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ABSTRACT

This study examines the spillover effects of minimum wages on suicide mortality in Japan using vital statistics data from 2000 to 2016. The possibility of competing income and unemployment effects motivates our research question as an empirical one. Our difference-in-differences regression framework exploits a minimum wage policy reform in Japan that was implemented in 2008, which mandated prefectures to incrementally increase their minimum wages to local living wages. The revision contributed to a decrease in male suicides by 4.58% that is concentrated among age groups with greater exposure to minimum wages. A supplementary analysis of the Comprehensive Survey of Living Conditions (2004-2016) suggests that increases in earned income in the absence of sizable adverse effects on labor at the intensive and extensive margins among low-wage earners could be an important mechanism driving these results.

Keywords: Well-being, Suicide, Minimum wage, Living wage, Japan

JEL Codes: I1, I31

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I. Introduction

Suicide is one of the leading causes of death worldwide, with nearly 800,000 suicide deaths and 16 million suicide attempts occurring each year (WHO 2017). In particular, Japan reports one of the highest suicide rates in the world, even among OECD nations (OECD 2020), with poor health, family problems, and financial distress reported as the three leading motives for suicide (Cabinet Office of Japan 2018). While no single intervention can fully address Japan’s remarkably high rates of suicide, a large body of research on the effect of economic conditions on well-being suggest that *economic policy* can play a central role in reducing suicide rates (Chang et al. 2013; Stuckler et al. 2009; Lundin and Hemmingsson 2009; Andrés 2005). With the lower-bound estimates for lifetime income lost from deaths by suicide in Japan at approximately 1.9 trillion JPY in 2009 (Kaneko and Sato 2010), and the inseparable link between well-being and suicide (Daly and Wilson 2009), the economic and welfare implications of preventing suicides are potentially substantial.

This paper aims to shed new light on the implications of an economic policy that has been the subject matter of heated debate—the minimum wage—for well-being in Japan, using suicide as a proxy for negative welfare. We view this research question as non-trivial because it allows us to study the well-being implications of minimum wages while taking into account the potential trade-offs between rises in income and unemployment that are posited in classical economic theory (Neumark and Wascher 2008). Due to the undeniable link between minimum wages and labor market outcomes, and economic factors and suicides,¹ the effect of minimum wages can theoretically go in either direction depending on the relative magnitude of the income and unemployment effects. We conjecture that any positive effects of minimum wages will be driven by income growth at the left tail of the wage distribution, in the absence of substantial surges in average unemployment. On the one hand, our data does not enable us to test this hypothesis directly and will likely reflect competing income and unemployment effects. On the other hand, our specification is flexible enough to allow for any adverse labor market effects. This is a favorable feature of our research design as it enables us to implicitly test whether potential gains from improvements in livelihood for low-wage earners who remain employed can exceed potential losses in welfare among those who might lose their job to higher minimum wages.

This paper contributes to a growing body of literature on the spillover effects and aggregate net effects of minimum wages on health and well-being. Increases in minimum wages can improve health and reduce mortality rates among the working age population (Lenhart 2017a, 2017b, 2020; Van Dyke et al. 2018) and lower the incidence of pre-term childbirths and neonatal mortality (Komro, Livingston, Markowitz, & Wagenaar, 2016; Majid, Mendoza Rodríguez, Harper, Frank, & Nandi, 2016). Higher minimum wages may also lead to worse health outcomes and less investment in healthy behavior (Horn, Maclean, and Strain 2017; Lenhart 2019; Andreyeva and Ukert 2018). A handful of studies examine how minimum wages influence workers’ mental health using survey data, but findings are mixed. While some report improvements in subjective well-being and mental health among low-wage workers (Horn, Maclean, and Strain 2017; Kuroki 2018; Reeves et al. 2017), others find no such effects on mental health (Kronenberg, Jacobs, and Zucchelli 2017). Meanwhile, the majority of studies in the United States find a negative association between minimum wages and suicide (Dow et al. 2020; Gertner, Rotter, and Shafer 2019; Kaufman et al. 2020). These studies on well-being and minimum wage, however, often suffer from an inability to fully account for various sources of omitted variable bias or selection into and out of employment.

¹ For instance, see Toffolutti and Suhrcke (2019); Reeves et al. (2012); Rachiotis et al. (2015); Antonakakis and Collins (2014); Corcoran et al. (2015); Antonakakis and Collins (2015); Daly, Wilson, and Johnson (2013); Gardner and Oswald (2007); Koo and Cox (2008); Hamermesh and Soss (1974); Marcotte (2003); and Cutler, Glaeser, and Norberg (2001).

Our study offers numerous advantages over previous studies on minimum wages and well-being. First, we address endogeneity concerns by leveraging plausibly exogenous variation in prefectural minimum wages introduced by a unique minimum wage policy revision in 2007, which was implemented in the fall of 2008. Through a comparison of prefectures that fulfilled the requirements of the revision to those that did not, we formulate a difference-in-differences (DD) regression framework. Second, the transparency of the minimum wage setting process in Japan coupled with the abrupt policy revision allows us to isolate both short-run and long-run dynamics of minimum wage effects on well-being in an event study specification. The third and foremost advantage of our study comes from a defining feature of the minimum wage policy revision, which mandated prefectures to gradually close the gap between local living wages and prefectural minimum wages. We view this as a unique and valuable parameter of interest in comparison to estimates given in previous studies, especially when considering the significance of non-linear minimum wage effects (Gorry and Jackson 2017).

We find that increasing minimum wages to a level that is at least as high as local living wages decreases male suicides by 1.79 deaths per 100,000 population, which is a 4.58% reduction,² but we do not find consistent evidence of an effect on women. Furthermore, we find that age groups' exposure to minimum wages, defined as the fraction of each demographic group's population earning a minimum wage or lower in the year preceding the reform, moderates the effect. We further investigate various channels behind our main findings by using multiple waves of the Comprehensive Survey of Living Conditions (CSLC) from 2004 to 2016 and examine how the minimum wage revision altered low-paid individuals' earned income, wages, hours worked, and employment. We find increases in wages across all demographic groups, but growth in earned income only among prime-aged men, a concurrent decrease in hours worked among prime-aged women, and a statistically insignificant change in employment across all groups. Taken together, our results suggest that the decrease in suicides observed among prime-aged men is driven primarily by increases in earned income in the absence of sizeable adverse effects at the intensive or extensive margins of labor.

The remainder of the paper is organized as follows. Section II describes the minimum wage system in Japan and the 2007 reform. In Section III, we describe the data sources and construction of key variables. Section IV outlines the econometric specifications. In Section V, we present the empirical evidence for the effect of minimum wages on suicide and explore potential mechanisms. In Section VI, we present multiple falsification tests. Section VII concludes.

II. Minimum Wages in Japan

Unlike the United States, Japan does not have a federal minimum wage. Instead, minimum wages are set at the prefectural-level through a two-stage process involving decision making by both national and prefectural minimum wage councils.³

In the first stage, the Central Minimum Wages Council sets four separate minimum wage benchmarks and assigns all 47 prefectures to one of the benchmarks according to a prefecture's designated rank. These ranks range from A to D depending on prefectures' relative economic capacity, with A indicating the best economic conditions and D signifying the worst economic conditions. The

² $\frac{1.79}{39.1} = 4.58\%$, where 39.1 is the mean male suicide rate in non-attainment prefectures before the policy.

³ There are also industrial minimum wages for a select group of occupations, but the higher between the industrial and prefectural minimum wage takes precedence. See Nakakubo (2009) for more details on the minimum wage system in Japan.

assignment of ranks is determined based on a score which measures each prefecture's fiscal conditions. Specifically, the score is computed using multiple economic factors related to the local cost of living, the salary of low-wage workers, and local businesses' financial ability to increase wages. Prefectural minimum wages are thus clustered by their designated rank (Nakakubo 2009; Aoyagi, Ganelli, and Tawk 2016).

In the second stage, prefectural minimum wage councils choose an appropriate minimum wage, using the national council's recommendations as a benchmark. As Aoyagi et al. (2016) and Hara (2017) elaborate, the local councils' deliberation process involved only three factors prior to the 2007 reform: (1) the local cost of living, (2) the average wages of part-time workers, and (3) the local businesses' financial capacity to raise wages. The 2007 reform introduced a fourth criteria, which came into effect October 1, 2008, requiring (4) the estimated after-tax monthly income for a minimum wage earner be at least as high as the estimated income earned through public assistance for a single-member household, which is regarded as the living wage.⁴ Prefectures that met the fourth requirement followed the standard two-stage procedures for deciding their prefectural minimum wage without any further considerations, while the 12 prefectures that failed to meet the fourth requirement were required to prioritize the raising of their minimum wage to their public assistance levels. Although prefectures were initially given a span of two to three years to fulfill the new requirements, prefectures that had unfavorable local economic conditions for drastic minimum wage hikes were given additional time to meet the requirement. In practice, it took up to seven years for all 12 prefectures to meet the fourth requirement, in part due to revisions in the way the minimum income necessary for a basic living was calculated.⁵

Finally, we find strong evidence that the reform brought about a notable degree of dispersion in prefectural minimum wages. Figure 1 depicts nominal minimum wages from 2000 to 2016 and demonstrates that minimum wages did not vary substantially prior to the reform, while after the reform, there is a clear increase in variation in minimum wages between and within prefectures. A formal quantification of the reform's impact on minimum wages reveals that the average minimum wage growth rate in real terms was approximately 25 JPY or 3.69% higher for non-attainment prefectures than for attainment prefectures (Figure 2).⁶ The impact of the policy reached its peak in 2011, which is when prefectures began to fulfill the requirements of the minimum wage reform (Table A.1).⁷

⁴ The after-tax monthly income for a minimum wage earner in 2008 was computed as prefectural minimum wage \times 173.8 (standard monthly working hours) \times 0.8598 (disposable income ratio after considering social security fees. The monthly public assistance levels cited in the 2007 reform is the minimum earnings required for a single-member household whose household head is aged 12 to 19. Local public assistance levels are computed based on the generosity of local livelihood assistance and housing subsidies; thus, expected income from public assistance levels are regarded as analogous to living wages.

⁵ Some prefectures that had at one point raised their minimum wage to meet the requirements, such as Aomori and Chiba became non-attainment prefectures again in subsequent years due to these revisions. However, no prefectures that were initially attainment prefectures prior to the 2007 reform changed their status to non-attainment after the reform. Prefectures did not respond to the reform by lowering the public assistance levels for single household members. See Supporting Information (A) for more details.

⁶ $\frac{25}{678.18} = 3.69\%$, where 678.18 is the mean real minimum wage in non-attainment prefecture before the policy.

⁷ We used Eq. (2) to estimate the effect of the minimum wage reform on prefectural minimum wages.

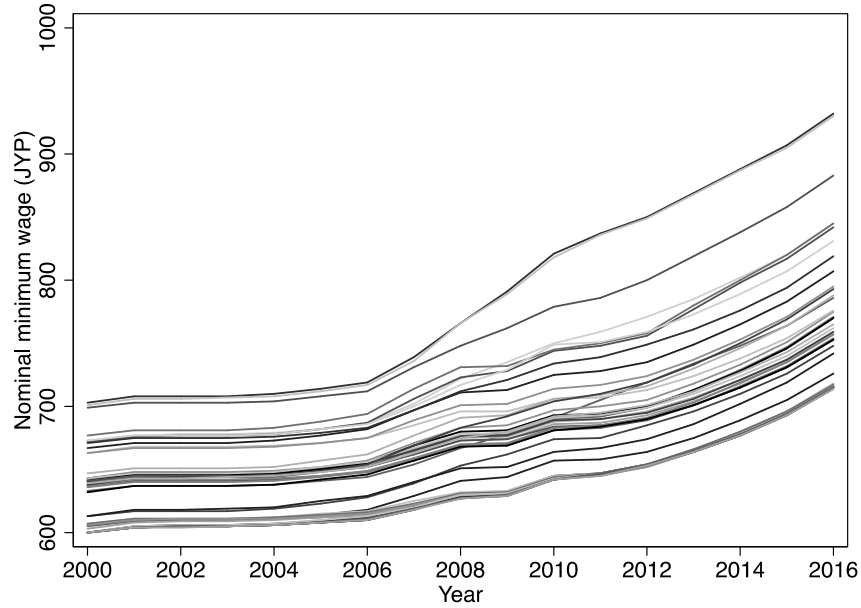


Figure 1. Nominal prefectural minimum wages from 2000 to 2016.

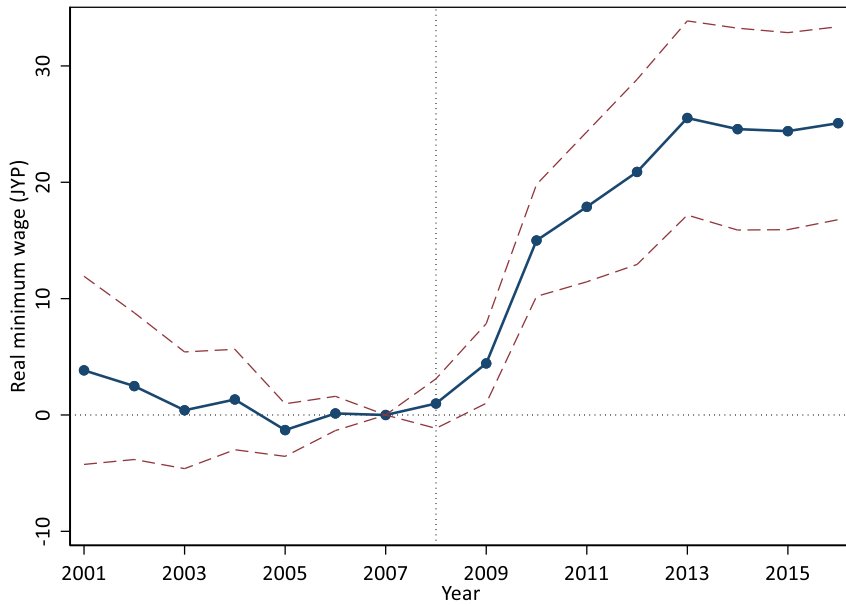


Figure 2. Impact of 2007 revision on real prefectural minimum wages. Point estimates are obtained from Eq. (2) with the one-year-lag of real minimum wages as the dependent variable. Dashed lines are 95% confidence intervals.

All in all, the new fourth criteria introduced a source of plausible exogeneity into the minimum wage determination process that can be captured using prefectures' pre-revision attainment status. The primary identification strategy of this paper rests on the *additional* variation in minimum wages introduced by the fourth criteria in a difference-in-differences regression framework, which compares

prefectures that did not meet the fourth criteria (treated) to prefectures that had already met the requirement prior to the reform (control).⁸

III. Data

Our main data sources are at the prefecture and prefecture-demographic group-level. For both types of data, the number of prefectures is 47 and the number of observational periods is 17 years (2000-2016). Hence, our prefecture-level data consist of a 17-year panel of 47 prefectures for a total of 799 observations. The prefecture-demographic-group-level data, where each demographic group is a five-year age bin (15-19, 20-24, ..., 75-79, 80 and older) stratified by gender, is a 17-year panel of 28 demographic groups within each of the 47 prefectures, leading to a total of 22,372 observations. These two types of data were matched by prefecture-year identifiers.

We collected suicide data from Vital Statistics-Mortality Files (2000-2016), which is compiled by the Ministry of Health, Labour, and Welfare (MHLW). We regarded ICD-10 codes X60–X84 as deaths by intentional self-harm (suicide). Individual mortality records were collapsed into year-prefecture-demographic group cells and divided by the total population counts within each cell. Table 1 summarizes the definitions and data sources for the main variables we utilized.

Table 1. Definitions and data sources

Variable name	Definition	Source(s)
<i>Panel A. Prefecture-level variables</i>		
Suicide rate	The number of suicides per 100,000 population.	(A) and (D)
Real minimum wage	Regional minimum wages adjusted to 2015 level prices in each prefecture.	(B) and (C)
Prefectural rank	The designated rank of each prefecture.	(B)
Public assistance recipients (per 1000)	The number of individuals receiving public assistance per 1000 population.	(E) and (D)
Unemployment rate	Estimations of the seasonally adjusted unemployment rate.	(F)
Real GNP per capita	Real gross national product divided by the population size.	(B) and (D)
Per capita investment in unemployment countermeasures	Local government investment in unemployment countermeasures divided by the population size.	(G) and (D)
Public assistance expenditure	Local government expenditure on public assistance divided by the number of public assistance recipients.	(G) and (E)

⁸ Hara (2017), Okudaira, Takizawa, & Yamanouchi (2019), Higuchi (2013) and Kawaguchi & Mori (2021) also utilize this policy revision for identification.

Per capita expenditure on social welfare	Local government expenditure on social welfare divided by the population size.	(G) and (D)
Population (thousands)	The size of the population divided by 1000.	(D)
Population by age group	Annual population estimates.	(D)

Panel B. Prefecture-demographic group level variables

Divorce rate	The number of separated individuals divided by the population size.	(H)
Unemployment rate	Number of employed individuals divided by the size of the labor force.	(H)
Home ownership rate	The number of homeowners divided by the population size.	(H)

Panel C. Individual-level variables

Hourly wage variable	$\frac{\text{Previous years' earned income}}{\text{hours worked a week} \times 52}$	(I)
Earned income	Previous year's income earned from work in ten thousands JPY.	(I)
Employment status	Employment status indicator.	(J)
Hours worked	Hours worked a week.	(J)
Age	Age of respondent in years.	(J)
Household size	Number of members in household.	(J)
Marital status	Indicator variables for divorced, married, widowed, and not married.	(J)

Data sources

- (A) Vital Statistics, Ministry of Health, Labor, and Welfare
- (B) Press Release Materials, Ministry of Health, Labor, and Welfare
- (C) Consumer Price Index (CPI), Ministry of Internal Affairs and Communications
- (D) Population Census, Statistics Bureau of Japan
- (E) National Survey on Public Assistance Recipients, Ministry of Health, Labor, and Welfare
- (F) Labor Force Survey, Statistics Bureau of Japan
- (G) Survey of Local Fiscal Conditions, Ministry of Internal Affairs and Communications
- (H) Census, Statistics Bureau of Japan
- (I) Income and Savings Questionnaire, Comprehensive Survey of Living Conditions
- (J) Household Questionnaire, Comprehensive Survey of Living Conditions

Prefecture-level data include prefectural minimum wages and prefectures' designated ranks from official public announcements on the website of the MHLW, which are released each year. We collected essential control variables such as prefectures' population size, proportion of the population over the age of 65, and the proportion of the population under the age of 15. We also collected public assistance take-up rates, seasonally adjusted unemployment rates, real gross national product (GNP) per capita, per capita government investment in unemployment countermeasures, expenditure on public assistance per recipient, and per capita expenditure on social welfare programs. All prefecture-level wage and expenditure variables are converted into real terms at 2015 price levels. To avoid the possibility of a bad control problem, we used the lag of control variables (Angrist and Pischke 2008). Therefore, our regressions exclude our earliest year available, leaving 16 years.

Prefecture-demographic group level control variables include a vector of demographic group indicators (for each of the 28 age-sex groups), population size, divorce rates, unemployment rates, and home ownership rates. These covariates can control for changes in local economic conditions or well-being that are specific to demographic groups within each prefecture. To account for the possibility that any of these additional control variables could lead to an over-control problem, we compute the one-year lag for control variables and estimate models with and without the additional cell-level socioeconomic controls. Data on divorce rates, unemployment rates, and home ownership rates are from the Census, which is conducted every five years in Japan, so we linearly interpolated values for missing cells using the latest information available. Data on cell-level population size was obtained from annual population estimates by the Statistics Bureau. Cell-level population size was utilized to weigh observations, lower the variance in regression errors by including it as a control, and to generate our cell-level variable for suicide rate per 100,000 population.

We also utilize individual-level repeated cross-sectional data from the Comprehensive Survey of Living Conditions (CSLC) to explore changes in wages, earned income, and labor at the intensive and extensive margins as potential mechanisms. We control for respondent age (a continuous measure), marital status (indicators for divorced, married, widowed, and not married), and number of household members in our individual-level analyses. The CSLC is a nationally representative survey of the Japanese population that is conducted in June on a tri-annual basis and has been conducted since 1986 by the Ministry of Health, Labour, and Welfare. The survey applies a two-stage random sampling procedure and consists of four questionnaires on households, respondent health, income/savings, and long-term care. In this study, we utilize the household and income/savings questionnaires. The main household questionnaire covers all respondents, comprising of 600,000-800,000 individuals from approximately 300,000 households that are randomly selected in each survey wave. The sample for the income and savings questionnaire is extracted from the main household survey and includes approximately 60,000 respondents each year and 295,963 respondents over five waves. In our wage, earned income, and hours worked regressions, the analytical sample was restricted to those who were earning a wage rate less than or equal to 1.5 times that of the one-year-lag of prefectural minimum wages. We believe that these individuals are most likely to be sensitive to changes in minimum wage. In our employment regressions, our analytical sample was restricted to those who were either unemployed or who were employed but earning a wage rate equal to or less than 1.5 times that of one-year-lag of prefectural nominal minimum wages. We apply sample weights provided by the CSLC to ensure that our survey analysis is

representative of the average population, and cluster standard errors at the prefecture level.⁹

To investigate the effect of the reform on suicides by demographic groups' exposure to the minimum wage, we computed an exposure variable as follows. Using the 2007 wave from the CSLC, we identified individuals earning a wage rate equal to or less than their local prefectural minimum wage and coded these individuals as exposed, and zero otherwise. We then collapsed the data into 14 five-year age bins by gender, resulting in 28 variations in exposure to the minimum wage.¹⁰ The resulting values are the number of respondents exposed within each demographic group. We then divided these values by the total number of survey respondents within each demographic group to measure exposure. A cursory look at the relationship between demographic group and exposure in Figure 3 shows that, in line with our expectations, the proportion of individuals earning a wage rate equal to or less than their prefectural minimum wage are primarily prime-aged individuals, which is defined as those within the age range of 20-54. Those with the least exposure are teens aged 15-19 and adults over the minimum retirement age at 60 years old. This is likely because the majority of teens are still in school, while the majority of older adults have left the labor force. Among prime aged individuals, exposure to the minimum wage is negatively correlated with age, with those in their twenties and thirties most likely to be affected. While women are more "exposed" to the minimum wage, the majority of women are not the main breadwinner of their household, and minimum wage workers who are men are more likely to be from low socioeconomic households than that of women.

⁹ We restricted the sample to individuals earning a wage rate less than or equal to 1.5 times the prefectural minimum wage as opposed to the exact prefectural minimum wage for two reasons. First, there may be measurement error in our hourly wage rate variable because we computed it ourselves using self-reported hours worked in a week, days worked in a week, and previous years' earned income. Hourly wages were computed assuming 52 work weeks a year. Due to this measurement error, our estimates for wages are imprecisely estimated, and using exact prefectural minimum wages as cutoffs may exclude a proportion of minimum wage earners in the sample who are earning near the threshold. Second, we hope to capture potential wage spillover effects in the lower end of the wage distribution, as it is plausible that the minimum wage reform affected not only those earning less than or equal to the statutory minimum wages, but also indirectly affected those earning a wage rate slightly above their prefectural minimum wage.

¹⁰ The denominator for the exposure variable is the total number of CSLC respondents in each demographic group for the 2007 wave, as opposed to the total number respondents in the labor force, because the outcome variable in our study is the population suicide rate rather than the employment rate.

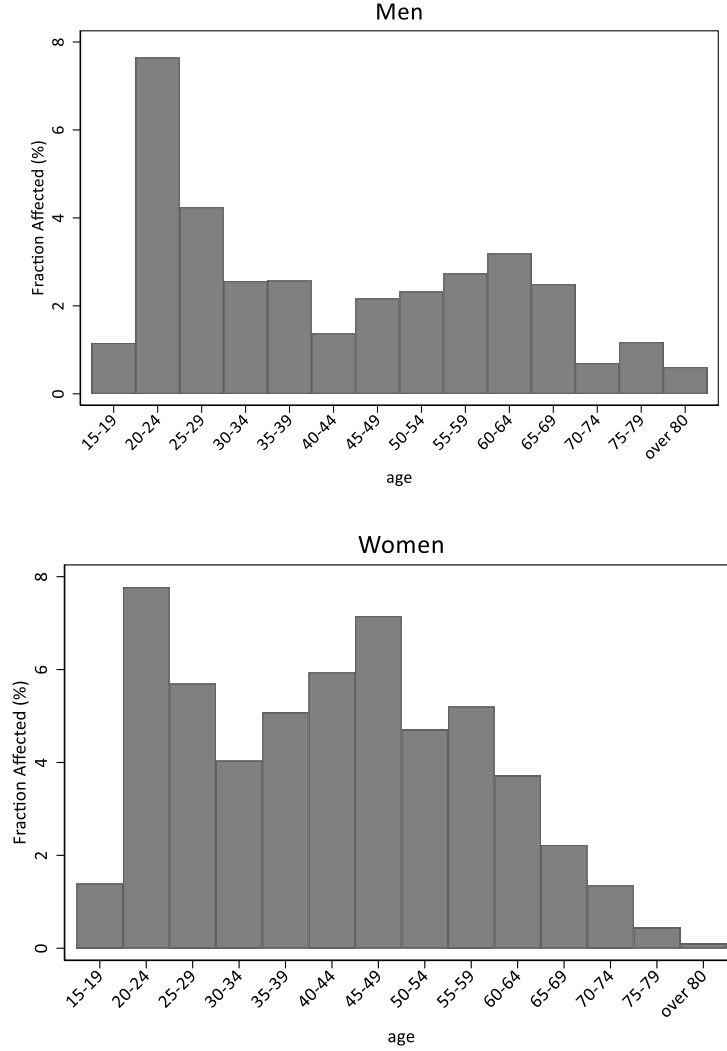


Figure 3. Distribution of fraction affected. The fraction affected, which is our proxy for exposure to minimum wage, is the fraction of respondents within a demographic group that are earning a wage rate that is equal to or less than their prefectural minimum wage. Data is taken from the 2007 wave of the CSLC.

IV. Econometric Models

While the majority of minimum wage studies from the United States rely on state-level variation in minimum wage, this approach is considered to be endogenous to omitted time-varying factors such as political movements, local economic shocks, or contemporaneous poverty-alleviation policies (Neumark and Wascher 2008). Given that prefectural minimum wages in Japan are determined primarily by local economic conditions, which might lead to similar endogeneity issues if we were to follow the commonly used fixed effects method, we leverage the fourth criteria in the minimum wage setting process introduced by the 2007 minimum wage reform and exploit prefectures' pre-revision attainment status in a difference-in-differences (DD) specification. The idea is to compare changes in outcomes in prefectures where the minimum wage increased because the minimum wages were lower than living wages (non-attainment), to prefectures that did not experience an additional increase in

minimum wages because their minimum wages were already at least as high as living wages prior to the reform (attainment).

A. Difference-in-Differences Specification

Our baseline difference-in-differences (DD) specification is denoted as follows:

$$y_{i,j,t} = \beta_0 + \beta_1 Treated_j \times 1(\tau \geq 2008) + X'_{i,j,t} \omega + \xi_j + \theta_t + \eta_{k,t} + \epsilon_{i,j,t} \quad (1)$$

$y_{i,j,t}$ is the suicide rate per 100,000 population for demographic group i in prefecture j in year t . The DD coefficient of interest, β_1 , is an interaction term between an indicator variable equal to 1 if a prefecture is designated as non-attainment, $Treated_j$, and an indicator variable equal to 1 in years after the minimum wage reform was implemented, $1(\tau \geq 2008)$. $X'_{i,j,t}$ is a set of control variables including those at the prefecture level, comprising of log population, proportion over the age of 65, proportion under the age of 15, prefectural rank (which can vary over time), public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, and expenditure on social welfare programs. $X'_{i,j,t}$ also includes controls at the prefecture-demographic level, including age (an indicator for each of the 14 categories), male indicator variable, divorce rates, unemployment rates, and home ownership rates. ξ_j and θ_t are vectors of prefecture and year fixed effects, respectively. Prefecture fixed effects capture prefecture-specific time-invariant factors such as fixed features of social or economic institutions, while year fixed effects capture factors that are correlated with changes in suicide that emerge over time at the national level. This can include macroeconomic trends, national social welfare policies, or changing attitudes toward suicide.

If local economic conditions changed differentially for treatment and control prefectures at the timing of the 2007 revision's implementation in 2008, then there may exist omitted variable bias in our natural experiment. Ideally, we would incorporate prefecture-by-year fixed effects to flexibly control for sources of time-varying endogeneity, but this is not possible in Eq. (1) due to multicollinearity with our DD coefficient (Wolfers 2006). Instead, Eq. (1) includes a vector of rank-by-year fixed effects, $\eta_{k,t}$, as an institutional control. This absorbs the year-by-year variation in minimum wages associated with annual guidelines for each rank. As discussed earlier, rank-designated minimum wages are determined from local economic conditions—a potential source of time-varying omitted variable bias in our setting. We therefore believe that rank-by-year fixed effects can proxy for these plausible sources of year-by-year endogeneity and strengthen the validity of our results through their inclusion. An added benefit is a lower the variance in the regression error and smaller standard errors. Taken together, $\eta_{k,t}$ enables us to isolate the variation in minimum wages caused by the minimum wage reform, from potential co-occurring increases in minimum wages driven by changing fiscal conditions that arise locally. Finally, $\epsilon_{i,j,t}$ is the idiosyncratic error.

We also estimate regressions with region-specific linear time trends, but the results do not change substantially. In fact, we prefer a specification without region-specific linear time trends because, as pointed out by Meer & West (2016), a specification with region-specific time trends can affect employment growth, which could lead to a bad (over) control problem in the context of this paper if employment growth is an important mechanism driving the relationship between minimum wages and suicide.

DD estimation requires the common trend assumption to be met for there to be internal validity (Angrist and Pischke 2008). An indirect test for this assumption can be conducted by empirically checking for pre-existing trends in an event-study specification (Autor 2003). We therefore examine dynamic treatment effects in the following specification:

$$y_{i,j,t} = \beta_0 + \sum_{k \neq -2007} \beta_{1,k} Treated_j \times 1(\tau = k) + X'_{i,j,t} \omega + \xi_j + \theta_t + \eta_{r,t} + \epsilon_{i,j,t} \quad (2)$$

Here, $1(\tau = k)$ is an indicator variable taking the value of 1 if the observation year is equal to the k th year. $\beta_{1,k}$ is our main coefficient of interest measuring the effect of treatment status, $Treated_j$, at year k , with 2007 as the comparison year. To informally satisfy the common trend assumption, $\beta_{1,k}$ should be statistically insignificant in all time periods prior to the implementation of the reform in 2008.

A potential concern of DD specification, even if we observe parallel trends, is that confounding policies could contaminate the results. Consider the possibility that recent national mandates to delay mandatory retirement improved the well-being of full-time employees. Since the retirement policy had the greatest impact on full-time workers in large firms (Kondo and Shigeoka 2017), coupled with the fact that the majority of large firms are located in urbanized prefectures which make up a large proportion of treated prefectures, estimates could remain endogenous to such policies. To address this concern, we first run a series of falsification tests in Section VI. We then consider a difference-in-difference-in-differences (DDD) specification to test the robustness of our main findings. We describe this specification in detail in the next section.

B. Difference-in-Difference-in-Differences Specification

Previous studies use various proxies for exposure to minimum wages in a DDD specification, such as low educational attainment or comparing those at the working age to those over the retirement age (Horn, Maclean, and Strain 2017). However, these proxies have their limitations. For instance, those without a college degree may still be affected by concurrent poverty-alleviation policies. Moreover, retired individuals are completely isolated from any labor market trends, which prevents one from differentiating between minimum wage effects and local labor market trends. Due to these reasons, coupled with the fact that Japanese vital statistics data do not provide information on educational attainment in death certificates, we make full use of the demographic information provided in death certificates and estimate a DDD specification, which examines the impact of the minimum wage revision by demographic groups' exposure to minimum wage:

$$y_{i,j,t} = \alpha_0 + \alpha_1 Treated_j \times 1(\tau \geq 2008) \times Exposure_i + \alpha_2 Treated_j \times Exposure_i + \gamma_t \times Exposure_i + X'_{i,j,t} \omega + \xi_j + \theta_t + \rho_{j,t} + \epsilon_{i,j,t} \quad (3)$$

There are a few notable differences in Eq. (3) compared to Eq. (1). $Exposure_i$ measures each demographic groups' exposure to the minimum wage in 2007 and is treated as a continuous variable to make full use of the variation in demographic groups' exposure. α_1 is the DDD coefficient of interest, which measures the effect of the minimum wage reform for every 1 percentage point increase in exposure to the minimum wage. $Treated_j \times Exposure_i$ allows for different correlations between exposure to minimum wages and suicide for treatment and control prefectures. $\gamma_t \times Exposure_i$ are

year-specific exposure effects, which capture unobserved year shocks to demographic groups' suicide rates that are specific to their exposure to the minimum wage. For instance, recessions may have an adverse effect on adults in their twenties or thirties, who are more likely to be earning close to a minimum wage, compared to older adults or those approaching the retirement age. Finally, the prefecture-by-year fixed effects, $\rho_{j,t}$, replaces the rank-by-year fixed effects in Eq. (1) and (2), $\eta_{r,t}$, to control for prefecture-year shocks that could be correlated with both the reform and suicides.

Within a treatment prefecture, demographic groups with greater exposure to the minimum wage are subject to the same social welfare programs as demographic groups with less exposure, but demographic groups with lower exposure are on average less likely to be impacted by the minimum wage. Our DDD is thus beneficial because it can control for these plausible confounding policies that co-occur within prefectures.

We take the leads and lags of the DDD estimator in the following triple differences event study specification:

$$y_{i,j,t} = \alpha_0 + \sum_{k \neq 2007} \alpha_{1,k} Treated_j \times 1(\tau = k) \times Exposure_i + \alpha_2 Treated_j \times Exposure_i + \gamma_t \times Exposure_i + X'_{i,j,t} \omega + \xi_j + \theta_t + \rho_{j,t} + \epsilon_{i,j,t} \quad (4)$$

Where our DDD event study coefficient of interest, $\alpha_{1,k}$, measures the effect of treatment status at the k th period for every 1 percentage point increase in exposure to minimum wages.

V. Results

A. Summary Statistics

We next report the mean of the dependent and control variables of the prefecture-level data by prefectures' non-attainment status in Table 2, and the pre-revision means of the dependent and control variables of prefecture-demographic-group-level data of high-exposure demographic groups, by prefectures' non-attainment status in Table 3. Table 2 shows that treated prefectures have a larger population, public assistance recipients, and GNP per capita than control prefectures, suggesting that non-attainment prefectures tend to be more urbanized than attainment prefectures. Table 3 reveals that there are no significant differences in means between high exposure groups in attainment prefectures and high exposure groups in non-attainment prefectures for these socioeconomic controls.

Table 2. Mean of dependent and prefecture-level control variables

	All	All prefectures pre-2008	Attainment pre-2008	Non- attainment pre-2008
Suicide rate (per 100,000 population)	25.9	27.7	28.7	26.8
Population size	5244.7	5124.9	2701.3	7454.2
Public assistance recipients (per 1000)	13.4	10.6	7.8	13.3
Seasonally adjusted unemployment rate	4.3	4.6	4.2	5.1
GNP per capita	4062.4	4043.2	3769.1	4306.6
Per capita investment in unemployment countermeasures	56.8	33.4	40.9	26.3
Expenditure on public assistance per recipient	1906.2	1970.9	1950.6	1990.4
Expenditure on social welfare	1425.4	993.1	1013.7	973.3
Population over 65 (%)	22.0	19.3	20.8	17.9
Population under 15 (%)	13.4	14.0	14.5	13.4
No. of prefectures	47.0	47.0	35.0	12.0
Observations	22372	10528	7840	2688

Notes: Observations weighted by the population size in each cell.

Table 3. Mean of dependent and cell-level control variables

	All	High exposure groups in attainment prefectures pre-2008	High exposure groups in non-attainment prefectures pre-2008
Suicide rate (per 100,000)	15.9	15.7	16.1
Divorce rate	4.0	4.2	3.9
Proportion unmarried	39.6	36.0	42.8
Unemployment rate	5.1	4.7	5.4
Home ownership rate	64.9	69.1	61.1
Observations	2961	2205	756

Notes: Observations weighted by the population size in each cell. High exposure groups in this table are defined as demographic groups with 4.2% (70th percentile) or more of the population earning less than or equal to their prefectural minimum wage. Low exposure groups are demographic groups with less than 4.2% of the demographics' population earning less than or equal to the prefectural minimum wage.

Figure 4 reports trends in the suicide rate for high- and low- exposure demographic groups in attainment and non-attainment prefectures from 2000-2016, using the same binary variable for high exposure (note that exposure is continuous in our regressions). There is a clear divergence in the male suicide rates between high exposure groups in attainment prefectures and high exposure groups in non-attainment prefectures following to the minimum wage reform. Prior to the reform, the suicide rates of men within exposure groups in attainment and non-attainment prefectures show a similar trend. However, there seems to be some evidence of pre-existing trend for women. For this reason, the results for women in the baseline DD specification should be taken with a grain of salt.

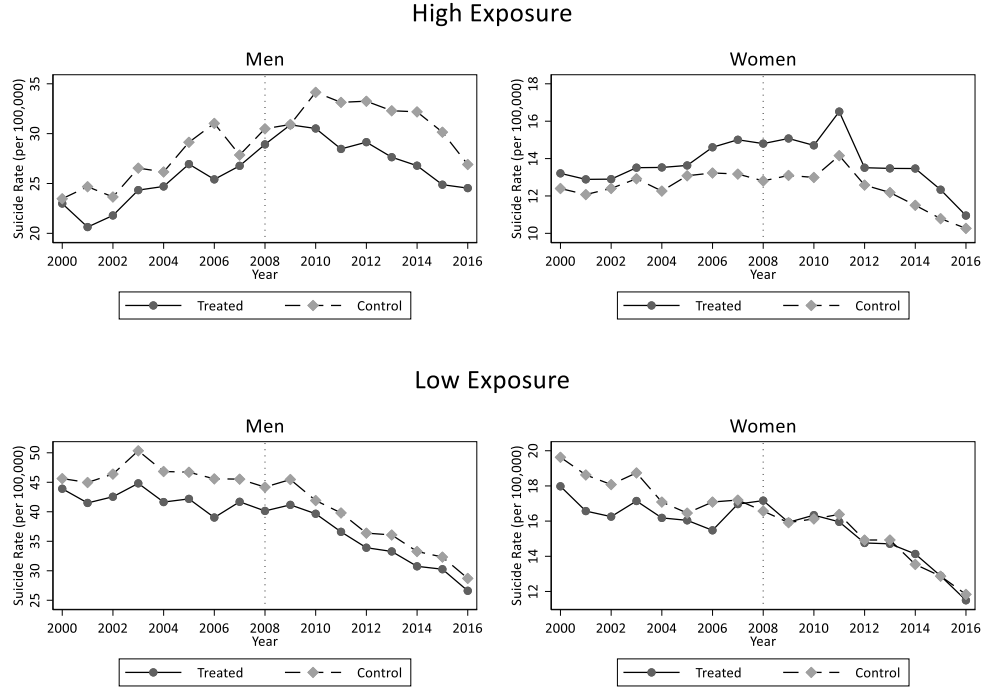


Figure 4. Trends in suicide rate by treatment and control prefectures and high- and low-exposure age groups, 2000-2016. The dependent variable is the suicide rate per 100,000 population. Observations weighted by the population size in each cell. High exposure groups in this figure are defined as demographic groups with 4.2% (70th percentile) or more of the population earning less than or equal to their prefectural minimum wage. Low exposure groups are demographic groups with less than 4.2% of the demographic's population earning less than or equal to the prefectural minimum wage.

B. Suicide Mortality

We next examine the formal estimation results for suicide mortality. Table 4 reports the aggregate effect of the minimum wage reform on suicide by gender using a DD specification in Panel A and a DDD specification in Panel B. Column 2 of Panel A suggests that the effect of the minimum wage reform reduced the male suicide rate by 1.79 suicide deaths per 100,000 population, which is a 4.58% decrease in suicides. To make the DD and DDD coefficient magnitudes comparable, we can once again consider a demographic group having a “high exposure” to minimum wages as 4.2% of the population earning a wage equal to or lower the minimum wage (i.e., the 70th percentile of exposure to minimum wage). Under this classification, the DDD coefficient for men is $-0.38 (4.2) = -1.596$, which is slightly smaller than the DD coefficient. As a whole, both the economic and statistical significance of this finding is generally robust to a DDD specification, even after the inclusion of additional socioeconomic controls. Meanwhile, none of the specifications imply a statistically significant effect on the female population. The estimates from Poisson and negative binomial regressions using suicide counts as the dependent variable are presented in Panels A and B of Table A.4, respectively, while estimates using the inverse hyperbolic sine transformation on the suicide rate is presented in Panel C of the same table. The results from a fixed effects model with prefecture-specific linear time trends are reported in Table A.5 and suggests an elasticity of -1.67 for prime-aged men. Regardless of functional form and specification, our main conclusions point in the same general direction—a sizable reduction in

the suicide rate among men from the closing gap between minimum wages and living wages.¹¹

Table 4. Aggregated results from DD and DDD

	(1)	(2)	(3)	(4)	(5)	(6)
	Men		Women			All
Panel A. Difference-in-differences						
<i>Treated_j</i>	-2.300***	-1.792**	0.142	-0.171	-1.014**	-0.995**
$\times 1(\tau \geq 2008)$	(0.785)	(0.814)	(0.274)	(0.294)	(0.471)	(0.491)
Panel B. Difference-in-difference-in-differences						
<i>Treated_j</i>	-0.292*	-0.380**	-0.191	-0.204	-0.225	-0.289*
$\times 1(\tau \geq 2008)$	(0.166)	(0.177)	(0.138)	(0.141)	(0.157)	(0.167)
$\times Exposure_i$						
<i>N</i>	10,528	10,528	10,528	10,528	21,056	21,056
Additional Controls		Y		Y		Y

Notes: The dependent variable is the suicide rate per 100,000 population. Panel A reports the DD results from Eq. (1) while Panel B reports the DDD results from Eq. (3). All models control for one-year lagged variables including log of population at the cell-level, variables at the prefecture-level (proportion over the age of 65, and proportion under the age of 15), prefecture and year fixed effects, and rank-by-year fixed effects. Additional one-year lagged socioeconomic controls include those at the prefectural level (public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and cell-level controls (demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged. All triple difference models include prefecture-year fixed effect and year-specific exposure effects. Observations weighted by the population size in each year-prefecture-demographic group cell. Robust standard errors are clustered at the prefecture level.

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

We further disaggregate our regressions into two different age groups: the prime-aged population who are within the age range of 20-54, and older population, defined as those over the age of 54, in Table 5. The DD coefficient with full controls for prime-aged men, in Column 2 of Panel A, is statistically significant at the 5% level. This is a larger effect size than the DD coefficient for the entire male population, increasing in absolute terms from 1.79 to 2.25 suicides per 100,000 population. The DDD specification in Column 2 of Panel B, which exploits variation in exposure to the minimum wage among prime-aged men, is also statistically significant, albeit at the 10% level. This is expected, as our analysis by age group mechanically decreases the variation in our exposure variable, which relies on

¹¹ Specifically, the specification for our prefecture-fixed effects model is:

$$y_{i,j,t} = \sigma_0 + \sigma_1 \log \text{minwage}_{j,t-1} + X'_{i,j,t} \omega + \xi_j + \theta_t + \delta_j T + \epsilon_{i,j,t}$$

Where our coefficient of interest, σ_1 , is the log real minimum wage for prefecture j at time $t - 1$. We estimate separate models with and without prefecture-specific linear time trends, denoted $\delta_j T$.

variation across age groups. On the contrary, we do not find any statistically significant results for older men in any of our specifications. This finding is consistent with our conjecture that younger men, including those in their prime age, are more likely to be exposed to changes in the minimum wage than older men, who have likely left the labor force through retirement. Our regressions on women show no statistically significant effects, regardless of age.

Table 5. Disaggregated by demographic group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
Panel A. Difference-in-differences								
$Treated_j$	-2.793***	-2.255**	-1.743	-1.323	-0.027	-0.088	0.533	-0.129
$\times 1(\tau \geq 2008)$	(0.810)	(0.909)	(1.094)	(1.056)	(0.297)	(0.328)	(0.630)	(0.579)
Panel B. Difference-in-difference-in-differences								
$Treated_j$	-0.863**	-0.708*	0.008	0.456	0.188	0.194	-0.031	-0.201
$\times 1(\tau \geq 2008)$	(0.398)	(0.401)	(0.854)	(0.916)	(0.159)	(0.157)	(0.221)	(0.256)
$\times Exposure_i$								
N	5264	5264	4512	4512	5264	5264	4512	4512
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable is the suicide rate per 100,000 population. Panel A reports the DD results from Eq. (1) while Panel B reports the DDD results from Eq. (3). All models control for one-year lagged variables including log of population at the cell-level, variables at the prefecture-level (proportion over the age of 65, and proportion under the age of 15), prefecture and year fixed effects, and rank-by-year fixed effects. Additional one-year lagged socioeconomic controls include those at the prefecture-level (public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and cell-level controls (demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged). All triple difference models include prefecture-year fixed effects and year-specific exposure effects. Observations weighted by the population size in each year-prefecture-demographic group cell. Robust standard errors are clustered at the prefecture level.

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

Next, we examine the results from our dynamic treatment effect specifications, Eq. (2) and Eq. (4), in Table 6, Figure 5, and Figure 6. Table 6 summarizes the event study coefficients for the prime-aged population in both DD and DDD specifications. Columns 1 and 2 reveal that the minimum wage revision decreased the suicides of prime-aged men regardless of the specification employed. A notable feature of our findings is that effect of the minimum wage reform on suicides was not immediate, but rather, gradual. The DD and DDD event study coefficients, $\beta_{1,k}$ and $\alpha_{1,k}$, were negative but not statistically significant from 2008 to 2010 but reached statistical significance from 2011 onwards. Importantly, the timing of the effect on suicides is consistent with the timing of the reform's impact on actual prefectural minimum wages, which operated with a significant lag due to the national governments' relaxed enforcement of the policy in the early years of the reform. According to Table

A.1, prefectures only began to fully satisfy requirements of the reform from 2011. In both DD and DDD event study estimates for prime-aged men, we do not observe any pre-existing trend, providing implicit evidence that the common trend assumption was met. Turning to the effect of the reform on prime-aged women in columns 3 and 4, we find statistically significant results for women in the DD event study specification, but also statistically significant pre-trend. This suggests that we may not be able to interpret the results for women as causal at face value. Furthermore, the results for prime-aged women are not robust to the DDD event study specification, where we do not observe pre-existing trends. Figure 5 displays the DD event study coefficients on the prime-aged population, $\beta_{1,k}$, while Figure 6 displays the DDD event study coefficients, $\alpha_{1,k}$, on the entire population over the age of 14. All in all, the results of Figure 5 and 6 are consistent with our primary findings in Table 6, which reveal an increasing effect on suicides over time for prime-aged men.

Table 6. Timing of the effect on suicide rates

	(1)	(2)	(3)	(4)
	Prime aged men		Prime aged women	
	DD	DDD	DD	DDD
$\tau = 2001$	-1.811 (1.716)	-0.579 (0.381)	-1.310** (0.508)	-0.253 (0.442)
$\tau = 2002-2004$	0.606 (0.918)	-0.001 (0.276)	-0.468 (0.403)	-0.451 (0.343)
$\tau = 2005-2007$ (comparison)	-	-	-	-
$\tau = 2008-2010$	-0.405 (0.755)	-0.346 (0.330)	0.383 (0.374)	-0.060 (0.351)
$\tau = 2011-2013$	-3.527*** (1.149)	-1.017** (0.486)	-1.023** (0.438)	-0.123 (0.338)
$\tau = 2014-2016$	-4.224*** (1.205)	-1.057** (0.432)	-1.076** (0.406)	0.065 (0.334)
N	5264	5264	5264	5264
Additional Controls	Y	Y	Y	Y

Notes: The dependent variable is the suicide rate per 100,000 population. Columns (1) and (3) report bunched event study coefficients from Eq. (2) while columns (2) and (4) report event study coefficients, $\alpha_{1,k}$, from Eq. (4). All models control for one-year lagged prefecture-level variables (proportion over the age of 65, proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and cell-level controls (log population, demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged. All triple difference models include prefecture-year fixed effects and year-specific exposure effects. Observations weighted by the population size in each year-prefecture-demographic group cell. Robust standard errors were clustered at the prefecture level.

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

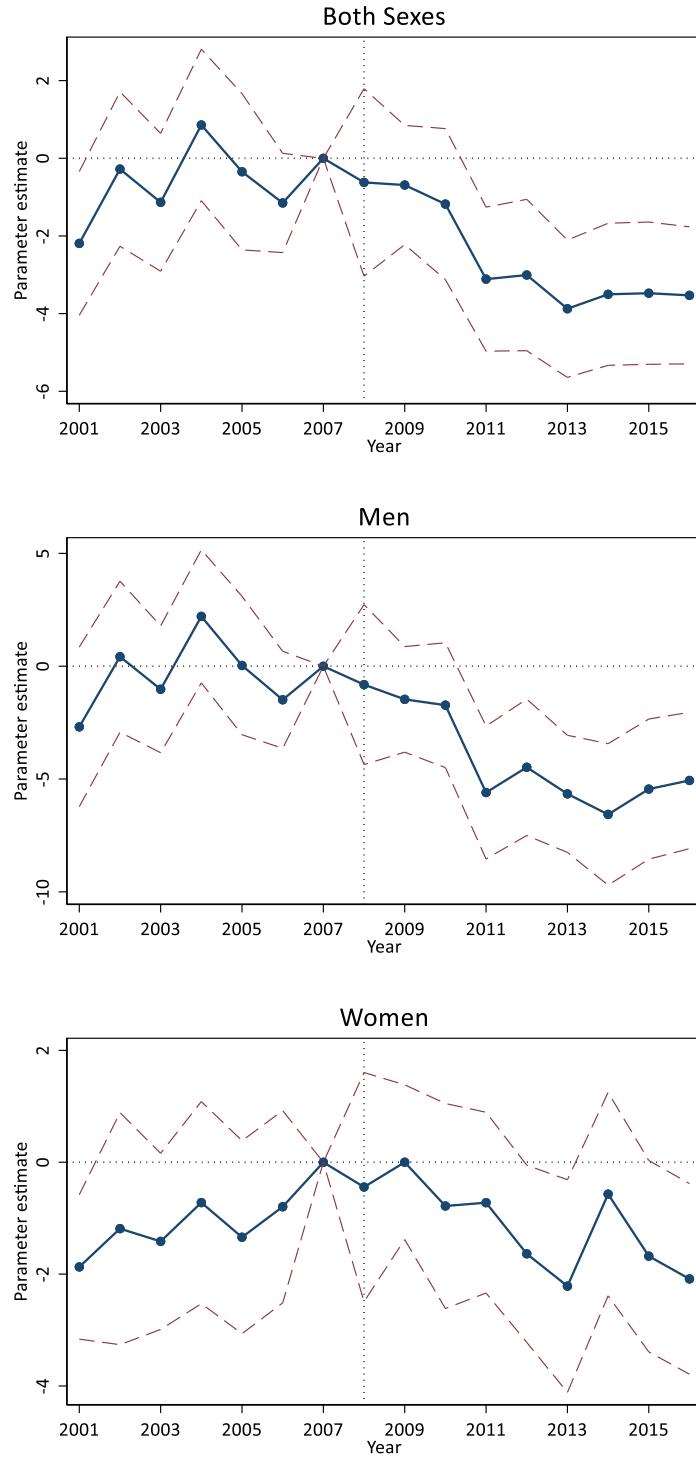


Figure 5. Difference-in-differences event study models of suicide. The dependent variable is the suicide rate per 100,000 population. The event study estimates show the results of $\beta_{1,k}$ from Eq. (2) on the prime-aged population. Dashed lines are 95% confidence intervals.

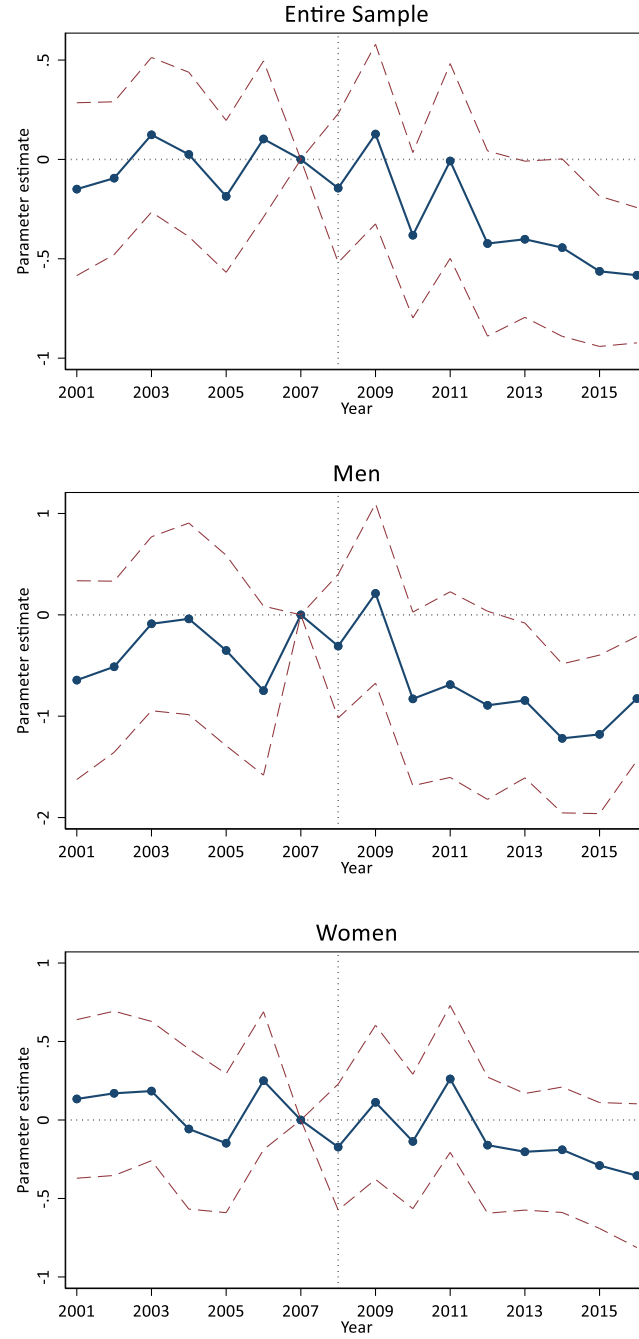


Figure 6. Triple difference event study models of suicide. The dependent variable is the suicide rate per 100,000 population. The event study estimates show the results of $\alpha_{1,k}$ from Eq. (4) on the entire population over the age of 14. Dashed lines are 95% confidence intervals.

C. Mechanisms

The link between minimum wages and unemployment, and unemployment and suicide found in previous studies merits an investigation into labor related outcomes including wages, earned income, and labor at the intensive and extensive margins as candidate mechanisms behind the relationship between minimum wages and suicide. This section reports results from Eq. (1) and Eq. (2) using individual-level repeated cross-sectional data from the CSLC (2004-2016).¹² We no longer need to rely on exposure to minimum wage across demographic groups in our individual-level analysis because we are able to directly restrict our analysis to those earning a low wage.

Panel A of Table 7 shows that the minimum wage reform had a statistically significant positive effect on hourly wages for both prime aged men and women earning a low wage, although the magnitude of the effect is larger for men. After the inclusion of controls, men experience a 40 JPY increase in hourly wages while women experience a 25 JPY increase. This result is consistent with our finding that the reduction in suicide is concentrated among men. Panel B of Table 6 shows that the minimum wage reform had a positive effect on the earned income of prime aged men, amounting to 135,000 JPY a year, or 1,350 USD under an exchange rate of 1 USD = 100 JPY. Interestingly, while we observe an increase in wage rates for both men and women, we find an increase in earned income only among men and not among women. This provides suggestive evidence for earned income as a potential mechanism behind the effect on suicide.

¹² For this analysis, we examine outcome y for individual i in prefecture j at year t . The vector of controls, $X'_{i,j,t}$, now includes individual-level controls such as respondents' age, marital status, and household size.

Table 7: Effect on earned income and wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
Panel A. Wages								
<i>Treated_j</i>	57.388**	40.880*	19.428	56.983*	35.005**	25.024*	2.754	4.378
× 1($\tau \geq 2008$)	(23.324)	(23.838)	(27.453)	(32.902)	(13.639)	(14.001)	(21.161)	(22.435)
<i>N</i>	6498	6498	3524	3524	9770	9770	4254	4254
Additional Controls		Y		Y		Y		Y
Panel B. Earned income								
<i>Treated_j</i>	19.580**	13.563*	5.333	9.300	2.129	-0.164	-0.924	-2.112
× 1($\tau \geq 2008$)	(7.984)	(7.376)	(7.374)	(8.760)	(3.105)	(3.334)	(3.628)	(4.423)
<i>N</i>	6498	6498	3524	3524	9770	9770	4254	4254
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable in Panel A is hourly wage rate in JPY and the dependent variable in Panel B is an earned income in the past year in ten thousands JPY. The table reports difference-in-differences results from Eq. (1) on a sub-sample of workers earning a low wage (i.e. a wage rate equal to or less than 1.5 times the prefectural minimum wage). All models control for prefecture and year fixed effects, and rank-by-year fixed effects. Additional controls include lagged variables at the prefecture-level variables (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

We next examine the effect of the minimum wage reform on labor at the intensive and extensive margins using data from the CSLC. According to Panel A of Table 8, there is no significant selection into or out of low-wage employment for any of the sub-populations. This result is consistent with our main findings on suicide and suggest that the minimum wage reform may have increased wages and earned income in Japan with only minor dis-employment effects. However, we acknowledge that our results could mask heterogeneity in minimum wage effects, or that our estimates on employment are underpowered due to the relatively small number of minimum wage workers in Japan, which may not be captured fully in the household survey data. Nevertheless, the coefficients from our fully specified models for prime aged groups point in the negative direction, suggesting that there may have been minor dis-employment effects among prime-aged individuals.

In contrast to the estimates on employment, Panel B of Table 8 reveals that the minimum wage reform contributed to a statistically significant decrease in the hours worked a week among prime aged women who earn a low wage by approximately 1.5 hours per week. This suggests that the increase in earned income observed among prime-aged men are the result of higher wages in the absence of changes in the quantity of work hours a week. By the same token, the absence of an increase in earned income among prime-aged women could be the result of a reduction in work hours, despite higher wages.

We next turn to the dynamic effects of prefectural minimum wages on individuals' wages, earned income, employment, and hours worked and investigate whether the timing of these effects correspond with the timing of the policy's impact on prefectural minimum wages and suicide. Table 9 reports Eq. (2) using hourly wage rate as the dependent variable. Consistent with the timing of the effect on minimum wages and suicide, we find that the minimum wage reform increased wage rates gradually after the reform and reached statistical significance after 2010. The magnitude of the effect on wages, which are higher than the actual minimum wage hikes, are also consistent with findings from previous studies which suggest that minimum wage hikes lift the earnings of those at the bottom of the wage distribution by a magnitude that is greater than actual minimum wage increases (Cengiz et al. 2019; Dube 2019; Kawaguchi and Mori 2021; Rinz and Voorheis 2018). The dynamic effects on earned income are reported in Table 10. We find that the rise in earned income among prime aged men co-occurs with the peak of the reform's impact on prefectural minimum wages. Considering that earned income reported in the CSLC is the income from the previous year, and that new minimum wages are implemented each year in the autumn, it is understandable that the event study coefficient for 2010, which reflects earnings from 2009, is positive but statistically insignificant. In Tables 11 and 12, we further find slightly negative effects on employment and hours worked on prime-aged women.

Table 8: Effect on employment and hours worked

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
Panel A. Employment								
$Treated_j$ $\times 1(\tau \geq 2008)$	0.002 (0.017)	-0.005 (0.019)	0.009 (0.011)	0.014 (0.009)	0.019 (0.015)	-0.003 (0.019)	0.002 (0.004)	0.004 (0.006)
N	12500	12500	35595	35595	29464	29464	60150	60150
Additional Controls		Y		Y		Y		Y
Panel B. Weekly hours worked								
$Treated_j$ $\times 1(\tau \geq 2008)$	0.984 (1.162)	0.352 (1.153)	-0.195 (0.792)	-0.771 (0.998)	-1.606** (0.725)	-1.516* (0.801)	0.263 (1.285)	0.393 (1.549)
N	6498	6498	3524	3524	9770	9770	4254	4254
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable in Panel A is an employment indicator and the dependent variable in Panel B is hours worked in a week. The table reports difference-in-differences results from Eq. (1) on a sub-sample of the population who are either unemployed or are workers earning a low wage (i.e. a wage rate equal to or less than 1.5 times the prefectural minimum wage). All models control for prefecture and year fixed effects, and rank-by-year fixed effects. Additional controls include lagged variables at the prefecture-level variables (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

Table 9. Timing of the effect on wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
$Treated_j$ $\times 1(\tau = 2004)$	42.427 (28.921)	46.649 (31.198)	42.321 (35.615)	9.224 (40.462)	6.778 (20.764)	14.717 (17.372)	26.890 (38.556)	33.037 (39.758)
$Treated_j$ $\times 1(\tau = 2007)$ (comparison)	-	-	-	-	-	-	-	-
$Treated_j$ $\times 1(\tau = 2010)$	48.222 (29.838)	45.324 (31.601)	15.131 (28.684)	9.644 (31.168)	21.321 (17.251)	35.579* (19.034)	17.612 (35.191)	16.971 (31.625)
$Treated_j$ $\times 1(\tau = 2013)$	92.677*** (30.904)	78.817*** (25.900)	17.809 (36.513)	59.544* (34.493)	43.993** (19.245)	34.488 (20.875)	-0.080 (53.371)	4.523 (52.165)
$Treated_j$ $\times 1(\tau = 2016)$	144.346*** (45.851)	134.626*** (44.434)	37.434 (36.875)	65.687 (41.284)	56.063*** (15.533)	52.774*** (17.284)	28.858 (24.917)	18.124 (28.462)
N	6498	6498	3524	3524	9770	9770	4254	4254
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable is the hourly wage rate in JPY. The table reports $\beta_{1,k}$ from an event study specification identical to Eq. (2) on a sub-sample of workers earning a low wage (i.e. a wage rate equal to or less than 1.5 times the prefectural minimum wage). All models control for prefecture and year fixed effects, and rank-by-year fixed effects. Additional controls include lagged variables at the prefecture-level variables (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

Table 10: Timing of the effect on earned income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
$Treated_j$ $\times 1(\tau = 2004)$	7.767 (7.000)	13.305 (8.434)	1.099 (7.382)	-2.353 (10.023)	0.115 (3.824)	1.695 (4.138)	8.913 (7.130)	13.345* (7.825)
$Treated_j$ $\times 1(\tau = 2007)$ (comparison)	-	-	-	-	-	-	-	-
$Treated_j$ $\times 1(\tau = 2010)$	15.755 (10.756)	12.177 (10.693)	4.763 (9.961)	4.453 (9.568)	2.498 (4.020)	2.302 (4.138)	4.379 (7.885)	5.973 (8.583)
$Treated_j$ $\times 1(\tau = 2013)$	28.323** (13.017)	26.640** (10.779)	6.218 (9.453)	12.361 (9.749)	-3.937 (4.512)	-6.332 (4.832)	5.664 (7.422)	4.029 (6.959)
$Treated_j$ $\times 1(\tau = 2016)$	26.916** (10.621)	26.093** (11.679)	6.247 (8.098)	10.963 (9.468)	6.287 (3.782)	4.423 (4.152)	-0.064 (4.174)	-1.416 (5.138)
N	6498	6498	3524	3524	9770	9770	4254	4254
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable is the annual earned income in ten thousands JPY. The table reports $\beta_{1,k}$ from an event study specification identical to Eq. (2) on a sub-sample of workers earning a low wage (i.e. a wage rate equal to or less than 1.5 times the prefectural minimum wage). All models control for prefecture and year fixed effects, and rank-by-year fixed effects. Additional controls include lagged variables at the prefecture-level variables (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

Table 11: Timing of the effect on employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
$Treated_j$ $\times 1(\tau = 2004)$	0.042 (0.037)	0.053 (0.038)	-0.021* (0.011)	-0.014 (0.010)	-0.024 (0.020)	-0.020 (0.018)	-0.008 (0.009)	-0.011 (0.009)
$Treated_j$ $\times 1(\tau = 2007)$ (comparison)	-	-	-	-	-	-	-	-
$Treated_j$ $\times 1(\tau = 2010)$	0.035 (0.031)	0.029 (0.033)	-0.008 (0.017)	0.004 (0.012)	0.033* (0.017)	0.027 (0.020)	-0.004 (0.010)	-0.006 (0.011)
$Treated_j$ $\times 1(\tau = 2013)$	0.007 (0.033)	0.020 (0.037)	0.014 (0.016)	0.021 (0.013)	0.038 (0.023)	0.030 (0.025)	0.014 (0.010)	0.016 (0.011)
$Treated_j$ $\times 1(\tau = 2016)$	-0.012 (0.039)	0.006 (0.040)	-0.014 (0.019)	-0.002 (0.016)	-0.040** (0.016)	-0.045* (0.023)	-0.016** (0.007)	-0.014* (0.008)
N	12500	12500	35938	35938	29464	29464	61171	61171
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable is an indicator for employment. The table reports $\beta_{1,k}$ from an event study specification identical to Eq. (2) on a sub-sample of the population who are either unemployed or are workers earning a low wage (i.e. a wage rate equal to or less than 1.5 times the prefectural minimum wage). All models control for prefecture and year fixed effects, and rank-by-year fixed effects. Additional controls include lagged variables at the prefecture-level variables (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

Table 12: Timing of the effect on hours worked

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men				Women			
	Prime aged		Older		Prime aged		Older	
$Treated_j$ $\times 1(\tau = 2004)$	-2.084 (1.409)	-1.081 (1.118)	-2.011 (1.307)	-1.437 (1.867)	0.225 (0.911)	0.107 (0.943)	1.573 (1.723)	2.470 (1.942)
$Treated_j$ $\times 1(\tau = 2007)$ (comparison)	-	-	-	-	-	-	-	-
$Treated_j$ $\times 1(\tau = 2010)$	-0.207 (1.463)	-0.559 (1.358)	-2.928* (1.708)	-2.658* (1.583)	-0.508 (0.934)	-0.882 (1.061)	0.544 (2.822)	1.138 (2.658)
$Treated_j$ $\times 1(\tau = 2013)$	0.414 (1.208)	0.543 (1.307)	1.271 (1.511)	0.899 (1.793)	-3.361*** (0.807)	-3.240*** (0.891)	3.049** (1.371)	2.883* (1.588)
$Treated_j$ $\times 1(\tau = 2016)$	-0.581 (1.152)	-0.354 (1.155)	-1.606 (1.255)	-1.403 (1.687)	-0.776 (0.698)	-0.626 (0.809)	-0.203 (1.845)	0.316 (2.035)
N	6498	6498	3524	3524	9770	9770	4254	4254
Additional Controls		Y		Y		Y		Y

Notes: The dependent variable is hours worked in a week. The table reports $\beta_{1,k}$ from an event study specification identical to Eq. (2) on a sub-sample of workers earning a low wage (i.e. a wage rate equal to or less than 1.5 times the prefectural minimum wage). All models control for prefecture and year fixed effects, and rank-by-year fixed effects. Additional controls include lagged variables at the prefecture-level variables (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs), and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

VI. Robustness Tests

A general concern with our DD estimates is that they may not reflect changes in minimum wages, but rather broader local economic conditions. One way to test for the validity of our research design is to examine whether our DD estimates are mediated by actual changes in prefectural minimum wage. Therefore, in Columns (1), (3), and (5) of Table 13, we replicate the results on suicide mortality from Table 4, Panel A, but with the inclusion of one-year-lagged log minimum wage. Columns (2), (4), and (6) of the same table report the effects of the log minimum wage without the DD coefficients. Reassuringly, our DD coefficients on suicide are no longer statistically significant with the inclusion of the minimum wage variable. Table 14 further demonstrates that for both prime-aged men and women, the inclusion of log minimum wage removes the statistical significance of the DD coefficient on labor outcomes. Thus, our mediation analysis suggests that our results for suicide mortality, hourly wages, earned income, and hours worked are driven by actual changes in the minimum wage.

Table 13. Suicide rate mediation analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Men		Women		All	
$\log \text{minwage}_{j,t-1}$	-0.318*** (0.115)	-0.408*** (0.139)	-0.276*** (0.054)	-0.238*** (0.048)	-0.289*** (0.069)	-0.317*** (0.078)
$Treated_j$ $\times 1(\tau \geq 2008)$	-1.068 (0.770)	-	0.451 (0.322)	-	-0.339 (0.493)	-
N	10,528	10,528	10,528	10,528	21,056	21,056
Additional Controls	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is the suicide rate per 100,000 population. Columns (1), (3), and (5) report the results from Eq. (1) with the inclusion of a lagged log minimum wage covariate. Columns (2), (4), and (6) report the results from a specification that replaces the DD covariates with a lagged log minimum wage covariate. All models control prefecture and year fixed effects, rank-by-year fixed effects, and one-year lagged control variables including those at the prefecture-level (proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and cell-level controls (log population, demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged. Observations weighted by the population size in each year-prefecture-demographic group cell. Robust standard errors are clustered at the prefecture level.

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

Table 14: Earned income, wage rate, and work hours mediation analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Men		Women			All
Panel A. Wage rate						
$\log\text{minwage}_{j,t-1}$	12.112*** (4.215)	13.630*** (3.355)	2.717 (3.510)	5.703** (2.812)	6.520** (2.659)	8.974*** (2.171)
$Treated_j$ $\times 1(\tau \geq 2008)$	13.504 (25.933)	-	25.903 (18.358)	-	21.499 (15.833)	-
Panel B: Earned income						
$\log\text{minwage}_{j,t-1}$	3.137* (1.661)	3.551** (1.401)	0.520 (0.682)	0.285 (0.645)	1.845** (0.845)	1.500* (0.832)
$Treated_j$ $\times 1(\tau \geq 2008)$	3.436 (8.793)	-	-1.918 (3.430)	-	-2.829 (4.585)	-
Panel C: Hours worked						
$\log\text{minwage}_{j,t-1}$	-0.069 (0.287)	0.000 (0.181)	-0.196 (0.192)	-0.301** (0.143)	-0.085 (0.186)	-0.218 (0.161)
$Treated_j$ $\times 1(\tau \geq 2008)$	0.573 (1.723)	-	-0.855 (1.085)	-	-1.087 (0.986)	-
N	6498	6498	9770	9770	16268	16268
Additional Controls	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is hourly wage rate in JPY in Panel A, annual earned income in ten thousands JPY in Panel B, and hours worked in a week in Panel C on prime-aged individuals. Columns (1), (3), and (5) report the results from Eq. (1) with the inclusion of a lagged log minimum wage covariate. Columns (2), (4), and (6) report the results from a specification that replaces the DD covariates with a lagged log minimum wage covariate. All models control prefecture and year fixed effects, rank-by-year fixed effects, and one-year lagged control variables including those at the prefecture-level (log population, proportion over the age of 65, and proportion under the age of 15, public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and individual-level controls (age, marital status, and number of household members). Observations weighted using survey weights offered by the CSLC. Robust standard errors are clustered at the prefecture level.

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

As a second robustness test, we estimate models of cancer mortality. This robustness test intends to test for unobserved shifts in access to healthcare or expansions in health insurance coverage, such as Japan's long-term care insurance policies. We examine cancer mortality because it is less likely to be affected by the minimum wage compared to other forms of mortality, such as cardiovascular disease (Lenhart 2017b). The models using Eq. (2), presented in Figure 7, do not detect any significant shifts in cancer mortality on any of the demographic groups. Similarly, the DD and DDD event study models, presented in Figure 8, do not detect meaningful shifts in cancer mortality when estimated on the entire population that is over the age of 14.

In our third and fourth robustness tests, we check for changes in prefecture-level unemployment rates and migration, respectively. Reassuringly, as reported in Figure 9, we find no endogenous changes in prefecture-level mean unemployment, nor do we find substantive evidence of endogenous changes in the ratio of in-migration to out-migration with our identification strategy (Eq. (2)).

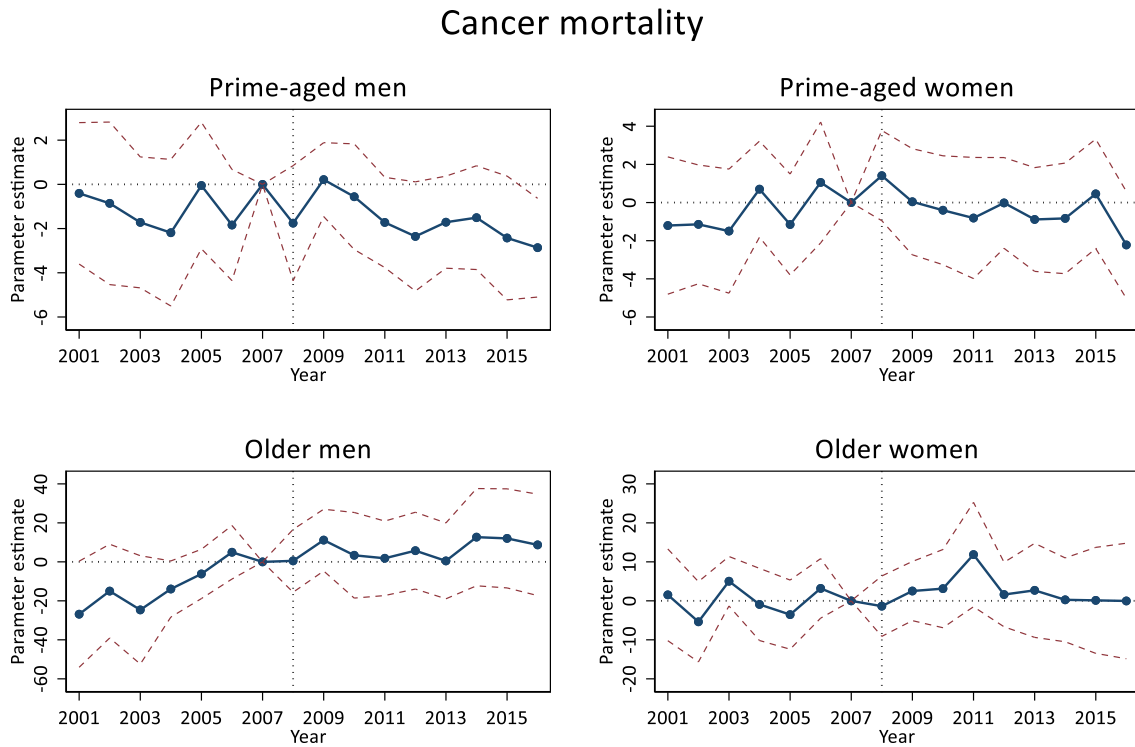


Figure 7. Difference-in-differences event study models of cancer. The dependent variable is cancer mortality per 100,000 population. Event study point estimates are from Eq. (2).

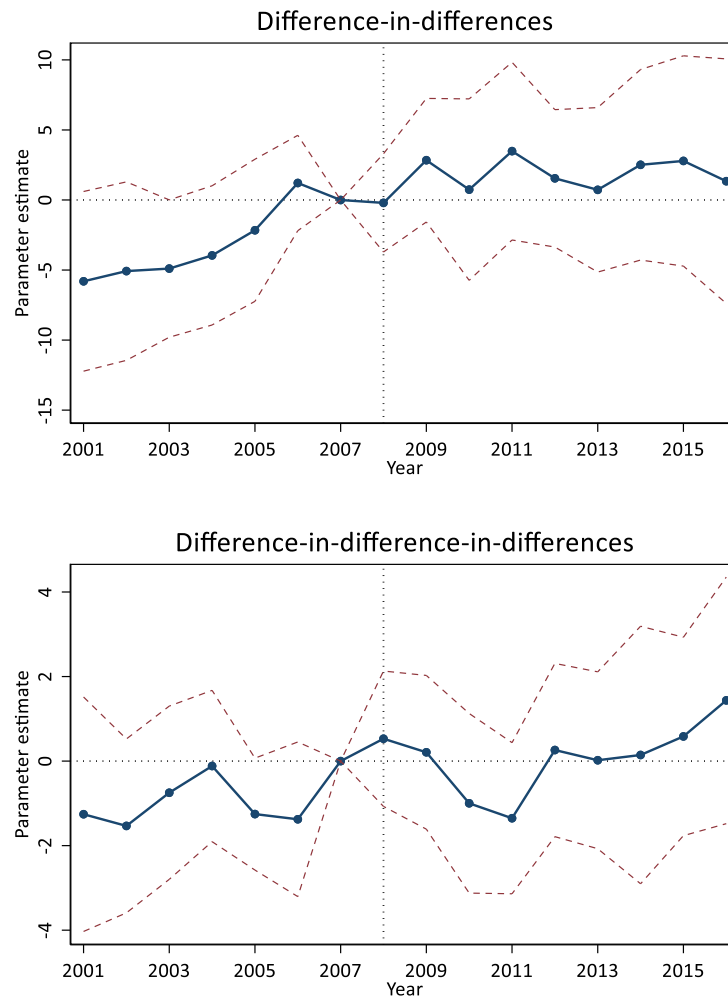


Figure 8. Difference-in-differences and triple difference event study models of cancer. The dependent variable is cancer mortality per 100,000 population. Event study point estimates include the entire population that is over the age of 14.

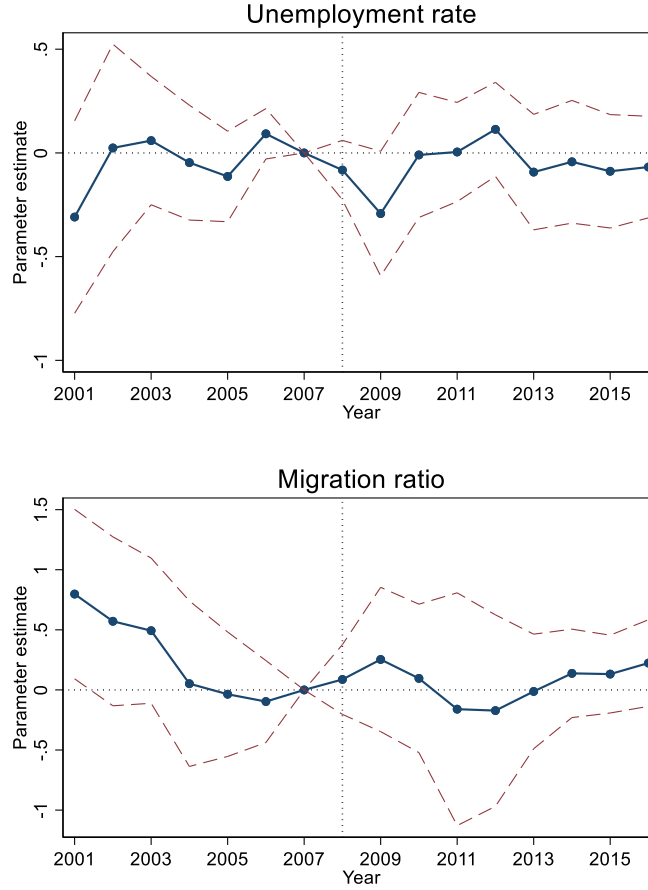


Figure 9. Difference-in-differences event study models of prefecture-level unemployment (top panel) and migration ratio in ten-thousands population (bottom panel). Event study point estimates are from Eq. (2).

VII. Conclusion and Discussion

This study contributes to our understanding of the potential benefits and tradeoffs of economic policy for social well-being. Namely, we examine the effect of a policy revision which closed the gap between minimum wages and living wages on suicide rates in Japan. We use difference-in-differences and triple differences methods, whereby we compare attainment and non-attainment prefectures before and after the revision's implementation in 2008 and examine its effects across demographic groups with varying degrees of exposure to minimum wage. Our estimates suggest that the closing the gap between minimum wages and living wages contributed to a reduction in male suicides by 1.79 deaths per 100,000 population, which is a 4.58% reduction, and an even larger decrease of 2.25 deaths per 100,000 population among the prime-aged male population, which is a 6.3% reduction. This is an economically significant magnitude, considering that a 3.69% increase in minimum wages is attributable to the reform. In contrast, studies in the United States find that a 10% increase in minimum wages lower the suicide rate by 3% (Dow et al. 2020; Gertner, Rotter, and Shafer 2019; Kaufman et al. 2020).

A simple economic model of suicide can explain the discrepancy we observe between the

magnitude of our estimates and those found in the United States (Hamermesh and Soss 1974; Marcotte 2003). While minimum wage hikes in the U.S. are announced in the year of each minimum wage hike, the minimum wage policy we examine measures both actual increases in minimum wage and higher expectations that minimum wages will increase during the five-year grace period stipulated by the Japanese government. In other words, the unique policy we study can have an impact on employees' expected future earnings. This increase in lifetime expected earnings would increase expected lifetime utility, contributing to a large decrease in the propensity of suicide.

The inconsistent evidence of an effect on women may be due to their unique socioeconomic background in Japan. Women who earn a minimum wage are from middle-class families and are typically not the main breadwinner in a family, whereas men who earn a minimum wage are usually the maximum earner in their household (CSLC, 2007). Nonetheless, failing to reject the null does not imply that we can accept the null that minimum wages have no effect on women. A more granule analysis using mortality data with information on educational attainment or detailed occupational records could uncover a statistically significant effect on women from low socioeconomic households.

Our findings on suicide, wages, earned income, and labor at the intensive and extensive margins are consistent with a large body of research which examine the economic determinants of psychological health and well-being. Unemployment is strongly positively correlated with suicides while higher income is negatively associated with suicides (Hamermesh and Soss 1974; Lundin and Hemmingsson 2009; Marcotte 2003). Our results find a modest increase in wages and income from work which directly correspond with the timing of the policy's impact on minimum wages, and a neutral effect on aggregate employment. Furthermore, we find that reductions in suicide are observed only among demographic groups that experienced increases in both earned income and wages without decreases in labor at the intensive or extensive margins. On the contrary, we find no decrease in suicide among demographic groups that did not experience increases in earned income, despite higher wages, which we found to be driven by reductions in labor at the intensive and extensive margins. This is broadly consistent with recent evidence suggesting that minimum wage hikes increase the wages of those at the left tail of the wage distribution by a magnitude greater than actual wage hikes, even for those earning above the minimum wage (Cengiz et al. 2019; Rinz and Voorheis 2018; Kawaguchi and Mori 2021). This may also suggest that the elasticity of suicidal behavior with respect to earnings is lower for women than it is for men, at least within the socio-cultural context of Japan.

There are several limitations to this paper which must be addressed. First, our supplementary analysis of labor outcomes may suffer from sample selection due to our use of repeated cross-sectional data. We tested for selection into and out of low-wage employment and controlled for various prefecture and individual-level factors to mitigate these concerns. Nevertheless, it is conceivable that this source of endogeneity bias persists. Second, our analysis of mechanisms does not provide direct evidence that increases in earned income in the absence of adverse labor market effects are the sole drivers of reductions in suicide. Nonetheless, against the backdrop of economic literature on minimum wages, we perceive hourly wages, earned income, and labor at the intensive and extensive margins as necessary outcomes to examine. Third, while we conducted several falsification tests and treated endogeneity with care in our main specifications for suicide mortality, it is possible that there remain unobserved confounding policies or economic conditions in our estimates.

Furthermore, our results do not necessarily suggest that increases in minimum wages are the best economic policy available for achieving this particular policy goal of improving welfare, nor does it imply that minimum wages are a sustainable solution. The minimum wage can incur undesirable

outcomes in the labor market such as a loss in job availability in the long run (Neumark and Wascher 2008). Our study offers no rationale for higher minimum wages that greatly exceed public assistance levels, or for sharp increases in minimum wage that do not consider the wage-paying capacity of local businesses, which are defining features of Japan's minimum wage setting process.

Despite these potential limitations, our paper offers several policy implications that may be of substantive interest to policy makers. We provide suggestive evidence that despite the potential negative consequences of minimum wages on labor at the intensive and extensive margins, the net decreases in suicides observed in our study may be indicative that the welfare improvements from increases in earned income among those who remain employed offset the losses in welfare that might accompany higher minimum wages. Furthermore, one of the conclusive policy implications of our findings is that the returns to minimum wages extend beyond higher wages when they can increase income without (or, despite) driving some workers out of employment. Future studies should thus be devoted to identifying the precise mechanisms and local economic conditions by which minimum wages lead to positive or negative spillovers to our well-being. Another avenue of research can analyze the welfare implications of possible substitutes or complements to the minimum wage, such as the earned income tax credit.

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Appendix

A. Details of 2007 Minimum Wage Reform

The minimum wage reform was ratified in 2007 and took effect in October of 2008. Non-attainment prefectures took varying amounts of time to reach attainment status. This is primarily for two reasons: (1) to minimize the damage to local economic conditions and (2) recalculations in the amount of income that would be necessary to make a basic living. Table A.1 reports that Hokkaido, Miyagi, Tokyo, Hyogo, and Hiroshima took the longest time to fulfill the criteria, while some prefectures such as Aomori, Akita, and Chiba met the requirements as early as 2011. According to Table A.2, Tokyo and Kanagawa had the largest gap between minimum wages and public assistance earnings in nominal terms. However, some of these prefectures tend to be more urbanized, which could be a source of downward bias in our estimates if we were to impose a strict linear functional form on a treatment intensity variable. Hence, we prefer a more flexible binary non-attainment variable.

The annual rank-specific benchmarks for minimum wage is reported in Table A.3. The gaps in annual minimum wage benchmarks set by the Central Minimum Wage Committee between the rank A and rank D prefectures is not always large. For instance, in 2010 and 2012, the gap between rank A and rank D benchmarks is only 1 JPY or less.

Table A.1. Treated prefectures' timing of attainment

	2008	2009	2010	2011	2012	2013	2014	2015	2016
Hokkaido	X	X	X	X	X	X	X		
Aomori	X	X	X		X	X			
Miyagi	X	X	X	X	X	X	X		
Akita	X	X	X						
Saitama	X	X	X	X	X	X			
Chiba	X	X	X		X	X			
Tokyo	X	X	X	X	X	X	X		
Kanagawa	X	X	X	X	X	X			
Kyoto	X	X	X	X	X	X			
Osaka	X	X	X	X	X	X			
Hyogo	X	X	X	X	X	X	X		
Hiroshima	X	X	X	X	X	X	X		

Notes: X is non-attainment status. In 2008, 2009, 2010, 2011, and 2012, there were minor recalculations to the monthly salary one would earn from public assistance. Data taken from: https://www.mhlw.go.jp/stf/shingi/shingi-tingin_127939_old.html (last accessed Sep 9, 2020)

Table A.2. Remaining gap between welfare and minimum wage by year

	2008	2009	2010	2011	2012	2013	2014	2015	2016
Hokkaido	53	47	39	31	30	22	11		
Aomori	11	9	6		5	2			
Miyagi	20	20	14	8	19	9	1		
Akita	9	3	5						
Saitama	41	23	14	9	12	6			
Chiba	16	5	5		6	1			
Tokyo	80	60	40	16	20	13	1		
Kanagawa	89	66	47	23	18	9			
Kyoto	33	23	20	1	8	3			
Osaka	34	26	17	7	15	8			
Hyogo	22	16	13	3	10	4	1		
Hiroshima	22	16	13	6	12	11	4		

Notes: Designated remaining gap between hourly earnings from public assistance and minimum wage in JPY. Data taken from: https://www.mhlw.go.jp/stf/shingi/shingi-tingin_127939_old.html (last accessed Sep 9, 2020)

Table A.3. Rank-specific minimum wage benchmarks

	No. of Pref	2008	2009	2010	2011	2012	2013	2014	2015	2016
Rank A	16	15	15	10	4	5	19	19	19	25
Rank B	16	11	11	10	1	4	12	15	18	24
Rank C	10	10	10	10	1	4	10	14	16	22
Rank D	5	7	7	10	1	4	10	13	16	21

Notes: Minimum wage guidelines set by the Central Minimum Wage Committee for each rank. Data taken from: https://www.mhlw.go.jp/stf/shingi/shingi-tingin_127939_old.html (last accessed Sep 9, 2020).

Table A.4. Alternative functional forms

	(1)	(2)	(3)	(4)	(5)	(6)
	Men		Women			All
Panel A. Poisson model of suicide counts						
$Treated_j$	-0.045***	-0.036**	0.017	-0.003	-0.027*	-0.026*
$\times 1(\tau \geq 2008)$	(0.015)	(0.015)	(0.016)	(0.018)	(0.014)	(0.014)
Panel B. Negative binomial model of suicide counts						
$Treated_j$	-0.049***	-0.043***	0.014	-0.004	-0.026*	-0.030**
$\times 1(\tau \geq 2008)$	(0.015)	(0.015)	(0.018)	(0.019)	(0.015)	(0.015)
Panel C. Linear model of inverse hyperbolic sine of suicide rate						
$Treated_j$	-0.043**	-0.036**	0.002	-0.011	-0.020	-0.026*
$\times 1(\tau \geq 2008)$	(0.018)	(0.017)	(0.017)	(0.020)	(0.015)	(0.015)
N	10,528	10,528	10,528	10,528	21,056	21,056
Additional Controls		Y		Y		Y

Notes: The dependent variable is the suicide count for Panels A and B, and the inverse hyperbolic sine of the suicide rate per 100,000 population in Panel C. All estimates show the results of Eq. (1) on those over the age of 14. All models control for one-year lagged variables including log of population at the cell-level, variables at the prefecture-level (proportion over the age of 65, and proportion under the age of 15), prefecture and year fixed effects, and rank-by-year fixed effects. Additional one-year lagged socioeconomic controls include those at the prefecture-level (public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and cell-level controls (demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged. Robust standard errors are clustered at the prefecture level.

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

Table A.5: Panel fixed effects models

	(1)	(2)
DV: Inverse hyperbolic sine of suicide rate (per 100,000)		
Panel A: Prime aged men		
$\log \text{minwage}_{j,t-1}$	-1.116*** (0.369)	-1.676** (0.682)
Panel B: Prime aged women		
$\log \text{minwage}_{j,t-1}$	-1.445** (0.345)	-1.945 (0.402)
N	5264	5264
Additional controls	Y	Y
Prefecture-specific linear time trends		Y

Notes: The dependent variable is the inverse hyperbolic sine of the suicide rate per 100,000 population. All estimates show the results from a specification which replaces the difference-in-differences coefficients of Eq. (1) with the log of the one-year-lag real minimum wage on the prime-aged population for Men (Panel A) and Women (Panel B). All models control for one-year lagged variables including log of population at the cell-level, variables at the prefecture-level (proportion over the age of 65, and proportion under the age of 15), prefecture and year fixed effects, and rank-by-year fixed effects. Additional one-year lagged socioeconomic controls include those at the prefecture-level (public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and cell-level controls (demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged. Column (2) controls for additional prefecture-specific linear time trends. Robust standard errors are clustered at the prefecture level.

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

Table A.6: Placebo exposure variables

	(1)	(2)	(3)
	Men	Women	All
Panel A. Exposure to low wages above minimum wages			
<i>Treated_j</i>	-0.085	-0.054	-0.069
$\times 1(\tau \geq 2008)$	(0.068)	(0.043)	(0.049)
$\times Exposure_above_i$			
Panel B. Exposure to employment			
<i>Treated_j</i>	0.963	-2.262	-0.069
$\times 1(\tau \geq 2008)$	(2.044)	(1.386)	(1.579)
$\times Exposure_employed_i$			
<i>N</i>	10,528	10,528	21,056
Additional Controls	Y	Y	Y

Notes: The dependent variable is the suicide rate per 100,000 population. All estimates show the results from Eq. (3) using two placebo exposure variables. Panel A shows the results from Eq. (3) using demographic groups' exposure to earning a wage rate greater than minimum wages but lower than twice the minimum wage. Panel B shows the results from Eq. (3) using demographic groups' exposure to employment. All models control for one-year lagged variables including log of population at the cell-level, variables at the prefecture-level (proportion over the age of 65, and proportion under the age of 15), prefecture and year fixed effects, and rank-by-year fixed effects. Additional one-year lagged socioeconomic controls include those at the prefecture-level (public assistance take-up rate, unemployment rate, GNP per capita, expenditure on public assistance per recipient, expenditure on social welfare programs) and cell-level controls (demographic group, divorce rate, unemployment rate, and home ownership rate), which, excluding demographic group, are also lagged. Robust standard errors are clustered at the prefecture level.

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

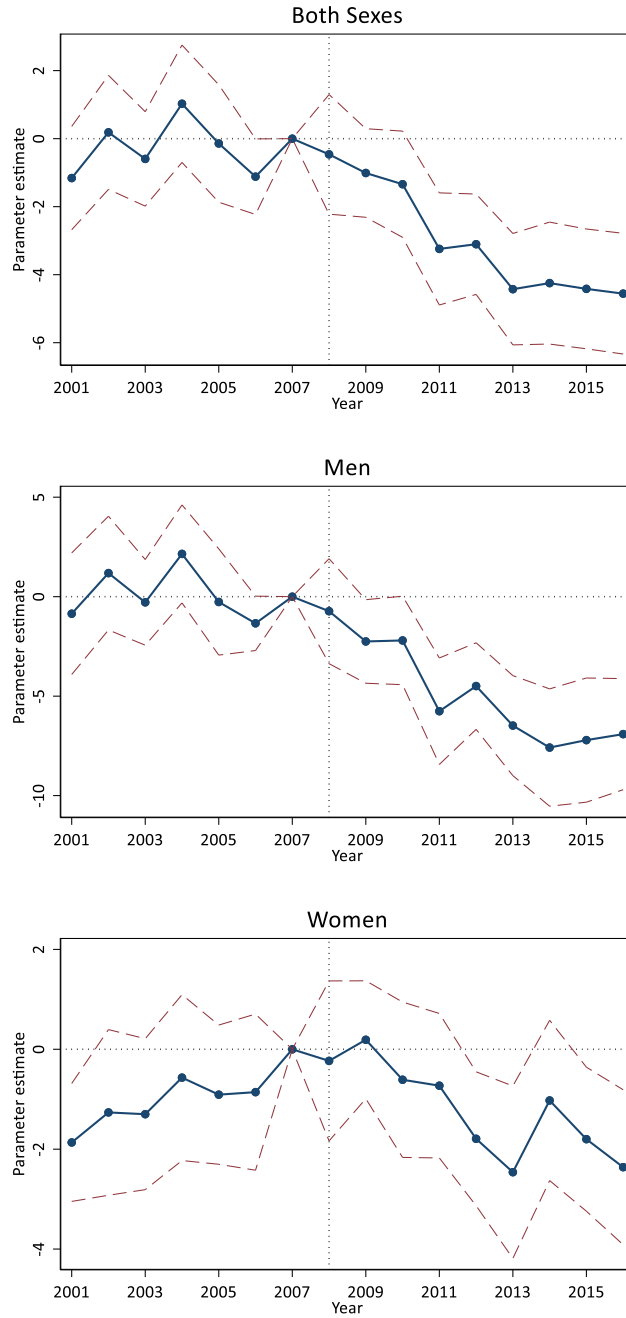


Figure A.1: Event study models of suicide with region-specific linear time trends. The dependent variable is the suicide rate per 100,000 population. The event study estimates show the results from Eq. (2) with region-specific linear time trends on the prime-aged population. 95% confidence intervals presented.

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