Minimum Wages and Housing Rents: Theory and Evidence*

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January 10, 2021

Abstract

I analyze the effect of minimum wage hikes on housing rents using exogenous variation in minimum wages across local labor markets in Japan. I estimate that in low-quality rental housing market, a 10% minimum wage increase induces a 2.5%-4.5% increase in rents. Minimum wage hikes benefit workers in light of a spatial equilibrium model showing that changes in housing market rents work as a sufficient statistic for measuring utility changes arising from changes in minimum wages. The increase in housing rents also implies an unintended benefit for homeowners.

JEL classification: H70, J38, J61, R23, R38

Keywords: Minimum Wages, Housing Rents, Capitalization, Spatial Equilibrium, Price Levels

*I am grateful to Gabriel Ahlfeldt (the handling editor) and anonymous referees for their constructive comments and suggestions. I also thank Taiyo Fukai, Koichi Fukumura, Allison Green, Daiji Kawaguchi, Keisuke Kawata, Henrik Kleven, Ilyana Kuziemko, Hikaru Ogawa, Chigusa Okamoto, Dan Sasaki, Yasuhiro Sato, Takatoshi Tabuchi, Kohei Takeda, Jos van Ommeren, Jiro Yoshida, and all participants of Tokyo Labor Economics Workshop and Urban Economics Workshop at the University of Tokyo. In obtaining the dataset on apartment rents in Japan, I was supported by Joint Research Program No. 821 at CSIS, UTokyo (Real Estate Database provided by At Home Co. LTD). I was financially supported by JSPS Graint-in-Aid for JSPS Research Fellows (18J20441) and the Funai Foundation for Information and Technology. I am responsible for any remaining errors.

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1 Introduction

Despite extensive research, the efficacy of minimum wage as a redistributive policy tool is still a controversial issue (e.g., Card and Krueger, 1994, 2000; Neumark and Wascher, 2000; Dube, Lester, and Reich, 2010, 2016; Neumark, Salas, and Wascher., 2014; Meer and West, 2016; Cengiz et al. 2019; Clemens and Wither, 2019; Harasztosi and Lindner, 2019; Dustmann et al. 2020). Most studies on this topic focus on the extent of adverse effects on employment. However, minimum wages may have an impact on other dimensions of the economy, such as hours worked, price levels, and consumption schedules. Assessing minimum wages becomes an even more complicated task once these effects are also taken into account.

In this paper, I analyze the effectiveness of the minimum wage as a redistributive policy along a novel dimension – its impact on housing rents. To the best of my knowledge, this is one of the first studies to formally analyze this issue.[1]

This yields two important implications on the value of minimum wages.[2] First, if minimum wage hikes increase housing costs, it increases living costs for low-income households who are primarily renters while unintentionally benefiting higher-income homeowners. Since housing costs take up a large share of budgets for low-income households, measuring the price increase for housing is essential in evaluating the minimum wage as a policy to help the poor. The increased housing rents also imply that some benefits of minimum wage hikes are captured by homeowners, who are generally much wealthier than minimum wage workers. Thus, this study is informative about unintentional distributional impacts of a minimum wage.

Second, by developing a spatial equilibrium model à la Rosen (1979) and Roback (1982), I can measure the welfare effects of a minimum wage hike. I show that changes in housing rents are sufficient for revealing qualitative information on whether and how much a minimum wage change increases the utility of minimum wage workers. The basic idea is that if the minimum wage hike in a region is attractive, this region experiences increased housing demand leading to higher housing costs.[3] Notably, the cost and benefit captured by this approach is inclusive in the sense that the effects of minimum wages through various channels, such as employment and the consumption re-scheduling (e.g., labor-leisure choice), are incorporated. Moreover, under some restrictive assumptions, a quantitative (money-metric) welfare implication can also be obtained.

In light of the theoretical framework, I investigate the effect of minimum wage hikes on housing rents utilizing a natural experiment in Japan. I focus on a low-quality rental housing market, which is likely to be

[1] See section 5.2 for discussions on the recent independent studies econometrically estimating the impact of minimum wages on housing rents. At the journalistic level, the impact of minimum wages on housing rents has attracted attention and been discussed in informal ways (e.g., Greenblatt, 2015; Ozimek, 2015). In this paper, I first formalize and extend the theoretical arguments and then reveal the causal relationship between the two using Japanese data.

[2] To be precise, minimum wage hikes have no spatial effect if minimum wage increase affects every region in the same way. In reality, there are often local variations in minimum wage rates and even the uniform minimum wage can have heterogeneous impact due to regional heterogeneity.

[3] Numerous studies have shown that minimum wages affect spatial effects. I discuss this literature later in this introduction.

relevant for minimum wage workers, and exploit local minimum wage changes caused by a national policy change. I combine the event-study specification and the difference-in-difference (DD) specification to obtain the causal impact of minimum wage increase on housing rents.

I estimate that low-quality apartments experience around a 2.5-4.5% rent increase in response to a 10% minimum wage increase. I then show that these results are reasonably stable across specifications and there is no indication that pre-trends might drive the results. Finally, I show that there is some evidence that the rent increases are particularly concentrated on the lowest-quality housing units.

The positive effect on rents implies the increased housing demand, which, in turn, implies that minimum wage hikes induce a better local labor market for workers. Thus, the detrimental effects of minimum wage increases, such as unemployment, are not so large that they lower the utility of workers. Moreover, my model also allows me to present a quantitative welfare impact of minimum wages under some strong assumptions. In particular, if households spend about 30% of their budget on housing, the estimated elasticity implies that 10% increase in minimum wages makes workers as well-off as giving them transfers of 0.75-1.35% of the total earnings *even in the presence of the adverse effects of minimum wages.*

On the other hand, the positive impact on rents also implies an unintended distributional consequence. If a 10% increase in minimum wages leads to a 10% increase in earnings, around 7.5%-13.5% of the increased earnings go to the pocket of homeowners. Thus, as a policy instrument, minimum wages is not as redistributive as it is in the absence of housing cost adjustments. This is arguably not very large but still non-negligible. It would be informative in assessing the distributional consequence of minimum wage hikes.

This paper relates to several active strands of literature beyond that of minimum wages and unemployment. First, it contributes to the growing literature of evaluating minimum wages in a spatial framework (e.g., Cadena, 2014; Giulietti, 2014; McKinnish, 2017; Zhang, 2017; Ahlfeldt, Roth, and Seidel, 2018; Pérez, 2018; Monras, 2019; Dustmann et al. 2020). Like these studies, I analyze the impacts of a minimum wage using a spatial quantitative framework, but I introduce a novel focus on the role of the housing market in assessing the impacts of minimum wages. I theoretically argue that identifying the effect of minimum wages on housing rents is appealing. Moreover, housing rents are also informative in detecting the unintended distributional consequence for homeowners, which provides additional information on the effectiveness of redistribution through minimum wages.

This study also contributes to growing macroeconomic evidence regarding the effect of minimum wages on price levels (e.g., Aaronson, 2001; Aaronson, French, and MacDonald, 2008; Lemos, 2008; Fougère, Gautier, and Le Bihan, 2010; MaCurdy, 2015; Allegretto and Reich, 2018; Harasztosi and Lindner, 2019; Cooper, Luengo-Prado,and Parker, 2020). In general, these studies tend to focus on the impact of minimum wages on supplier's cost while investigating the effect on commodity prices. In contrast, my focus is on how the minimum wage hikes affect the housing market through the channel of housing demand. This question is practically

important because housing represents one of the largest expenditure categories for low-income households. Furthermore, there is a growing interest in local price levels and their welfare implications (e.g., Moretti, 2013; Diamond, 2016). In analyzing the impact of minimum wages on local prices, this paper is closest in spirit to Cooper, Luengo-Prado, and Parker (2020) showing the positive impact of minimum wages on local commodity prices in the United States. I estimate the impact of minimum wages on housing rents, which contribute greatly to local price levels but are not emphasized in Cooper, Luengo-Prado, and Parker (2020).[4]

Finally, this paper relates to the literature of evaluating public policies using housing rents (e.g., Brueckner, 1982; Cellini, Ferreira, and Rothstein, 2010; Kuminoff, Smith, and Timmins, 2013; Coate and Ma, 2017). I apply this approach to the novel setting of evaluating minimum wages. Moreover, my theoretical results could also be applied to policies other than minimum wages.

After completing the first draft of this paper, I discovered a number of studies written around the same time as my paper that estimate the impact of minimum wages on housing rents using data from different countries. Tidemann (2018), Agrawal, Ambrose, and Diop (2019), and Hughes (2020) analyze the United States. Tidemann (2018) obtains the negative impact on housing rents, while Agrawal, Ambrose, and Diop (2019) and this paper agree on the positive impact. Hughes (2020) also finds the positive impact but only in areas where housing supply is inelastic. There is, however, less agreement on the magnitude of the positive impact. Other than the US context, Ahlfeldt, Roth, and Seidel (2019) find that Germany's national-level minimum wage introduction in 2015 induced higher housing rents in regions where minimum wages bite more strongly, again qualitatively consistent with my findings. My empirical analysis contributes to this growing evidence by exploiting Japanese institutional settings. Also, my theoretical results might be helpful in understanding what these empirical results imply in terms of welfare. Section 5.2 discusses these papers in more detail.

This paper is organized as follows. In section 2, I present the model and derive several propositions to better understand the empirical results. In section 3, I describe relevant institutional details and empirical strategies to exploit a Japanese natural experiment. Section 4 presents the estimated causal impact of minimum wages on housing rents. Section 5 contains robustness checks and additional discussions. Section 6 concludes.

2 Theoretical Framework

2.1 Overview of Theoretical Results

My model shows that housing rents in a region increase if and only if the minimum wage increase improves the welfare of residents. The key intuition comes from the Rosen-Roback spatial equilibrium model. Suppose that a region increases its local minimum wage rate. If this policy change does not cause significant adverse

[4]More specifically, since my paper deals with the impact on low-quality rental housing, this paper is more concerned with the local price levels relevant for the poor. See Handbury (2019) for the evidence that the rich and the poor have different tastes and hence effectively face different local price indices.

effects, then the improvement in labor market condition for workers would cause their willingness-to-pay to live in this region to increase. This shifts up the demand in the housing market and raises the rents, as long as the housing supply is inelastic. On the other hand, if the distortions from a minimum wage hike cause relative large negative effects such as increased unemployment, the labor market becomes less unattractive for workers. Thus, they are willing to pay less to live in this region. This decreases the housing demand and lowers the rent under inelastic housing supply. In this way, the utility effects of minimum wages and housing rents have a tight connection at the spatial equilibrium. Moreover, under certain conditions, I can show that the increase in housing rents equals the money-metric welfare gain from minimum wage hikes. Note that this argument assumes that the change in the minimum wage is local, and so it applies to local minimum wage hikes such as state-level ones in the United States and prefecture-level ones in Japan.[5]

In the following section, I formalize this argument allowing for endogenous housing demand and supply. I derive the explicit formula for the response of housing rents to minimum wage increase and show that the qualitative welfare implication is correctly obtained when the assumption of free mobility is reasonable. I also clarify under what conditions the response of housing rents can convey quantitative (i.e., money-metric) welfare implications.

2.2 A Formal Argument

Consider a small region i. The amount of available housing in region i is denoted by $H(R_i)$, where R_i is the rental price per unit of apartment quality. I assume $H' \geq 0$ so that the supply function is upward-sloping. When $H' = 0$ for all $R_i \geq 0$, the housing supply is perfectly inelastic. This assumption is widely adopted in the literature because housing supply is mainly driven by stocks, but I do not impose it from the outset because some supply responses to housing rents may be expected in reality. Since the housing supply elasticity would be different in the short-run and the long-run, the function H might depend on the time horizon in consideration.

Let minimum wage workers be homogeneous with utility function $u(x, l, q)$, where x is the numeraire (non-housing) consumption, l is the amount of leisure, and q denotes the amount of housing consumption. The unit price of q is R_i.[6] Each worker is assumed to be infinitesimally small. I assume that $u_x, u_l, u_q > 0$ and that u is quasiconcave.

Workers face the risk of unemployment. Noting that the minimum wage rate may affect the unemployment, I assume that with probability $p(\underline{w}_i) \in [0, 1]$, workers can work at the minimum wage rate $\underline{w}_i$. On the other hand, with probability $1 - p(\underline{w}_i)$, workers cannot find a job and earn nothing. I do not restrict the sign of p',

[5] I make two remarks. First, a similar argument would work for a nationally uniform minimum wage as long as it heterogeneously affects different regions (c.f., Ahlfeldt, Roth, and Seidel, 2018, 2019). Second, the effect of minimum wages on housing rents in the empirical analysis might be more moderate than what my model suggests because every local governments experience some increase in minimum wages and it attenuates regional differences through affecting the outside utility level. In the subsequent empirical analysis, year fixed effects are expected to mitigate this concern.

[6] The simplest way to interpret q is to regard it as the amount of land consumption, where R_i is the unit cost of land.

implying that minimum wages can improve or worsen labor market conditions. In a competitive labor market, minimum wages reduce employment, while under monopsony, employment can increase. Here, I try to capture both situations in a reduced-form way. I assume workers receive M_e (M_u) of endowment when employed (unemployed). M_e and M_u can include the assets, taxes, unemployment insurance premium, and transfers such as unemployment benefits.[7]

If employed, workers can choose their preferred working hours. I normalize the price of private goods to be one.[8] Therefore, conditional on a worker finding a job, the budget constraint is $x + R_i q + \underline{w}_i l = \underline{w}_i T + M_e$, where x is the consumption of numeraire, l is the amount of leisure time, and T is the endowment of time. If she is unemployed, the budget constraint is $x + R_i q = M_u$. Note that $l = T$ in this case. Denoting the income by I, let $V^e(\underline{w}_i, R_i, I)$ and $V^u(R_i, I)$ be the indirect utility function when a worker is employed and unemployed, respectively. I denote the optimal leisure choice by l^* and the optimal housing consumption when employed (resp. unemployed) by q^{e*} (resp. q^{u*}).

Assuming that workers are expected utility maximizers, the expected utility can be written as

$$pV^e(\underline{w}_i, R_i, \underline{w}_i T + M_e) + (1-p)V^u(R_i, M_u). \tag{1}$$

Each worker maximizes (1) by choosing the location i. Then, in the equilibrium, the following migration equilibrium condition must hold for those living in region i:

$$pV^e(\underline{w}_i, R_i, \underline{w}_i T + M_e) + (1-p)V^u(R_i, M_u) \geq \bar{u} - \theta, \tag{2}$$

where $\bar{u}$ is the exogenous outside utility level and θ is an idiosyncratic parameter that affects the desirability of living outside region i.[9] Examples of the cost incurred to live outside region i include migration cost or the cost of living apart from their family and friends, which would be particularly relevant for workers born in region i. θ might also be an idiosyncratic preference toward living outside region i, in which case $\theta < 0$ and its absolute value describes the attractiveness of the life outside region i.

θ varies at the individual level and is distributed according to $G(\theta)$ with $dG/d\theta \equiv g \geq 0$. $G(\theta)$ represents the number of workers with the taste θ. g is the size of the migration response when the utility changes by one

[7] I make two remarks. First, M_e can be negative, implying the payment. Second, the Public Assistance (PA) in Japan might also be included in M_u. However, the PA involves a means test and welfare stigma. Thus, the take-up might be endogenous to minimum wages (see Appendix C.1). For simplicity, I do not explicitly consider the PA in the main analysis.

[8] Here, I implicitly assume that the good price is nationally determined and thus exogenous to each region. As shown Appendix A.3, the results remain the same even if local commodity prices depend on minimum wage rates.

[9] By rearranging (2) as $pV^e(\underline{w}_i, R_i, \underline{w}_i T + M_e) + (1-p)V^u(R_i, M_u) + \theta \geq \bar{u}$, I can also define θ as the desirability of living in region i and follow the same analysis to obtain the analogous results. One technical issue is that I should exclude θ in calculating the welfare because the utility from living in region i inclusive of θ is always equalized at $\bar{u}$ due to the assumption of small economy, preventing me from analyzing the welfare impact of minimum wages in region i's labor market. The exclusion might be plausible when θ is the moving cost of migrating into or out of region i because at the equilibrium, nobody moves and migration cost is unpaid. Note that even when θ is included as welfare, I can obtain qualitatively the same implications when $\bar{u}$ is increasing in the attractiveness of region i, which is the case when region i is not infinitesimally small.

unit, which can be interpreted as the reduced-form degree of friction in geographical mobility. To see this point, consider the case of free mobility. Since the utility should be equalized everywhere at the outside utility level $\bar{u}$, everyone has $\theta \simeq 0$. Now, suppose that the utility $pV^e(\underline{w}_i, R_i, \underline{w}_i T + M_e) + (1-p)V^u(R_i, M_u)$ marginally increases. In a small open economy under free mobility, infinitely many people want to live in region i since everyone has $\theta \simeq 0$, corresponding to the case of $g \simeq \infty$. On the other hand, in a closed economy nobody wants to move into or out or region i. This case corresponds to $\theta \simeq \infty$ for all people in region i and $\theta \simeq -\infty$ for all people outside region i. Since nobody has the intermediate θ in this case, the marginal migration response is zero, i.e., $g \simeq 0$. An intermediate g captures situations between these two extremes.

When (2) holds with equality, it defines the marginal worker who is indifferent between region i and the outside. I denote such worker's θ by $\overline{\theta}$. The population level in region i is now written as $N_i \equiv \lim_{\theta \to \infty} G(\theta) - G(\overline{\theta})$, where I assume that the first term is finite. In equilibrium, the following housing market clearing condition should also hold:

$$N_i \left(p(\underline{w}_i) q^{e*} + (1 - p(\underline{w}_i)) q^{u*} \right) = H(R_i), \tag{3}$$

The equilibrium conditions (2) and (3) determine two endogenous variables $(R_i, \overline{\theta})$.

Now, I show that measuring changes in R_i helps in understanding the welfare impact of minimum wages. Applying the implicit function theorem to (2) and (3) and rewritting terms using Roy's identity, I obtain

$$\begin{pmatrix} p\frac{\partial V^e}{\partial R_i} + (1-p)\frac{\partial V^u}{\partial R_i} & 1 \\ N_i(p\frac{\partial q^{e*}}{\partial R_i} + (1-p)\frac{\partial q^{u*}}{\partial R_i}) - H' & -g\left(pq^{e*} + (1-p)q^{u*}\right) \end{pmatrix} \begin{pmatrix} dR_i \\ d\bar{\theta} \end{pmatrix} = \begin{pmatrix} -\left(p'(V^e - V^u) + p\frac{\partial V^e}{\partial I}(T - l^*) \right) \\ -N_i \left(p\frac{dq^{e*}}{d\underline{w}_i} + p'(q^{e*} - q^{u*}) \right) \end{pmatrix} d\underline{w}_i, \tag{4}$$

where $\frac{dq^{e*}}{d\underline{w}_i} = \frac{\partial q^{e*}}{\partial \underline{w}_i} + \frac{\partial q^{e*}}{\partial I} T$.[10] From (4), the response of the rents R_i can be expressed as follows:

$$\frac{\partial R_i^*}{\partial \underline{w}_i} = \frac{\left(p'(V^e - V^u) + p\frac{\partial V^e}{\partial I}(T - l^*) \right) + \frac{N_i}{g}\left(p\frac{dq^{e*}}{d\underline{w}_i} + p'(q^{e*} - q^{u*}) \right)}{\left(p\frac{\partial V^e}{\partial I} q^{e*} + (1-p)\frac{\partial V^u}{\partial I} q^{u*} \right) + \frac{H' - N_i(p\frac{\partial q^{e*}}{\partial R_i} + (1-p)\frac{\partial q^{u*}}{\partial R_i})}{g(pq^e + (1-p)q^u)}}. \tag{5}$$

The denominator is positive in sign unless housing consumption responds quite negatively to the increase in rents. I assume the regularity of the model by imposing that the denominator is positive.[11] Note also the $H' \simeq \infty$ (i.e., perfectly elastic housing supply) implies (5) = 0. This is intuitive as the housing price equals the marginal production cost under perfectly elastic supply, independently of the demand side.

The first term of the numerator captures the benefit of the minimum wages in the labor market, which I am interested in. The term $p\frac{\partial V^e}{\partial I}(T - l^*) > 0$ captures the effect that existing workers enjoy more earnings.

[10] Note that $\frac{dq^{u*}}{d\underline{w}_i} = 0$ since the wage rate does not affect the decision of unemployed workers.

[11] Formally, the regularity condition is violated when $\frac{-N_i(p\frac{\partial q^{e*}}{\partial R_i} + (1-p)\frac{\partial q^{u*}}{\partial R_i})}{g(pq^e + (1-p)q^u)}$ is negative and large in absolute value so that it dominates the other two positive terms in the denominator.

The term $p'(V^e - V^u)$ captures the employment effect. Assuming $V^e - V^u \geq 0$ (i.e., the indirect utility is larger when employed), it is negative if $p' < 0$ so that minimum wages cause unemployment, which is the case in the competitive labor market. Thus, the housing rents can respond negatively if the unemployment effect is significant. If $p' \geq 0$, so that minimum wage increases do not increase the unemployment rate, it is positive because of the increased employment. This can happen in a monopsony model.

In line with the Rosen-Roback logic, the first term in the numerator shows that better local labor market outcomes are associated with higher housing rents. The second term of the numerator, capturing the effect of minimum wages on housing demands, confounds the relation. This term is ambiguous in sign because minimum wages may or may not increase the housing demand in a region. Conditional on employment demand for housing increases as due to increased earnings. [12] If minimum wage hikes decreases employment, housing demand for the newly unemployed decreases. The welfare impact inferred from housing rents is over-estimated if the first effect is dominant, and vice versa.

Under free mobility ($g \simeq \infty$), the ambiguity of this effect goes away. Intuitively, the rent change must ensure that the utility of workers must be exactly compensated due to free mobility. The change in rents adjust to keep the individual utility the same as before, irrespective of how individual housing consumption responds. Due to continuity, the assertion also holds when g is sufficiently large. Summarizing the above arguments, I obtain the following proposition:

Proposition 1. *Suppose that the economy is sufficiently close to the case of free mobility. Then, housing rents increase in response to the minimum wage increase if and only if the minimum wage increase improves the welfare of workers.*

Importantly, the cost and benefit captured by Proposition 1 are inclusive of various margins where minimum wages might matter. In this model, it includes employment effects and re-optimization of the labor-leisure choice in response to the new wage rate. In making migration decisions, workers only care about a single number: the welfare level of living in a region, and therefore have taken into account any possible effects minimum wage hikes might have. The housing rents capitalize such welfare effects and Proposition 1 provides a method to measure such inclusive benefits of minimum wages. Applying this logic, one can also include other effects of minimum wages and obtain similar conclusions.[13]

While Proposition 1 is a qualitative result, a quantitative welfare implication can also be obtained under more restrictive assumptions. Suppose first that $q^{e*} = q^{u*} = q^*$ where q^* is fixed so that everyone consumes the same amount of housing. This assumption makes the second term of the numerator zero. Also, assume either that (i) $H' = 0$ so that housing supply is perfectly inelastic or (ii) $g \simeq \infty$ so that there is no migration friction.

[12] Hughes (2020) recently finds that minimum wages hikes increase workers' housing consumption in the US.

[13] Other effects might include the effect on non-wage benefits (Clemens, Kahn, and Meer 2018).

Such assumptions make the second term of the denominator zero. Then, from (5),

$$q^* \frac{\partial R_i^*}{\partial \underline{w}_i} = \frac{p'(V^e - V^u) + p\frac{\partial V^e}{\partial I}(T - l^*)}{p\frac{\partial V^e}{\partial I} + (1-p)\frac{\partial V^u}{\partial I}}. \tag{6}$$

The denominator of (6) is the (expected) marginal utility gain from receiving a dollar. Thus, (6) shows the welfare impact of the minimum wage hike in unit of the dollar.[14] Noting that $q^* \frac{\partial R_i^*}{\partial \underline{w}_i}$ is the response of total housing spending in response to minimum wages, I obtain the following corollary:

Corollary 1. *Under the assumptions validating expression (6), the response of housing spending equals the money-metric welfare impact of minimum wages.*

The assumptions behind Corollary 1 are stringent. However, since it makes the response of housing spending the sufficient statistic for welfare analysis, I invoke Corollary 1 to talk about the quantitative welfare impact of minimum wages.[15]

Note that when both $H' = 0$ and $g \simeq \infty$ are violated, the second term of the denominator in (5) is positive while the second term of the numerator is still zero. In this case, the money-metric welfare gain is underestimated when the welfare effect is positive, and the welfare loss is overestimated when the welfare effect is negative. This paper presents an empirical result of the positive impact on housing rents, implying that the true money-metric welfare impact is larger than my estimate suggests.

Appendix A provides extensions to see how the interpretation of my empirical results might be modified by including additional factors. First, Appendix A.1 discusses migration responses, which have been analyzed in the literature (e.g., Cadena 2014; Giulietti 2014; Monras, 2019). Second, Appendix A.2 introduces inter-regional commuting (e.g., McKinnish 2017; Ahlfeldt, Roth, and Seidel. 2019; Dustmann et al. 2020). Third, Appendix A.3 considers the effect of local inflation. Finally, Appendix A.4 generalizes the model with heterogeneous workers and derive the generalized formula for the rent gradients in the form of a weighted sum (or a weighted average in important special cases).

3 Background and Empirical Strategies

Following the theoretical framework, I investigate the causal effect of a minimum wage increase on housing rents using a Japanese policy reform. I first describe the Japanese minimum wage system and a Japanese low-quality rental housing market. I then introduce my empirical strategy.

[14] When $q^{e*} \neq q^{u*}$ but the expression (6) is valid, $\partial R_i^* / \partial \underline{w}_i$ shows the welfare impact in unit of a lottery that pays out $\$q^{e*}$ if a worker is employed and $\$q^{u*}$ if unemployed.

[15] Note, however, that Corollary 1 is about marginal minimum wage changes. See Chetty (2009) for methods and qualifications about obtaining welfare impacts of non-marginal policies from the assessment of marginal policy changes.

3.1 Minimum Wages in Japan

Japan has forty-seven prefectures and each has a different minimum wage rate. There is no difference in the minimum wage rate within a prefecture.[16] In particular, unlike the United States, there is no city-level minimum wage.

A major endogeneity concern over determining the causal relationship between the wage level and housing rents arises if policymakers have discretionary power over minimum wages. For instance, a local government might raise its minimum wage as policy instrument to support the poor if housing costs are increasing. This could upwardly bias the estimate of housing rents on minimum wage changes. Japan is an appealing context since in the relevant period, local policymakers have limited discretionary power over the minimum wage.[17]

First of all, the minimum wage setting in Japan is highly centralized and unresponsive to trends in local housing markets due to institutional features. Japanese prefectural minimum wages are determined by the following process. First, the central government classifies prefectures into four categories, and it assigns the targeted amount of minimum wage increase to each category. The categorization is reviewed only once every five years and changes in the classification are rare. This crude classification implies that it is difficult for the central government to fine-tune minimum wages in response to prefecture-specific trends. Given the targeted rate, local governments can make adjustments to the minimum wage increase. A concern for the endogeneity is that local governments may take into account trends in housing markets. However, in practice, the targeted rate explains almost all variations of the actual minimum wage increase and local economic factors have only a very limited impact (Tamada, 2011). In this sense, while there is a variation in minimum wage rates across prefectures, the Japanese minimum wage setting is similar to a centralized system and largely unaffected by the trends of local housing markets. See Appendix C.1 for further discussion.

Furthermore, I can exploit a quasi-experimental variation in local minimum wage rates. From 2007 to 2012, a new consideration took the primary role in setting minimum wages due to the national policy change.[18] As explained in detail by Hara (2017), Kawaguchi and Mori (2019), and Okudaira, Takizawa, and Yamanouchi (2019), after the revision of the Minimum Wage Law in 2007, the primary consideration in setting the minimum wage rate became closing the gap between the quality of life of minimum wage workers and people relying on Public Assistance (*seikatsu-hogo*, PA henceforth).[19] As a result, since 2007, prefectures with large pre-existing earnings gaps between minimum wage workers and PA recipients experienced larger increases in minimum

[16] Strictly speaking, different minimum wage rates are applied for some specified types of jobs, which Appendix C.1 explains in more detail. In this paper, I focus on the effect of the generally applied minimum wage rates.

[17] Appendix C.1 contains more details on Japanese minimum wage settings. See also Kambayashi, Kawaguchi, and Yamada (2013), Aoyagi, Genelli, and Tawk (2016), and Hara (2017).

[18] On the other hand, I am unaware of concurrent other policy changes that might affect low-quality housing markets in a spatially heterogeneous way.

[19] Appendix C.1 introduces the institutional features of Japanese PA.

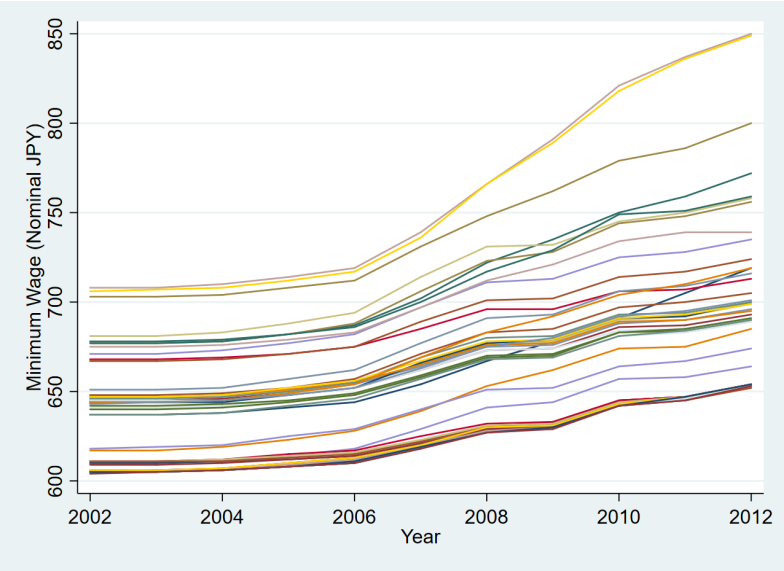

Figure 1: **Japanese Prefectural Minimum Wage Rates: 2002–2012.**
Notes: the figure shows the nominal minimum wage rates for each of 47 Japanese prefectures during 2002-2012.

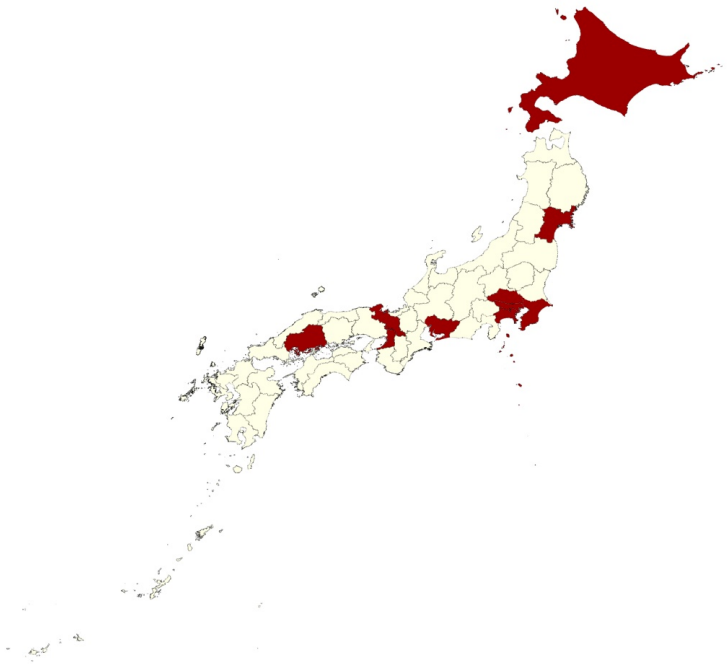

Figure 2: **Prefectures that Experienced the Largest Minimum Wage Increase.**
Notes: Prefectures that experienced top 10 largest minimum wage increase are colored, where the amount of the increase is defined as the nominal minimum wage rate in 2012 divided by that in 2006.

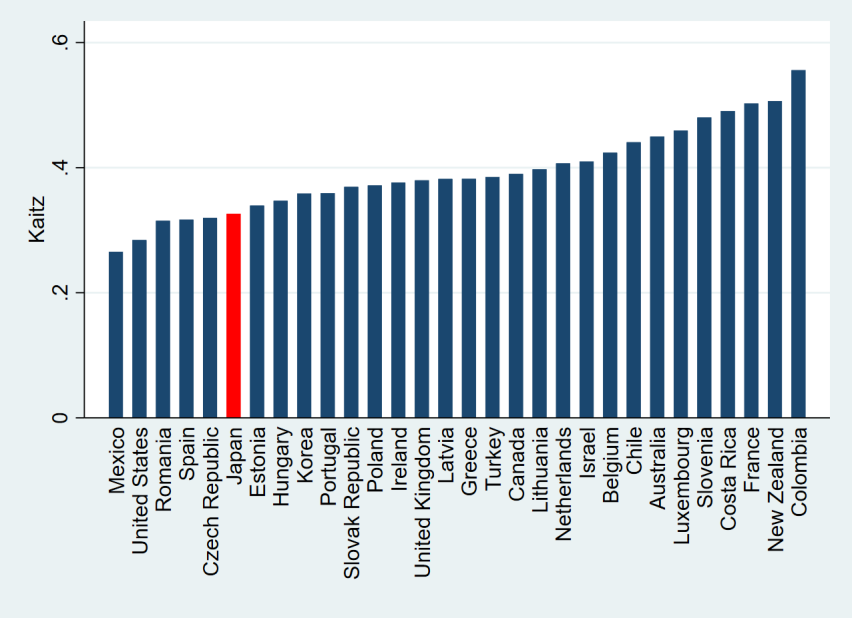

Figure 3: **Kaitz index of selected countries in 2010.**
Notes: Kaitz index is defined as the minimum wage level divided by the average wage of full-time workers. Source: OECD statistics (`https://stats.oecd.org/Index.aspx?DataSetCode=MIN2AVE`).

wage rates to ensure that minimum wage workers have higher income than those receiving the PA.[20]

Since the gap was generally larger in urban areas, the policy resulted in a plausibly exogenous minimum wage increase in urban prefectures.[21] Such a policy is likely to weaken the connection in policymaking between the minimum wage increase and the trend in housing markets, which in turn mitigates the endogeneity problem. Figure 1 shows the evolution of minimum wage rates during 2002-2012 by prefectures. Minimum wage rates were kept almost constant until 2007, but they started increasing since 2007 due to the policy change. The heterogeneous speed of the minimum wage increase across prefectures created a large variation in minimum wage rates.

Figure 2 maps prefectures that experienced top 10 largest minimum wage increase due to the policy change. They are mainly urban areas including the three largest urban employment areas (i.e., Tokyo, Osaka, and Nagoya). However, since they are geographically distant, each of them faces significantly different economic situations, which again mitigates the concern for policy endogeneity. Given the arguments so far, I treat 2007 policy change as a natural experiment that increased minimum wages in some prefectures.

To assess the external validity of my results, it is useful to see how high Japanese minimum wage rates were. Figure 3 shows the Kaitz index for Japan and other selected countries in 2010, which is an indicator

[20]Presumably, this policy change was unexpected and my empirical results are inconsistent with the hypothesis that markets respond prior to the policy change. See Nakakubo (2009) for backgrounds behind the revision.

[21]Kawaguchi and Mori (2019) confirm that the policy endogeneity about 2007 policy change is unlikely to be a concern in analyzing the effect on unemployment. Okudaira, Takizawa, and Yamanouchi (2019) also argue that explicitly accounting for the policy endogeneity does not change their main conclusions.

about the level of the minimum wage. The Kaitz index for Japan was comparable with countries such as Spain, but lower than many other countries. Therefore, I view that the analysis concerns a country with the relatively low minimum wage rate.[22] Note, however, that the index was higher than the US, which had the second lowest Kaitz index among countries in Figure 3.

3.2 Japanese Apartment Market and Data

I describe my data and some features of the Japanese housing market. I focus on a Japanese apartment rental market from October 1, 2002 to September 30, 2013. Denote $t = 2002, .., 2012$.[23]

The Japanese apartment rental market is mainly for non-rich people and thus suitable for detecting the effect of minimum wages on housing markets. Japanese rental markets are mainly for singles and couples because wealthier families with children typically own a house or an apartment unit. Moreover, Japanese market-based housing assistance program is much less extensive than the United States and the supply of public housing is limited. This implies that minimum wage workers participate in the rental market without subsidies.[24] In addition, the apartment market in Japan does not include buildings made of high-quality material.[25] Thus, the rental apartment market is likely to reflect the effect of minimum wages.

The dataset contains all apartments posted on At Home, which is one of the most popular online real estate search engines in Japan. At Home covers all prefectures in Japan and deals with a wide variety of rental apartments. However, since the dataset is not a random sample of all rental apartments in Japan, the representativeness relative to all stocks of rental housing may not be guaranteed. In particular, two types of housing units might be under-represented because they do not need to find a resident in an online search engine (So, 2017). First, some housing units are old and no longer seek new residents. Second, housing units that are significantly under-priced than the market price keep residents for a long time and find a new resident immediately without resorting to online search engines even if they become vacant. Arguably, both types of apartments are irrelevant for workers who are trying to rent in a new location simply because they

[22] Still, the minimum wage was binding. In 2009, which was in the middle of the minimum wage increase, 28% of part time workers had the wage rate less than 1.15 times the minimum wage rate (The Japan Institute of Labour Policy and Training, 2011).

[23] The maximum value is 2012 because Japanese minimum wage in year t is implemented from around October and is effective until September of year $t+1$. Thus, I call an observation during October 1 of year t and September 30 of year $t+1$ as an observation in year t. Strictly speaking, there is a slight timing difference in the implementation across prefectures, but I do not consider such difference because minimum wage rates is already determined by October 1. Moreover, since the data is about posted advertisement, the actual transaction must occur with some time lag. Thus, it is likely that advertisers anticipate the market conditions at the time of the transaction in posting advertisements. Appendix C.2 explains why I focus on this sample period.

[24] Only the unemployed are applicable to the housing assistance program, called *jutaku kakuho kyu-fu kin*. This program was not very well-known and likely to be underutilized. Moreover, the length of the program was three months and could not be extended for more than nine months.

[25] In Japan, high-quality apartments that use better building material are called "mansions" and they are explicitly distinguished from what are called "apartments" in Japanese. "Apartments" in Japanese are apartments (in the sense of the English word) that are of lower quality than "mansions". Note that the word "mansion," which originates from English, has a different meaning in Japanese. In the main analysis, I focus on housing units that are called as "apartments" in Japanese since I am interested in a low-quality housing market to capture the effect of minimum wages. See Appendix C.5 for the analysis of the mansion market.

	Mean	Standard deviation	Median	90th percentile
Monthly rent (Nominal JPY)	55667.29	16012.14	54000	75000
Apartments' age	13.97	8.68	14	25
Square footage (m^2)	34.00	14.05	31.05	54
Fraction of wooden buildings	0.535	-	-	-
Fraction of light gauge steel buildings	0.324	-	-	-
Observations	5696368			

Table 1: **Summary Statistics**

are unavailable in the market. As my theoretical model tries to capture the determination of housing rents by market demand and supply conditions, At Home data might be more relevant for my purpose than data on all stocks of rental housing in Japan.[26]

For each apartment unit, the dataset records the posted monthly rent, the prefecture in which the apartment is located, the floor on which it is located, construction year, square footage, and the material of which the building is made. Utility costs are not included in the rent. The unit of the observation is the posted advertisement and so the rent may not necessarily equal to the contracted price. However, the posted price should be a close proxy for the contracted price because the former plays a very important role in determining the latter.[27] To mitigate the influence of misrecorded values, I delete certain outliers and eliminate duplicate observations in the raw data to the extent possible (see Appendix C.2 for details). Summary statistics are provided in Table 1.[28]

While the apartment market at issue can be regarded as a low-quality housing market, we might expect that the impact of minimum wages is more salient in lowest quality apartments that are likely to be occupied by minimum wage workers. Therefore, it is interesting to seek a way to identify the quality of an apartment unit even within a low-quality housing market. Unfortunately, I do not have data on renters' characteristics because the observational unit is each apartment advertisement posted on the online real estate search engine. Thus, I cannot directly observe which apartment units are occupied by minimum wage earners and are more likely to be affected by the minimum wage.

However, a unique feature of Japanese apartments might be helpful in detecting a low-quality apartment. Specifically, I exploit the fact that Japanese apartments depreciate very fast compared with those of other

[26]Ambrose, Coulson, and Yoshida (2015) construct a rent index using new contracts with new tenants that better reflect the contemporaneous market conditions. Advertisements on the online search engine are conceptually close to the new contracts with new tenants, where the advertisements describe the rents just before the negotiation between owners and renters.

[27]Anecdotally, the negotiation between homeowners and renters is not very active in Japan. Moreover, in analyzing income segregation in Tokyo, Tabuchi (2019) finds that qualitative implications are very similar regardless of whether he uses data from an online search engine or transaction data of land and buildings.

[28]Note that the monthly rent is sufficiently low that the apartments in this market are available to minimum wage workers. For example, the median rent is 54,000 yen. If one assumes 750-yen minimum wage, a full-time minimum wage worker monthly earn around $750 * 170 = 127,500$ yen. Given that the household income is likely to be larger and the actual wage rate is not exactly equal to the minimum wage, this housing market is likely to be relevant for minimum wage workers.

countries. Generally speaking, Japanese residential structures depreciate very fast. Quantitatively, Yoshida (2020) finds that residential properties annually depreciate around three to five times faster in Japan than in the United States. There are several possible explanations for the difference, but one of the most important reasons might be the building material. In Japan, most apartments are made of woods and light-gauge steel, which are cheap but not durable. More than 85% of the apartments in my sample are made of these materials. Legally, buildings of this variety are completely depreciated within 20–40 years.[29] Such buildings are not likely to go through major repairs and renovations, and they are typically demolished when they are sufficiently damaged. The absence of major renovations implies that most unobserved quality of the apartment, such as the quality of the interior, the exterior, infrastructure, fire and earthquake resistance, and sanitation, are accurately predicted by the age of the apartment. This feature is helpful in my identification strategy because the observable age of the building is a predictor of the unobserved quality of the apartment unit.

To confirm this point in my data, I conduct a simple regression analysis to predict housing rents.[30] The result shows that, even controlling for various observable characteristics, rent decreases by around 1% as the building becomes one year older. A 25-year-old apartment room, which is around the 90th percentile of my sample, is approximately 25% cheaper than a new apartment. This result confirms the conjecture that the age of the apartment is a strong predictor of housing quality.

Housing quality, which includes cleanliness, safety, and the appearance of the building, are generally deemed as normal goods. Given the large price difference by age of apartment, minimum wage workers are likely to sort into old apartments while non-minimum wage workers sort into new apartments. In the empirical analysis, after controlling for observable characteristics, I use apartment age as a proxy for the overall housing quality. In some specifications introduced in the next subsection, I allow for heterogeneous impacts of minimum wages according to the age of the unit. Note, that minimum wage workers do not necessarily belong to a poor household because many of them are secondary wage earners (Kawaguchi and Mori 2009). If this is the case, the residential sorting according to housing quality would not be salient. See Appendix C.1 for more discussions on the characteristics of minimum wage workers.

3.3 Empirical Strategies

Overview

I use two empirical specifications – an event study and difference-in-differences (DD). In both specifica-

[29] I exclude apartments made of reinforced-concrete, which are rare except in Okinawa prefecture, because they are more durable than typical apartments in Japan.

[30] The regression equation is

$$\ln(rent)_{ijt} = \delta age_i + \beta X_i + pref_j + year_t + \varepsilon_{ijt},$$

where X_i are apartment-level control variables, $pref_j$ are prefecture fixed effects, and $year_t$ are time fixed effects. X_i includes the square footage of apartment i and its square, which floor the apartment unit is on, and the material of which the apartment is made. I omit the detailed estimation results.

tions, I exploit that due to the policy change, some prefectures experienced a large minimum wage hike while others experienced only a small minimum wage increase. Both specifications capture the elasticity of housing rents with respect to minimum wages and so are directly comparable. As shown later, the different specifications produce similar impacts on housing rents.

The event study serves as a non-parametric way of inspecting the temporal structure of the treatment effect. For example, it tells me the treatment effect is gradual or instantaneous, and the effects accumulate over time or fade out. It also allows me to assess the presence of the pre-trend, a threat to the DD specification, in an intuitive way. After confirming that the event study specification indicates the plausibility of the simple before-after comparison as an approximation, I turn to the DD specification that estimates the time-invariant treatment effect of minimum wages on housing rents. It saves the degrees of freedom and is more precisely estimated using all available data.

Throughout all specifications, $pref_j$ denotes the fixed effect of prefecture j, $year_t$ denotes the year fixed effect of year t, X_i denotes the vector of characteristics of each apartment unit i, and ε_{ijt} is the error term where t represents year t. I assign a different i for the same unit across years so that j and t are uniquely identified by specifying i. X_i includes the apartment's age and its square, the square footage of the apartment i and its square, the floor the apartment unit is located on, and the material of which the building is made. All estimations are done by OLS. In every specification, I cluster standard errors at the prefecture level since the minimum wage, the policy of interest, varies at this level (Bertrand, Duflo, and Mullainathan, 2004).[31]

Event Study

I use an event-study design with continuous treatment variable in line with Ahlfeldt, Roth, and Seidel (2018, 2019):

$$\ln(rent)_{ijt} = \sum_{z \neq 2006} \delta_z (\Delta T_j \times I(t=z)_t) + pref_j + year_t + \beta X_i + \varepsilon_{ijt}, \tag{7}$$

where i is each apartment observed in prefecture j in year t and $I()$ is the indicator function.[32] ΔT_j, the treatment intensity defined as the minimum wage rate in 2012 divided by the 2006 minimum wage rate, so that prefectures that experienced a large minimum wage increase have the stronger treatment intensity. This way, (7) exploits the magnitude of the treatment.

The coefficients of interest are δ_z capturing the differential movement of housing rents in treated prefectures in year z. In the event-study specification, the treatment effect can be time-dependent. I set the base year to 2006 ($\delta_{2006} = 0$) since the treatment starts from 2007. The usual common trend assumption requires $\delta_z = 0$ prior to 2007, while we have no sign prediction from 2007 since the theory does not determine the sign of the

[31] The number of prefectures in Japan is 47, which is within the "safe zone" to use clustered standard errors (Angrist and Pischke, 2009).

[32] Since the minimum wage rate in year t is effective from October of year t to September of year $t+1$, I refer to observations during this period as those in year t.

treatment effect. Note that δ_z has the elasticity interpretation: when the minimum wage rate increases $x\%$ from 2006 to 2012 (i.e., ΔT_j increases by $0.01x$), the housing rent in year z increases by $x \times \delta_z\%$.

Difference-in-Difference (DD):

I estimate the following simple DD regression:

$$\ln(rent)_{ijt} = \delta_1 \ln(MW)_{jt} + pref_j + year_t + \beta X_i + \varepsilon_{ijt}, \tag{8}$$

where $year_t$ is the year fixed effects. $\ln(MW)_{jt}$ denotes the log of the minimum wage rate of prefecture j in year t, which can be regarded as the treatment intensity given that the assignment of Japanese local minimum wage rates during the sample period was exogeneous.[33] The parameter of interest is δ_1, the elasticity of the housing rents with respect to the minimum wage rate.

I have argued that minimum wage hikes are likely to have a larger impact on lower-quality housing, but (8) assumes the homogeneous effect for all apartments. To allow for the heterogeneity, I estimate the following equation:

$$\ln(rent)_{ijt} = \delta_1 \ln(MW)_{jt} + \delta_2 \ln(MW)_{jt} \times age_i + pref_j + year_t + class_i \times pref_j + class_i \times year_t + \beta X_i + \varepsilon_{ijt}. \tag{9}$$

I include age_i and the interaction of it with the minimum wage rate. δ_1 and δ_2 are parameters of interest. I call δ_1 the "primary effect," which indicates the effect for brand-new apartments. On the other hand, I refer to δ_2 as the "differential effect," meaning the heterogeneity of the effect of depending on apartment age. The total impact on an x-year-old apartment's rent in response to 1% minimum wage increase is $\delta_1 + \delta_2 \times x\%$. The equation (9) controls for prefecture-specific and year-specific effects of apartments' age to reduce concerns about endogeneity.[34] To control for the interaction effects of apartments' age without imposing functional-form assumptions, I define an indicator variable $class_i$ that can take three categorical values, namely $old, medium$, and new. I classify apartment i with $0 \leq age_i \leq 11$ as new, $12 \leq age_i \leq 24$ as medium, $age_i \geq 25$ as old.[35]

I also examine the sensitivity of my result to prefecture-year level controls. First, I control for the male and female log average wage rates in prefecture j at time t.[36] While controlling for the average wages allows me to better incorporate the relative significance of minimum wage rates and local economic conditions, it

[33]This treatment variable does not have to be interacted with the indicator variable for the treatment years because the variable $\ln(MW)_{jt}$ has already captured the temporal structure of treatments in Figure 1. Appendix C.4 estimates an alternative specification requiring such an interaction.

[34]In particular, the term $class_i \times pref_j$ is important because it mitigates the concern that apartments' age might be correlated with the treatment status due to the potential correlation between the age and the local economic conditions.

[35]These classifications are chosen given that wooden apartments and light-gauge steel framed apartments, which are the most common type of apartments in my sample, completely depreciate in around 25 years. Other classifications also lead to similar results.

[36]The data are taken from Basic Survey on Wage Structure, which reports the average total earnings and working hours in every June. I calculate the average wage rate from these variables and use year t's information to explain observations from October of year t to September of year $t+1$. The specification in (9) is more flexible than regressing the log of the Kaitz index on housing rents, as argued in Card and Krueger (1995).

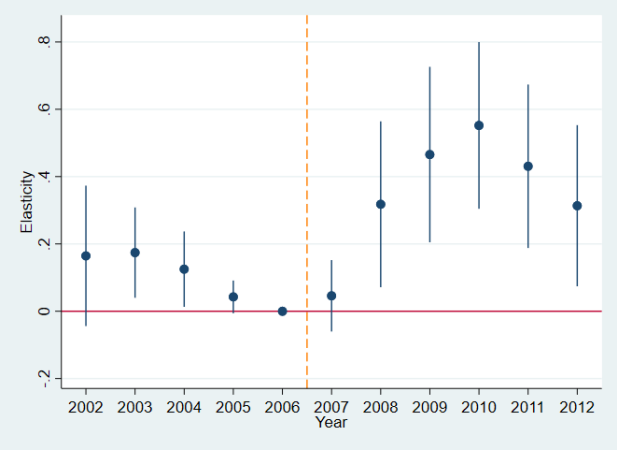

Figure 4: **Results from the Event-Study Specification.**
Notes: The regression equation is (7): $\ln(rent)_{ijt} = \sum_{z \neq 2006} \delta_z (\Delta T_j \times I(t = z)_t) + pref_j + year_t + \beta X_i + \varepsilon_{ijt}$, where i indicates each apartment unit, j indicates the prefecture, and t indicates the year. The figure plots the estimated coefficient and the 95% confidence of interval of δ_z in (7) for each year. The coefficient in 2006 is normalized to 0. The vertical dashed line separates coefficients before and after the policy change.

may introduce "over-controlling" because average wages are also affected by minimum wages to some extent. This issue is more serious for the inclusion of the female average wage because women are more likely to be minimum wage workers (Kawaguchi and Mori 2009). Thus, I report the results without controls, controlling for only the male average wage, and controlling for both male and female average wages. Second, I include the prefecture-year dummies. Essentially, this amounts to a difference-in-difference-in-differences (DDD) specification that utilizes the difference between the old and new apartments in each prefecture. Thus, the primary effect relevant for all apartments can no longer be estimated, but I can still estimate the differential effect δ_2.

In the DD specification where the treatment variable is $\ln(MW)_{jt}$, I implicitly account for the heterogeneous timing of the minimum wage increase.[37] The timing might be important as each prefecture annually experienced the minimum wage increase and the amount of the increase in each year has variations (see Figure 1). Alternatively, in Appendix C.4, I estimate an specification analogous to (7) but assuming the time-invariant treatment effect. While intuitive and transparent, this specification, like the event-study specification, ignores the fact that different prefectures might experience a minimum wage increase at a different timing.

4 Estimation Results

In Figure 4, I present the estimation result of the event-study specification (7). The figure shows the estimate of δ_z and its 95% confidence interval for each year.

The figure shows a positive causal impact on housing rents. Note that the coefficients after the 2008 pol-

[37] Note that $\ln(MW)_{jt}$ has already captured the temporal structure of the treatment in Figure 1.

icy change are all significantly positive. In particular, coefficients after 2008 are between 0.3 and 0.55. This implies a 10% minimum wage increase raises apartment rents by 3%-5.5%. The range is around 3%-4.5% when the largest estimate in 2010 is ignored. The coefficient in 2007 is slightly larger than zero but statistically insignificant. I interpret this pattern as plausible because the treatment effect may arise with some time lag. Indeed, migration intentions and the following demand changes in housing markets would need some adjustment time. Wheaton (1990), for example, shows that search frictions in the housing market might slow down the reallocations. Even in a non-spatial context, Meer and West (2016) demonstrate that the employment effect of minimum wage hikes takes some time to arise. These factors would prevent the realization of the treatment effect immediately after the policy change.[38]

As for the pre-trend, there is some indication of a *negative* trend. Indeed, the point estimates decrease steadily from 2003 to 2006. Thus, if in the absence of intervention the negative trend were to persist, the estimated positive impact might be understated. This result also suggests that the policy change was indeed an unexpected event: the coefficients might start increasing before the actual policy change was anticipated.

I now turn to the DD specification in (8). The results are reported in Table 2. In column 1, I show the result from the simple DD specification (8). It shows a statistically significant result that the rent increases by 2.6% in response to 10% minimum wage increase. Thus, even when I do not include the heterogeneity of the treatment effect, the specification (8) detects a positive impact on rents. This is plausible given that I have already focused on a low-quality apartment market and consistent with the event-study result in Figure 4.

In column 2, I estimate (9) without prefecture-year level controls. The primary effect (δ_1) is 0.16, which implies that a 10% minimum wage hike increases the rent of new apartments by 1.6%. The estimate is somewhat imprecise and indistinguishable from zero. The differential effect (δ_2), on the other hand, is positive and significantly different from zero at the 10% level. The coefficient is 0.0072, implying that x-year-old apartments experience a $0.072x$% larger increase than the new apartments in response to 10% minimum wage increase. However, since it is significant only at the 10% level, its robustness requires caution. I discuss this issue in section 5.1. I report the marginal effect of minimum wage hikes on old (25-year-old) apartments and very old (35-year-old) apartments . An old apartment experiences a rent increase of around 3.3% when the minimum wage increases by 10%, which is statistically significant at the 1% level. A very old apartment experiences an increase of around 4%, which is also significant at 1% level. Overall, the result reveals the larger impact on the rents of lower-quality apartments.[39]

In columns 3 and 4, I add the average wage rates as controls. Including them in the regression equation

[38]On the other hand, the large part of the effect is apparent after two or three years from the policy change, which some readers might find as surprising since the time lag is unexpectedly small. I speculate that this is partially because the policy change was roughly formula-based and it was easy to expect future minimum wage changes.

[39]These effects on the old and the very old apartments are statistically different from the estimated primary effect at the 10% level because the p-values exactly equal those for the differential impacts.

	(1) $\ln(rent)$	(2) $\ln(rent)$	(3) $\ln(rent)$	(4) $\ln(rent)$	(5) $\ln(rent)$
$\ln(MW)$	0.2624^{***}	0.1555	0.1878^{*}	0.1909^{*}	
(δ_1)	(0.0970)	(0.1144)	(0.1094)	(0.1095)	
$\ln(MW) \times age$		0.0072^{*}	0.0072^{*}	0.0072^{*}	0.0088^{**}
(δ_2)		(0.0038)	(0.0038)	(0.0038)	(0.0038)
Marginal effect (old)		0.3343^{***}	0.3680^{***}	0.3705^{***}	
($\delta_1 + 25\delta_2$)		(0.09686)	(0.09146)	(0.09265)	
Marginal effect (very old)		0.4058^{***}	0.4401^{***}	0.4424^{***}	
($\delta_1 + 35\delta_2$)		(0.1138)	(0.1093)	(0.1106)	
Prefecture-year controls	No	No	M average wage	M&F average wages	Dummies
Observations	5696368	5696368	5696368	5696368	5696368

Standard errors in parentheses are clustered at the prefecture level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: **The Effect of Minimum Wage on Apartment Rents**
Notes: The regression results from regression housing rents of each apartment unit on $\ln(MW)$, the natural logarithm of the minimum wage rate, and $\ln(MW) \times age$ (the interaction with the age of the apartment unit). The marginal effect for the old (very old) apartment is the estimated elasticity of the housing rents with respect to minimum wages for 25-year-old (35-year-old) apartment units. M (F) average wage stands for the male (female) average prefectural wage. The unit of observation is the apartment unit.

might help in distinguishing minimum wage effects and local economic conditions, while potentially introducing over-controlling problems. In column 3, I estimate (9) while controlling for the log of the male average income. I further add the log of the female average income in column 4. In both cases, the results are consistent with column 1. After including both terms, old apartments are estimated to experience around 3.7% increase in rents and very old apartments experience around 4.4% increase. In column 5, I replace the prefecture-year level controls with prefecture-year dummies to see the sensitivity of the differential effect to prefecture-year level confounders. The differential effect increases slightly: it is now 0.0088 instead of 0.0072 in specification (9), but it is not very different from the previous estimate. Overall, controlling for prefecture-year dummies does not substantially alter the results, which is reassuring because the influence of confounding factors seems limited.

From Proposition 1, the positive impact on rents implies the positive welfare impact for some minimum wage workers. I further investigate the quantitative welfare impact by assuming that Corollary 1 is valid. For an illustration, assume that a minimum wage worker spends 30% of his earnings on housing. Since my estimates suggest that 10% minimum wage increase raise housing rents by 2.5%-4.5%, it implies the increase in housing spending by 0.75%-1.35% of the total earnings. Thus, Corollary 1 implies that in terms of workers' welfare, 10% increase in minimum wages is equivalent to lump-sum transfers of 0.75%-1.35% of the total earnings. While Corollary 1 relies on strong assumptions, this might provide a useful number for policymakers. Note that the positive impact is inclusive of various margins such as unemployment. Thus, minimum wage hikes

indeed helped workers even in the presence of potential adverse effects.

On the other hand, the increase in rents also implies the unintentional benefit on homeowners. Assuming away any side effects of minimum wages, 10% minimum wage increase mechanically increases the total earning by 10%, which is the total benefit of the minimum wages for workers. However, the housing rent increases by 0.75%-1.35% of the total earnings. Thus, 7.5%-13.5% of the increase in total earnings unintentionally goes to homeowners.[40] While this number might be moderate, it still uncovers a neglected distributional consequence of minimum wages.

Note that the estimated results in different specifications are consistent with each other. In particular, the policy impact on housing rents implied by the estimated elasticity is close to what I get in the event-study design. Indeed, the event study results suggest the elasticity between 0.3-0.55. The range becomes 0.3-0.45 when the largest estimate in 2010 is ignored. On the other hand, the DD results suggest the range 0.25-0.45. Although the DD results imply a little smaller elasticity, both estimates are close to each other. I take the range 0.25-0.45 as a plausible estimate for the elasticity of housing rents with respect to minimum wages.

5 Further Robustness Checks and Discussions

5.1 Placebo Tests

I conduct placebo tests to check whether my results in Table 2 can be interpreted as describing the causal relationship. The most important concern for DD identification strategies is that the effect of minimum wages may erroneously reflect prefecture-specific trends rather than the true effect of minimum wages. In particular, if prefectures experiencing rapid minimum wage hikes have a particular type of the trend, the DD results are likely to be biased. While the graphical inspection of Figure 4 does not indicate the positive pre-trend, I take a more systematic approach in this subsection.

In conducting the placebo tests, I exploit the fact that during 2002–2006, minimum wage rates were kept almost constant (see Figure 1).[41] I assume that minimum wage rates were increased in this period just as they were increased during 2008–2012, during which minimum wages were significantly increased due to the policy change.[42] Then, I repeat the same DD analysis as in the previous subsection, replacing the actual minimum wage rates with hypothetical ones and limiting the sample to 2002–2006. Since the policy change in 2007 was not pre-announced, rents in this period would not be affected by the expectation of the subsequent minimum wage hikes. Thus, the estimated coefficient in this placebo test is expected to pick up the trends of prefectures

[40] If there are other adverse effects of minimum wages such as unemployment, the effective size of the benefit would be smaller than 10% of the total earnings. It makes the fraction of the benefit going to homeowners larger.

[41] The sample size in this period is relatively small because online real estate search engines were less popular during this period. As a result, some prefectures do not have many observations. The results are similar, however, if I drop samples from prefectures with few observations.

[42] Formally, for $t \in \{2002, 2003, 2004, 2005, 2006\}$, I replace $\ln(MW)_t$ with $\ln(MW)_{t+6}$.

	(1) $\ln(rent)$	(2) $\ln(rent)$	(3) $\ln(rent)$	(4) $\ln(rent)$	(5) $\ln(rent)$
$\ln(MW)$	-0.3371**	-0.4675	-0.4676	-0.4263	
(δ_1)	(0.1621)	(0.3186)	(0.3186)	(0.2602)	
$\ln(MW) \times age$		0.0088	0.0088	0.0087	0.0084
(δ_2)		(0.0109)	(0.0109)	(0.0109)	(0.0109)
Marginal effect (old)		-0.2474**	-0.2473**	-0.2101**	
($\delta_1 + 25\delta_2$)		(0.0990)	(0.9947)	(0.1114)	
Marginal effect (very old)		-0.1593	-0.1592	-0.1235	
($\delta_1 + 35\delta_2$)		(0.1225)	(0.1232)	(0.1553)	
Prefecture-year controls	No	No	M average wage	M&F average wages	Dummies
Observations	681386	681386	681386	681386	681386

Standard errors in parentheses are clustered at the prefecture level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: **Placebo Tests**

Notes: The regression results from regression housing rents of each apartment unit on $\ln(MW)$, the natural logarithm of the hypothetical minimum wage rate (i.e., the minimum wage rate after six years) while restricting the sample to apartment units during 2002–2006. $\ln(MW) \times age$ means the interaction with the age of the apartment unit. The marginal effect for the old (very old) apartment is the estimated elasticity of the housing rents with respect to minimum wages for 25-year-old (35-year-old) apartment units. M (F) average wage stands for the male (female) average prefectural wage. The unit of observation is the apartment unit.

with large minimum wage hikes, but not the effect of minimum wages since they were almost constant.

The results presented in Table 3 confirm that my results are unlikely to be driven by an increasing pre-trend. In column 1, I estimate (8) using the hypothetical minimum wage rates for the 2002-2006 period where there were no minimum wage increase. The estimated marginal effect is reassuringly non-positive, and more strikingly, it is significantly negative at the 5% level. The negative trend is also observed in the specification (9) reported in columns 2-4, with some statistically significant negative coefficients. This negative pre-trend is consistent with the graphical inspection of Figure C.2. It suggests that the positive effect on rents is not driven by a pre-trend, and it might be somewhat larger if one takes the potential negative trend into account.

Although the main conclusion that the increase in rents of low-quality apartments is unlikely to be driven by pre-trends, the placebo tests imply that the composition of the marginal effects might be affected by the presence of pre-trends because the differential effect in columns 2-5 is close to those estimated in Table 2. The estimates are quite imprecise and I do not view this result as the definite evidence that the differential effect is zero. Still, it raises a concern that the differential effect is actually closer to zero than estimated in Table 2, which is consistent with the view that minimum wage workers may not necessarily belong to a poor household and housing quality does not allow me to isolate housing units relevant minimum wage workers (see also Appendix C.1).

Taken together, the placebo tests generally reinforce my main finding that a minimum wage hike causes an increase in rents for low quality housing and if anything the impact is higher than estimated due to a negative

relative pre-trend for the treatment prefectures. However, I note that since the estimated differential effect is close to the pre-trend, new apartments in the sample might also have experienced increase in rents as much as old apartments.

5.2 Relation to Other Empirical Results in the Literature

Minimum Wages and Migration:

There is a potential concern that the positive impact on housing rents seems inconsistent with Cadena (2014) and Monras (2019) who find that in the United States, low-skilled workers "vote with one's feet" and move out of the states experiencing minimum wage hikes. Indeed, these results imply that a minimum wage increase is harmful for workers by significantly destroying jobs. The difference may come from the difference in institutional contexts or details of empirical specifications, as other studies document *positive* impact on the number of immigrants (e.g., Giulietti, 2014). That said, the positive response of housing rents may still seem somewhat contradictory to their results.

There are several ways to reconcile this tension of increased rents with the acceleration of out-migration. First, endogenous housing consumption might matter. In particular, suppose that minimum wage hikes increases housing demand, which has empirical evidence in the US (Hughes, 2020). Then, this can lead to a situation where a minimum wage hike improves worker welfare leading to a housing rent increase even as the net migration response is negative. Intuitively, housing rents increase with demand. On the other hand, since the amount of available housing in the short-term is fixed, the population must decrease as each individual consumes more housing (see Appendix A.1 for more discussions). Second, out-migration might be driven by additional factors not included in the canonical models. For example, unlike my model and the models of Cadena (2014) and Monras (2019), workers may have a chance to migrate after seeing whether they are employed or not. In particular, the unemployed people may be more likely to move to seek a job (Greenwood, 1985).[43] In such a case, the out-migration is likely to be more stimulated than these models predict. Third, I argue in Appendix A.4 that it is involved to obtain welfare implications from migration responses when workers are heterogeneous.

Exactly identifying why we may get the contrasting welfare conclusions by focusing on housing rents or migration is beyond the scope of this paper. However, note that the magnitude of the unintended benefit on homeowners provides a useful quantitative implications even if I discard the theoretical framework.

Recent Independent Estimates on Housing Rents from Other Countries:

In addition to this paper, four independent recent papers (Tidemann 2018; Agrawal, Ambrose, and Diop, 2019; Ahlfeldt, Roth, and Seidel, 2019; Hughes, 2020) estimate the impact of minimum wages on housing

[43]Other studies, however, suggest that the unemployed might not be particularly more mobile than others (e.g., Fendel 2014; Caliendo, Künn, and Mahlstedt, 2017).

rents. Three papers use data from the United States. First, Tidemann (2018) finds that the minimum wage significantly *decreases* housing rents. However, in an earlier version of this paper (Yamagishi, 2019), I show that a significant and positive estimate is obtained when samples are restricted to densely populated counties using the same data and specification as Tidemann (2018).[44] This result is broadly consistent with Hughes (2020), who finds that minimum wages have a small but positive impact on housing rents where housing supply is inelastic. Note that Japanese policy change I analyze induced the minimum wage increase mainly in urban prefectures such as Tokyo, where population density is high and the housing supply would not be very elastic. Thus, I view these results are broadly consistent with my finding.[45] Finally, Agrawal, Ambrose, and Diop (2019) find a large positive impact of minimum wage hikes on housing rents for newly-contracted leases in the US. While they obtain qualitatively the same results as mine, the magnitude is different. In their preferred estimate, housing rents increase soaks up 66% of the mechanical earnings increase, while I estimate the fraction to be 7.5-13.5%.

Ahlfeldt, Roth, and Seidel (2019, ARS henceforth) find a positive impact on rents by utilizing Germany's minimum wage introduction in 2015. Interestingly, because Germany has a uniform minimum wage rate, their identifying variation comes from the salience of the uniform rates across regions. This differs from this paper and the others in the literature that leverage variation in the minimum wage rate across space. ARS estimate that a 10 percentage point increase in the minimum wage salience is associated with 3% increase in housing rents. ARS's result is consistent with my Proposition 1 that the increase in housing rents implies the welfare improvement for workers. Indeed, ARS report that the unemployment marginally *decreased* by Germany's minimum wage introduction. My view is that ARS qualitatively reinforce the validity of my theoretical and empirical results from a different institutional setting. Regarding the magnitude of the impact, however, the direct comparison between ARS and my paper is difficult as the estimands are conceptually different.

Overall, while the consensus has not been reached, studies tend to confirm the positive impact on housing rents, at least in densely populated areas where housing supply is inelastic. The magnitude of the impact is more debatable. My empirical analysis, which is one of the earliest in the literature, contributes to this new discussion by presenting evidence from a Japanese quasi-experiment. Also, my theoretical results might be useful in connecting the estimates in the literature to welfare implications.

5.3 Supplementary Empirical Results

In Appendix B, I investigate the impact of a minimum wage increase on housing supply and migration, as these outcome variables are suggested to be important by the theoretical framework developed in section 2. In a

[44] Yamagishi (2019) also argues that the negative impact in Tidemann (2018) loses statistical significance when standard errors are clustered at the state level, the level of the identifying variation.

[45] Note also that studies using the US data might be likely to find no impact on housing rents because the US has exceptionally elastic housing supply in comparison with other OECD countries such as Japan (Caldera and Johansson, 2013).

nutshell, I find an increase in housing supply but somewhat complex effects on migration. However, the data is limited and the results should be viewed as only suggestive.

As an additional robustness check to the placebo test in section 5.1, I re-estimate my DD specification while controlling for linear time trends in Appendix C.3. I show that by controlling for the linear trend, the positive impact of the minimum wage hikes on rents is magnified, which is consistent with the negative pre-trend in section 5.1.

Finally, Appendix C.5 repeats the empirical analysis in a somewhat better-quality housing market that still overlaps with the low-quality market I have analyzed so far. I find some evidence of a positive effect, but it is smaller and more nuanced in terms of statistical significance, which is expected since a higher-quality housing market would be less relevant for minimum wage workers.

6 Concluding Remarks

I contribute to the debate over the effectiveness of the minimum wage as a redistributive policy from a new perspective: its effect on housing rents. It is valuable mainly for two reasons. First, since housing costs account for a large share of budgets, minimum wage workers are significantly harmed if housing costs increase in response to minimum wage hikes, although homeowners benefit. Second, changes in housing rents are informative about how minimum wage hikes affect workers' welfare, which I show in a spatial equilibrium model.

Based on the theoretical framework, I empirically investigate how much minimum wages affect housing rents using a Japanese natural experiment. In the low-quality rental housing market, 10% increase in minimum wage rates induces around 2.5%-4.5% increase in housing rents. This result implies that minimum wage hikes improve workers' welfare even when various effects of minimum wages, such as unemployment and labor-leisure rescheduling, are incorporated. Under some strong assumptions, I also quantify the welfare improvement for workers: 10% minimum wage increase is as good as receiving lump-sum transfers of 0.75-1.35% of the total earnings. The positive impact on housing rents also imply that minimum wage hikes unintentionally benefit homeowners. On the other hand, 7.5%-13.5% of the benefits go to homeowners. The figure is arguably moderate but non-negligible.

In my framework, minimum wages affect housing rents by changing the spatial equilibrium condition. While the variation in local minimum wages provided an interesting identifying variation, an important question is whether we should have differential local minimum wages in the first place. Especially, one might argue that local minimum wage rates should be equalized to eliminate spatial distortions and the unintended benefit for homeowners (i.e., adopting the national-level minimum wage). Interestingly, some media reported in March 2019 that Japanese central government had started discussing movement toward the nationally uniform

minimum wage rates to avoid distortions in migration decisions.[46] Unfortunately, it might also be spatially distortionary because even the uniform minimum wage rate can have heterogeneous impacts due to regional heterogeneity (c.f., Ahlfeldt, Roth, and Seidel, 2018, 2019; Dustmann et al., 2020). At the same time, decentralizing minimum wage setting can also involve distortions due to intergovernmental competition (c.f., Fukumura and Yamagishi, 2020; Simon and Wilson, 2020). Therefore, achieving the full efficiency would, in general, require the central government to impose the minimum wage rates tailored for each region, but finding the optimal rates with considering general equilibrium effects is a difficult task. It would require a carefully quantified spatial economic model, which is left for future research.

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Appendix (Not for Publication)

A Theoretical Extensions

In this Appendix, I extend the theoretical framework in section 2 in several ways. Except for Appendix A.1, I abstract from the endogenous housing consumption by assuming that everyone always consumes one unit of housing (i.e., $q^{e*} = q^{u*} = 1$).

A.1 Migration Responses

As the migration responses have been focused on to infer the qualitative welfare implication of minimum wages, it is interesting to compare housing rents to migration responses (e.g., Cadena, 2014; Giulietti, 2014; Monras, 2019). The idea is that in-migration (resp. out-migration) is induced if the minimum wage hike is desirable (resp. undesirable) due to the "voting with one's feet" mechanism. While this idea is useful and interesting, I show that unlike Proposition 1, migration responses might not yield a qualitatively correct welfare implication even if we are willing to assume the free mobility.

From (4), I have

$$\frac{\partial N_i^*}{\partial \underline{w}_i} = -g\frac{\partial \bar{\theta}}{\partial \underline{w}} = -\frac{\left(p'(V^e - V^u) + p\frac{\partial V^e}{\partial I}(T - l^*)\right)\left(N_i(p\frac{\partial q^{e*}}{\partial R_i} + (1-p)\frac{\partial q^{u*}}{\partial R_i}) - H'\right) + \left(p\frac{\partial V^e}{\partial R_i} + (1-p)\frac{\partial V^u}{\partial R_i}\right)\left(N_i(p\frac{dq^{e*}}{d\underline{w}_i} + p'(q^{e*} - q^{u*}))\right)}{\left(p\frac{\partial V^e}{\partial I}q^{e*} + (1-p)\frac{\partial V^u}{\partial I}q^{u*}\right) + \frac{H' - N_i(p\frac{\partial q^{e*}}{\partial R_i} + (1-p)\frac{\partial q^{u*}}{\partial R_i})}{g(pq^e + (1-p)q^u)}}$$

The first term of the numerator relates the migration response to welfare implications. This term is multiplied by the response of net housing supply to the increase in rents. An important special case is $H' \simeq \infty$, where the first term becomes dominant so that the migration response is tightly connected to welfare implications. Focusing on migration responses is thus appealing when the housing supply is very elastic. On the other hand, if the housing supply is completely inelastic ($H' = 0$) and the housing consumption does not respond to rents ($\partial q^{e*}/\partial R_i = \partial q^{u*}/\partial R_i = 0$), the migration response is completely unrelated with the welfare impact of housing rents.

The connection to welfare is contaminated by the second term of the numerator. Notably, the second term might drive down the number of people when minimum wage hikes increase the individual demand for housing. Thus, the outflow of workers might occur despite that the minimum wage is beneficial for workers. Intuitively, a region can accommodate less people when each individual consumes more housing space. Hughes (2020) finds that minimum wage workers demand more services in response to minimum wage hikes, suggesting the empirical relevance of this case.

The important difference from housing rents is that this second term does not disappear even when $g \simeq \infty$. Thus, the migration response is not guaranteed to yield qualitatively correct welfare implications even if we are willing to assume free mobility. The intuition is as follows. The number of people in the region is the available amount of housing divided by the amount of housing an individual demands. Therefore, the population level changes in response to minimum wage hikes when the individual demand is endogenous to minimum wages. However, since the amount of housing consumption each worker chooses is unrelated with the inter-regional mobility, imposing free mobility does not solve the issue.

A.2 Cross-prefectural Commuting

My main model ignores the commuting that crosses prefectures. In this subsection, I theoretically argue how my result should be modified in the presence of such commuting. To illustrate this issue in a simple case, suppose two prefectures: A and B. In prefecture A's labor market, some workers live in prefecture A while the others commute from prefecture B. I assume the inelastic housing demand in both prefectures. Let R_A and R_B be the respective housing rents. I also assume that there is no job in prefecture B so that all workers in prefecture B commute to prefecture A.[47] Commuting entails fixed time cost c, but commuters and non-commuters are otherwise in a equal condition in the labor market and firms do not present different offers to them. Finally, I assume the quasi-linear utility ($u(x,l,q) = x + v(l,q)$) to make the expressions simple.

Consider a minimum wage hike in prefecture A. For workers, there are three possibilities: live and work in prefecture A, live in prefecture B and commute to A, and live elsewhere and enjoy the outside utility ($\bar{u}$). For simplicity, let me assume that some workers have two options of living in prefecture A or living elsewhere, and the others have the options of living in B or living elsewhere. This allows me to assume away the migration condition between prefecture A and B (i.e., location choice within an metropolitan area) and focus on the effect of the inter-city migration condition.[48] Each worker has an idiosyncratic term (θ) for living elsewhere. By differentiating the indifference conditions, I obtain the following:

$$\frac{\partial R_A}{\partial \underline{w}_i} = p'(V_A^e - V_A^u) + p(T - l_A^*) \tag{10}$$

$$\frac{\partial R_B}{\partial \underline{w}_i} = p'(V_B^e - V_B^u) + p(T - l_B^* - c). \tag{11}$$

where V_A^* (resp. V_B^*) and l_A^* (resp. l_B^*) denote the indirect utility and the optimal leisure choice if a person lives in prefecture A (resp. B). e represents the employed case and u represents the unemployed case.

[47] This assumption eliminates an incentive for the unemployed in prefecture A to commute to prefecture B.

[48] Note that if the equilibrium outside utility is the same for both types of workers, the utility is equalized and workers have no incentive to move from one prefecture to the other.

These expressions provide some insights on the role of commuting. First, suppose that there is no unemployment effect $p' = 0$. Intuitively, the rent in prefecture A should increase more as it "directly" benefits from the minimum wage hike, while prefecture B benefits only through commuting. This is indeed the case: (10) $>$ (11) when $p' = 0$ and $l_A^* < l_B^* + c$. This is because of the time cost of commuting. The monetary value of time endowment is $\underline{w}_A T$ for non-commuters but $\underline{w}_A(T-c)$ for commuters. As commuters have less effective time endowment, minimum wages, which increase the value of time, have a smaller welfare impact for commuters.

On the other hand, if the minimum wage hike worsens the labor market condition, the rent in A should fall more as it is the directly affected region. This can again be seen in (10) and (11). To observe this, note first that $V_A^u = V_B^u$ since the home production is the same in both prefectures. Moreover, $V_A^e > V_B^e$ since commuting and non-commuting workers are the same except that commuters have less effective time endowments. It implies that when $p' < 0$, $p'(V_A^e - V_A^u)$ is greater in absolute value than $p'(V_B^e - V_B^u)$ (i.e., the rent decrease is larger in prefecture A). Intuitively, as non-commuters benefit more from working, losing the job opportunity is more costly for them.

The total welfare effect of the minimum wage should be the sum of the commuters and non-commuters. In the current setting, the impact of minimum wage increase in A on the total welfare is

$$\frac{\partial R_A}{\partial \underline{w}_i} \times \#\text{non-commuters} + \frac{\partial R_B}{\partial \underline{w}_i} \times \#\text{commuters}.$$

Thus, in this example, ignoring commuters underestimates the true welfare improvement. The labor market improvement in prefecture A has a positive spill-over effect in the presence of commuting, and ignoring it leads to a conservative estimation of welfare improvement.

While drastically simplified, this situation is consistent with my empirical setting where urban prefectures experienced the relatively large minimum wage increase. Prefecture A is an urban area attracting commuters from suburban prefecture B. The above argument implies that when it experiences the minimum wage increase, it would also increase the housing rents in the suburb. This leads to a violation of the stable unit treatment value assumption (SUTVA). Since the rent of the control group also increases, the estimated impact using the DD strategy would underestimate the true impact on rents.

Of course, the reality is substantially more complicated than this simple setting. First, the labor market in prefecture B is assumed away in this example. Second, the commuting flow is bilateral in reality. Third, geography within a prefecture is ignored. In reality, places near prefectural borders would face lower cost of commuting. This introduces heterogeneity of apartment units within a prefecture in terms of the job opportunity available in other prefectures. I leave these issues for future work.

A.3 Local Commodity Prices

In the main analysis, the commodity price are determined at the national level and local minimum wages have no effect on it. However, it is plausible that minimum wages may affect the price of local goods through increasing the supply cost.

Here, I explore the case where minimum wages affect the local price of commodities. On top of the national good x_i, let y_i be the consumption vector of local goods and $\psi_i(\underline{w}_i)$ be the associated price vector. Then, (5) is modified as follows:

$$\frac{\partial R_i}{\partial \underline{w}_i} = \frac{p'(V^e - V^u) + p\frac{\partial V^e}{\partial I}(T - l^*) + \left(p\frac{\partial V^e}{\partial \psi_i} + (1-p)\frac{\partial V^u}{\partial \psi_i}\right)\frac{\partial \psi_i}{\partial \underline{w}_i}}{p\frac{\partial V^e}{\partial I} + (1-p)\frac{\partial V^u}{\partial I} + \frac{H'}{g}} \gtreqless 0. \tag{12}$$

As long as $\frac{\partial \psi_i}{\partial \underline{w}_i} > 0$, the third term in the numerator is negative, implying that local inflation makes the region unattractive and the housing rent decreases. The presence of this term tends to negatively bias the qualitative welfare implication in the labor market. However, the local inflation would also be an important negative aspects of minimum wage hikes. If one is interested in the total welfare impact of minimum wages, focusing on housing rents does not cause a trouble. In particular, the positive impact on rents uncovered in the paper suggests that the negative impact of the local inflation does not completely offset the benefit for minimum wage workers.

Minimum wages can induce local inflation because it raises the price of labor, which is needed for producing local goods. The above argument has considered local goods other than housing, but implicitly assumes that housing services do not utilize labor in production since the housing supply function H depends on minimum wages only through housing rents.[49] In practice, although housing is a very capital-intensive industry, it still requires some labor cost (e.g., carpenters). Naturally, the rental price of housing should rise when the marginal provision cost (e.g., the maintenance cost for existing structures) rises, which is increased by the minimum wage hikes. To the extent that this effect is strong, the positive impact on housing rents overestimates the welfare improvement.

In Appendix B, I show that new housing constructions positively responded to the minimum wage hikes. It might suggest that the increase of housing rents due to the marginal cost increase is of limited importance. To see this point, imagine the canonical demand-supply diagram. The increase of the marginal cost means the upward shift of the supply curve. However, as long as the demand curve stays the same, it implies the increase in rents and the decrease in the equilibrium quantity of housing. Then, we should expect fewer new housing constructions, although my analysis says the opposite. Rather, my results are consistent with the demand curve

[49]The similar argument holds if housing supply requires non-labor local goods because the marginal cost increases when non-housing local goods become more expensive by minimum wage hikes, which might ultimately be caused by the increased labor cost.

shifting upward, which means minimum wage hikes improve workers' welfare.

A.4 Workers' Heterogeneity

I introduce a model with minimum wage workers' heterogeneity. Using the general utility functions $u^k(x,l)$, for each k, the utility maximization problem leads to the indirect utility functions $V^{ek}(\underline{w}_i, I)$ if employed and $V^{uk}(I)$ if unemployed. The amount of the endowment when employed (M_e^k) and unemployed (M_u^k) are also heterogeneous. The expected utility of type k is written as

$$p_i^k V^{ek}(\underline{w}_i, M_e^k + \underline{w}_i T - R_i) + (1 - p_i^k) V^{uk}(M_u^k - R_i). \tag{13}$$

I define $EU_i^k \equiv p_i^k V^{ek}(\underline{w}_i, M_e^k + \underline{w}_i T - R_i) + (1 - p_i^k) V^{uk}(M_u^k - R_i)$, which is the expected utility of residing in region i. Let $G_i^k(\theta)$ be the number of type k workers in region i with the preference parameter not larger than θ, and let g_i^k be its derivative. g_i^k is large when the population of the group k is large and the group k is mobile. We obtain the following general rent function:

Proposition 2. *In response to marginal a minimum wage increase $\underline{w}_i$, the change in rents $\partial R_i / \partial \underline{w}_i$ can be written as $\sum_k (\omega_i^k \partial EU_i^k / \partial \underline{w}_i)$, where $\omega_i^k \equiv g_i^k / \left(H' - \sum_k (g_i^k \partial EU_i^k / \partial R_i)\right)$.*

Proof. Let θ_i^k be the idiosyncratic parameter for the group k affecting the attractiveness of living elsewhere. For each k, The migration condition is rewritten as

$$EU_i^k = \overline{u}^k - \overline{\theta}_i^k, \tag{14}$$

where $\overline{u}^k$ is the outside utility for type k, which is assumed to be exogenous. The marginal resident has the attachment level $\overline{\theta}_i^k$.

The housing market equilibrium condition is

$$N_i = H(R_i). \tag{15}$$

(14) and (15) define the equilibrium.

In the absence of changes in rents in response to a marginal minimum wage increase, (14) shows that $\partial \overline{\theta}_i^k / \partial \underline{w}_i = -\partial EU_i^k / \partial \underline{w}_i$, which translates into the population change $g_i^k \partial EU_i^k / \partial \underline{w}_i$ of type k workers. On the other hand, $\partial \overline{\theta}_i^k / \partial R_i = -\partial EU_i^k / \partial R_i$, implying that the population change of type k in response to the marginal increase of rents is $g_i^k \partial EU_i^k / \partial R_i$. Note that the population level $N_i \equiv \sum_k \left(\lim_{\theta \to \infty} G^k(\theta) - G^k(\overline{\theta}^k) \right)$

The differentiation of (15) with respect to $\underline{w}_i$ yields

$$\frac{\partial R_i}{\partial \underline{w}_i} \left(\sum_k (g_i^k \partial EU_i^k / \partial R_i) \right) + \sum_k (g_i^k \partial EU_i^k / \partial \underline{w}_i) = \left(\frac{\partial R_i}{\partial \underline{w}_i} \right) H', \tag{16}$$

which can be rewritten as

$$\frac{\partial R_i}{\partial \underline{w}_i} = \sum_k (\omega_i^k \partial EU_i^k / \partial \underline{w}_i), \tag{17}$$

where $\omega_i^k \equiv g_i^k / \left(H' - \sum_k (g_i^k \partial EU_i^k / \partial R_i)\right)$. ■

Proposition 2 shows that the gradient of the rent function can be expressed as a weighted sum of the changes in the expected utility of each group of workers. To facilitate interpretation, I focus on the special case where the housing supply is inelastic ($H' = 0$) and utilities are quasi-linear. The quasi-linearity ensures that the marginal utility is in monetary unit so that the marginal increase of EU is interpreted as the willingness-to-pay (WTP) to live in the region. These assumptions allow me to write the rent function as the weighted average of the WTP.

$$\frac{\partial R_i}{\partial \underline{w}_i} = \sum_k (z_i^k \partial WTP_i^k / \partial \underline{w}_i), \tag{18}$$

where $z_i^k \equiv g_i^k / (\sum_k g_i^k)$ and $\sum_k z_i^k = 1$. Noting that g_i^k reflects both the population size and the mobility of type k, I obtain the following corollary.

Corollary 2. *In response to a marginal minimum wage increase $\underline{w}_i$, the change in rents $\partial R_i / \partial \underline{w}_i$ can be written as $\sum_k (z_i^k \partial WTP_i^k / \partial \underline{w}_i)$. Thus, $\partial R_i / \partial \underline{w}_i$ is a weighted average of changes in WTP of each group. The weight for group k is larger when it has larger share of population and is more mobile.*

Since the weight is determined by g_i (i.e., the marginal change of the number of workers when the utility changes), the rent gradient tends to reflect large and mobile groups' WTP. This result can be intuitively understood in the following way. Suppose there are two groups, k_1 and k_2, and the minimum wage increase improves the welfare of group k_1, but it harms the welfare of group k_2 because unemployment is more detrimental to group k_2. Suppose also that group k_1's mobility cost is much lower than group k_2's. Then, the minimum wage increase induces massive inward migration of people of group k_1, while little migration response is induced for group k_2. Since a fixed housing supply implies constant population in the region, the inward migration of people of group k_1 should be offset by the increase in rents. That is, the amount of the increase should be determined so that it offset the increase of $WTP_i^{k_1}$, implying that it reflects the WTP of group 1.

An important implication is that, as long as housing supply is inelastic, the minimum wage hike must create the winner and the loser. For example, suppose that the "rich" minimum wage workers are secondary wage earners, while the poor workers are truly poor. Suppose the students are less likely to suffer from the unemployment because they may be able obtain financial support from other sources. Thus, even when both groups positively value minimum wage increase, the relative valuation differs. In this case, the poor workers are likely to suffer. The argument is summarized in the following Corollary:

Corollary 3. *The group with the lowest $\partial WTP_i^k / \partial \underline{w}_i$ experiences the decrease in utility due to the adjustment of rents. As a result, the population of type k decreases.*

Corollary 3 might be a rationale to focus on housing rents rather than migration responses. Corollary 3 implies that some group of workers might move out even when every group of workers benefits from the minimum wage increase.[50] If one focuses on housing rents, however, it always increases when everyone benefits from it.

Corollary 3 might also be helpful to understand the effect of minimum wages on relatively high-skilled workers who are not directly affected by the minimum wages.[51] To see this, suppose there are two groups of workers: L and H. The group H is high skilled and they are not affected by the minimum wage in the labor market, implying $\partial WTP_i^H/\partial \underline{w}_i = 0$. However, as long as the group L benefits from this (i.e., $\partial WTP_i^L/\partial \underline{w}_i > 0$), Corollary 3 suggests that the high-skilled workers suffer due to the increase in housing rents. As for the low-skilled workers, their welfare improves more than what the increase in housing rents suggest.[52]

B Housing Supply and Migration in Japan

In this section, I report suggestive evidence about the effect of minimum wages on housing supply and migration. In the main text, I have focuses on housing rents but I have not directly examined whether my theoretical framework captures the driving forces behind the responses of rents. Additional analyses on housing supply and migration responses might facilitate a deeper understanding of how minimum wage hikes affect the economy.

I begin with reviewing the theoretical predictions about housing supply and migration responses. As for housing supply, it is likely to be positively related to housing rents because the supply function is upward-sloping. Given the evidence that minimum wage hikes increase housing rents, the supply of rental housing is likely to be increased.[53]

The impacts on migration responses are more complicated. In the simplest case, migration responds positively to the desirability of minimum wages. However, when housing consumption is endogenous, I have argued in section A.1 that migration responses might be negative even when minimum wages are desirable for workers. Thus, in my model, both positive and negative migration responses are consistent with the positive response of housing rents.

I use prefectural panel data to investigate the effect of minimum wages on rental housing supply and migration responses. Unfortunately, detailed information on these outcomes is not available on an annual basis, and

[50] Such a problem might be avoided if one does not focus on a subgroup of minimum wage workers and analyze the total size of migration responses. However, in practice, it might be difficult to identify who is and is not affected by minimum wages, forcing researchers to focus on subgroups of minimum wage workers.

[51] The following argument assumes that the high skilled workers also participate in the same housing market (i.e., no market segmentation according to income).

[52] Formally, $\frac{\partial R_i}{\partial \underline{w}_i} = z_i^L \partial WTP_i^L/\partial \underline{w}_i < \partial WTP_i^L/\partial \underline{w}_i$, where L denotes low-wage workers. Thus, the increase in housing rents does not fully offset the benefit of minimum wage hikes in the labor market.

[53] A minimum wage increase may also induce the shift in the supply function itself by increasing construction and maintenance costs. It shifts up the supply function. See Appendix A.3 for more discussions on the shift of the supply function.

I use an aggregate measure of them. I obtain the annual number of newly constructed rental housing from the Survey of Building Construction Work Started, conducted by the Ministry of Land, Infrastructure, Transport, and Tourism. The data are available separately for private and public rental housing. Investigating the supply response of public housing is informative on how governments respond to housing demand shifts induced by minimum wages. The annual number of inward and outward Japanese migrants is obtained from the Basic Resident Register, collected by Ministry of Internal Affairs and Communications.

Since the responses of both migration and construction requires some time, I allow for some time lag for the effect to appear. More specifically, I use the minimum wage rate from October of year $t-1$ to September of year t to explain the number of newly constructed rental apartments during April of year t and March $t+1$. Similarly, I use such minimum wage rates to explain the level of migration during January t and December $t+1$. Accordingly, I use the average wage rates in year $t-1$ (at the time of minimum wage settings) as control variables. I collect the relevant data during 2003–2013 because the outcomes during this period are explained by minimum wage rates during 2002–2012.

My data does not allow me to identify the groups of rental housing or people most likely to be affected by minimum wages, which prevents me from investigating the heterogeneity of treatment effects. I instead estimate the following simple DD regression equation:

$$\ln Z_{jt} = \delta \ln(MW)_{jt-1} + \beta Y_{jt-1} + pref_j + year_t + \varepsilon_{jt}, \tag{19}$$

where Z_{jt} is the explained variable in prefecture j in year t and Y_{jt} is a vector of control variables. In this section, I control for the log of male average wage rates.[54]

To test the significance of the pre-trends, I also report the results of placebo tests. As explained in section 3.2, Japanese minimum wage rates are determined without referring to the error term in (19), but some effects might be driven by trends rather than the true effect of minimum wages. As seen below, however, the pre-trends are identified only imprecisely; hence, one should be careful about quantitative implications in this section. That said, even if the large fraction of effects is driven by trends other than changes in minimum wages, the correlations between minimum wages and the outcome variables are, at least to some extent, informative in interpreting my main results. Standard errors are clustered at the prefectural level.

The results are reported in Table B.1. Column 1 shows that a minimum wage increase induces the increase of newly constructed private rental housing. Of course, the share of new housing to the stock of all housing is small and the supply adjustment takes time. Still, the result indicates that housing supply is increased, which is consistent with the theoretical prediction.

Column 2 shows the supply response of public rental housing. It shows a much stronger positive response than in private rental markets. Japanese public housing is primarily targeted toward poor households, who are

[54]The results are similar if I drop this control variable or add the log of the female average wage rate.

	(1)	(2)	(3)	(4)
	$\ln(new\ private)$	$\ln(new\ public)$	$\ln(inflow)$	$\ln(outflow)$
$\ln(MW)$	4.100***	11.069***	0.351	0.893***
	(1.024)	(2.976)	(0.213)	(0.234)
Observations	517	505	517	517

Standard errors in parentheses are clustered at the prefecture level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.1: **New Construction of Rental Housing, and Migration Responses**
Notes: $\ln(MW)$ is the natural logarithm of minimum wages. The outcome variable in column 1 (resp. column 2) is the number of new constructions of private (resp. public) rental housing units. The outcome variable in column 3 (resp. column 4) is the number of Japanese in-migrants (resp. out-migrants).

	(1)	(2)	(3)	(4)
	$\ln(new\ private)$	$\ln(new\ public)$	$\ln(inflow)$	$\ln(outflow)$
$\ln(MW)$	1.049	0.562	0.400	-0.424
	(1.723)	(5.661)	(0.366)	(0.310)
Observations	188	186	188	188

Standard errors in parentheses are clustered at the prefecture level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: **Placebo Tests: New Construction of Rental Housing and Migration Responses**
Notes: $\ln(MW)$ is the natural logarithm of the hypothetical minimum wage rate (i.e., the minimum wage rate after six years). The outcome variable in column 1 (resp. column 2) is the number of new constructions of private (resp. public) rental housing units. The outcome variable in column 3 (resp. column 4) is the number of Japanese in-migrants (resp. out-migrants).

likely to be affected by minimum wage increase. The strong increase in supply might imply the possibility that governments might try to mitigate the increase in rents by supplying more public housing. Thus, it may be the case that governments implicitly consider redistributive policies and housing market responses as intertwined, although more direct evidence is needed to fully justify this interpretation.

Columns 3 and 4 report the effect on inward and outward migration, respectively. The results show that both inward and outward migration are promoted by minimum wage hikes, although the effect on the inward migration is marginally insignificant at the 10% level ($p \simeq 0.11$). Thus, taken literally, both inward migration and the outward migration are stimulated. I analyze the migration pattern in more detail later in this section.

To see if the results are contaminated by trends, I conduct the placebo test analogous to section 5.1. I confine the sample to the pre-treatment period (2003–2006) and use minimum wage rates during 2009–2012 to explain the outcomes.[55] If prefectures experiencing rapid minimum wage increase have a particular pre-trend, the placebo test is likely to capture it.

[55]That is, I use the minimum wage rate in year $t+6$ to explain outcomes in year t ($t = \{2003, 2004, 2005, 2006\}$).

The results in Table B.2 confirm that there are no statistically significant pre-trends. As to the estimates for construction, both private and public construction exhibit positive but much smaller point estimates. However, the large standard errors might imply that the causal effects revealed in Table B.1 might be smaller. As for migration responses, the effect on immigration cannot be statistically distinguished from the roughly estimated pre-trend. Although the statistical significance is limited, the result suggests that emigration might be relatively more strongly promoted by minimum wage hikes, consistent with Cadena (2014) and Monras (2019). As discussed in Appendix A.1, this pattern is consistent with the positive response of housing rents when housing consumption is endogenous.

Limitations of the analysis

While I believe that the analysis in this section is suggestive, the results should be taken with a grain of salt due to the limitations of the data.

Regarding the housing supply, note that the outcome variable I use is the number of new constructions. It does not necessarily correspond to the change in the total housing supply because it does not take into account the demolition of the existing housing units. As far as minimum wage increase affects the destruction of the existing units, the net impact on the housing supply might not be captured.

The data limitations are severer for the migration analysis. First, the data do not include non-Japanese people. Although the fraction of foreigners in Japan is smaller than other developed countries, this is unsatisfactory especially given that foreigners might be more mobile. Second, anecdotally, some people who expect to live in a place only temporarily (e.g., attending a college) do not report the movement to the government, leaving no record of moving in my data.[56] For example. in Appendix C.1, I argue that the migration decision of students might be affected by minimum wage workers. It is likely that some of students' movements are missing from my data. Finally, my data do not contain the characteristics of movers, preventing me from focusing on those likely to be affected by minimum wages.

These limitations imply that the results on housing supply and migration should be taken only as suggestive. I leave a more comprehensive analysis on these two outcomes for future research.

C Details and Further Robustness Checks on the Empirics

C.1 Further Institutional Details

More on the Determination of Minimum Wages:

Relying on Tamada (2011), I first explain the process in which prefecture-specific general minimum wages are determined in Japan. The basic timeline is that each year, the central government first provides a recom-

[56]Those who do not report the movement to the government within 14 days may be subject to a fine up to 50,000 Japanese yen. However, those who often go back to their original house and those who expect to live in the new place for a short period of time are exempt from reporting.

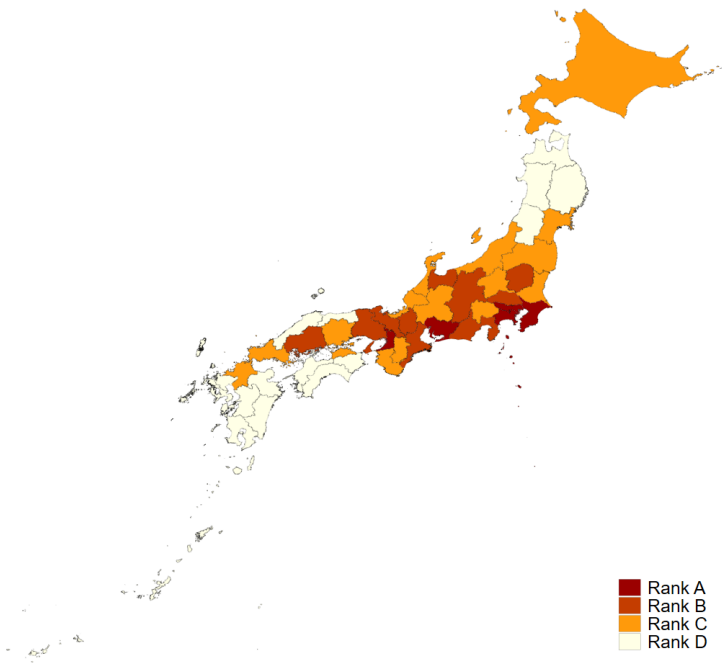

Figure C.1: **Classification of prefectures as of 2010**

mended level of minimum wage increase. Referring to the guideline level, each prefecture then determines the actual amount of minimum wage increase.

The central government classifies Japanese 47 prefectures into four ranks—Rank A, Rank B, Rank C, and Rank D—with the guideline increase for each rank. The classification, which is reviewed every five years, is made on the basis of twenty indices: five benchmark indices related to income and consumption, ten indices for salaries, and five indices related to business performances. All of these indices are evaluated at levels and weighted equally. Importantly for my purpose, no index is about the trend of local economy and no index is explicitly about the housing price, suggesting that trends in the housing market play a very limited role at this stage.[57] Prefectures are ranked according to the total index points and divided into four ranks. The Rank A region is the set of the highest index points, while the Rank D region corresponds to prefectures with the lowest index points. As of 2010, five prefectures belonged to Rank A, 10 prefectures belonged to Rank B, 13 prefectures belonged to Rank C, and 19 prefectures belonged to Rank D. Figure C.1 maps the classifications. While there are some concentrations, it shows that the classification is geographically mixed.

The regional (prefectural) councils set the final minimum wage levels. Although a regional council is not bound by the guideline increase when determining regions' actual minimum wage increase, in most cases, the recommended increase is adopted with only minor adjustments. For instance, in 2018, fifteen prefectures added one Japanese Yen per hour, eight prefectures added two Japanese Yen to the recommended amount of increase,

[57] Out of twenty indices, two indices on local price indices reflect housing rents only partially.

and the remaining twenty-four prefectures adopted the recommended amount as it is.[58] The discrepancy was slightly larger in my sample period since on top of considering the recommended amount for each class, local councils were expected to lift the level of minimum wages up to the benefit level of the public assistance (PA). Still, using the data from 2001 to 2010, Tamada (2011) finds that the correlation between the recommended amount of the increase and the actual increase is very close to one.

For my empirical analysis, it is important that minimum wage increase is unrelated with trends in the housing market.[59] Given that the rank classification is not based on housing market indicators, the claim seems reasonable. However, it might still be the case that prefectures in different ranks have different characteristics, thereby having differential trends in housing. Although my event-study result and placebo tests do not indicate differential pre-trends, in principle, this might threaten my identification.

To assess this concern, I look at the heterogeneity of prefectures across and within each rank. Table C.1 summarizes selected characteristics of prefectures for each rank.[60] For each rank, Table C.1 shows the mean value of a characteristic and a standard deviation within the class. Column 1 shows that the population density is strongly associated with the rank. However, the rank is not necessarily associated with economic characteristics. Columns 2 and 3 present the fraction of workers in manufacturing and service, respectively. In both cases, the mean value is non-monotonic to the rank. The non-monotonicity is also observed for the unemployment rate in column 4. Overall, while it is true that highly-ranked prefectures are more densely populated, it does not mean that economic conditions are systematically associated with the rank. This is reassuring as we could expect that the housing market trend would also be unrelated with the rank insofar as the economic trends in the local economy is unrelated.

The standard deviations in Table C.1 imply the significant variation within each rank. This is consistent with the presumption that the central government does not well consider the specific local conditions in recommending minimum wage increase. As the classification remains coarse, prefectures within each rank have considerable heterogeneity. This alleviates a concern for endogeneity that the central government takes into account prefecture-specific local conditions.

Having seen that the central government does not tailor the minimum wage increase for local trends, the remaining concern is whether the regional councils, the final decision makers, take into account the local trends. Tamada (2011) shows that this is unlikely to be the case. Using prefectural panel data, she regresses the minimum wage increase on the recommended amount of increase, the rate of increase in the average cost of living, the rate of increase in the relative regional consumer price index, and other control variables.

[58] The recommended amount this year ranged from 24-27 Japanese yen per hour.

[59] Note that prefecture-specific fixed effects absorb the unobserved permanent difference of prefectures. Thus, my DD-style empirical strategy requires that unobserved local trends are unrelated with minimum wage increases.

[60] I use the classification of prefectures in 2010. The data is taken from 2010 population census. All data are taken from e-Stat (`https://www.e-stat.go.jp/en`).

	Density ($/km^2$)	% Manufacturing	% Service	% Unemployment
Rank A	5321.48	22	68.76	6.18
	(3182.923)	(5.982892)	(5.111066)	(1.084896)
Rank B	1357.27	28.46	62.82	5.73
	(754.1843)	(4.281277)	(3.859418)	(0.5538753)
Rank C	885.3563	26.1375	65.05625	6.5125
	(386.6934)	(4.361938)	(3.938522)	(0.8452811)
Rank D	638.3125	21.7625	65.425	7.09375
	(218.57)	(3.50502)	(2.936551)	(1.414435)
All Prefectures	1373.591	24.70213	65.1	6.508511
	(1738.307)	(4.949242)	(3.959743)	(1.14471)

Standard deviations within each rank are in parentheses.

Table C.1: **Characteristics of Prefectures across Different Ranks.**

She finds that both the rate of increase in the average cost of living and the rate of increase in the relative regional consumer price index are statistically insignificant, implying that regional councils do not take into account changes in price levels. Thus, the housing cost increase would not affect the minimum wage increase. Moreover, as I discussed, the amount of adjustments is usually small.

Overall, the central government does not take into account prefecture-specific economic trends in recommending the amount of the minimum wage increase. It is true that the recommendation of the central government is associated with density, but market conditions in each prefecture do not seem to be associated with the recommendation. The local councils are also unresponsive to the local economic trends and closely follow the central government's recommendation. As such, trends in the local housing market would be unrelated to the amount of the minimum wage increase.

Characteristics of Minimum Wage Workers:

Kawaguchi and Mori (2009) document characteristics of minimum wage workers in Japan. Their main finding is that minimum wage workers do not necessarily belong to poor households: approximately half of them belong to an household with annual earnings of 5 million Japanese yen (approx. 45,000-50,000 US dollar) or more.

Their result might seem inconsistent with my theoretical model ignoring the possibility that the household a minimum wage worker belongs to might be non-poor. Unfortunately, since I do not have data on residents, I cannot fully take account of this possibility in this paper. However, a slight re-interpretation of my model might still allow me to incorporate such secondary wage earners.

To see this, I focus on teenagers and married women as they are two prominent groups that are likely to be minimum wage workers (Kawaguchi and Mori, 2009). First, consider teenagers who are considering renting their own room and starting working. Another option for them is to keep living off of their parents. For such teenagers, the outside utility ($\bar{u}$ in the model) corresponds to the utility from living with their parents. On

the other hand, if they choose to initiate a new household, they have to enter the labor market and obtain the (expected) utility to work as a minimum wage worker. In this interpretation, equation (2) is an indifference condition between these two options. Thus, when the labor market prospects become better due to minimum wage increase, more teenagers choose to rent a new apartment, hence driving up the housing rents.

Second, suppose women cohabiting with their partner. Since their current relationship is not very happy, they are considering breaking up and living apart from the partner by renting a new room. They should enter the labor market in this event to live off. Now, the outside utility ($\bar{u}$) is the utility from continuing the relationship, while they get the (expected) utility from the minimum wage job. Thus, equation (2) is the indifference condition between breaking up and not breaking up.

These illustrations suggest that my model might accommodate the above situations. In both cases, housing demand can rise if and only if the minimum wage improves labor market conditions for workers. Thus, the rise in rents is still associated with welfare implications, as shown in my main model.[61] Combined with minimum wage workers belonging to poor households, one could explicitly account for such heterogeneity of workers using the model in section A.4.

However, such a re-interpretation of the model has limitations. In particular, the above argument does not encompass married women who do not consider breaking up as an outside option. For such minimum wage workers, their primary choice is to work at the minimum wage or to focus on housework. However, choices between these two do not seem to involve a new demand for low-quality housing. Rather, since the household income rises, their housing demand might rise in a higher-quality housing market. In Appendix C.5, I show that in a higher-quality rental housing market, the positive response to housing rents might be observed although its significance is limited. As illustrated by this example of married women, such a positive response is expected if households minimum wage workers belong to are not necessarily poor.

A more complete analysis of the minimum wages on the housing market should take into account the entire market while utilizing the information on residents. Due to the limitation of data, this study focuses on a low-quality housing where the effect of minimum wages would be most salient.

Specific Minimum Wages by Industry:

In Japan, there are two types of minimum wages: prefecture-specific general minimum wages (*chiiki betsu saitei chingin*) and specific minimum wages by industry (*tokutei saitei chingin*). In this paper, I have focused on prefecture-specific general minimum wages. This minimum wage is applied to all workers with few exceptions. Specific minimum wages are also set at the prefectural level, but it applies only to certain industries. Which industry has the specific minimum wage is defined at a very granular level (JSIC 4-digit classifications) and differs across prefectures. Moreover, specific minimum wages often involve exceptions since they are meant

[61] This happens without cross-prefectural migration in the data. On the other hand, since housing rents are now bid up, migration inflow from outside would be curbed. This might explain the limited evidence on increased migration inflow in section B.

to be applied only to "core workers." For example, workers below 18 or above 65 are typically exempt from the specific minimum wage. Workers in a training period are also ineligible. Finally, workers engaging in basic tasks (e.g., cleaning) are typically ineligible. As minimum wage workers are generally low-skilled, such exceptions might be relevant. When a worker is eligible for both minimum wages, the higher minimum wage applies.

Due to these features, it is very challenging to assess the level of specific minimum wages. Following previous studies utilizing the policy change in 2007 (e.g., Hara, 2017; Kawaguchi and Mori, 2019; Okudaira, Takizawa, and Yamanouchi, 2019), I focus on the prefecture-specific general minimum wages.[62] While this is a limitation of my study, I suspect that the potential bias from this ignorance is minimal for two reasons. First, the coverage of the specific minimum wages is much smaller than the prefecture-specific general minimum wages: At the beginning of 2001, the former covered around 4.5 million workers while the latter covering 52 million workers (Kawaguchi and Mori, 2009). Indeed, Kambayashi, Kawaguchi, and Yamada (2013) state that during 1994-2003, any meaningful variation in minimum wage rates did not come from industry-specific minimum wages exactly because they cover only a very small fraction of workers. Second, the specific minimum wages are often increased in response to the general minimum wages increase, implying a parallel movement of both minimum wages. As long as they move parallel, the prefecture-specific general minimum wage is an accurate proxy for the minimum wage level that also accounts for the specific minimum wages by industry. Having said this, incorporating the specific minimum wages is left for future work.

Public Assistance (PA):

I first briefly describe the public assistance (PA) system in Japan.[63] The PA system is meant to secure the minimum level of living based on the 25th article of the Constitution of Japan. The recipients of the PA are secured the "minimum cost of living," which is derived from seven categories of expenses: livelihood, housing, educational, medical, maternity, occupational, and funeral expenses. The calculation of the minimum cost of living considers the number of children and adults, household members' ages, and residency area. The amount of assistance is calculated by subtracting the household's final income from the minimum cost of living. If the minimum cost of living exceeds the final income, the difference is given as assistance. Those eligible are required to fully exhaust their available resources such as financial support from family and relatives and the benefits are provided only if the minimum cost of living cannot be covered even after utilizing all resources. As such, benefits are provided only after a careful examination of the financial situation of applicants.

The PA is important for this paper because the policy change in 2007 is related to the PA. In the early 2000's,

[62] Kambayashi, Kawaguchi, and Yamada (2013) is an important exception in that they account for the industrial minimum wages (*sangyou betsu saitei chingin*), which is the old name of the specific minimum wage by industry, using the number of workers in the relevant industry as weights. This adjustment is made possible thank to an access to confidential governmental statistics, to which I unfortunately do not have an access. Note that even they do not consider exceptions to industry-specific minimum wages.

[63] The explanation relies on Yugami, Morimoto, and Tanaka (2017) and Hayashi (2019).

the benefit of the PA exceeded the earnings that minimum wage earners could earn (Abe and Tamada, 2007). Minimum wage increase since 2007 was meant to close this gap. On the other hand, the PA system did not experience a major change during this period so that no concurrent policy change took place. In particular, two major benefits of the PA, livelihood and housing assistance, experienced almost no change during 2005-2012.

A natural question is that why many minimum wage workers choose to work despite the generous PA program. While it is difficult to answer this question, many reasons are conceivable. First, the PA program imposes a very strict means test. Although all citizens have a right to claim the assistance, the person is required to use all available resources, including assets, ability to work, as well as assistance from those who are required to support the person by law. The standard is strict: for example, the recipients are, in principle, not allowed to own a car. Thus, even if a worker faces a temporal financial difficulty, he might not be eligible for the PA. Second, the PA involves a strong stigma and many people are hesitant to take up (Abe, 2003). Consequently, in 2007, the percentage of the PA recipients is 0.88% for people below 65 despite that receiving PA is more advantageous than working at the minimum wage.[64] Even among those eligible for the PA, the fraction of people who are not on the PA is estimated to be around 75%-90% (e.g., Komamura, 2003).

Thus, minimum wage workers seem to be hesitant or unable to take up the PA, even though the take-up rate should be 100% if they are eligible and only consider the economic advantages. This implies that the take-up decision of the PA might not be very responsive to the relative benefit level of the PA. Thus, minimum wage hikes are expected to have only minor impacts on the PA take-up rate. Indeed, using prefectural panel data, Abe and Tamada (2007) find that the ratio of the minimum wage rates to the benefit level does not affect labor supply.

However, one could still consider a situation where the PA take-up is endogenous to minimum wage rates. For instance, given the strong stigma associated with the PA, it is possible that the likelihood of taking-up the PA depends on the strength of stigma, which might in turn depend on minimum wage levels (c.f., Besley and Coate, 1992). For example, higher minimum wage rate might increase the stigma because taxpayers believe that minimum wage workers could live off without relying on the PA. However, since how welfare stigma is formed is unknown and I do not have data on the strength of welfare stigma, I abstract from this issue in this paper.[65]

[64] The percentage for the population above 65 is 2.25%, mainly because this population is more likely to unable to work. These numbers are taken from National Institute of Population and Social Security Research (`http://www.ipss.go.jp/s-info/j/seiho/seiho.asp`, in Japanese).

[65] One reduce-form way to model the endogenous welfare stigma is to assume that fraction $\eta(\underline{w}) \in [0,1]$ of all workers choose to rely on the PA. The others enter the labor market and the mass $p(1-\eta)$ of workers find the job while $(1-p)(1-\eta)$ of them are unemployed. This situation might be plausible if the life condition under the PA is better and people want to pick it up, but the welfare stigma, whose psychological cost is heterogeneous across people, prevents it. While the coverage (i.e., η) would be small in Japan, the response of housing rents might be greatly influenced by the PA if η' is large in absolute value.

C.2 Sample Selection Procedure

I focus on the data from October, 2002 to September, 2013, implying that $t = 2002, ..., 2012$. This time period includes five years before and after the policy change in 2007. I do not use the data with $t \geq 2013$ for several reasons. First, the policy change was intended to end at $t = 2012$ and the source of the minimum wage increase thereafter is unclear (Kawaguchi and Mori, 2019). Second, two important national policy changes may affect the market. The first change is the increase of the VAT in 2014. Since rental apartments are exempt from VAT, it changes the relative price between housing and other goods. The second change is the major revision on the amount of housing assistance to the PA recipients in 2015. On the other hand, during my sample period, the level of the housing assistance was nearly held fixed. Third, beginning from 2013, minimum wages were increased relatively parallelly across prefectures. Such a parallel increase is not likely to affect the cross-sectional spatial arbitrage condition and bias the elasticity estimates toward zero. I do not use data between January, 1999 and September, 2002 because there was some minimum wage increase during this period, which makes it inadequate to conduct the placebo tests. Moreover, the sample size in this period is small because online real estate search engines were not very popular in Japan.

Turning to the sample selection criteria, I delete observations with either (i) floor area smaller than $5m^2$ or larger than $80m^2$ (which is very rare in rental apartments in Japan), (ii) monthly rents higher than 150,000 yen, (iii) negative apartment's age, or (iv) missing values on the located floor or the building material. I also drop apartments made of reinforced concrete (RC) or steal reinforced concrete (SRC) to make sure that the apartments in the sample are not very durable. I also delete apartments older than 45 to ensure that my results are not driven by potentially mis-recorded extreme values. Since my dataset on rental apartments comes from raw data of the online search engine, some observations are re-posted multiple times when they are vacant for a long time. Since my dataset records all postings, a re-posted observation is counted as a new observation even though the same observation is already included in the dataset. To mitigate this concern, I randomly keep only one observation out of all repeated observations of the same unit within one year. Qualitative implications remain the same even when I work with the data without the above-mentioned data cleaning processes.

C.3 Prefecture-Specific Linear Trends

In this section, I re-estimate the results in Table 2 while including the prefecture-specific trends ($pref_j \times t$). As seen in Figure 1, Japanese minimum wages are adjusted gradually. Thus, including the linear trend may unintentionally absorb the true effect of the minimum wage hikes. Still, it is likely to alleviate the difference in trends while utilizing the full sample. Moreover, it may also alleviate the problem that all prefectures experience some increase in minimum wages. My model in section 2 assumes that the outside utility is fixed, but it may actually be endogenous and causes underestimation about the true positive impact. By controlling

	(1) ln(*rent*)	(2) ln(*rent*)	(3) ln(*rent*)	(4) ln(*rent*)
ln(*MW*)	0.7624***	0.6604***	0.6446***	0.6324***
	(0.2155)	(0.2038)	(0.2052)	(0.2079)
ln(*MW*) × *age*		0.0076*	0.0076*	0.0076*
		(0.0038)	(0.0038)	(0.0038)
Prefecture-year controls	No	No	M average wage	M&F average wages
Observations	5696368	5696368	5696368	5696368

Standard errors in parentheses are clustered at the prefecture level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: **Re-estimation of Table 2 with prefecture-specific linear trends.**
Notes: ln(*MW*) is the natural logarithm of the minimum wage rate, and ln(*MW*) × *age* is the interaction with the age of the apartment unit.

for the linear trend, the effects of minimum wage increase with small minimum wage adjustments are likely to be better absorbed, although year fixed effects also absorb this effect.

The results are reported in Table C.2.[66] Compared with Table 2, the primary effect is much larger. According to column 1, 10% minimum wage increase raises the apartment rents by 7.6%. This result is consistent with the negative pre-trend discussed in section 5.1. Indeed, the difference between the coefficient in the column 1 of Table 2 and that in the column 1 of Table 3 is approximately 0.6, which is close to the coefficient in Table C.2. The differential effect is slightly larger, but similar to the estimates in Table 2. As discussed in section 5.1, taking into account the presence of the trends seems to strengthen the positive impact on housing rents in the DD analysis. Note, however, the standard errors are larger than my main results and taking the point estimates literally might require additional caution.

C.4 Time-Invariant Treatment Effect under an Alternative Specification

In this section, I estimate a DD regression equation using an alternative treatment variable. Recall that in the event-study specification (7), the treatment intensity is measured by ΔT_j, which is the relative increase of minimum wage rates between 2006 and 2012. While the event-study specification allows for the time-dependent treatment effects, I assume the time independence here. The estimated equation is

$$\ln(rent)_{ijt} = \delta \Delta T_j \times I(t \geq 2007) + pref_j + year_t + \beta X_i + \varepsilon_{ijt}, \tag{20}$$

which replaces the treatment variable $\ln(mw)_{jt}$ in equation (8) with $\Delta T_j \times I(t \geq 2007)$. Since $\Delta T_j \times I(t \geq 2007)$ implicitly assumes that all minimum wage increase took place in 2007, it ignores the gradual nature of the minimum wage increase shown in Figure 1.

[66] The results on the difference-in-difference-in-difference analysis is omitted because the linear trend is completely absorbed by the prefecture-year specific dummies.

	(1)	(2)	(3)	(4)	(5)
$\Delta T_j \times I(t \geq 2007)$	0.3152**	0.2735**	0.3068**	0.3146***	
(δ_1)	(0.1320)	(0.1244)	(0.1167)	(0.1106)	
$\Delta T_j \times I(t \geq 2007) \times age$		0.0007***	0.0007***	0.0007***	0.0007***
(δ_2)		(0.0002)	(0.0002)	(0.0002)	(0.0002)
Marginal effect (old)		0.2908**	0.3241***	0.3318***	
($\delta_1 + 25\delta_2$)		(0.1241)	(0.1163)	(0.1102)	
Marginal effect (very old)		0.2978**	0.3311***	0.3387***	
($\delta_1 + 35\delta_2$)		(0.1240)	(0.1161)	(0.1101)	
Prefecture-year controls	No	No	M average wage	M&F average wages	Dummies
N	5696368	5696368	5696368	5696368	5696368

Standard errors in parentheses are clustered at the prefecture level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: **DD under an Alternative Treatment Variable**

Notes: $\Delta T_j \times I(t \geq 2007)$ is the ratio of the minimum wage rate in 2012 to that in 2006 times the post-treatment dummy. $\Delta T_j \times I(t \geq 2007) \times age$ is the interaction with the age of the apartment unit. The marginal effect for the old (very old) apartment is the estimated elasticity of the housing rents with respect to minimum wages for 25-year-old (35-year-old) apartment units. M (F) average wage stands for the male (female) average prefectural wage.

Similarly, I use a modification of (9) to allow for heterogeneity according to apartments' age:

$$\begin{aligned} \ln(rent)_{ijt} = {} & \delta_1 \Delta T_j \times I(t \geq 2007) + \delta_2 \Delta T_j \times I(t \geq 2007) \times age_i \\ & + pref_j + year_t + class_i \times pref_j + class_i \times year_t + \beta X_i + \varepsilon_{ijt}, \end{aligned} \tag{21}$$

where δ_1 is the primary effect and δ_2 is the differential effect.

Table C.3 presents the results. Column 1 shows that 10% increase in the minimum wage raises housing rents by 3.1%, which is consistent with my main estimation results. This conclusion does not change in the regression equation (21), as seen in columns 2-4. One important difference from my main result is that the differential effect is strongly significant ($p < 0.01$), but its economic importance is much smaller. This can be seen in the effects for old apartments and very old apartments. Essentially, the small differential effect implies that new and old apartments experienced the increase in rents by almost the same magnitude. This result does not change in a difference-in-difference-in-difference (DDD) specification reported in column 5.

Overall, the results show the robustness of my main conclusion that 10% minimum wage increase causes 2.5-4.5% increase in rents. However, due to the small differential impact, the estimate is in the bottom-half of the range but is relevant for all apartments in the market. On the other hand, the results suggest that the estimates of the differential impact might not be robust. Recall that the placebo tests in section 5.1 suggest that differential impact might be limited but all apartments in my data have experienced the increase in housing rents. The results in table C.3 are in line with this conclusion.

C.5 The Impact of Minimum Wages in a Higher-Quality Housing Market

As the effect of minimum wages is expected to be seen in low-quality markets, the effect is expected to be smaller in a higher-quality housing market. In the main analysis, I have focuses on the rental apartment market, which is considered to be a low-quality housing market. In this section, I analyze a market of higher-quality rental housing.

In particular, I focus on "mansions." In Japan, what are called "mansions" are high-quality apartments that are distinguished from what are called "apartments" in Japanese, which I have analyzed in the main text.[67] Although the distinction between "mansions" and "apartments" is somewhat ambiguous, the former typically use high-quality building material (e.g., reinforced concrete) while the latter are made of lower-quality material such as wood. As in my main analysis, the data comes from the advertisements posted in At Home.[68]

Table C.4 shows the summary statistics for the mansion data in a comparable form to Table 1. As can be seen, the fractions of wooden buildings and light gauge steel buildings, which are two primary building material in the apartment market, are almost zero. It verifies that mansions use higher-quality building material. As for the housing rents, mansions have higher housing rents. However, the large standard deviation implies the considerable overlap between the apartment market (analyzed in the main analysis) and the mansion market.[69] Mansions are also a little more spacious but a little older. Overall, mansions are higher-quality than apartments in my main analysis, although the markets significantly overlap.

Figure C.2 shows the event study result, which is depicted in the same way as Figure 4. The figure might seem to suggest a significantly positive impact on rents. However, I claim that the effect of minimum wages is smaller on two grounds. First, the point estimates since 2008 are smaller than Figure 4. In particular, during 2009-2011, the estimates exceed 0.4 in Figure 4, while they never exceed 0.4 in Figure C.2. Second, the positive pre-trend might overestimate the true impact. In fact, the lowest point estimate is in 2005 and estimates and estimates rise since 2006, not 2007 in which the policy change took place. Thus, even in the absence of the policy change, the positive estimates might be observed. These informal arguments are formalized in Table C.5, which replicates Table 2 using the mansion market data. The results indicate the positive impact on housing rents, but it is smaller than my main result and statistically insignificant (except for very old units).

To sum up, a higher quality housing market, which overlaps the low-quality housing market in my main analysis, exhibits the smaller and insignificant positive impact on housing rents. This result is consistent with the expectation that higher-quality markets are less influenced by minimum wage hikes.

[67] Thus, the word "mansion" has a different meaning in Japanese despite that the word originates from English. See also footnote 25.

[68] To keep comparability of results, I apply the same sample selection procedure described in section C.2 except that (i) I do not delete apartments made of reinforced concrete (RC) or steal reinforced concrete (SRC) and that (ii) I also keep apartment units with the rent up to 300,000 yen since the rents are generally higher in the mansion market.

[69] The 25th percentile of housing rents is around 55,000 Japanese yen, which is roughly the mean/median housing rents of the apartment market in the main text.

	Mean	Standard deviation	Median	90th percentile
Monthly rent (Nominal JPY)	79154.98	37113.19	70000	127000
Apartments' age	15.52	9.59	15	28
Square footage (m^2)	37.46	16.84	32.55	62.62
Fraction of wooden buildings	0.0002144	-	-	-
Fraction of light gauge steel buildings	0.0005226	-	-	-
Observations	15653147			

Table C.4: **Summary statistics for a Higher-Quality Rental Housing Market.**

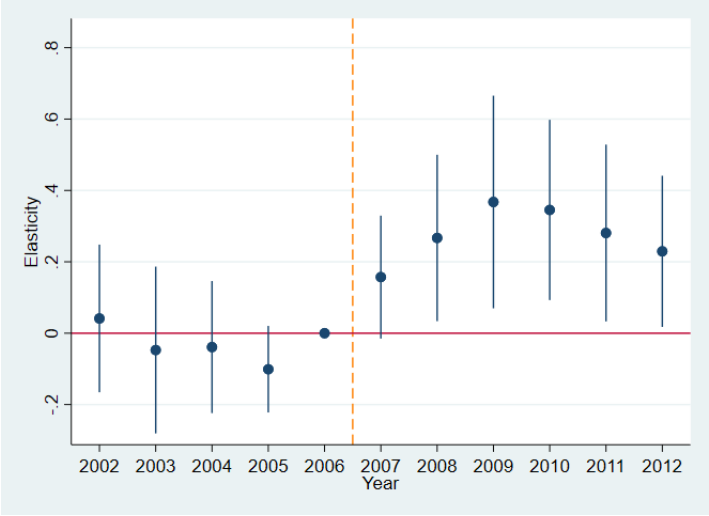

Figure C.2: **Results from the Event-Study Specification in a Higher-Quality Rental Housing Market.** *Notes:* The regression equation is (7): $\ln(rent)_{ijt} = \sum_{z \neq 2006} \delta_z(\Delta T_j \times I(t=z)_t) + pref_j + year_t + \beta X_i + \varepsilon_{ijt}$. The figure plots the estimated coefficient and the 95% confidence of interval of δ_z in (7) for each year. The coefficient in 2006 is normalized to 0. The vertical dashed line separates coefficients before and after the policy change.

	(1)	(2)	(3)	(4)	(5)
$\ln(MW)$	0.1849	-0.1076	-0.0802	-0.0779	
(δ_1)	(0.1236)	(0.2137)	(0.2110)	(0.2083)	
$\ln(MW) \times age$		0.0106*	0.0106*	0.0106*	0.0115*
(δ_2)		(0.0059)	(0.0059)	(0.0059)	(0.0059)
Marginal effect (old)		0.1582	0.1853	0.1877	
($\delta_1 + 25\delta_2$)		(0.1264)	(0.1374)	(0.1364)	
Marginal effect (very old)		0.2645**	0.2915**	0.2939**	
($\delta_1 + 35\delta_2$)		(0.1280)	(0.1446)	(0.1449)	
Prefecture-year controls	No	No	M average wage	M&F average wages	Dummies
N	15653147	15653147	15653147	15653147	15653147

Standard errors in parentheses are clustered at the prefecture level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: **DD analysis for a Higher-Quality Rental Housing Market.**
Notes: $\ln(MW)$ is the natural logarithm of the minimum wage rate, and $\ln(MW) \times age$ is the interaction with the age of the apartment unit. The marginal effect for the old (very old) apartment is the estimated elasticity of the housing rents with respect to minimum wages for 25-year-old (35-year-old) apartment units. M (F) average wage stands for the male (female) average prefectural wage.

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