Housing Demand & Affordability for Low-Wage Households: Evidence from Minimum Wage Changes

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Abstract

Rent-to-income ratios have risen over the past two decades with large increases at the bottom of the income distribution, prompting concern about a housing affordability crisis. This paper uses minimum wage changes as a natural experiment to study the relationship between housing demand & policies affecting low-wage households. If their housing demand is relatively inelastic, an increase in income will causally decrease rent-to-income ratios. The results suggest a 10% increase in minimum wages increases income for affected households by 1.9%, increases housing consumption by 0.5%, and decreases rent-to-income ratios by 1.4%. These estimates suggest that housing demand is fairly income inelastic, and preferences over housing demand are non-homothetic. In a modeling exercise, this paper suggests that homothetic models may not match housing demand behavior and may underestimate welfare gains to low-wage households.

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1. Introduction

Over the last several decades, wages for the bottom half of the distribution have stagnated or fallen (see e.g. Autor, 2019). Rents and house prices have risen. As a result, rent-to-income ratios have risen in nearly every U.S. city, and have risen most for low-income people (see e.g. Albouy, Ehrlich, & Liu, 2016). These changes—often referred to in relation to the housing affordability crisis—vary across the income distribution and over time, as shown in Figure 1. The figure plots average rent-to-income ratios for renter households within each percentile in the national household income distribution for each Census year from 1980 to 2016 (Ruggles et al. 2019). For the highest income households, the housing share of income is around 10%, and has been constant or declined. For low-income households, rent-to-income ratios have increased starkly over the past 35 years—increasing from 50% to 60% for households around the 15th income percentile.

In news media and policy advocacy, the housing affordability crisis is often connected to local minimum wages. The National Low Income Housing Coalition releases an annual report, written about in *The Washington Post* among other newspapers, on the number of work hours needed to afford an apartment while working a minimum wage job in each state or county.[2] Based on this analysis and others like it, one politician notes "[t]here are 3,000+ counties in the United States. A minimum wage worker can afford to rent a one-bedroom home in just 28 of them. That is a national crisis."[3] While some advocates and politicians advance this approach to descriptive analysis, at least one economist and political commentator has argued that connecting minimum wages and median rents to discuss housing affordability is a "pointless comparison."[4] There is limited empirical analysis at the nexus of minimum wages and housing affordability. Agarwal, Ambrose & Diop (2019) find that increasing the minimum wage reduces rental housing default, Yamagishi (2019) finds that minimum wages increase rents, and Tidemann (2018) finds that minimum wages decrease rents.

This paper uses changes in minimum wage laws as a natural experiment in order to study the interaction between the labor market and the housing market, while providing insights into how minimum wages are

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[2] NLIHC, "Out of Reach 2019," available at https://reports.nlihc.org/oor. See e.g. https://www.washingtonpost.com/news/wonk/wp/2015/06/09/what-youd-need-to-earn-in-every-state-to-afford-a-decent-apartment/; https://www.washingtonpost.com/news/local/wp/2018/06/20/d-c-residents-must-earn-34-48-an-hour-to-afford-a-two-bedroom-home-report-says/; https://www.washingtonpost.com/news/wonk/wp/2017/06/08/heres-how-much-you-would-need-to-make-to-afford-housing-in-your-state/; https://www.washingtonpost.com/news/wonk/wp/2018/06/13/a-minimum-wage-worker-cant-afford-a-2-bedroom-apartment-anywhere-in-the-u-s/.

[3] See https://twitter.com/BernieSanders/status/1174344065317580800?s=20.

[4] See https://www.forbes.com/sites/jeffreydorfman/2018/07/27/the-pointless-comparison-of-minimum-wage-and-median-rent/#143506b477f5.

related to housing affordability. The motivating idea behind this approach is that active labor market policies can affect household welfare through their interaction with the housing market. For working households, minimum wages appear to significantly decrease rent-to-income ratios for low-wage earners, while they have, on average, no statistically significant effect on higher-wage households. There is some evidence that rental prices increase in markets where housing supply is inelastic.

To estimate how minimum wages affect housing demand, I use a triple difference strategy exploiting variation across time, and within and across states. The within-state variation compares households whose head works for a wage near the minimum wage versus households whose head works for a wage too high to be plausibly affected by the minimum wage (consistent with Dube, 2019; Cengiz et al. 2019; Autor, Manning & Smith, 2016; Tidemann, 2018). These comparisons are within-occupation (i.e. comparing near-minimum wage cashiers to cashiers making 125%+ of the minimum wage), and are robust to controlling for household and individual characteristics. The data used is annual repeated cross-section Census data from the American Community Survey for 2005-2017 (Ruggles et al. 2019).

A problem in identifying the effects of minimum wages on the housing market is that minimum wage laws may make housing more affordable for those workers who receive a raise, but it may also cause the local price level to rise either in response to a general equilibrium increase in demand or if landlords are able to price discriminate. The empirical approach in this paper attempts to disentangle the income effects on housing demand from the 'general equilibrium' price effects on rental housing. The triple difference strategy is important because minimum wages may both raise the demand for housing of individual households and raise the prices that households must pay for any given unit in a local housing market. In a standard difference-in-difference regression of log rent on log minimum wage, these two effects will be combined.

In response to a 10% minimum wage increase, incomes increase by 1.9%, housing consumption increases 0.5%, and rent-to-income ratios fall by 1.4% for households at the bottom of the wage distribution. For this affected group of households, average rent-to-income ratios are 47%, so the average minimum wage increase causes a decline of around 0.63 percentage points in their rent-to-income ratios. Results are consistent when considering a subsample of likely affected renters whose households are headed by workers in cashier, clerk, and salesperson occupations. By comparing similar workers, this should provide some additional evidence that the results are not driven solely by differing behavior or location choices of low-wage versus higher-wage households.

The results suggest that housing is a normal good (demand rises with income), but demand is fairly inelastic and non-homothetic. In other words, a one percent increase in income results in a less than one percent increase in housing consumption (measured by changes in rents), and an increase in income appears to causally decrease rent-to-income ratios. A further section analyzes heterogeneity in the rental price effect. Measures of housing supply elasticity are interacted with the minimum wage effect. This interaction shows the extent to which minimum wage effects are heterogeneous in a way predicted by theory—on average, minimum wages increase rental prices in supply inelastic cities, while they may decrease rental prices in very elastic cities.

Dynamic specifications suggest that household incomes and rental prices adjust within 1-2 years, but housing demand adjusts more slowly over 3-4 years after the minimum wage change. Because incomes rise almost immediately after the minimum wage change and demand among low-wage households appears to adjust more slowly, rent-to-income ratios fall with income then stabilize at a lower level for low-wage households. Rent-to-income ratios for higher-wage households are not significantly affected by minimum wages. The lead and lag terms in these dynamic specifications are fairly noisy, so substantial work remains to be done to parse the consumption versus price effects of minimum wage changes. These dynamic specifications do not use log state minimum wage *levels* as the independent variable. Instead, they include distributed leads and lags of *differenced* log minimum wages. This specification has two advantages: it only uses variation in minimum wage differences rather than levels, which mitigates the concern that minimum wage levels are systematically correlated with unobserved differences between states; and it provides some insight into the timing of changes in incomes, rent, and rent-to-income ratios. The specification choice decision and robustness to different specifications is a point of contentious debate in the literature on the employment effects of minimum wages as recently noted in Cengiz et al. (2019) and Neumark (2019).

The welfare implications of these empirical estimates are then discussed in the context of a consumption model, focusing on the suitability of the homotheticity assumption. Homotheticity, meaning that the utility function is a monotonic transformation of a homogeneous function, is convenient for modeling consumers—utility functions are homogeneous of degree one, demand is unit elastic, and expenditure shares are fixed, invariant to changes in income. However, empirically, housing demand appears to be inelastic and expenditure shares vary strongly by income (Albouy et al., 2016). Thus, to the extent that expenditure shares causally decline with income, homothetic or Cobb-Douglas models of utility may not capture household behavior well.

An important contribution of this paper is to estimate the elasticity of the housing expenditure share with respect to income. The theory section of the paper includes a brief discussion of the importance of this elasticity and the merits of different utility representations. The size of the change in welfare depends on underlying preferences—especially the functional form of utility. Welfare estimates vary between commonly-used models which assume housing demand is homothetic versus in non-homothetic models. The theory section considers one form of non-homotheticity, namely that households must pay for some basic, subsistence level of housing in order to participate in the local housing market. A straightforward approach to modeling subsistence housing is using Stone-Geary preferences, which provide intuitive and qualitatively similar comparative statics relative to the empirical results.

To measure the welfare effect of an income change, the theory section develops a sufficient statistics-style formula which I use to show that welfare estimates using only the percent change in consumption or the percent change in income can severely underestimate the changes in welfare by 50-250%. As household income increases, housing expenditure shares causally decline, and income gains provide disproportionately large welfare gains for low-wage households who are close to the subsistence housing level.

In order to speak to the longer-term relationship between minimum wages and housing affordability, decennial Census data is used (analogous to Figure 1) to regress decade-over-decade differences in log rent-to-income ratios on log state minimum wages. This decade-over-decade analysis is limited because there is very little state-level identifying variation prior to the 1990s (nearly every state was bound by the federal minimum wage). Qualitatively the results of this long-run approach are similar to those using more recent annual ACS data. I conclude by noting that this correlational evidence suggest that raising minimum wages may contribute to lower rent-to-income ratios for the bottom decile of household income.

In a back-of-the-envelope calculation, increasing the federal minimum wage from $7.25 to $10, or about 35%, would decrease average rent-to-income ratios approximately 1.5 percentage points for the bottom quarter of renter households. This is significant, but relatively small compared to the increase in average rent-to-income ratios over the past 25 years. Figure 1 provides descriptive evidence that for households at the bottom of the income distribution, rent-to-income ratios have increased by ten percentage points or more. Thus, a long-term decline in minimum wages are likely only directly responsible for a noticeable but small portion of the housing affordability crisis—an heuristic upper bound suggests that it can only explain 15% of the long-term increase in housing cost burdens.

Contribution to the Literature

This paper contributes to the literature on (1) behavior of housing demand, and (2) the effects of minimum wages. This paper provides new evidence to the literature on the behavior of housing demand and local housing market responses to policy changes, including housing demand elasticities with respect to income and income elasticities of the housing expenditure share. For example, several papers used HUD's Housing Allowance Demand Experiment and Housing Assistance Supply Experiment to estimate housing demand responses as summarized in Mayo (1981). The results in this paper confirm that housing demand is inelastic, especially for lower-income households. It is also related to a continued debate in the literature on the appropriate functional form for preferences for housing demand. Davis & Ortalo-Magné (2011) argue for using Cobb-Douglas preferences over housing. In contrast, Albouy, Ehrlich, & Liu (2016) presents a model with non-homothetic constant elasticity of substitution preferences over housing. These functional form assumptions may be important in the context of spatial equilibrium and structural models (e.g. Moretti, 2013; Diamond, 2016; Couture, Gaubert, Handbury & Hurst, 2019), which disagree on whether it is relevant to incorporate non-homotheticities explicitly in modeling preferences. This paper provides empirical evidence for the class of non-homothetic models—income increases appear to causally decrease housing expenditure shares.

I provide new estimates of the consumption effects of minimum wages. This is in a similar spirit to the work on consumption in Aaronson, Agarwal & French (2012), who incorporate housing by regressing mortgage debt on the minimum wage. The results here add plausible estimates of income gains and housing stability to this literature. Existing research estimates the effects of minimum wages on income, poverty, and employment (e.g. Card & Krueger, 2000; Autor, Manning & Smith, 2016; Dube, 2018; Cengiz, Dube, Lindner & Zipperer, 2019). Recently, many papers have also related minimum wages to health, crime, education, and other outcomes. My estimates may motivate additional work on causal channels of minimum wage effects—housing stability could affect health, education, crime, and other economically important behavior. Pilkauskas & Michelmore (2019) also investigate how labor market policy affects housing stability, showing that the EITC improves various measures of housing stability for single mothers.

This paper contributes to a growing literature on parsing the effect of minimum wages on prices and consumption, e.g. Wadsworth (2010); Aaronson et al. (2012), and Renkin et al. (2019). Several papers have also related spatial equilibrium models to minimum wages focusing on immigration, (Cadena, 2016; Monras, 2018) and commuting (Perez Perez, 2018; Zhang, 2019). At the intersection of research on minimum wage effects and housing market prices, Yamagishi (2019) and Tidemann (2018) also measure the effect of minimum wages on rental prices. Their main empirical results are contradictory—using an event study approach, Tidemann finds minimum wages reduce rents, while Yamagishi finds they increase rents using a distributed lag approach. Agarwal, Ambrose & Diop (2019) also estimate the extent to which minimum wage increases lead to fewer rental housing defaults among tenants, and provide short-run estimates of within-property rental price increases from RentBureau, a selected sample of data from large property managers who reported rent histories to credit rating agencies. Agarwal, Ambrose & Diop find substantial rent increases, and declines in late rental payments in this selected sample of rent histories. Likely the first attempt to estimate how minimum wages pass through to rental prices is Appendix Graph 3 in the Seattle Minimum Wage Study Baseline Report (Vigdor et al., 2016) analyzing web-scraped Zillow rents in zip codes inside Seattle versus those just outside of Seattle after the city's minimum wage hikes. They find little or no change between rents just inside and outside the city.

While those papers focus on primarily rental prices, this paper directly addresses housing consumption and focuses on the empirical and theoretical implications of rent-to-income ratios in the context of minimum wage changes. Results in Table 5, explained in Section 5, finds evidence in favor of one theoretical prediction in Yamagishi (2019) and Tidemann (2018)—that the effect of minimum wages on rental prices may depend on local housing supply elasticities. By explicitly allowing for heterogeneity in minimum wage effects with respect to city-level supply elasticity, this evidence is consistent with the prediction that minimum wages increase rents in housing supply inelastic cities, while they may decrease, on average, in supply elastic cities.

Section 2 presents a theoretical framework for the housing demand effect of an income change. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 discusses the empirical results. Section 6 interprets the welfare implications using the theoretical framework and empirical results. Section 7 discusses the results and Section 8 concludes.

2. Theoretical Intuition

Under a straightforward model of housing demand, welfare gains from the minimum wage are large for households whose income increases. In percentage terms, the welfare gain can be much larger than the increase in consumption or in income. This is because of the nonhomotheticity of housing demand. The substantial declines in housing expenditure shares documented in the empirical results provide evidence in favor of, and a measure of the size of this effect.

This section discusses an intuitive model of housing demand building from a homothetic model to a non-homothetic model that captures the welfare effects for workers whose incomes increase in response to a minimum wage increase.

Consider a household of type i that makes a continuous decision about consumption of housing and a good, X_i. Housing is expressed as a linear function of the price level q, and housing services H_i, which the household consumes. This assumption is likely unrealistic, but yields an intuitive and calculable expression for welfare changes.

The prediction of a homothetic model is that housing expenditure shares do not change in response to a change in income. To build intuition, we can observe the constrained optimization of a homothetic Cobb-Douglas utility function $\max_{X_i,H_i} U(X_i, H_i) = {X_i}^{\delta}{H_i}^{1-\delta} \quad s.t. \quad m(i) = pX_i + qH_i$. Plugging the first-order conditions back into the budget constraint and solving for housing demand, H_i, and the housing expenditure share, s_{H_i}: $H_i = (1-\delta)\frac{m(i)}{q}$; $s_{H_i} = \frac{qH_i}{m(i)} = (1-\delta)$. This equation provides the straightforward intuition that an increase in household income causes an increase in housing demand, which does not appear to be consistent with the empirical results in this paper.

In this model, we would be able to measure welfare changes caused by minimum wage increases in terms of percent changes in housing consumption, if we assume the ratio of housing prices to prices of other goods is not affected by the minimum wage change:

$$V(p,q,H_i) = \left(\frac{\delta}{(1-\delta)}\frac{q}{p}\right)^{\delta} H_i \quad \rightarrow \quad \%\Delta V = \frac{V^{post} - V^{pre}}{V^{pre}} = \frac{{H_i}^{post} - {H_i}^{pre}}{{H_i}^{pre}} = \%\Delta H_i$$

If the price ratio is affected by the minimum wage change, then we can interpret the direction of the effect on welfare relative to the case where prices are unchanged:

$$\%\Delta V = \frac{V^{post}}{V^{pre}} - 1 = \frac{\left(\frac{q^{post}}{p^{post}}\right)^{\delta} {H_i}^{post}}{\left(\frac{q^{pre}}{p^{pre}}\right)^{\delta} {H_i}^{pre}} - 1 \gtreqless \%\Delta H_i \iff \left(\frac{q^{post}}{q^{pre}}\right)^{\delta} \gtreqless \left(\frac{p^{post}}{p^{pre}}\right)^{\delta}$$

In the case that the rental housing price level increases more than non-housing price level, measuring welfare solely through its effect on housing consumption underestimates the welfare gain and vice versa. A well-developed literature provides estimates of non-housing price-level effects of minimum wages. Many find small increases in inflation, similar in scale to the average rental housing price-level effects estimated in the difference-in-difference regression coefficients. This provides some reassurance that the relative price-level change may be fairly small.[5]

To make the model of utility more realistic, we can introduce a modeling change by adding a non-homothetic Stone-Geary term, so utility is expressed ${X_i}^{\delta}(H_i - \theta)^{1-\delta}$. This may better reflect that households must pay for some minimal subsistence level of shelter. Notice that consumption of housing services is continuous, rather than discretized. Conceptually, I view housing services as a combination of location amenities and structure characteristics. Allowing for a continuous choice over housing services, while unrealistic, allows for a conveniently-estimable expression of welfare. We can express Stone-Geary housing demand and the housing expenditure share as: $H_i = (1-\delta)\frac{m(i)}{q} + \delta\theta$; $s_{H_i} = \frac{qH_i}{m(i)} = (1-\delta) + \delta\frac{q\theta}{m(i)}$.

The housing expenditure share is now decreasing smoothly in income, consistent with the descriptive graph in Figure 1. Another implication of this simple model is that, holding the housing price level fixed, as $m(i) \to \infty$, $s_{H_i} \to (1-\delta)$. Consider the welfare change in terms of housing consumption (again, for exposition, suppose that the price ratio does not change). Assuming that $\theta > 0$, compare the Stone-Geary expression to the percent change in housing consumption used in the homothetic model, which we can measure empirically:

$$\%\Delta V = \frac{V^{post} - V^{pre}}{V^{pre}} = \frac{\left({H_i}^{post} - \theta\right) - ({H_i}^{pre} - \theta)}{({H_i}^{pre} - \theta)} = \frac{{H_i}^{post} - {H_i}^{pre}}{({H_i}^{pre} - \theta)} \geq \frac{{H_i}^{post} - {H_i}^{pre}}{{H_i}^{pre}}$$

This implies that if we are correctly empirically estimating the percent change in housing demand and the Stone-Geary term is positive, then the percent change in housing consumption is a lower bound for the percent change in welfare.[6] Unfortunately, housing services and the Stone-Geary term are not money-metric.

[5] Cooper, Luengo-Prado & Parker (2019) find inflation increases of 0.25-0.38% on all goods (0.53% on food away from home) with heterogeneous effects by the share of low-wage workers; Leung (2018) finds grocery store prices increase 0.6-0.8%; MacDonald & Nilsson (2016) find food away from home prices increase 0.25-0.39%; Aaronson, French & MacDonald (2008) find food away from home prices increase by 0.7% and the effect varies by establishment type; Wadsworth (2010) estimates retail prices rose by 0.4-0.7% using UK minimum wage changes; Ganapati & Weaver (2017) estimate non-significant grocery price effects between -0.89% and 0.29%; and Renkin, Montialoux & Siegenthaler (2019) find grocery prices increase 0.2% and argue the price increases erode the income gains of affected households by 3-12%, though their welfare calculations appear to be based on a homothetic linear expenditure system.

[6] For intuition, consider the case where we can map housing services directly to rents: ${H_i}^{post} = 1200$, ${H_i}^{pre} = 1000$, and $\theta = 500$. A homothetic model would imply that welfare increases by 20% ((1200-1000)/(1000)=1/5). In contrast, the Stone-Geary model implies that welfare increases by 40% ((1200-1000)/(1000-500)=2/5).

In order to map theory into calculable welfare estimates, the theoretical welfare change from increasing household income can be expressed in terms of observable and estimable parameters. The following expressions use the above housing demand functions, and assume the rental price level, q, does not change (relative to non-housing prices, p). The utility change is:

$$\frac{1}{\%\Delta V}=\frac{H_i^{pre}}{H_i^{post}-H_i^{pre}}-\frac{\theta}{H_i^{post}-H_i^{pre}}=\frac{1}{\%\Delta H}-\frac{q\theta}{(1-\delta)}\frac{1}{\Delta m}$$

This provides a measure of the welfare change in terms of the percent change in housing consumption and the change in income, but because the Stone-Geary term is a theoretical construct, we still cannot calculate the change in welfare. Notice that $q\theta$ is an important term in the equation for the housing expenditure share. Taking the first derivative of the housing expenditure share with respect to income: $\frac{\partial s_{H_i}}{\partial m(i)}= -\delta\frac{q\theta}{m(i)^2}$.

Using this derivative as a first-order approximation for how a change in income affects the housing share, we can express the percent change in the housing expenditure share relative to the percent change in income as an elasticity:

$$\varepsilon_{s_H,m}=\frac{\Delta s_{H_i}}{\Delta m(i)}\frac{m(i)}{s_{H_i}}= -\delta\frac{q\theta}{m(i)^2}\frac{m(i)}{s_{H_i}}=-\delta\frac{q\theta}{qH_i^{pre}}\Leftrightarrow q\theta=-\varepsilon_{s_H,m}\frac{qH_i^{pre}}{\delta}$$

Now we can rewrite the change in utility in terms of the percent change in housing consumption ($\%\Delta H$), the average pre-minimum wage-change rent (qH_i^{pre}), the estimated percent change in housing expenditure share with respect to income ($\varepsilon_{s_H,m}$), and the average change in income (Δm). There is one unknown preference parameter, $\delta\in[0,1]$.

$$\frac{1}{\%\Delta V}=\frac{1}{\%\Delta H}+\frac{\varepsilon_{s_H,m}(qH_i^{pre})}{\delta(1-\delta)}\frac{1}{\Delta m} \qquad (\mathbf{1})$$

According to my interpretation of the estimates above, the coefficient on log minimum wage multiplied by the binary variable for below 125% of the minimum wage (i.e. below the 25th wage percentile) gives a measure of the causal effect of the minimum wage on household income, rental housing consumption, and housing expenditure shares.

Section 6 returns to this calculation after presenting the results. Equation (1) is calculated using:

(1) $\%\Delta H$: the percent change in housing consumption from a 10% minimum wage increase (10 times the coefficient on log minimum wage interacted with the binary variable for affected workers);

(2) qH_i^{pre}: the average rent for the group of interest;

(3) Δm: the average change in monthly income calculated as the average income multiplied by 1/10 of the coefficient on log minimum wage-binary variable interaction in the log household income regression;

(4) $\varepsilon_{s_H,m}$: the income elasticity of the housing expenditure share (the coefficient on log minimum wage-binary variable interaction in the log rent-to-income ratio regression, divided by the coefficient on minimum wage-binary variable interaction in the log household income regression).

3. Data

The primary source of data is the American Community Survey (ACS) from IPUMS (Ruggles et al. 2019). ACS is useful in this context because it is among the only datasets that includes data on both individual household labor market outcomes and housing consumption decisions. Annual data that includes state geographic identifiers cover the 2005-2017 time period. The primary variables used are income (total household and personal wage and salary income), tenure choice (renting or owning), monthly rent or owner housing costs, and demographic characteristics. The main subject of this paper is the behavior of rental housing, so most analyses are subset to only renters. Each table in the main results includes summary statistics for the dependent variable in the bottom rows of the table. Table 1 (columns 2, 4, and 6) shows that renters in the bottom 25% of the wage distribution spend an average of \$688 on rent, make \$1,694 per month in household income, and have rent-to-income ratios of 40.6%. Households above the 25% wage percentile spend an average of \$872 on rent, make \$4,081 per month, and have rent-to-income ratios of 21.4%.

There are two limitations to this data worth mentioning here. First, the repeated cross-section structure of Census data makes it difficult to address some dynamics in the housing market. It is not possible to observe individuals as their income grows within or across jobs, and their mobility across homes and neighborhoods is unobserved. This unobserved variation makes it difficult to provide detail about what type of housing services individual households are consuming—in other words, low-wage workers may consume more housing services by moving to better neighborhoods or higher quality structures (or may improve the structures they currently live in) in ways that cannot be observed in the public-use Census data. To infer changes in consumption of housing services, this paper observes changes in gross rent for households whose incomes are affected by the minimum wage relative to households whose incomes are not affected by the minimum wage, but who live in the same area and are observably similar. Second, the timing of the Census data collection makes it difficult to learn much about the timing of minimum wage effects. The collection of income and labor market data asks respondents about their annual income from the prior year, so it is unclear when exactly we should expect to observe minimum wage effects. Godøy & Reich (2019) deal with this timing issue by connecting each Census year to the prior year minimum wage change. There is a related problem with learning about housing consumption & prices, because it is unclear when rental prices and changes in consumption should occur. Agarwal, Ambrose & Diop (2019) use new rental contracts from a group of landlords in 6-month windows around minimum wage changes to argue that the adjustment occurs fairly quickly in their sample. This paper does not offset the Census data and includes the population of renters, self-reporting their rent to Census. The Appendix figures include dynamic specifications showing all of the 3 leads and 4 lags to provide some evidence about timing, but it is difficult to provide better measures of the dynamics of these effects.

Data on federal and state minimum wage changes comes from Neumark (2019), Zipperer & Vaghul (2019), and data on 2018 and 2019 law changes from U.S. Department of Labor and National Council of State Legislatures. The levels and changes in minimum wages over time and across states is displayed in Figure 2. Minimum wages change in every state over the analysis time period (2000-2017), though many changes are due to the federal minimum wage change that occurred over 2007-2009. The average change is approximately 11%. Most minimum wage changes occur in several consecutive years, and between 2010 and 2019 more than 20 states have passed legislation either indexing minimum wages to some measure of inflation or cost of living, or have passed legislation setting a multi-year schedule of minimum wage increases. This provides relatively rich state-level variation in minimum wages.

4. Empirical Strategy

The effect of minimum wages on housing demand is identified using a triple-difference strategy by regressing log outcomes on log minimum wages to identify a percent change in income or rents with respect to a percent change in minimum wages. The three outcomes of interest are log contract rents, log household incomes, and log gross rent-to-household income ratios (RTIR). Changes in log rents provide a measure of the percent change in housing consumption in response to a percent change in income. Rent-to-income ratios are the closest approximation available to the housing expenditure share, which is the theoretical object of interest.

The three differences used in the triple difference analysis are: (1) between states that raise the minimum wage and those that do not; (2) within states that raise the minimum wage before and after the policy change; and (3) between workers whose wages are above the level affected by the minimum wage change and workers below the level affected by the minimum wage. The regressions allow for spillover effects from the minimum wage for workers who are, on average, up to 125% of the local minimum wage, consistent with results in Cengiz, Dube, Lindner & Zipperer (2019) and Autor, Manning & Smith (2016).

The workers affected by the minimum wage change are identified by imputing hourly wages, then finding the percentile in the wage distribution above which workers should no longer be affected by the minimum wage change. Wages are imputed using ACS variables on salary and wage earnings last year, weeks worked last year, and usual hours worked each week. Individuals' nominal wages are divided by the nominal state minimum wage.[7] Then, within each state-year cell, I find the point in the wage distribution that corresponds to 100% of the minimum wage—i.e. the percentile in the average state wage distribution where workers make below 100% of the minimum wage. Workers affected by the minimum wage are classified with a binary indicator variable for households with wages below the percentile corresponding to 100% of the minimum wage for the average state-year cell: $\mathbf{1}[\text{wage}_i < aff_{s,t}]$. For private sector wage and salary workers who are renters, that percentile is the 16th percentile of the average state wage distribution for those below 100% of the minimum wage.

To verify that these results are not spurious changes unrelated to the minimum wage, additional specifications raise the bar for identifying affected workers. Instead of 100% of the minimum wage, these specifications find the percentile in the average state-year wage distribution corresponding to 125% of the minimum wage. Raising this bar should attenuate triple difference coefficient, β, in Equation 2. The 25th wage percentile corresponds to 125% of the minimum wage in the average state-year cell. In addition, I verify in Appendix Table 1 that the density of observations below the 'affected' threshold is not changing in some way correlated with the minimum wage; and in Appendix Table 2, I collapsed the household-level data to the state-by-year level and verify that the size of the total population of renter households is not changing systematically with the minimum wage (regressing log population on the log minimum wage).

The main regression equation can be expressed as:

$$\ln(y_{i,s,t}) = \theta_s * t + \theta_{rt} + \varphi \ln(MW_{s,t}) + \gamma\, \mathbf{1}[\text{wage}_i < aff_{s,t}] + \beta \ln(MW_{s,t}) * \mathbf{1}[\text{wage}_i < aff_{s,t}] + \varepsilon_{i,s,t} \quad \mathbf{(2)}$$

[7] Note that the wage imputation is very conservative in that it includes all categories of full- and part-time workers, and will likely result in the binary "affected" variable including workers not affected by minimum wage changes. Weeks are reported in categories (in variable "wkswork2"). The values assigned to impute wages are: 51 weeks for wkswork2=6; 48.5 weeks for wkswork2=5; 43.5 weeks for wkswork2=4; 33 weeks for wkswork2=3; 21 weeks for wkswork2=2; and 6.5 weeks for wkswork2=1. Weeks worked is then multiplied by usual weekly hours worked over the last year (uhrswork).

The coefficient on the minimum wage variable alone, φ, should provide a measure of the average effect of minimum wages on prices and other common economic factors that affect all households in the sample. The coefficient on the interaction of minimum wages with the indicator, β, should provide a measure of the average response of households who are most intensively affected by the minimum wage change. Additional controls include state-year linear trends and fixed effects by year-Census division, occupation-year and state-occupation fixed effects. This helps ensure the regression is comparing workers within location and occupation, which may be important as explained below. Standard errors are clustered at the state level, which is the level of identifying variation.

There are at least two potential sources of bias related to using this strategy. First, wages are poorly measured in the Census. Workers only report their earnings from the prior year, and only include categorical variables for usual hours worked. This will introduce measurement error, creating attenuation bias—the coefficient of interest, β, will be biased toward zero. To begin deal with this, the sample is subset to households who (1) have a household head employed in a private sector wage- or salary-earning occupation; and (2) non-zero/non-negative household incomes, gross rents, and RTIR. In addition, because more reliable data sources show that some occupations have many minimum wage workers, while others have none, the 2010-vintage occupation codes in the ACS are used. By including year-occupation and state-occupation, the regressions are comparing changes in rents, incomes, and RTIR within occupations.

The second potential concern is that the repeated cross-section nature of the data means that there may be composition changes to the sample. Ideally, effects could be estimated using panel data, by including individual fixed effects. One of the goals of the third difference is to keep fixed the group of workers who are in the 'affected' group. As explained above, a binary variable is created to identify workers below the 16th (or 25th) percentile in the state wage distribution (of renters who are household heads), and comparing this portion of the distribution over time within and across states. To the extent that this binary 'affected' variable captures too many workers, it should bias the main estimates towards zero. In fact, this attenuation prediction is shown explicitly by shifting the percentile determining the affected variable from the 16th to 25th wage percentile.

As robustness checks, I show two other sets of results. First, I show the same regressions sub-setting the sample to only a few occupations that include high concentrations of minimum wage workers, cashiers, clerks, and salespersons (OCC=4720-4760). This should ensure that workers are similar—they work in similar occupations, the fixed effects ensure that we are comparing workers within region and time period. One concern about the housing consumption results is that regressing log rents on minimum wages may still be picking up the effect of the policy change on the local price level because the housing market is segmented. By comparing cashiers to cashiers, the results show that if a state increases the minimum wage from \$7 to \$8, cashiers who make \$8 per hour are changing their housing behavior while those who make \$10 are not. This does not guarantee that landlords are not discriminating between \$8 cashiers and \$10 cashiers, but it should provide some assurance of the reliability of the results.

Second, in Appendix Table 3, results are shown for a regression where indicators for each occupation group are interacted with the main affected-worker coefficient of interest. Along with the regression results, summary statistics for the share of workers whose imputed wages are below 100% of the minimum wage are displayed. The results show that occupations who should have few or no hourly-paid near-minimum wage workers (e.g. financial/business specialists or social scientists), have coefficients in the 'wrong' direction—minimum wages decrease their average incomes. This is likely due to changes in the occupation composition of workers below the binary 'affected' variable, but the composition issues should be muted in the main results because those occupations are relatively small and have very few low-wage workers in the first place.

The standard difference-in-difference identification strategy requires a common trends assumption. Robustness checks show pre-minimum wage change trends. To check the trends, each model is estimated with a set of distributed leads and lags on changes in the log minimum wage independent variable, including saturated interactions with each of the binary variables for those affected by the minimum wage. The regression is expressed:

$$\ln(y_{i,s,t}) = \theta_s * t + \theta_t + \sum_{k=-3}^{k=4} \varphi_k * [\ln(MW_{(t-k+1),s}) - \ln(MW_{(t-k),s})] + \gamma\, \mathbf{1}[\text{wage}_i < aff_{s,t}]$$
$$+ \sum_{k=-3}^{k=4} \beta_k * [\ln(MW_{(t-k+1),s}) - \ln(MW_{(t-k),s})] * \mathbf{1}[\text{wage}_i < aff_{s,t}] + \varepsilon_{i,s,t} \qquad \mathbf{(3)}$$

The advantage of this approach is that the regression is only exploiting variation in minimum wage changes around those changes, instead of using minimum wage levels which introduces cross-sectional variation in the level of minimum wages and variation outside of the window directly around minimum wage changes. Equation 3 captures the variation in outcomes 3 years prior to and 4 years after minimum wage changes. Following Allegretto, Dube, Reich & Zipperer (2017), the leading coefficients and the lagging coefficients are separately averaged, then their difference is reported to evaluate the cumulative effects of the minimum wage. The coefficients are graphed to show the possibility of correlated or non-parallel leading variation in Appendix Figures 1 and 2.

5. Housing Demand Results

The main hypothesis of this paper is that minimum wages affect housing demand. The first test this hypothesis are in Table 1. The common effect of the minimum wage on all private sector workers is near zero for log rents, household income, and rent-to-income ratios. The interaction effect of the minimum wage implies that households below the 16th percentile in the local wage distribution of renters increase their consumption of housing by 0.54% in response to a ten percent increase in the minimum wage (significant at the 95% level). Their incomes increase by 1.89% and their rent-to-income ratios decline by 1.35%. The 16th wage percentile corresponds to worker households who are, on average, at or below 100% of the state minimum wage. The table also shows results for households below the 25th percentile in the local renter wage distribution, corresponding to 125% of the state minimum wage. On average, the increase in log rents, log household income, and log RTIR is smaller after including the slightly higher-wage households.

In each table, summary statistics are reported at the bottom of the table for the outcome variable for each subgroup of interest. I interpret the differential changes in rents, incomes, and rent-to-income ratios (RTIR) measured by the coefficients on the interaction terms as causal effects of the minimum wage on consumption. Multiplying the interaction coefficients by the average of the outcome variables (in 2016 dollars), low-wage households increase their rental consumption by $4 per month, increase household incomes by $330 per year, and decrease rent-to-income ratios by 0.63 percentage points on an average RTIR of 47.0%.

The results from several different specifications in Appendix Table 4 are shown as a simple robustness check to Table 1. With each outcome variable stacked as a separate panel, Appendix Table 4 shows the main specification from Table 1, then simple difference-in-difference specifications with two-way fixed effects and with linear time trends and occupation controls. The takeaway from this simple check is that a difference-in-difference regression does not recover any significant effect from minimum wages on rents, incomes, or their ratio. Once the triple-difference is included there is an identifiable change in incomes and rent-to-income ratios. The final column includes household and individual level controls, which only increases the precision of the main results from Table 1 and has very little effect on the coefficients. The housing consumption effects from Table 1 (Column 1) are not evident in the specifications that drop occupation-state and occupation-year fixed effects, but they are statistically significant once controlling for occupation & household and individual level controls.

Table 2 shows results for households whose heads are cashiers, clerks, and salespersons. The results are broadly similar to Table 1, though the interaction effect on housing consumption where log rents are the outcome variable is only significantly different from zero at the 90% level. The interactions imply that a ten percent minimum wage increase affects housing consumption by increasing log rents by 1.2% ($7.74 per month), increasing household incomes by 2.6% ($388 per year), and decreasing RTIR by 1.39% (-0.71 pp.). In this specification, again the common effect of the minimum wage is not statistically significantly different from zero for any outcome.

The combination of Tables 1 and 2 provide evidence that minimum wages increase household incomes and decrease rent-to-income ratios. The interaction coefficients for log rents are noisy and not statistically significantly different from zero in some specifications. It is possible that this marginal significance reflects attenuation bias, but it could also reflect inelastic housing demand and slow adjustment of housing demand to changes in income. The possibility of slow adjustment is demonstrated by re-estimating these specifications in Equation 2, where distributed leads and lags are included on both the common minimum wage effect and interaction effect isolating low-wage workers affected by the policy change. The explanatory variables are differences of the minimum wage, which isolates the variation in the years leading up to and following the policy change (rather than cross-sectional variation in the level of minimum wages across states).

Table 3 summarizes the distributed leads and lags, and shows statistically significant effects of the minimum wage on housing consumption.[8] The coefficients on distributed leads and lags are noisy, so it is not clear which leads and lags to compare. This is a similar issue to that discussed in Allegretto, Dube, Reich & Zipperer (2017), which they address by averaging the leading and lagging coefficients and considering the main effect of policy as the difference between the average lags and average leads.

I find that the common effect of a ten percent minimum wage change raises rents by 0.32%, though this effect is not statistically significant. The interaction effect of the change on low-wage workers increases household incomes by 0.96%, increases housing consumption by 0.47% and decreases RTIR by 0.49%. These effects are similar to the average effects for households below 125% of the minimum wage in Table 1, though the income change is slightly smaller: 0.096 compared to 0.13. Table 4 shows the average dynamic response for cashiers, clerks, and salespersons (analogous to Table 2). The coefficients imply that a ten percent minimum wage increase causes a 1.97% increase in income, a 1.65% increase in housing consumption, and a 0.31% decline in rent-to-income ratios.

[8] Note that because three leads are included, this specification does not use data from the 2017 ACS, so the sample is smaller than Table 1 and covers 2005-2016.

The rental demand response in Tables 3 and 4 is statistically significant and large relative to the change in income. This may suggest that the rental demand response is gradual, consistent with renters readjusting their housing consumption over time as they move to new homes. To observe this, the coefficients on distributed leads and lags are graphed in Appendix Figure 1 (results for the cashiers, clerks, and salespersons subsample are shown in Appendix Figure 2). One way to interpret noisy leading coefficients is as non-parallel pre-event trends. I follow the approach used in Monras (2018) by running a linear regression on the pre-policy-change leading coefficients and projecting that linear trend onto the post-policy-change coefficients. These results are shown side-by-side with the raw coefficients in Appendix Figures 1 and 2. The graphical evidence suggests that the change in rents, income, and RTIR grow over 2 to 3 years following the minimum wage change, and the long-run effects are larger than the in static difference-in-difference regression.

Because of the concern about wage mismeasurement and composition changes, a robustness check in Appendix Table 3 interacts indicators for each of the 24 main occupation groups with the minimum wage and with the below-25th percentile binary variable classifying affected workers. This specification is identical to Table 1 except for the interaction effect with occupation group identifiers. The right-most columns of Appendix Table 3 show the number and percent of workers below the 25th percentile in the state renter wage distribution. The occupation group results are sorted by the share of workers below the 25th percentile in the local wage distribution (i.e. the proxy for those likely affected by the minimum wage change).

These results provide several insights. In occupation groups with very few low-wage workers (the top 10 rows), the effect of the minimum wage on household incomes is in the 'wrong' direction (i.e. negative). This is likely due to the mix of wage mismeasurement and composition effects. In more reliable datasets like the Current Population Survey, other descriptive work has shown that these occupation groups should have few or no sub-minimum wage workers.[9] On the other hand, for larger occupation groups where there are larger concentrations of low-wage workers, the triple difference approach appears to do a better job of identifying the effect of the minimum wage, similar to that in Tables 1-4—minimum wages increase household income, increase housing consumption shown by log rents, and decrease rent-to-income ratios.

In order to further investigate the rental price effects, Table 5 considers the hypothesis that minimum wages differentially affect rental price levels depending on the local housing supply elasticity. In inelastic cities, we might expect that rental prices will rise more in response to a change in minimum wages. Both Tidemann (2018) and Yamagishi (2019) suggest that this is a possibility, although neither explicitly measure

[9] See e.g. https://www.bls.gov/opub/reports/minimum-wage/2017/home.htm.

this heterogeneity. Columns 1, 3, and 5 show the main results analogous to Table 1, except that Table 5 only includes Census observations from regions with identified metropolitan statistical area (MSA) codes. Estimates of housing supply elasticity from Saiz (2010) are then merged (using a crosswalk of 1980 MSA codes to 2010 PUMAs to 2010 MSA codes). The regression then supplements Equation (2) by adding an interaction between log minimum wage and the Saiz supply elasticity. This empirical estimation is similar to the approach to minimum wage heterogeneity in Azar et al. (2019), which estimate heterogeneity in minimum wage employment effects according to the local labor market concentration prior to minimum wage increases.

The results of Table 5 suggest that minimum wages increase rents more in inelastic cities—the coefficient on ln(MW)*Saiz Elasticity is negative and significantly different from zero. The average housing supply elasticity in this dataset is 1.6, so, to interpret the interaction in the average city, one can multiply the coefficient, -0.059, by 1.6 and add to the common coefficient on log minimum wage. In the average city, minimum wages have a -0.014 elasticity with respect to the minimum wage. These results imply that the minimum wage-rental price elasticity is 0.046 in the most inelastic cities whose housing supply elasticity is approximately 0.6. This source of heterogeneity also appears to be important to changes in incomes for households higher in the wage distribution. This suggests that households are compensated for these higher rental housing prices, though this change is less than one-for-one because the Saiz interaction coefficient in the rent regression (-0.059) is larger in magnitude than in the income regression (-0.035). These results are also consistent with the possibility that minimum wages may cause declines in rental prices in some regions—Tidemann (2018) argues that this decline occurs in areas where minimum wage increases have a significant negative effect on jobs or employment opportunities.

6. Welfare Implications of an Increase in Income

Table 5 shows the inputs into the welfare calculation in the top rows. The welfare calculation is performed for several different values of δ. I compare the welfare change from the increase in housing consumption alone, as one would use to learn about the welfare gains in a homothetic model, to the welfare change in the non-homothetic Stone-Geary model. The range of plausible welfare gains from a homothetic model are between 0.25-1.65%. My calculations suggest that the Cobb-Douglas functional form may understate the welfare gains by more than 50-250%. Welfare gains in a non-homothetic model are in the range of 0.5-5.0%.

In Figure 3, I plot the welfare change along the possible values of δ. These graphs show that the welfare changes asymptote—as δ approaches the asymptote, welfare gains approach infinity; after the point at which they asymptote, they grow from negative infinity towards 0 on the outer possible values of δ (0 and 1). This analysis provides evidence about the *curvature* of the utility function, and the level of subsistence levels of housing services, θ. For households who are only able to consume near subsistence levels of housing—whose spending on housing takes up an exceptionally large share of their income—even a small increase in income can have very large effects on their welfare in the model. For the values of δ where welfare changes are negative, the implication from the model is that observed household behavior can only be reconciled if subsistence consumption, θ, is negative. Because the subsistence level of housing services is likely positive, this section focuses on welfare changes in the range of δ where welfare changes are positive, and θ is positive.

As mentioned in the introduction, one goal of this analysis is to show that when measuring the welfare effects of policy changes that shift household incomes, accounting for non-homotheticity is important. Table 5 reports the implied elasticities of the housing expenditure share with respect to income for groups of low-wage households. Those elasticities imply that a one percent increase in income causes a 0.2 to 0.8 percent decline in rent-to-income ratios.

Theoretical models in the class of Klein-Rubin or Stone-Geary utility appear necessary to provide a reasonable guide to the key facts and empirical findings in this paper. One subject of interest is whether the empirical results can provide a guide for the fundamental parameters in the model, especially δ. The Stone-Geary model imposes that housing expenditure shares approach $1-\delta$ as income m grows. In Figure 1, housing expenditure shares for households at the very top of the income distribution fall to 5-10% (implying $\delta = \{0.90, 0.95\}$). The asymptote in Figure 3 is around .91, implying that the empirical results suggest plausible levels of δ between 0.80 and 0.90. If this were the case, then the welfare gains to giving low-income households an additional dollar in income are very large.

7. Discussion

This discussion section summarizes the results from the prior sections, and addresses an additional perspective on the relationship between minimum wages and housing affordability. Taking a longer term view, how important is the longer-term decline in the real value of the minimum wage as a contributing factor in the recent housing affordability crisis? In the following paragraphs, I discuss prior research related to this topic and present correlational relationships using longer-term Census data that provide some insight to this question. I conclude by noting that higher minimum wages would reduce housing cost burdens for many low-wage households, but it can only explain a small portion of the recent rise in rent-to-income ratios. There are also trade-offs that policymakers must consider—most notably that increasing minimum wages may raise rental prices for everyone in a local housing market, while mostly benefitting the lowest-wage workers.

In the context of discussing the minimum wage, it is important to note two long-run changes in the relevance of the minimum wage: (1) the share of workers paid at or below the minimum wage has declined from about 15% in 1980 to 2% today[10]; (2) the real value of the federal minimum wage has declined by approximately 20% ($9.02 in 1980 to $7.25 today[11]), though states and cities have raised their minimum wages so this real decline may be smaller when considering geographic heterogeneity.[12] Recent research has suggested that inflation rates are different across regions (especially due to housing costs, see e.g. Moretti, 2013), and across the income distribution within regions (see Jaravel, 2019). This paper takes a different approach estimating the relationship between minimum wages and prices and consumption at the same time by focusing on how housing expenditure shares respond to minimum wage changes.

Because the main empirical section of this paper only captures 2005-2017, it misses some of the substantial decline in the 'bite' of the minimum wage in terms of its real value and the share of workers covered by the minimum wage. Figure 4 provides a longer-run perspective. Using the same approach as Figure 1, decennial Census data is collapsed by household income decile, so it provides the mean household income, rent, and rent-to-income ratio.

Figure 4 graphs the decade-over-decade difference in minimum wages on the x-axis and on the y-axis graphs changes in mean income (Panel A) and rent-to-income ratio (Panel B) from 1960-2016 for the lowest household income decile and a middle income decile (5th decile). Panel B shows that changes in minimum wages are negatively correlated with changes in rent-to-income ratios for households in the bottom of the income distribution (1st decile), and only weakly positively correlated with rent-to-income ratios for households in middle of the income distribution (5th decile). Appendix Table 5 shows a more formal regression that includes the all deciles, with controls for year fixed effects and state-decile fixed effects. This again suggests that minimum wages are negatively correlated with rent-to-income ratios for the bottom of the income distribution, but weakly related to higher income deciles, consistent with minimum wages raising prices levels by a small amount. I hesitate to ascribe causal interpretation to these estimates because prior to

[10] See e.g. https://fred.stlouisfed.org/series/LEU0203127200A. This statistic is for all workers, whereas the analysis in this paper focuses on the household heads of renter households who are disproportionately lower-income and younger.

[11] See e.g. https://www.epi.org/publication/raising-the-federal-minimum-wage-to-15-by-2024-would-lift-pay-for-nearly-40-million-workers/.

[12] See e.g. https://www.nytimes.com/2019/04/24/upshot/why-america-may-already-have-its-highest-minimum-wage.html.

1990, they are primarily identified off of decade-over-decade changes in the federal minimum wage. This is observable in in Figure 4, where decade-over-decade changes for 1960 through 1990 are clustered along vertical lines representing the size of the federal minimum wage change on the x-axis. Many policy changes are likely correlated with federal minimum wage changes, so this descriptive work is left as a correlational exercise for the purposes of the analysis in this paper. The implied elasticity of rent-to-income ratios to the minimum wage for the bottom decile is -0.481, which is reasonable but larger than the main estimates (e.g. Tables 1 & 2 using different estimation samples over a different time period).

The results in Tables 1 find that a 10% minimum wage change increases households' incomes by 1.89% (1.30%) percent for renter households below the 16th (25th) percentile in the local wage distribution. Those households appear to increase their housing consumption over time by spending 0.54 (0.26) percent more on rent, though this estimate depends on the specification & sample of interest. The identified demand responses are arguably separate from any potential rental housing price level responses to the minimum wage, though disentangling these effects is difficult and relies on strong assumptions. Low-wage renter households are very cost-burdened, spending as much as 50 percent of their income on rent. A minimum wage increase of 10% causes a 1.35 (1.04) percent decline in their rent-to-income ratios, or 0.65 (0.41) percentage points.

Taking the estimates for renter households below the 25th percentile wage from Table 1 presented here, the elasticity of rent-to-income ratios to minimum wages is -0.104 (note that from Figure 4, the state-level correlation of the 2009-2016 change in log state minimum wage & the rent-to-income ratio for the bottom income decile is -0.17). If nominal minimum wages were increased by 35% (approximately increasing from $7.25 to $10), that implies that the average rent-to-income ratio for the bottom quarter of renter households would decline by 1.5 percentage points from 40.6% (see Table 1, Column 6) to 39.1%. As noted in the introduction, for households at the 15th percentile in the household income distribution, rent-to-income ratios have increased 10 percentage points from 50% to 60%. This back-of-the-envelope exercise suggests that minimum wages can explain an important but not a large portion of this longer-term increase in housing cost burdens. In addition, these cost burden increases have occurred through most of the income distribution. Minimum wages are only one tool, which can alleviate cost burdens for the lowest-income households, but may have trade-offs consistent with Table 5 showing that minimum wage changes may raise rental prices in some housing markets (or lower them in more housing supply elastic and less labor demand-monopsonistic markets).

8. Conclusion

This paper attempts to identify the housing demand response of low-wage households while disentangling the general equilibrium rental housing price increases in response to changes in state and federal minimum wage changes. The demand and price response using a triple difference strategy, exploiting variation between states that raise their minimum wage versus those that do not and variation within states that raise their minimum wage between households at the bottom of the state wage distribution versus those higher in the state wage distribution. To test the robustness of these results, this paper (1) tests several different specifications showing the timing of these effects, (2) shows different cutoffs in the within-state wage distribution to identify the average effects on households most likely to receive a wage raise, (3) presents results for different subsamples of renter households, including occupations most likely affected by minimum wages, and (4) controls for a wide set of individual-level variables, which change the point estimates little and increase the precision of the main estimates (Appendix Table 4).

These demand responses are contextualized in a non-homothetic consumer model—housing is a special case of a normal good, where demand increases in income, but demand is inelastic so that housing expenditure shares decrease in income. The elasticity of the housing expenditure share with respect to income is an important component to calculating welfare gains. Empirical estimates of the average elasticity of the expenditure share with respect to income are between -0.16 and -0.80 (see Table 6). The empirical results suggest that rent-to-income ratios decline sharply in income for low-wage households. The corresponding welfare gains are large. Under reasonable assumptions, welfare gains could be 50-250% larger than the percent gains in housing consumption or income.

This paper provides evidence that active labor market policy focused on increasing wages at the bottom of the income distribution has the potential to partly mitigate rising rent-to-income ratios. It also provides evidence that—to the extent that rising rent-to-income ratios are due to stagnating real incomes—the increase in housing cost burdens over the past several decades has had potentially large negative welfare consequences for low-income households.

The empirical and conceptual results presented in this paper are interesting and important, but leave substantial work to be done in the future. In particular, this line of research would benefit from improved data in order to (1) track individuals over time to observe whether they move to better neighborhoods or better homes, and (2) deal with rental housing market segmentation and landlord price discrimination which may be important to understanding the rental housing price level response.

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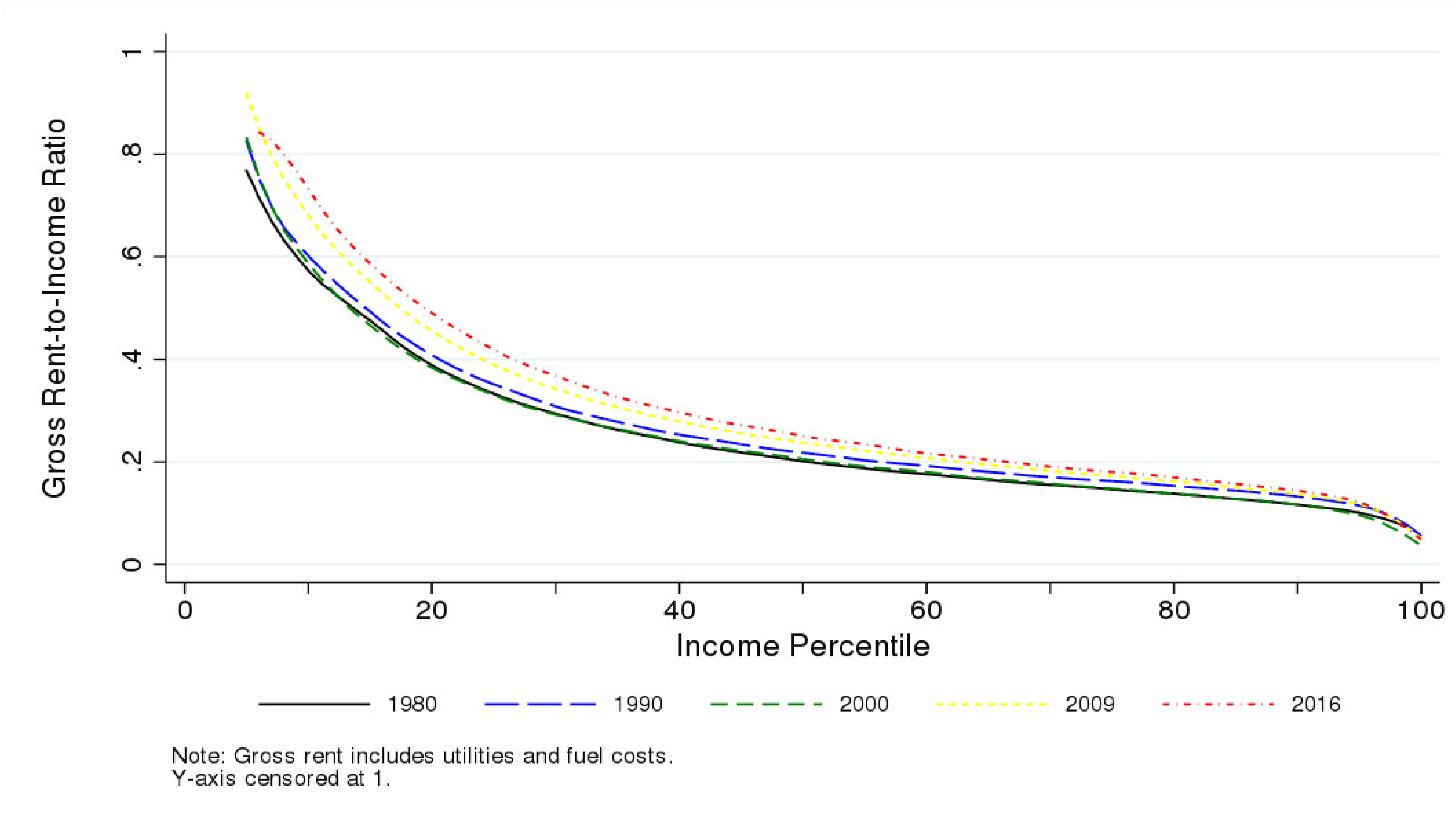
FIGURE 1. MOTIVATION: HOUSING EXPENDITURE SHARES ACROSS THE INCOME DISTRIBUTION AND OVER TIME

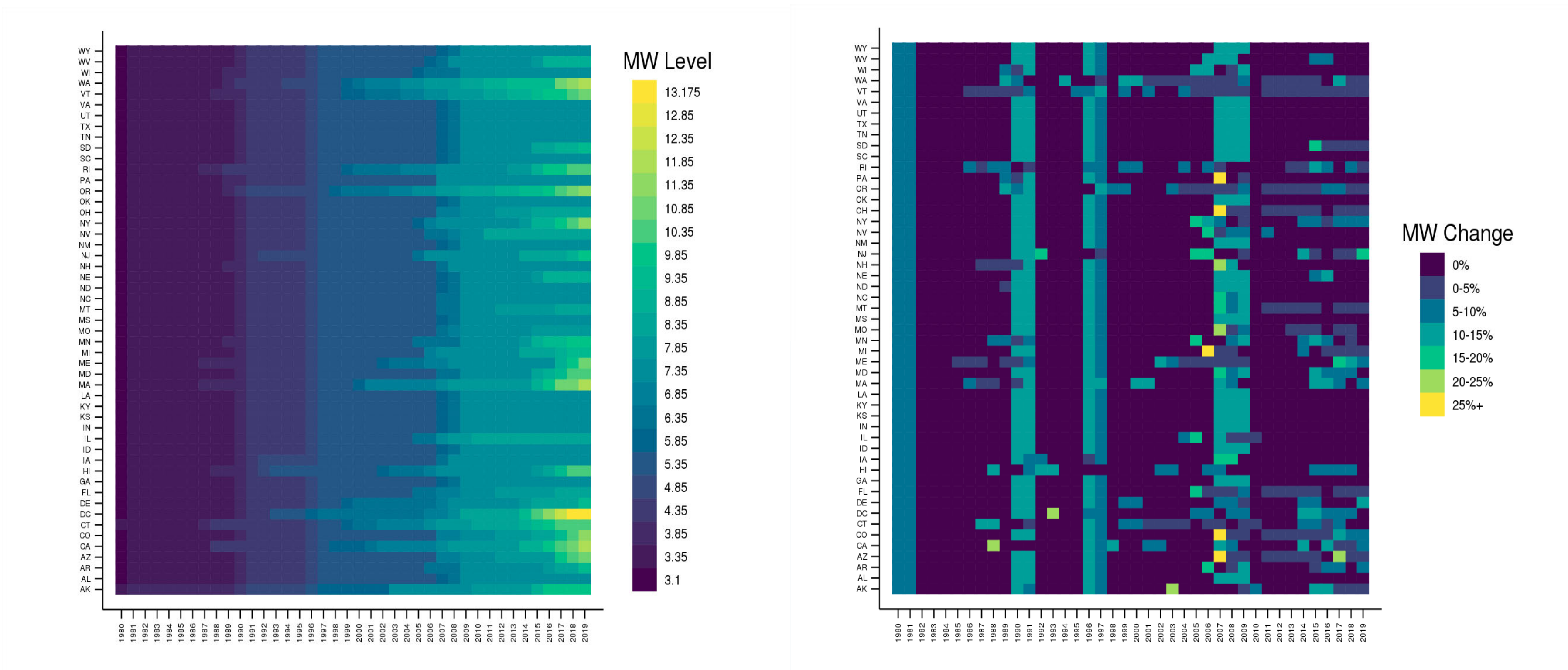


Note: Data comes from 1980, 1990, 2000 Census, and 2009 and 2016 5-year ACS from IPUMS, Ruggles et al. (2019). Data collapsed by state-year household income percentile. Gross rent-to-income ratio calculated as the average gross rent divided by annual household income divided by 12. Ratio averaged for renter households within each household income percentile.

FIGURE 2. IDENTIFYING VARIATION: LEVELS AND CHANGES IN MINIMUM WAGES ACROSS U.S. STATES, 1980-2019

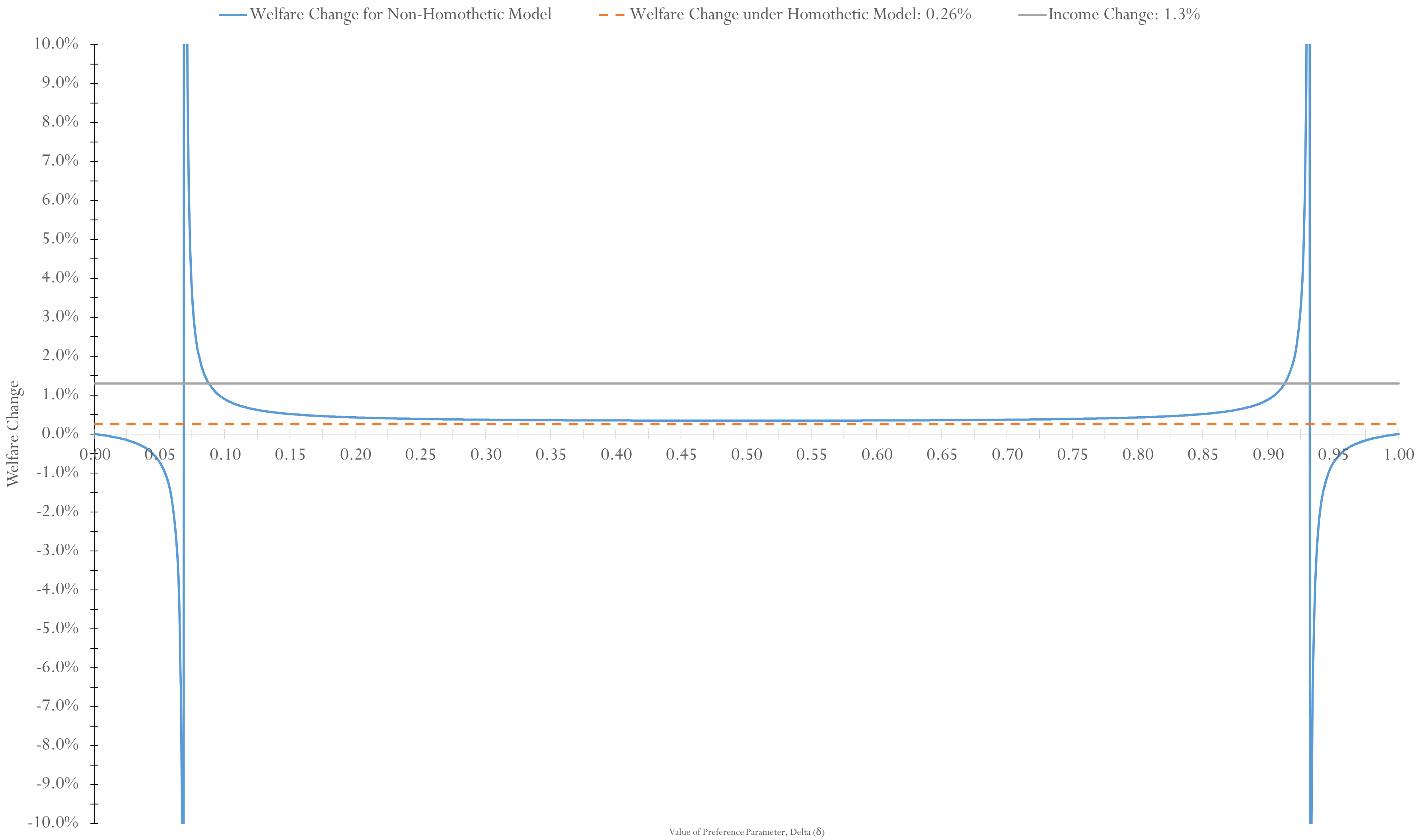
Panel A. Levels

Panel B. Changes



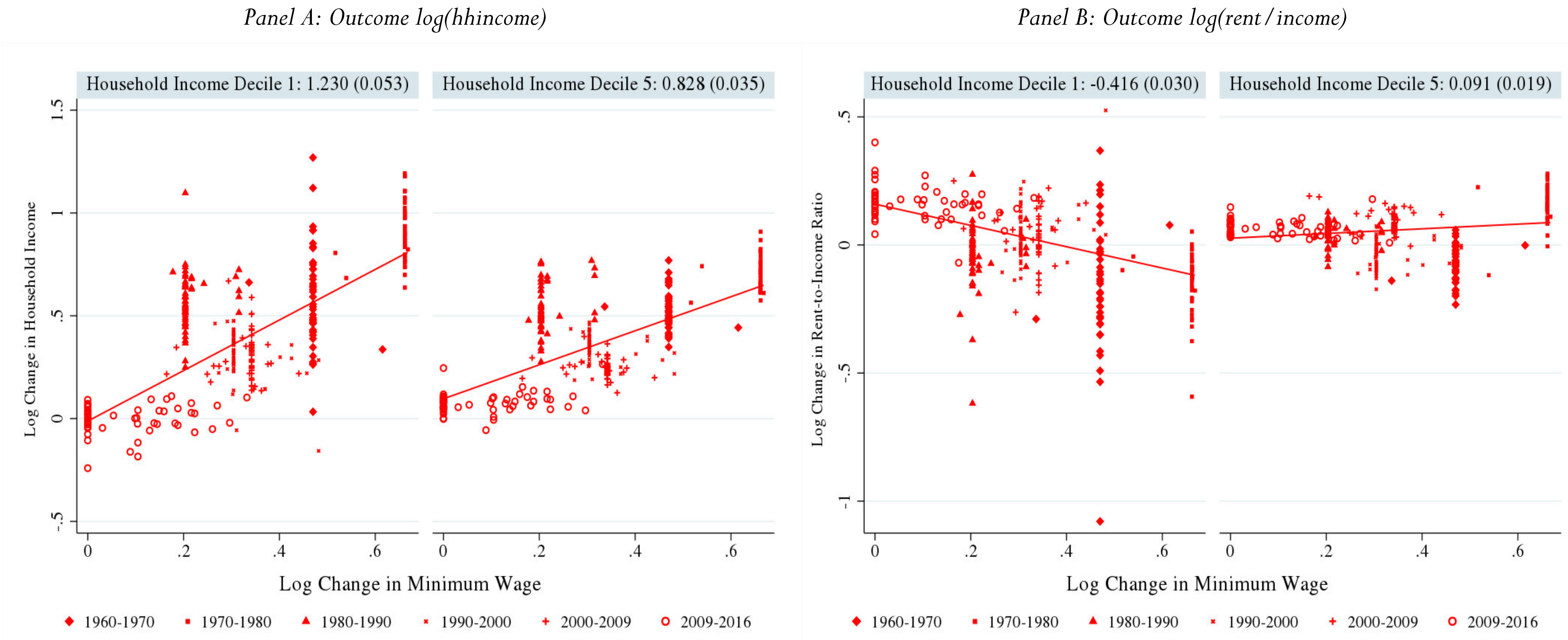
Note: Data on federal and state minimum wage changes from Neumark (2019), Zipperer & Vaghul (2019), and data on 2018 and 2019 law changes from U.S. Department of Labor and National Council of State Legislatures.

FIGURE 3. ESTIMATES OF WELFARE GAINS FOR WORKERS BELOW 25TH PERCENTILE IN STATE WAGE DISTRIBUTION DEPENDING ON PREFERENCE PARAMETER



Note: Calculation comes from Column 1, Table 5.

FIGURE 4. LONG-RUN AFFORDABILITY BY HOUSEHOLD INCOME DECILE



Note: Data is from IPUMS for 1960, 1970, 1980, 1990, 2000, 2009, and 2016 (Decennial Census & 5-Year ACS), collapsed by state-year household income deciles to take average values of annual household income, monthly gross rent and rent-to-income ratio for renters with each decile.

TABLE 1. MINIMUM WAGE EFFECT ON HOUSING FOR LOW-WAGE HOUSEHOLDS

		ln(rent)		ln(hhincome)		ln(rent/income)	
		(1)	(2)	(3)	(4)	(5)	(6)
Common Effect:	**ln(MW)**	-0.00783	-0.00619	-0.0106	-0.0158	0.00273	0.00965
		(0.0235)	(0.0237)	(0.0328)	(0.0328)	(0.0241)	(0.0244)
Interaction Effect:	**ln(MW)*Affected**	0.0543**	0.0256	0.189***	0.130***	-0.135***	-0.104***
		(0.0246)	(0.0234)	(0.0322)	(0.0277)	(0.0361)	(0.0254)
	Affected	-0.213***	-0.155***	-1.104***	-0.893***	0.892***	0.738***
		(0.0497)	(0.0473)	(0.0652)	(0.0585)	(0.0689)	(0.0513)
Observations		2,248,921	2,248,921	2,248,921	2,248,921	2,248,921	2,248,921
R-Squared		0.327	0.328	0.291	0.293	0.158	0.157
Affected Wage Relative to MW		100%	125%	100%	125%	100%	125%
Affected Wage Percentile Cutoff		16	25	16	25	16	25
Two-Way FE		X	X	X	X	X	X
State-Year Linear Trend		X	X	X	X	X	X
Mean of Outcome Below Affected Pctile		6.5261	6.5343	7.2810	7.4351	-0.7549	-0.9008
e^(Mean of Outcome)		$682.73	$688.35	$1,452.44	$1,694.43	47.0%	40.6%
Mean of Outcome Above Affected Pctile		6.7502	6.7711	8.2577	8.3141	-1.5075	-1.5430
e^(Mean of Outcome)		$854.23	$872.27	$3,857.21	$4,081.01	22.1%	21.4%

Note: Standard errors are clustered by state. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Affected' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 100% of the state minimum wage in Column 1, 3, and 5 (125% in Column 2, 4, and 6). In these regressions, the percentile corresponding to 100% of the minimum wage is the 16th percentile, and the percentile corresponding to 125% is the 25th percentile.

TABLE 2. MINIMUM WAGE EFFECT ON HOUSING FOR HOUSEHOLDS OF CASHIERS, CLERKS & SALESPERSONS

		ln(rent)		ln(hhincome)		ln(rent/income)	
		(1)	(2)	(3)	(4)	(5)	(6)
Common Effect:	**ln(MW)**	-0.0365	-0.0344	-0.0659	-0.103	0.0294	0.0683
		(0.0509)	(0.0518)	(0.116)	(0.119)	(0.112)	(0.113)
Interaction Effect:	**ln(MW)*Affected**	0.122*	0.0620	0.260***	0.201***	-0.139**	-0.139**
		(0.0664)	(0.0456)	(0.0826)	(0.0744)	(0.0656)	(0.0559)
	Affected	-0.327**	-0.209**	-1.152***	-0.933***	0.825***	0.724***
		(0.133)	(0.0916)	(0.170)	(0.152)	(0.129)	(0.113)
Observations		150,880	150,880	150,880	150,880	150,880	150,880
R-Squared		0.196	0.196	0.148	0.139	0.076	0.066
Affected Wage Relative to MW		100%	125%	100%	125%	100%	125%
Affected Wage Percentile Cutoff		31	47	31	47	31	47
Two-Way FE		X	X	X	X	X	X
State-Year Linear Trend		X	X	X	X	X	X
Mean of Outcome Below Affected Pctile		6.4528	6.4641	7.1273	7.2868	-0.6746	-0.8227
e^(Mean of Outcome)		$634.48	$641.69	$1,245.51	$1,460.89	50.9%	43.9%
Mean of Outcome Above Affected Pctile		6.5558	6.5717	7.8187	7.8692	-1.2629	-1.2975
e^(Mean of Outcome)		$703.31	$714.58	$2,486.67	$2,615.47	28.3%	27.3%

Note: Standard errors are clustered by state. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. In this specification only household heads working in occupation codes 4720-4760 are included. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Affected' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 100% of the state minimum wage in Column 1, 3, and 5 (125% in Column 2, 4, and 6). In these regressions, the percentile corresponding to 100% of the minimum wage is the 31th percentile, and the percentile corresponding to 125% is the 47th percentile.

TABLE 3. DYNAMICS OF EFFECTS FROM REGRESSION ON DISTRIBUTED LEADS/LAGS

		ln(rent)	ln(hhincome)	ln(rent/income)
ln(MW)	**Average of Leads**	-0.024	-0.008	-0.016
		(0.034)	(0.034)	(0.034)
ln(MW)	**Average of Lags**	0.008	0.013	-0.005
		(0.021)	(0.032)	(0.024)
ln(MW)	**Difference**	0.032	0.021	0.012
		(0.026)	(0.033)	(0.023)
ln(MW)*Affected	**Average of Leads**	-0.026	-0.042	0.015
		(0.024)	(0.029)	(0.040)
ln(MW)*Affected	**Average of Lags**	0.021	0.055	-0.034
		(0.019)	(0.023)	(0.022)
ln(MW)*Affected	**Difference**	0.047	0.096	-0.049
		(0.021)	(0.034)	(0.042)
Observations		2,063,501	2,063,501	2,063,501
R-Squared		0.324	0.290	0.157
Affected Wage Relative to MW		125%	125%	125%
Affected Wage Percentile Cutoff		25	25	25
Two-Way FE		X	X	X
State-Year Linear Trend		X	X	X
Mean of Outcome Below Affected Pctile		6.5343	7.4351	-0.9008
e^(Mean of Outcome)		$688.35	$1,694.43	40.6%
Mean of Outcome Above Affected Pctile		6.7711	8.3141	-1.543
e^(Mean of Outcome)		$872.27	$4,081.01	21.4%

Note: Standard errors are clustered by state. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2016. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Below 125% MW Pctile' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 125% of the state minimum wage. In these regressions, the percentile corresponding to 125% is the 25th percentile. See coefficients in Appendix Figure 1.

TABLE 4. DYNAMICS OF EFFECTS FROM REGRESSION ON DISTRIBUTED LEADS/LAGS FOR CASHIERS, CLERKS & SALESPERSONS

		ln(rent)	ln(hhincome)	ln(rent/income)
ln(MW)	**Average of Leads**	-0.084	-0.013	-0.071
		(0.088)	(0.146)	(0.135)
ln(MW)	**Average of Lags**	-0.059	0.019	-0.078
		(0.043)	(0.094)	(0.091)
ln(MW)	**Difference**	0.025	0.032	-0.007
		(0.073)	(0.126)	(0.115)
ln(MW)*Affected	**Average of Leads**	-0.011	-0.103	0.092
		(0.055)	(0.083)	(0.089)
ln(MW)*Affected	**Average of Lags**	0.154	0.094	0.061
		(0.050)	(0.101)	(0.074)
ln(MW)*Affected	**Difference**	0.165	0.197	-0.031
		(0.060)	(0.116)	(0.103)
Observations		140,156	140,156	140,156
R-Squared		0.194	0.138	0.067
Affected Wage Relative to MW		125%	125%	125%
Affected Wage Percentile Cutoff		47	47	47
Two-Way FE		X	X	X
State-Year Linear Trend		X	X	X
Mean of Outcome Below Affected Pctile		6.4641	7.2868	-0.8227
e^(Mean of Outcome)		$641.69	$1,460.89	43.9%
Mean of Outcome Above Affected Pctile		6.5717	7.8692	-1.2975
e^(Mean of Outcome)		$714.58	$2,615.47	27.3%

Note: Standard errors are clustered by state. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2016. All household heads who are private sector wage/salary workers are included; only renter households are included. In this specification only household heads working in occupation codes 4720-4760 are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Below 125% MW Pctile' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 125% of the state minimum wage. In these regressions, the percentile corresponding to 125% is the 47th percentile. See coefficients in Appendix Figure 2.

TABLE 5. EFFECT HETEROGENEITY BY SAIZ HOUSING SUPPLY ELASTICITY

		ln(rent)		ln(hhincome)		ln(rent/income)	
		(1)	(2)	(3)	(4)	(5)	(6)
Common Effect:	**ln(MW)**	-0.0286	0.0809*	-0.00495	0.0601	-0.0237	0.0208
		(0.0304)	(0.0409)	(0.0376)	(0.0451)	(0.0261)	(0.0234)
Interaction Effect:	**ln(MW)*Affected**	0.0241	0.0282	0.204***	0.206***	-0.180***	-0.178***
		(0.0255)	(0.0242)	(0.0398)	(0.0386)	(0.0486)	(0.0490)
Interaction Effect:	**ln(MW)*Saiz Elasticity**		-0.0590***		-0.0350***		-0.0239***
			(0.0166)		(0.0107)		(0.00773)
	Affected	-0.149***	-0.153***	-1.152***	-1.154***	1.003***	1.001***
		(0.0507)	(0.0486)	(0.0805)	(0.0781)	(0.0922)	(0.0932)
Observations		1,586,930	1,586,930	1,586,930	1,586,930	1,586,930	1,586,930
R-Squared		0.316	0.328	0.296	0.297	0.162	0.162
Affected Wage Relative to MW		100%	100%	100%	100%	100%	100%
Affected Wage Percentile Cutoff		16	16	16	16	16	16
Two-Way FE		X	X	X	X	X	X
State-Year Linear Trend		X	X	X	X	X	X

Note: Standard errors are clustered by state. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Affected' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 100% of the state minimum wage. In these regressions, the percentile corresponding to 100% of the minimum wage is the 16th percentile.

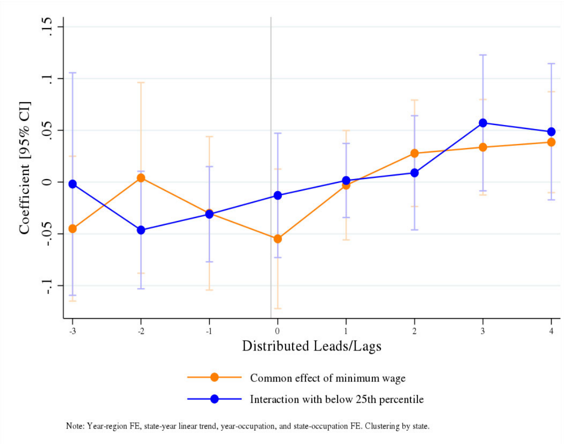
TABLE 6. WELFARE CALCULATION FROM INCREASE IN INCOME IN HOMOTHETIC VS NON-HOMOTHETIC MODELS

	All Private Sector		Cashiers, Clerks & Salespersons	
	<25th Percentile Wage (125% MW)	<25th Percentile Wage (125% MW)	<47th Percentile Wage (125% MW)	<47th Percentile Wage (125% MW)
Table Reference	Table 1	Table 3	Table 2	Table 4
Inputs to Welfare Calculation				
Income elasticity of expenditure share	-0.80	-0.51	-0.69	-0.16
Average rent ($2016)	$688	$688	$642	$642
Average change in monthly income ($2016)	$22	$16	$29	$29
Change in housing consumption	0.0026	0.0047	0.0062	0.0165
Welfare Change Calculations				
Homothetic model	0.26%	0.47%	0.62%	1.65%
Non-homothetic model				
Preference Parameter, 1-δ = 0.5	0.34%	0.79%	0.99%	2.15%
Percent Difference from Homothetic	34.45%	68.31%	59.94%	30.14%
Preference Parameter, 1-δ = 0.4	0.35%	0.81%	1.02%	2.17%
Percent Difference from Homothetic	36.41%	73.24%	64.04%	31.79%
Preference Parameter, 1-δ = 0.3	0.37%	0.91%	1.12%	2.28%
Percent Difference from Homothetic	43.89%	93.49%	80.56%	38.06%
Preference Parameter, 1-δ = 0.2	0.43%	1.28%	1.50%	2.59%
Percent Difference from Homothetic	66.77%	173.34%	141.31%	56.70%
Preference Parameter, 1-δ = 0.1	0.89%	-3.69%	-15.10%	4.63%
Percent Difference from Homothetic	246.92%	-885.00%	-2534.91%	180.31%

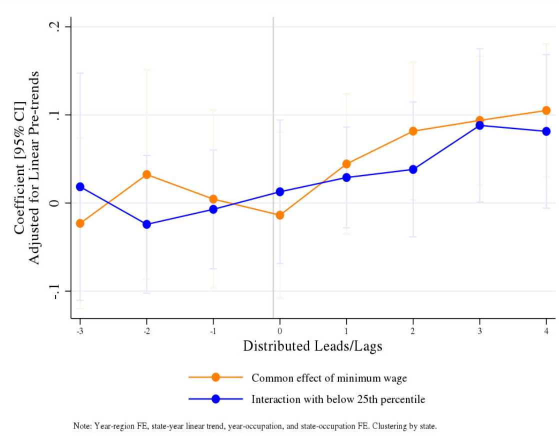
Note: Calculation described in text. Inputs come from Tables 1-4.

APPENDIX FIGURE 1. DYNAMICS OF HOUSING CONSUMPTION RESPONSE DISTRIBUTED LEADS/LAGS AND ADJUSTMENT FOR PRE-TRENDS

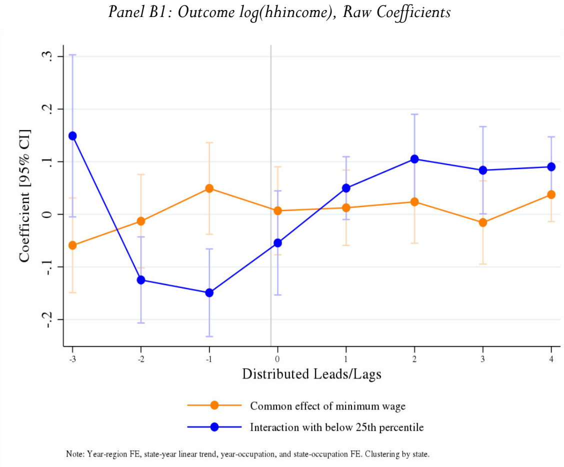
Panel A1: Outcome log(rent), Raw Coefficients



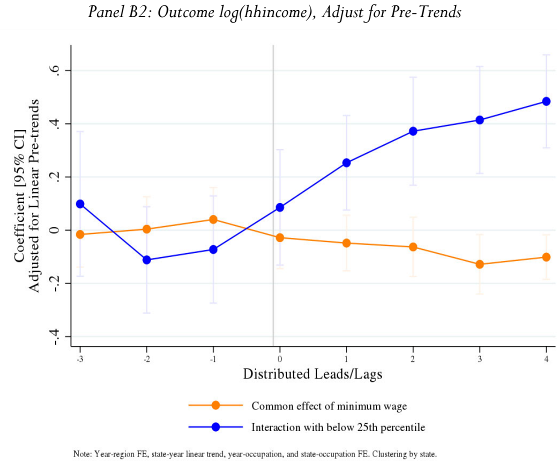
Panel A2: Outcome log(rent), Adjust for Pre-Trends



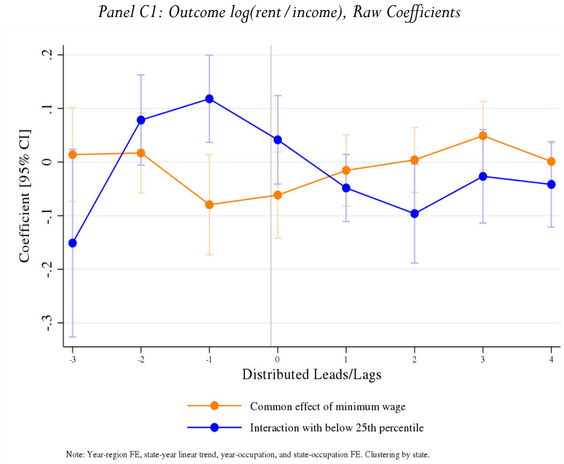
Panel B1: Outcome log(hhincome), Raw Coefficients



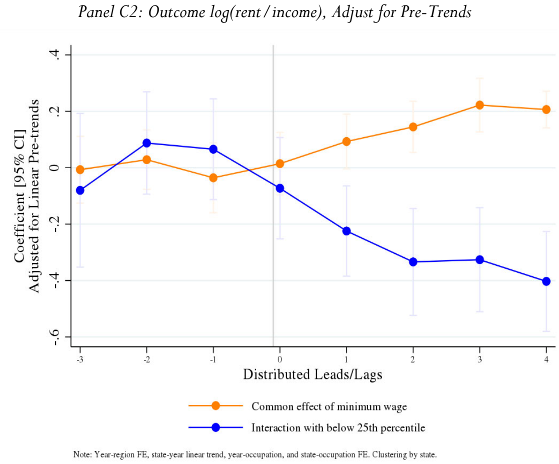
Panel B2: Outcome log(hhincome), Adjust for Pre-Trends



Panel C1: Outcome log(rent/income), Raw Coefficients



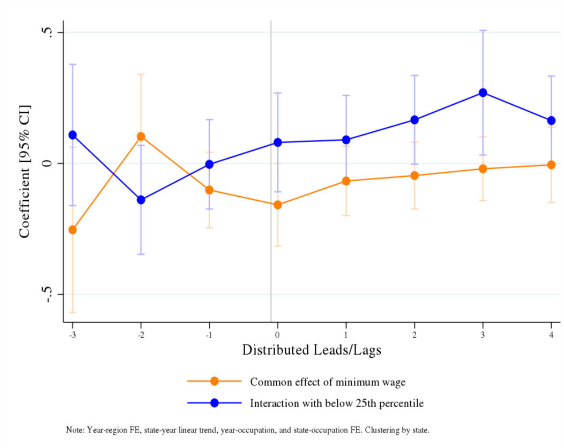
Panel C2: Outcome log(rent/income), Adjust for Pre-Trends



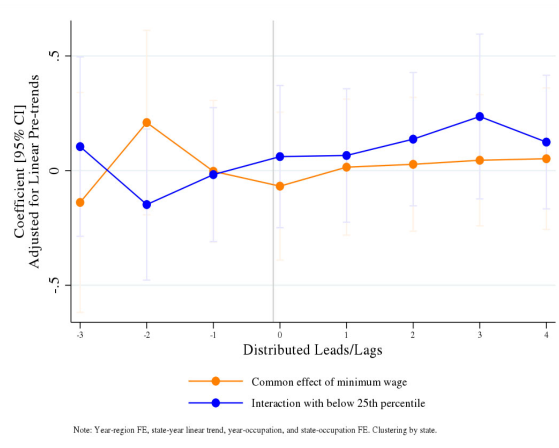
Note: Regressions described in Table 3. Bars shows 95% confidence intervals. All specifications include year by division, and state fixed effects with state-year linear trends. Regression in Panel A1, B1, and C1 show the raw coefficients. Panel A2, B2, and C2 coefficients are corrected for linear pre-event trends by projecting a linear regression through the pre-event coefficients.

APPENDIX FIGURE 2. DYNAMICS OF HOUSING CONSUMPTION RESPONSE FOR CASHIERS, CLERKS, & SALESPERSONS, DISTRIBUTED LEADS/LAGS AND ADJUSTMENT FOR PRE-TRENDS

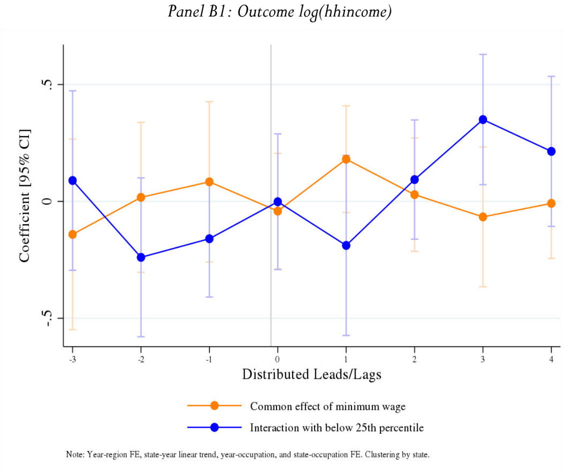
Panel A1: Outcome log(rent)



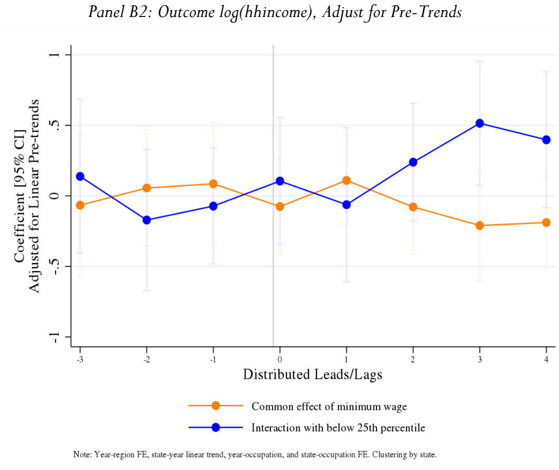
Panel A2: Outcome log(rent), Adjust for Pre-Trends



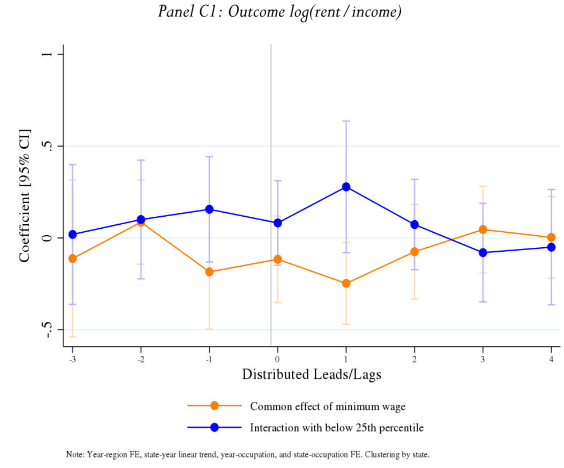
Panel B1: Outcome log(hhincome)



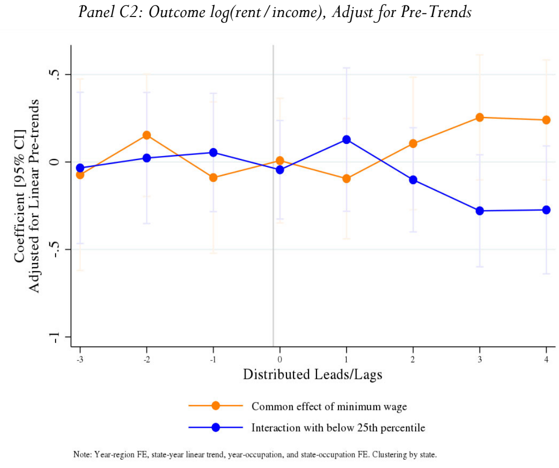
Panel B2: Outcome log(hhincome), Adjust for Pre-Trends



Panel C1: Outcome log(rent/income)



Panel C2: Outcome log(rent/income), Adjust for Pre-Trends



Note: Regressions described in Table 4. Bars shows 95% confidence intervals. Sample is subset to occupations for cashiers, clerks and salespersons. All specifications include year by division, and state fixed effects with state-year linear trends. Regression in Panel A1, B1, and C1 show the raw coefficients. Panel A2, B2, and C2 coefficients are corrected for linear pre-event trends by projecting a linear regression through the pre-event coefficients.

APPENDIX TABLE 1. VERIFY DENSITY DOES NOT CHANGE FOR "AFFECTED" VARIABLE

Outcome:	1(wage < Affected)			
	All Low-Wage Households		Cashiers, Clerks, Sales	
ln(MW)	0.00600	0.00246	-0.0103	-0.0541
	(0.00672)	(0.00712)	(0.0338)	(0.0344)
Observations	2,248,921	2,248,921	150,880	150,880
R-Squared	0.091	0.131	0.022	0.032
Affected Wage Relative to MW	100%	125%	100%	125%
Affected Wage Percentile Cutoff	16	25	31	47
Two-Way FE	X	X	X	X
State-Year Linear Trend	X	X	X	X

Note: Standard errors are clustered by state. Outcome variable is the binary variable for households whose head is a low-wage earner defined by their position in the state-year wage distribution. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Affected' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 100% (or 125%) of the state minimum wage.

APPENDIX TABLE 2. VERIFY WHETHER SIZE OF POPULATION CHANGES WITH MINIMUM WAGE

Outcome:	ln(pop)		
ln(MW)	0.0642	0.0347	-0.0277
	(0.0390)	(0.0489)	(0.0412)
Observations	663	663	663
R-Squared	0.999	0.999	0.999
Number of States	51	51	51
Number of Years	13	13	13
Year FE	X	X	X
State FE	X	X	X
Year-Division FE		X	X
State-Year Linear Trend			X

Note: Standard errors are clustered by state. Outcome variable is the log of the state population, where the population is the weighted sum of households who are renters whose households head is a private sector wage/salary-earners (i.e. the main sample for Table 1). Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017.

APPENDIX TABLE 3. EFFECT OF MINIMUM WAGE BY OCCUPATION GROUP

		ln(rent)	ln(hhincome)	ln(rent/income)	Below 125% MW Pctile		Above 125% MW Pctile	
					#	%	#	%
Architecture and Engineering	OCC*ln(MW)*Below 125% MW Pctile	0.0298	-0.186***	0.177***	1,686	4%	39,270	96%
		(0.0275)	(0.0205)	(0.0350)				
Computer and Mathematical	OCC*ln(MW)*Below 125% MW Pctile	-0.0108	-0.222***	0.180***	2,639	4%	64,414	96%
		(0.0241)	(0.0295)	(0.0214)				
Financial Specialists	OCC*ln(MW)*Below 125% MW Pctile	-0.0219	-0.0934***	0.0512	2,530	6%	39,514	94%
		(0.0281)	(0.0309)	(0.0315)				
Legal	OCC*ln(MW)*Below 125% MW Pctile	0.0289	-0.142***	0.134***	1,269	6%	18,616	94%
		(0.0240)	(0.0306)	(0.0362)				
Business Operations Specialists	OCC*ln(MW)*Below 125% MW Pctile	-0.0212	-0.0637**	0.0168	3,454	7%	45,240	93%
		(0.0285)	(0.0314)	(0.0273)				
Management, Business, Science, and Arts	OCC*ln(MW)*Below 125% MW Pctile	-0.0278	-0.0262	-0.0264	13,479	8%	148,686	92%
		(0.0246)	(0.0292)	(0.0266)				
Life, Physical, and Social Science	OCC*ln(MW)*Below 125% MW Pctile	0.0356	-0.111***	0.105**	1,409	9%	14,237	91%
		(0.0288)	(0.0317)	(0.0396)				
Healthcare Practitioners and Technicians	OCC*ln(MW)*Below 125% MW Pctile	0.0148	-0.00685	-0.00717	7,893	9%	77,721	91%
		(0.0241)	(0.0244)	(0.0247)				
Extraction	OCC*ln(MW)*Below 125% MW Pctile	0.0314	0.00140	-0.0149	374	10%	3,233	90%
		(0.0286)	(0.0417)	(0.0442)				
Arts, Design, Entertainment, Sports, and Media	OCC*ln(MW)*Below 125% MW Pctile	-0.00303	-0.0797**	0.0480	5,273	13%	36,575	87%
		(0.0230)	(0.0310)	(0.0304)				
Installation, Maintenance, and Repair	OCC*ln(MW)*Below 125% MW Pctile	0.0222	0.0721***	-0.0834***	8,641	13%	58,709	87%
		(0.0250)	(0.0245)	(0.0309)				
Community and Social Services	OCC*ln(MW)*Below 125% MW Pctile	0.00964	0.0320	-0.0478*	1,494	14%	9,222	86%
		(0.0229)	(0.0221)	(0.0274)				
Construction	OCC*ln(MW)*Below 125% MW Pctile	0.0447*	0.123***	-0.115***	20,946	18%	96,892	82%
		(0.0249)	(0.0285)	(0.0293)				
Office and Administrative Support	OCC*ln(MW)*Below 125% MW Pctile	0.0200	0.122***	-0.131***	66,230	20%	267,933	80%
		(0.0232)	(0.0267)	(0.0275)				
Production	OCC*ln(MW)*Below 125% MW Pctile	0.0382	0.154***	-0.149***	40,265	22%	144,217	78%
		(0.0235)	(0.0245)	(0.0325)				
Transportation and Material Moving	OCC*ln(MW)*Below 125% MW Pctile	0.0442*	0.156***	-0.148***	42,585	25%	128,982	75%
		(0.0248)	(0.0238)	(0.0302)				
Education, Training, and Library	OCC*ln(MW)*Below 125% MW Pctile	0.0237	0.113***	-0.120***	11,261	26%	31,626	74%
		(0.0222)	(0.0296)	(0.0291)				
Protective Service	OCC*ln(MW)*Below 125% MW Pctile	0.0384	0.170***	-0.165***	7,288	26%	20,551	74%
		(0.0251)	(0.0266)	(0.0292)				
Sales and Related	OCC*ln(MW)*Below 125% MW Pctile	0.0212	0.138***	-0.147***	80,109	27%	211,654	73%
		(0.0240)	(0.0264)	(0.0259)				
Healthcare Support	OCC*ln(MW)*Below 125% MW Pctile	0.0310	0.199***	-0.195***	24,449	29%	59,843	71%
		(0.0236)	(0.0265)	(0.0273)				
Building and Grounds Cleaning and Maintenance	OCC*ln(MW)*Below 125% MW Pctile	0.0536**	0.252***	-0.233***	39,991	38%	64,293	62%
		(0.0236)	(0.0266)	(0.0290)				
Personal Care and Service	OCC*ln(MW)*Below 125% MW Pctile	0.0449*	0.223***	-0.210***	29,213	40%	44,389	60%
		(0.0232)	(0.0278)	(0.0280)				
Food Preparation and Serving	OCC*ln(MW)*Below 125% MW Pctile	0.0394*	0.226***	-0.218***	80,390	43%	108,544	57%
		(0.0235)	(0.0265)	(0.0269)				
Farming, Fishing, and Forestry	OCC*ln(MW)*Below 125% MW Pctile	0.0551**	0.279***	-0.261***	10,867	47%	12,419	53%
		(0.0231)	(0.0263)	(0.0343)				
Observations		2,248,921	2,248,921	2,248,921	503,735	22%	1,746,780	78%
R-squared		0.329	0.298	0.178				

Note: Standard errors are clustered by state. Regression includes year by Census region fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Below 125% MW Pctile' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 125% of the state minimum wage. In these regressions, the percentile corresponding to 125% is the 25th percentile.

APPENDIX TABLE 4. MAIN MINIMUM WAGE EFFECT REGRESSION USING SEVERAL SPECIFICATIONS

Panel A. Outcome: ln(rent)									
Common Effect:	**ln(MW)**	-0.00783	0.0149	-0.00278	-0.000511	0.000115	0.0188	0.00318	-0.0111
		(0.0235)	(0.0363)	(0.0264)	(0.0239)	(0.0238)	(0.0386)	(0.0259)	(0.0220)
Interaction Effect:	**ln(MW)*Affected**	0.0543**					-0.0270	-0.0277	0.0579***
		(0.0246)					(0.0428)	(0.0424)	(0.0199)
	Affected	-0.213***				-0.104***	-0.159*	-0.158*	-0.210***
		(0.0497)				(0.00478)	(0.0850)	(0.0841)	(0.0399)
Observations		2,248,921	2,250,515	2,250,515	2,248,921	2,248,921	2,250,515	2,250,515	2,248,920
R-Squared		0.327	0.212	0.214	0.324	0.327	0.227	0.228	0.386
Panel B. Outcome: ln(hhincome)									
Common Effect:	**ln(MW)**	-0.0106	0.0393	-0.00351	0.0127	0.0171	0.0214	-0.0118	-0.0180
		(0.0328)	(0.0422)	(0.0359)	(0.0323)	(0.0321)	(0.0400)	(0.0358)	(0.0316)
Interaction Effect:	**ln(MW)*Affected**	0.189***					0.118***	0.118***	0.182***
		(0.0322)					(0.0403)	(0.0396)	(0.0320)
	Affected	-1.104***				-0.728***	-1.185***	-1.184***	-1.030***
		(0.0652)				(0.0135)	(0.0830)	(0.0816)	(0.0624)
Observations		2,248,921	2,250,515	2,250,515	2,248,921	2,248,921	2,250,515	2,250,515	2,248,920
R-Squared		0.291	0.047	0.048	0.226	0.291	0.169	0.169	0.400
Panel C. Outcome: ln(rent/hhincome)									
Common Effect:	**ln(MW)**	0.00273	-0.0244	0.000726	-0.0132	-0.0170	-0.00264	0.0150	0.00689
		(0.0241)	(0.0407)	(0.0250)	(0.0236)	(0.0239)	(0.0392)	(0.0269)	(0.0245)
Interaction Effect:	**ln(MW)*Affected**	-0.135***					-0.145***	-0.145***	-0.124***
		(0.0361)					(0.0405)	(0.0405)	(0.0359)
	Affected	0.892***				0.623***	1.026***	1.026***	0.820***
		(0.0689)				(0.0128)	(0.0765)	(0.0765)	(0.0678)
Observations		2,248,921	2,250,515	2,250,515	2,248,921	2,248,921	2,250,515	2,250,515	2,248,920
R-Squared		0.158	0.014	0.015	0.101	0.158	0.101	0.102	0.211
Affected Wage Relative to MW		100%	100%	100%	100%	100%	100%	100%	100%
Affected Wage Percentile Cutoff		16	16	16	16	16	16	16	16
State FE		X	X	X	X	X	X	X	X
Year FE		X	X	X	X	X	X	X	X
State-Time Trend		X		X	X	X		X	X
State-Occupation FE		X			X	X			X
Occupation-Year FE		X			X	X			X
Household/Individual Controls									X

Note: Standard errors are clustered by state. Regression includes year by Census division fixed effects, state fixed effects, and state-year linear trends. Data is from IPUMS ACS 2005-2017. All household heads who are private sector wage/salary workers are included; only renter households are included. State by occupation and occupation by year fixed effects are included using household head occupation code. Wages are imputed as annual wage and salary earnings divided by usual weekly hours worked times number of weeks worked last year. 'Affected' is a binary indicator created by finding the average percentile in the wage distribution that corresponds to wages that are 100% of the state minimum wage. In these regressions, the percentile corresponding to 100% of the minimum wage is the 16th percentile. Household/Individual Controls include a full set of fixed effects for number of household members, size of the household head's family, and age, sex, marital status, race, citizenship status, and education of the household head.

APPENDIX TABLE 5. LONG-RUN AFFORDABILITY BY HOUSEHOLD INCOME DECILE

	D.ln(rent)	D.ln(hhincome)	D.ln(rent/hhincome
D.ln(MW)*1st Decile	0.034	0.514***	-0.481***
	(0.088)	(0.089)	(0.055)
D.ln(MW)*2nd Decile	0.153*	0.180**	-0.027
	(0.091)	(0.084)	(0.048)
D.ln(MW)*3rd Decile	0.140	0.093	0.047
	(0.086)	(0.079)	(0.046)
D.ln(MW)*4th Decile	0.133	0.066	0.067
	(0.085)	(0.073)	(0.043)
D.ln(MW)*5th Decile	0.129	0.090	0.039
	(0.086)	(0.072)	(0.041)
D.ln(MW)*6th Decile	0.117	0.113	0.004
	(0.085)	(0.073)	(0.040)
D.ln(MW)*7th Decile	0.093	0.118	-0.025
	(0.085)	(0.073)	(0.041)
D.ln(MW)*8th Decile	0.076	0.113	-0.037
	(0.086)	(0.073)	(0.041)
D.ln(MW)*9th Decile	-0.005	0.090	-0.095**
	(0.086)	(0.073)	(0.042)
D.ln(MW)*10th Decile	-0.164*	-0.107	-0.057
	(0.083)	(0.066)	(0.044)
Observations	2,920	2,920	2,920
R-Squared	0.909	0.900	0.581
Year FE	X	X	X
State-Decile FE	X	X	X

Note: Standard errors are clustered by state. Data is from IPUMS for 1960, 1970, 1980, 1990, 2000, 2009, and 2016 (Decennial Census & 5-Year ACS), collapsed by state-year household income deciles to take average values of annual household income, monthly gross rent and rent-to-income ratio for renters.