($N = 383,107$) while Panel 2 is limited to households that moved sometime in the past two years ($N = 141,291$). Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

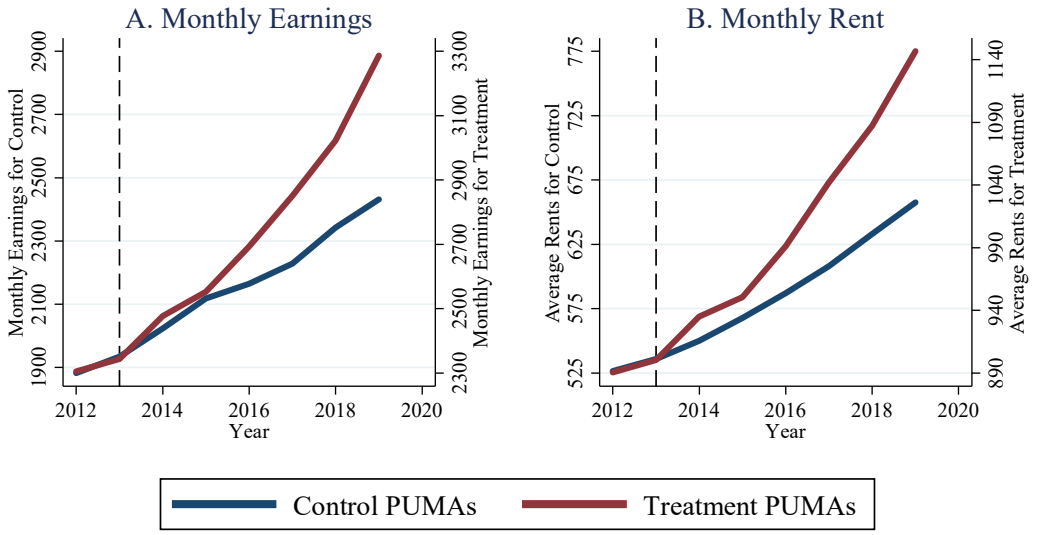
Table 10: Effects of Minimum Wages on Rent by Move Status

	Full Sample		**Movers**		**Non-Movers**	
	Spec 1	**Spec 2**	**Spec 1**	**Spec 2**	**Spec 1**	**Spec 2**
	(a)	(b)	(c)	(d)	(e)	(f)
ΔMW in Two Years	1.15	1.78	3.07	2.04	0.42	2.13
	(3.08)	(2.98)	(6.06)	(5.60)	(3.63)	(3.49)
ΔMW Next Year	3.33	2.93	6.23	5.18	3.69	3.92
	(2.83)	(2.96)	(5.62)	(5.56)	(3.26)	(3.26)
Minimum Wage This Year	20.43***	20.00***	29.48***	29.29***	19.39***	19.23***
	(1.60)	(1.60)	(2.98)	(2.94)	(1.56)	(1.57)
Cond. on Housing Characteristics	No	Yes	No	Yes	No	Yes
Observations	383,107		141,291		241,816	

Notes: Specification 1 differs from Specification 2 in that Specification 2 also conditions on housing characteristic variables to account for any changes in housing consumption. The specific housing characteristics variables included in Specification 2 include: the number of bedrooms, the number of rooms, whether the household resides in a house, the age of the residence, whether it is a new building (i.e. less than 10 years old), the number of people in the unit, and whether the rent includes electricity, gas, or water. A mover is defined as a household that moved sometime over the previous two years. The analytic sample is identical to the sample described in Table 3. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

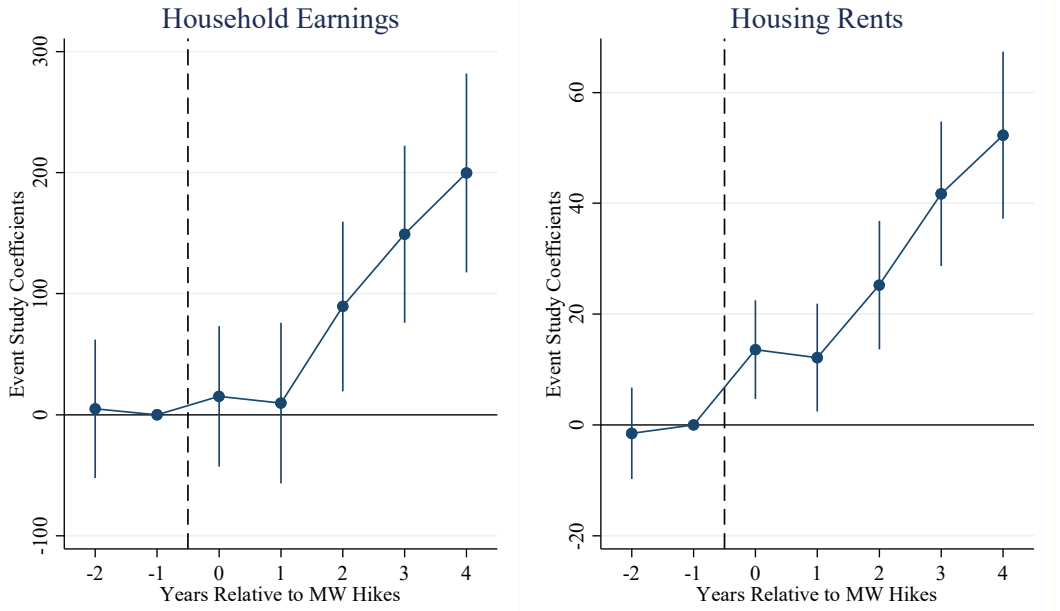
Figure 1: Impact of Minimum Wages on Earnings and Rent

2014 Hike PUMAs vs. Never Hike PUMAs



Note: This figure presents the average earnings and average rent of low-income households, comparing households in states that first increased their minimum wage in 2014 with states that did not increase their minimum wage at all over the sample period, 2012-2019. Treatment sample are limited to those PUMAs that experienced large increases in their minimum wage (greater than $0.50 per year) each year between 2014-2019. The vertical line in each figure is in 2013, the year before the first hike.

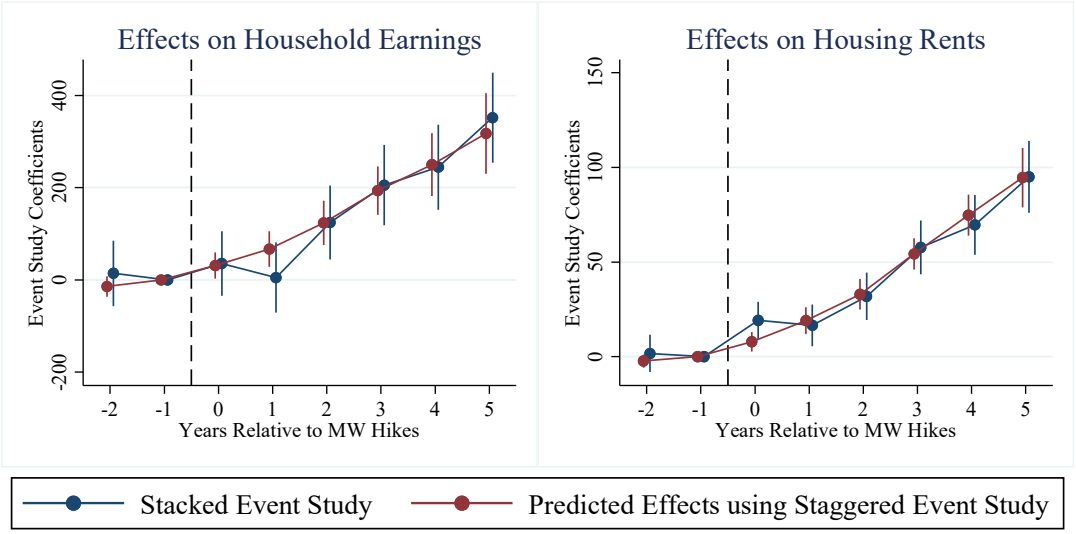
Figure 2: Stacked Estimates of Minimum Wages on Earnings and Housing Rents



Note: This figure presents estimates of the impact of minimum wage hikes on household earnings and housing rents using a stacked event study research design and a balanced panel using the 2014 and 2015 hike sample.

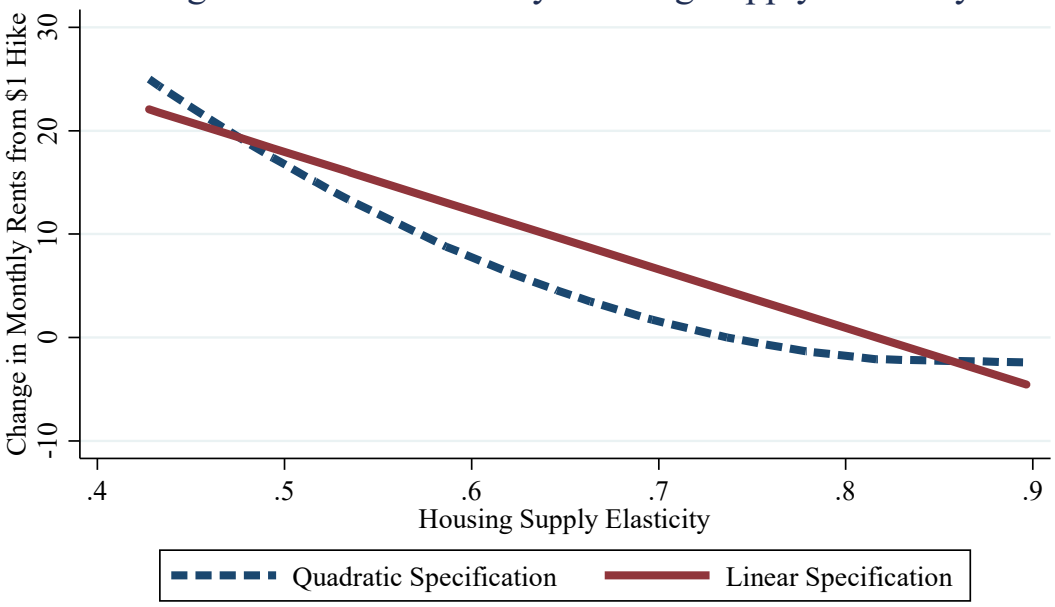
Figure 3: Stacked vs. Staggered Event Study Estimates

Comparing Estimates of the 2014 Experiment



Note: This figure presents the estimated effects of minimum wage hikes on household earnings and rents using two approaches. The stacked event study uses the 2014 treatment and control PUMAs to estimate the impact of those hikes. The staggered event study estimates use the complete analytic sample, but those coefficients are transformed into the predicted effects for the 2014 treatment sample (given the hikes they experienced) to compare the two sets of estimates. The predicted effects are all relative to the predicted effects for 2013 (t-1). The standard error bars capture the 95% confidence interval for estimated impact.

Figure 4: Rent Effects by Housing Supply Elasticity



Notes: This Figure plots the predicted rent effects of a $1 increase in the minimum wage by the housing supply elasticity in a PUMA. The predicted effects based on coefficient estimates using a quadratic interaction between the minimum wage and the housing supply elasticity and the actual ventiles of the housing supply elasticity distribution.

Appendix A: Supplementary Tables

Table A1: Effects of Minimum Wage Hikes on Hours, Earnings, and Rents by Sample

	Sample 1	**Sample 2**	**Sample 3**	**Sample 4**
	(a)	(b)	(c)	(d)
Panel 1: Impact on Household Hours Worked				
ΔMW in Two Years	1.29***	1.30**	1.26	1.44
	(0.37)	(0.51)	(0.81)	(1.02)
ΔMW Next Year	1.08***	0.83*	0.67	0.93
	(0.37)	(0.48)	(0.78)	(1.02)
Minimum Wage This Year	1.42***	1.30***	1.29***	1.53***
	(0.18)	(0.23)	(0.35)	(0.49)
Panel 2: Impact on Household Earnings				
ΔMW in Two Years	45.91***	37.36***	24.60	26.76
	(8.90)	(11.15)	(15.63)	(21.17)
ΔMW Next Year	16.80**	18.64*	13.28	16.33
	(8.42)	(10.22)	(14.13)	(18.06)
Minimum Wage This Year	64.99***	66.94***	62.98***	69.45***
	(4.09)	(5.01)	(6.35)	(8.43)
Panel 3: Impact on Household Earnings (Conditional on Hours Worked)				
ΔMW in Two Years	24.24***	17.91**	8.22	9.00
	(5.95)	(7.55)	(10.87)	(14.92)
ΔMW Next Year	-1.36	6.19	4.54	4.91
	(5.65)	(6.96)	(9.33)	(11.56)
Minimum Wage This Year	41.06***	47.52***	46.24***	50.61***
	(2.62)	(3.40)	(4.08)	(5.27)
Panel 4: Impact on Housing Rents				
ΔMW in Two Years	9.92***	9.92***	8.01***	1.15
	(2.20)	(2.20)	(2.56)	(3.08)
ΔMW Next Year	9.48***	9.48***	5.95**	3.33
	(1.85)	(1.85)	(2.43)	(2.83)
Minimum Wage This Year	27.19***	27.19***	23.28***	20.43***
	(1.41)	(1.41)	(1.34)	(1.60)
Observations	3,805,269	1,502,285	597,579	383,107
PUMA-Spec. Time Trends	No	No	No	No

Notes: Each column in each panel presents the estimates of a different regression, as the analytic sample (column) and dependent variable (panel) changes. The samples are descibed in detail in Table 1, with Sample 4 the preferred analytic sample because it is most-likely to be affected by minimum wage hikes. See Table 3 for a complete description of the empirical specification. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table A2: Stacked Estimates of Minimum Wage Hikes on Hours Worked and Earnings

	2014 Experiment		Stacked Estimates			
			All Treatment States		Excl. Small Hike States	
	DID	Event Study	DID	Event Study	DID	Event Study
	(a)	(b)	(c)	(d)	(e)	(f)
Panel 1: Impact on Hours Worked						
Treat*Post	5.1***		3.1**		4.5***	
	(1.6)		(1.4)		(1.6)	
Treat*Pre2		-1.8		-0.9		-0.5
		(2.5)		(2.0)		(2.4)
Treat*Pre1		Excl.		Excl.		Excl.
Treat*Post0		0.9		0.8		2.4
		(2.4)		(2.0)		(2.4)
Treat*Post1		-0.5		0.5		2.1
		(2.5)		(2.2)		(2.4)
Treat*Post2		5.5**		2.7		4.1
		(2.6)		(2.3)		(2.6)
Treat*Post3		6.8**		4.7**		6.6**
		(2.8)		(2.4)		(2.8)
Treat*Post4		6.3**		5.1**		6.2**
		(2.8)		(2.5)		(2.9)
Treat*Post5		6.7**				
		(3.0)				
Panel 2: Impact on Earnings (conditional on hours)						
Treat*Post	87.7***		50.1***		72.0***	
	(16.4)		(13.9)		(16.1)	
Treat*Pre2		36.9		15.3		11.6
		(23.2)		(18.5)		(22.1)
Treat*Pre1		Excl.		Excl.		Excl.
Treat*Post0		24.4		5.7		12.5
		(22.0)		(18.3)		(21.8)
Treat*Post1		11.4		4.2		13.5
		(23.5)		(20.1)		(24.3)
Treat*Post2		56.9**		56.4***		68.3***
		(24.0)		(20.9)		(24.6)
Treat*Post3		122.0***		91.6***		126.0***
		(26.6)		(22.8)		(25.9)
Treat*Post4		167.7***		138.5***		178.4***
		(30.8)		(26.7)		(31.0)
Treat*Post5		270.1***				
		(31.4)				
Observations	347,368		525,722		491,625	

Notes: The treatment samples differ across the estimates. Columns (a) and (b) are limited to geographic areas that first increased their minimum wage in 2014. Columns (c) and (d) present stacked estimates using both the 2014 and 2015 experimental sample. Columns (e) and (f) are identical to (c) and (d), except that geographic areas with small minimum wage hikes (average annual hike less than $0.50) are excluded, including Alaska, Arkansas, Connecticut, Delaware, Iowa City (IA), Louisville (KY), Nebraska, New Jersey, Michigan, South Dakota, and West Virginia. The empirical specifications also include PUMA dummy variables, year dummy variables, and controls for the demographic characteristics of the household head (as described in Table 3). Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table A3: Robustness of Effect of Minimum Wage Hikes on Earnings and Housing Rents

	Baseline Sample	**Exclude SNAP Participants**	**Exclude Cash Transfer Recipients**	**State-Level Hikes Only**	**Exclude Los Angeles & New York City**	**Exclude All of California**
	(a)	(b)	(c)	(d)	(e)	(f)
Panel 1: Effects on Household Earnings						
ΔMW in Two Years	26.76	19.99	14.16	59.89**	37.35	13.09
	(21.17)	(30.91)	(22.65)	(29.30)	(23.93)	(24.13)
ΔMW Next Year	16.33	22.21	14.63	19.63	37.44*	4.19
	(18.06)	(26.94)	(19.78)	(29.03)	(22.40)	(21.26)
Minimum Wage This Year	69.45***	85.04***	71.25***	78.27***	77.39***	46.93***
	(8.43)	(10.52)	(8.53)	(13.85)	(10.07)	(11.40)
Panel 2: Effects on Housing Rents						
ΔMW in Two Years	1.15	5.91	0.92	5.14	-2.26	-3.11
	(3.08)	(4.41)	(3.11)	(4.09)	(3.71)	(3.36)
ΔMW Next Year	3.33	7.49**	2.83	-0.86	-4.15	5.92*
	(2.83)	(3.74)	(3.02)	(4.48)	(3.56)	(3.17)
Minimum Wage This Year	20.43***	22.28***	21.16***	15.96***	21.11***	12.73***
	(1.60)	(1.79)	(1.58)	(3.02)	(2.49)	(1.88)
Observations	383,107	225,128	359,925	318,861	350,891	314,115

Notes: The baseline sample is our main analytic sample. The sample in column (b) excludes households reporting any food stamp receipt over the previous year. The sample in column (c) excludes any households reporting receipt of tranfer income. The samples in columns (d)-(f) report results when some subset of the geographic sample is excluded. The specifications are otherwise those described in Table 3. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table A4: Event Study Estimates of Minimum Wage Hikes on Moving Rates

				Move Last 12 Months		
	Move Last 12 Months	**Move Last 24 Months**	**Move Last 60 Months**	**Move within PUMA**	**Move within State but across PUMA**	**Move across States**
	(a)	(b)	(c)	(d)	(e)	(f)
Panel 1: Impact on All Households						
ΔMW Next Year	-0.005*	-0.009***	-0.015***	-0.007***	0.001	0.003***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)
ΔMW This Year	0.000	0.000	-0.007*	0.000	0.001	-0.002*
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)	(0.001)
ΔMW Last Year	0.005	0.001	-0.003	0.004	0.001	0.003**
	(0.004)	(0.004)	(0.005)	(0.003)	(0.001)	(0.001)
Minimum Wage Two Years Ago	-0.002	-0.006**	-0.012***	0.001	-0.001**	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Average	0.27	0.37	0.69	0.16	0.03	0.02
Panel 2: Impact on Younger Households						
ΔMW Next Year	-0.013***	-0.017***	-0.016***	-0.015***	0.000	0.004**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.002)	(0.002)
ΔMW This Year	0.011*	0.008	-0.003	0.005	0.004*	-0.004**
	(0.006)	(0.006)	(0.006)	(0.005)	(0.002)	(0.002)
ΔMW Last Year	0.001	-0.004	-0.008	0.001	0.000	0.005**
	(0.006)	(0.006)	(0.006)	(0.005)	(0.002)	(0.002)
Minimum Wage Two Years Ago	-0.007*	-0.010***	-0.012***	-0.001	-0.003**	0.000
	(0.004)	(0.004)	(0.004)	(0.003)	(0.001)	(0.001)
Average	0.34	0.47	0.80	0.20	0.04	0.03

Notes: The move variables used in this analysis come from two questions in the ACS: "mv" and "mig." They are described in detail in the notes to Table 6. The household level analysis also includes PUMA and year fixed effects as well as basic demographic controls for the household head, which are described in detail in Table 3. $N = 383,101$ in Panel 1 when all households in the analytic sample are included. $N = 176,858$ in Panel 2 when the sample is limited to household heads under age 40 in the analytic sample. Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Table A5: Heterogeneity in Effect of Minimum Wage Hikes on Housing Rents by PUMA Characteristics

	Effects on Rent			**Effects on Earnings**	
	Baseline	**Linear Interaction Terms**	**Quadratic Interaction Terms**	**Baseline**	**Linear Interaction Terms**
	(a)	(b)	(c)	(d)	(e)
ΔMW in Two Years	0.19	9.38	-27.95	28.26	-15.65
	(3.42)	(12.29)	(54.31)	(23.46)	(81.37)
ΔMW Next Year	2.99	2.56	-74.21*	6.95	62.55
	(2.98)	(9.85)	(40.46)	(19.38)	(63.34)
MW This Year	17.14***	46.36***	103.91***	68.25***	99.06***
	(1.76)	(5.21)	(20.03)	(9.33)	(24.30)
ΔMW in Two Years X Housing Supply Elast		-13.64	109.21		83.79
		(20.87)	(167.17)		(140.25)
ΔMW Next Year X Housing Supply Elast		-0.15	246.30*		-103.37
		(16.67)	(125.65)		(110.54)
MW This Year X Housing Supply Elast		-56.80***	-244.68***		-61.00
		(8.98)	(61.58)		(42.82)
ΔMW in Two Years X Housing Supply Elast Sq			-97.13		
			(118.15)		
ΔMW Next Year X Housing Supply Elast Sq			-184.09**		
			(89.72)		
MW This Year X Housing Supply Elast Sq			140.64***		
			(42.85)		

Notes: The housing supply elasticity comes from Baum-Snow and Han (2024). The analytic sample for this analysis is our main analytic sample but we drop areas where we do not have measures of the housing supply elasticities. The specifications are otherwise those described in Table 3. Standard errors are clustered at the PUMA level. $N = 237,996$ for all specifications. *p<0.1; **p<0.05; and *** p<0.01.

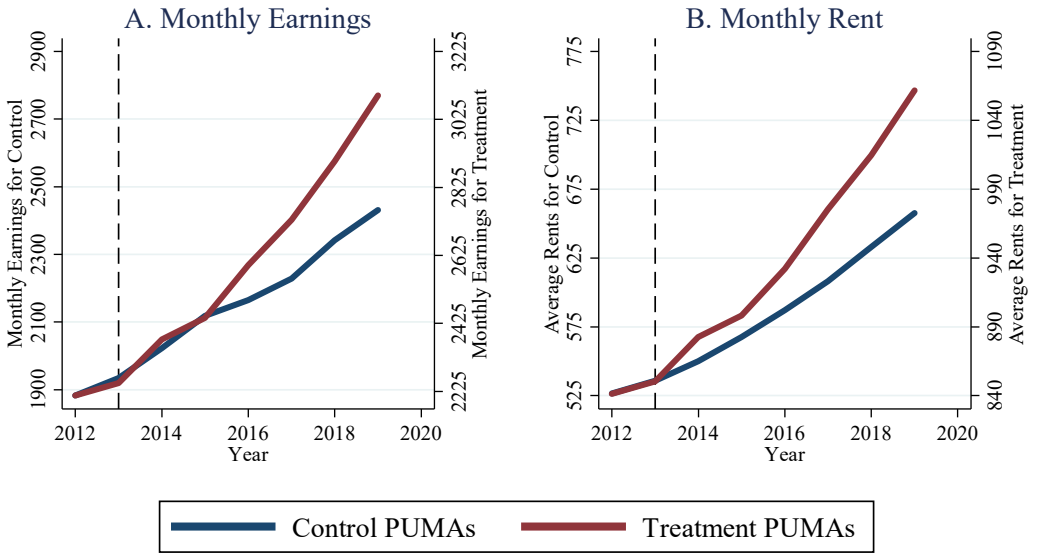
Table A6: Effects of Minimum Wage Hikes on Housing Consumption II

	Reside in Mobile Home	**Full Kitchen & Bathroom**	**Electricity Bill Included in Rent**	**Gas Bill Included in Rent**	**Water Bill Included in Rent**
	(a)	(b)	(c)	(d)	(e)
Panel 1: Full Sample					
ΔMW in Two Years	0.001	0.000	0.012***	0.018***	-0.002
	(0.001)	(0.001)	(0.003)	(0.005)	(0.008)
ΔMW Next Year	0.001	-0.001	0.000	0.001	0.011**
	(0.001)	(0.001)	(0.002)	(0.003)	(0.005)
Minimum Wage This Year	0.000	0.000	-0.006***	-0.017***	-0.027***
	(0.001)	0.000	(0.001)	(0.002)	(0.004)
Sample Mean	0.08	0.99	0.06	0.06	0.30
Panel 2: Movers Only					
ΔMW in Two Years	0.002	-0.001	0.007*	0.010**	0.000
	(0.003)	(0.002)	(0.004)	(0.005)	(0.009)
ΔMW Next Year	0.003	-0.003	0.003	-0.003	0.006
	(0.003)	(0.002)	(0.004)	(0.005)	(0.007)
Minimum Wage This Year	0.000	0.000	-0.008***	-0.018***	-0.031***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)
Sample Mean	0.08	0.98	0.07	0.06	0.31

Notes: The analytic sample for each of the specifications is identical to the sample described in Table 3. Panel 1 uses all observations ($N = 383,107$) while Panel 2 is limited to households that moved sometime in the past two years ($N = 141,291$). Standard errors are clustered at the PUMA level. *p<0.1; **p<0.05; and *** p<0.01.

Figure A1: Impact of Minimum Wages on Earnings and Rent

2014 Hike PUMAs vs. Never Hike PUMAs



Note: This figure presents the average earnings and average rent of low-income households, comparing households in states that first increased their minimum wage in 2014 with states that did not increase their minimum wage at all over the sample period, 2012-2019. Treatment sample includes all PUMAs that first increased minimum wage in 2014. The vertical line in each figure is in 2013, the year before the first hike.