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(分担)研究報告書

喫煙規制が受動喫煙と予防行動に及ぼす影響についての実証研究

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研究要旨

本研究の目的は，2018 年 4 月 24 日(承認番号：厚生労働省発政統 0424 第 3 号)によって提供を受けた，『国民健康・栄養調査』(2010-2016 年)を用いて，喫煙行動に焦点を当て，政策変更が，受動喫煙，及び，予防行動にどういった影響を及ぼすかについての検証を行うことにある．具体的には，2013 年に「不特定，または多数の人が出入りする公共的空間を有する全ての施設について」喫煙禁止措置が実施された「兵庫県受動喫煙の防止等に関する条例」の施行を自治体による「介入」と位置づけ，それを「自然実験」として，実施都道府県である兵庫県と他都道府県(但し，2010 年に同様の条例を実施した神奈川県を除く)において，2013 年前後で受動喫煙に対する曝露にどのような変化があったのかについて差の差(difference-in-difference:DID)分析を行った．結果，当該条例は，飲食店などの公的空間での非喫煙者の受動喫煙に対する曝露を統計学的に有意に改善させた一方で，家庭や職場での受動喫煙リスクが大幅に高まる傾向にあることがわかった．つまり，この結果は，喫煙行動が公的空間から私的空間へと単純に移行したことを意味している．さらに問題なのは，当該条例施行後，喫煙行動に統計学的に有意な変化が観察されなかったことである．

A. 研究目的

Exposure to secondhand smoke (SHS) may cause serious illnesses like lung cancer, heart disease, and respiratory disease. Thus, to prevent the exposure to SHS, many countries have implemented the legislation of smoking bans in public places (e.g., Ireland, New Zealand, Malaysia, and Korea). However, it is still controversial about the effectiveness of such legislative smoking bans because smokers can change their smoking locations from public places to private places without curbing their tobacco consumption.

Several studies have examined how legislative smoking bans influence SHS exposure in public places. Legislative smoking bans have found to be associated with reducing SHS exposure and improving health outcomes in public places (Frazer et al., 2016; Mayne et al., 2018), and they have also improved smoking behaviors at workplaces (Evans et al., 1999; Carpenter, 2009) and at bars and restaurants (Anger et al., 2011). Some studies also found that cotinine concentration decreased among hospitality workers (Farrelly et al., 2005; Mulcahy et al., 2005; Valente et al., 2007) and non-smoking pregnant women (Schechter et al., 2018) after the implementation of public indoor smoking bans. In contrast, Adda and Cornaglia (2010) found that smoking bans increased nonsmokers' exposure to SHS because there is a displacement of smokers from public places (e.g., bars and restaurants) to private places (e.g., households). However, to our best knowledge, few studies have examined how legislative smoking bans influence SHS exposure in private places like households.

To fill this gap, this paper investigates the impact of legislative smoking bans on SHS exposure at both public and private places using data from Japan. Among developed countries, Japan is often called a smokers' paradise and ranked the least protected countries by the World Health Organization because it does not have any binding laws controlling SHS. SHS exposure is estimated to claim 15,000 lives in Japan annually. Although Japan became a Party to the WHO Framework Convention on Tobacco Control on February 27 in 2005, tobacco control policies are still weak in Japan. Municipal regulation of street smoking bans are a common practice nationwide, while the health impact of exposure to SHS is not clearly articulated, street smoking bans were introduced mainly for environment purpose like littering prevention and connection with "beautification" (Ueda et al., 2011). At the national level, smoking is not restricted or prohibited by law in indoor public places, workplaces, or on public transport.

On the other hand, at the sub-national level, Japan's two large prefectures have enacted smoke free ordinances for indoor public places with associated penalties for non-compliance. Specifically, Kanagawa Prefecture is the first prefecture that passed an ordinance to restrict smoking in indoor public places in 2009 and implemented the legislative smoking ban in 2010 (Kashiwabara et al., 2011). Hyogo Prefecture followed as the second to adopt a similar ordinance (Hyogo Prefectural Ordinance on Prevention of Exposure to Secondhand Smoke) in March 2012, and enforce the smoking ban in April 2013 (Yamada et al., 2015). By now, Kanagawa and Hyogo Prefectures have been the only two sub-nations that implemented a legislation of smoking bans with penalty for non-compliance in Japan. While some studies found associations between the smoking ban and better health outcomes, the causal influence of the smoking ban on SHS exposure has never been sufficiently examined.

Thus, this paper attempts to better identify the causal impact of Hyogo legislative smoking ban on SHS exposure by employing a difference-in-differences (DID) approach. In this approach, the changes in SHS exposure among nonsmokers in Hyogo Prefecture are compared to the changes in SHS exposure among nonsmokers in other prefectures without any smoking ban. We use data from

National Health and Nutrition Survey (NHSN) in Japan. The data provide us a unique opportunity to examine the change of individuals' exposure to SHS in different locations including households, workplaces, and restaurants. Our results show that the implementation of the legislative smoking ban decreases the probability for nonsmokers to receive occasional exposure (being exposed to SHS once per week or once per month) at restaurants by 13 percentage points. In contrast, the smoking ban increases the probability for nonsmokers to receive frequent exposure (being exposed to SHS every day or several days per week) in households and workplaces by 9.8 percentage points and 14.3 percentage points, respectively.

Our findings may provide useful implications for future tobacco control policies in Japan for the 2020 Tokyo Olympic. Responding to international calls for a smoke-free games, Japanese government approved its first national smoking ban inside public facilities on July 18th in 2018. This ordinance will be implemented in phases and coming into full force by April 2020. The new national law bans indoor smoking at schools, hospitals and government offices. For other public facilities including restaurants and bars, however, a less rigorous measure will be applied. Larger and new eateries are allowed to set up segregated, well-ventilated rooms for smoking. Smaller eateries capitalized at 50 million yen or lower and with a floor space of up to 100 square meters (which includes more than half of Japanese establishments) are exempted from the ban. This policy design is similar to Hyogo smoking ban, and thus our findings may be useful to predict potential influences of the national smoking ban.

B. 研究方法

B-1. Data

We use nationally-representative, population-based repeated cross-sectional data from the National Health and Nutrition Survey (NHNS) in the years 2010, 2013 and 2016, which was conducted by the Japanese Ministry of Health, Labor and Welfare (MHLW). The NHNS collects information about health and nutritional intake annually in November since 1947. Although Kanagawa Prefecture is the first sub-nation to introduce a legislative smoking ban in Japan, because NHNS started to collect information about self-reported secondhand smoke (SHS) exposure since 2010, we do not have the exposure information before the smoking ban which was implemented in Kanagawa Prefecture in April 2010. Thus, we use data from NHNS in 2010, 2013 and 2016 to investigate the impact of a legislative smoking ban in Hyogo Prefecture which was implemented in April 2013 on self-reported SHS exposure. Moreover, we exclude respondents in Kanagawa Prefecture in our analytical sample due to the concern that the impact of smoking ban in Kanagawa would contaminate our control group. Regarding smoking status, the data has four categories to identify individuals' smoking status, including daily smoker, occasional smoker, quitted smoking for more than one month, nonsmoker. Since SHS exposure is mainly for nonsmokers, and smoking bans are often justified to protect nonsmokers rather than smokers, our sample excludes those who were either daily smokers or occasional smokers.

We use self-reported exposure to SHS as a measure of passive smoking. In the questionnaire, there are several places for passive smoking (household, workplace, school, restaurant, game hall, and others), respondents (who are 20 years old and above) were asked how often they have exposed to SHS in each place. The measurement is frequency of exposure to SHS, including: (1) every day; (2) several days per month; (3) once per month; (4) once per week; (5) no exposure. Also, there is a choice of "do not go there" for all the locations except for household, and we exclude respondents who reported that they did not go there in estimation part since they were unlikely to receive SHS exposure and they were not affected by the smoking ban if they did not go to the specific locations.

Some may argue that exposure to SHS in some places like restaurants or game halls could affect their probability to go there, as such, the choice "do not go there" might be related to our treatment variable, the legislative smoking ban. For example, people might go to restaurants more often if the smoking ban reduced SHS exposure there. Also, despite SHS exposure hardly affect people's propensity to work, high exposure to SHS in workplaces might lead workers to change their jobs. We assume that the smoking ban did not influence whether people go to the specific locations in our sample, and our robustness checks confirm that the probabilities of respondents whether go to the place are not associated with the implementation of the smoking ban (see Appendix A, table A1).

We also control individual socioeconomic characteristics including age, gender, household size, employment status, and occupation type. Table 1 reports the descriptive statistics of the key characteristics of our analytical sample. The treatment group consists of respondents who live in Hyogo Prefecture where a legislative smoking ban was implemented, and the control group consists of respondents who live in other prefectures (exclude Kanagawa Prefecture) where no such smoking bans were introduced.

B-2. Econometric Strategy

In this section, we first illustrate a difference-in-differences (DID) approach to estimate the causal impact of a legislative smoking ban on SHS exposure. Then, we present empirical evidences to validate the assumptions of our DID design.

B-2-1. Identification strategy

In our DID approach, we estimate the following model for each location (household, workplace, and restaurant):

$$Y_{it} = \alpha + \beta DID_{it} + \boldsymbol{X}_{it}\gamma + \boldsymbol{\lambda}_t + \boldsymbol{\theta}_p + \varepsilon_{it} \quad (1)$$

where Y_{it} is the SHS exposure for respondent i at survey year t, DID is the a dummy that equals 1 if respondents i is living in Hyogo Prefecture in year 2013 and 2016, and equals 0 otherwise. $\boldsymbol{X}$ represents a vector of control variables including age, gender, household size, employment status, and occupation type. We use a full set of year dummies δ_t and a full set of prefecture dummies θ_p to capture the time fixed effects and prefecture fixed effects respectively, we also control for linear prefecture-specific time trend. ε is the error term which has a zero conditional mean and constant variance.

Although our outcome variable of SHS exposure has an ordinal structure (everyday, several times per week, once per week, once per month, no exposure), we use a multinomial logit model instead of an ordered logit model for estimation because the parallel regression assumption (proportional odds assumption) of ordered logit or probit models is violated. In ordered models, coefficients of all independent variables (except for the constant term) are assumed to be the same across the values of the outcome variable. This assumption indicates that, for example, the influence of the smoking ban on the probability for nonsmokers to receive SHS exposure is the same regardless of the degree of SHS exposure. However, the effects of a smoking ban on the probability for nonsmokers to receive SHS exposure every day and the effect of that on the probability for them to receive no exposure are qualitatively different. We report the results of an ordered logit model and a Brant test in Appendix A table A2. Based on the binary response models discussed above, this parallel regression restriction is clearly rejected by a Brant test. Therefore, a multinomial logit model is more appropriate and employed as our main model.

B-2-2. DID assumption

The common trend assumption (parallel trend assumption) in a DID design requires that the outcomes show parallel trends between the control group and the treatment group. We have two supporting facts for this assumption, although we cannot test the validity of this identifying assumption by figure with only three time periods. First, respondents in the treatment group and the control group were faced with the same tobacco price and consumption tax. Thus, we may reasonably expect that their smoking behaviors were not substantially different. Second, the NHNS conducted survey in November annually, and the survey time does not vary across different regions. Thus, respondents' preferences would not be influenced by survey time.

The DID approach also assumes that there were no other policy changes or regional shocks that affect individuals' exposure to SHS when the Hyogo smoking ban was introduced. Although cigarettes price and other anti-smoking policies like tobacco tax hike could also influence smoking behaviors and exposure to SHS, these policy changes were applied to the entire country and cigarette prices are uniform across all over Japan. Thus, we may reasonably expect that there was no such changes that influenced only Hyogo prefecture. More detailed discussions are reported in the robustness checks section.

C. 研究結果

We first present our multinomial logit estimation results about the impact of Hyogo legislative smoking ban on nonsmokers' SHS exposure for each of household, workplace, and restaurant. We then examine the robustness of our main results in terms of the following two aspects: (1) the impact of a legislative smoking ban on smoking behaviors, and (2) the influences of other confounding anti-smoking policies. The first point aims to confirm that the impact caused by a smoking ban is reasonable because there should be no significant changes in smoking behaviors given that SHS exposure decreased in restaurants while increased in household and workplace. The second point checks whether there were other tobacco control policies introduced concurrently with the smoking ban and would confound our estimates.

C-1. Main results

Although our dependent variable (exposure to SHS) has five categories in original data, the categories “have exposure to SHS once per week” and “have exposure to SHS once per month” have too few observations. Thus, to avoid the convergence failure in estimating our multinomial logit model, we had to convert the five categories into three categories (i.e., frequent, occasional, and no exposure). Specifically, “every day” and “several times per week” are classified as frequent exposure, “once per week” and “once per month” as occasional exposure, and “no exposure” as no exposure. In addition, over 75% of respondents reported that they do not go to school or game halls (see table 1), and using the remaining 25% may cause serious selection bias. Thus, we focus on household, workplace and restaurant as the main locations for passive smoking.

Table 2 summarize our estimation results. The legislative smoking ban reduced the probability of receiving occasional exposure to SHS by 13 percentage points, and this reduction is statistically significant at the 10% level. On the other hand, following the implementation of the smoking ban, the probability of receiving frequent exposure to SHS increased by 9.8 percentage points in households (the 1% significance level) and 14.3 percentage points in workplaces (the 5% significance level). Moreover, the probability of no SHS exposure in households declined by 8.9 percentage points (the 5% significance level).

C-2. Robustness checks

C-2-1. Smoking behaviors

First, we investigate how the Hyogo smoking ban affected people’s smoking behaviors. If the smoking ban reduced the SHS exposure in restaurants while increase it in households and workplaces, the smoking ban should have not affected people’s smoking behaviors overall. To investigate this point, we use repeated cross-section data from the Comprehensive Survey of Living Conditions (CSLC) in years of 2001, 2004, 2007, 2010, 2013, 2016 because NHNS did not collect smoking intensity information in 2013 and 2016. The CSLC is also a nationwide survey conducted by the Japanese Ministry of Health, Labor and Welfare (MHLW). It collects information about household characteristics and health conditions every 3 years in the first week of June since 1986. To examine the impact of a legislative smoking ban on smoking behaviors, we use the latest six waves of data from the CSLC because data before 2001 does not have information about smoking intensity.

We examine two outcome variables: smoking status and smoking intensity. In the CSLC, respondents were asked whether they currently smoked, and if so, how many cigarettes they smoke on average per day. For smoking status, respondents were classified into four categories: (1) nonsmoker (I do not smoke); (2) daily smoker (I smoke every day); (3) occasional smoker (I smoke occasionally but not every day); (4) quitter (I have stopped smoking for more than one month). Regarding smoking intensity, smokers' daily cigarette consumption is classified into categories of 1-10, 11-20, 21-30, and ≥31 cigarettes. As control variables, we include age, gender, household size, household expenditure, marital status, employment status, occupation type, and a full set of prefecture dummies, year dummies, and linear prefecture-specific time trend. Since our outcome variable of smoking status is a nominal variable, we use a multinomial logit regression model. Although the measurement of smoking intensity shows a clear ordered structure, we use a multinomial logit model instead of an ordered logit model because the parallel line assumption is violated. The results of an ordered logit estimation and a Brant test are presented in Appendix A, table A2. Average marginal effects are calculated for interpretation.

Table 3 summarize our estimation results for smoking behaviors from the multinomial logit models. Neither smoking status nor smoking intensity were significantly affected by the legislative smoking ban. The results imply that respondents in Hyogo Prefecture did not change their smoking behaviors under the restriction of the smoking ban. These results provide indirect support for our main results that exposure to SHS decreased in public indoor places while increased in private indoor places.

C-2-2. Confounding tobacco control policies

If other prefecture-level tobacco control policies that might influence smokers' smoking behaviors and nonsmokers' exposure to SHS were implemented concurrently with the Hyogo legislative smoking ban, our main estimation results would be confounded. However, no such policy changes occurred during the period of 2010-2016. Cigarettes prices do not vary across prefectures or regions in Japan, and the price of a particular brand of cigarettes is the same across all vendors from cigarette machines to big supermarkets. Moreover, there are no discounts for bulk purchases. All taxes on cigarettes, such as consumption sales tax and tobacco tax, are uniform across prefectures. The legal age for smoking is 20 years old in Japan, and it did not change during our study period either. Although Japan introduced a tobacco tax increase in October 2010, this tax hike was uniform throughout the country. Thus, its effect should be captured by our time dummy variables.

D. 考察／E. 結論

This paper examined the impact of a

legislative smoking ban on SHS exposure of nonsmokers in Japan. Hyogo Prefecture implemented a legislative smoking ban with penal code in 2013, while all other prefectures in Japan except for Kanagawa prefecture have never implemented such smoking ban. We exploited this regional policy change as a natural experiment to identify the causal impact of the legislative smoking ban on nonsmokers' exposure to SHS in both public and private places. We employed a DID framework, using nationwide data from the HNHS for the years 2010, 2013, and 2016. We found a significant reduction of SHS exposure in restaurants and a significant increase of SHS exposure in households and workplaces after the implementation of the Hyogo smoking ban. Our findings are consistent with the study of Adda and Cornaglia (2010), which demonstrated that bans in workplaces, restaurants, and bars in United States have raised nonsmokers' exposure to SHS for those who share a household with smokers. And such smoking bans hardly affect smoking prevalence, smoking cessation, and attempted quits. As Yamada et al. (2015) pointed out, the Hyogo partial smoking ban failed to provide effective protection against SHS exposure because the ordinance mentioned only SHS in public places while ignored SHS in workplaces. This was because workplaces are covered by the Industrial Safety and Health Law (ISHL) rather than the health department. This also explains why exposure to SHS increased in workplace after the smoking ban in our study.

Our findings have at least two important policy implications. First, a legislative smoking ban should not only target at pubic indoor places but also private indoor places like household. Although Japan's new national law includes smoking ban in workplaces not only restaurants, such policy may increase nonsmokers' SHS exposure in households. Second, tobacco control policies should combine smoking bans and tobacco tax hikes since excise taxes have been found to be an efficient tool to curb passive smoking.

F. 健康危険情報

特に無し.

G. 研究発表

1. 論文発表

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2. 学会発表

特に無し.

H. 知的財産権の出願・登録状況(予定を含む)

1. 特許取得

特に無し.

2. 実用新案登録

特に無し.

3. その他

特に無し.

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Tables

Table 1: Descriptive statistics of analytical sample

Passive smoking	Full		Pre-treatment		Post-treatment	
	Treat	Control	Treat	Control	Treat	Control
	N = 1003	N = 29241	N = 273	N = 5064	N = 730	N = 24177
Household						
Every day	8.37	8.43	10.62	9.99	7.53	8.10
Several times per week	3.19	2.82	1.83	3.38	3.70	2.71
Once per week	0.80	1.88	0.73	2.29	0.82	1.80
Once per month	1.99	2.21	3.30	2.88	1.51	2.07
No exposure	85.64	84.65	83.52	81.46	86.44	85.32
Workplace						
Every day	4.89	6.56	4.76	8.85	4.93	6.08
Several times per week	5.88	6.24	8.42	7.01	4.93	6.08
Once per week	3.49	2.96	4.76	3.67	3.01	2.81
Once per month	3.79	3.52	5.86	3.79	3.01	3.46
No exposure	38.68	39.79	36.63	35.25	39.45	40.74
Do not go there	43.27	40.93	39.56	41.43	44.66	40.82
School						
Every day	0.00	0.13	0.00	0.14	0.00	0.13
Several times per week	0.40	0.26	0.73	0.22	0.27	0.27
Once per week	0.30	0.19	0.00	0.22	0.41	0.19
Once per month	0.60	0.32	0.37	0.36	0.68	0.32

No exposure	20.14	19.62	16.85	16.96	21.37	20.18
Do not go there	78.56	79.49	82.05	82.11	77.26	78.92
Restaurant						
Every day	0.70	0.53	0.37	0.63	0.82	0.51
Several times per week	2.69	2.66	2.56	3.32	2.74	2.52
Once per week	8.67	6.25	12.82	6.87	7.12	6.12
Once per month	23.13	19.27	27.84	20.06	21.37	19.10
No exposure	35.79	39.50	30.77	33.02	37.67	40.85
Do not go there	29.01	31.79	25.64	36.10	30.27	30.89
Game hall						
Every day	0.30	0.24	0.00	0.22	0.41	0.24
Several times per week	1.20	1.51	0.73	1.88	1.37	1.44
Once per week	1.99	2.51	2.20	2.59	1.92	2.49
Once per month	2.69	3.84	2.20	4.36	2.88	3.73
No exposure	16.15	15.62	15.38	15.56	16.44	15.63
Do not go there	77.67	76.28	79.49	75.39	76.99	76.46
Controlled covariates						
Age	56.51	58.14	52.82	57.25	57.88	58.33
	(18.13)	(17.99)	(16.08)	(17.65)	(18.66)	(18.05)
Household size	2.88	2.91	3.09	3.03	2.80	2.88
	(1.33)	(1.40)	(1.35)	(1.43)	(1.32)	(1.39)
Gender (Male=1)	0.39	0.39	0.40	0.39	0.39	0.39
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
Employment status (Employed = 1)	0.63	0.68	0.65	0.69	0.62	0.67

	(0.48)	(0.47)	(0.48)	(0.46)	(0.49)	(0.49)
Occupation type (%)						
Technological	10.47	10.92	13.19	10.21	9.45	11.07
Management	3.99	2.99	5.49	3.02	3.42	2.99
Officer	11.47	9.28	12.45	9.14	11.10	9.31
Salesperson	3.79	4.85	4.40	5.02	3.56	4.82
Service	5.28	7.96	5.13	8.18	5.34	7.92
Security guard	0.50	0.70	1.47	0.65	0.14	0.72
Agriculture	2.79	3.70	2.56	4.40	2.88	3.55
Machine operation	1.50	1.10	2.20	1.46	1.23	1.03
Production process	6.38	7.75	5.86	8.04	6.58	7.68
Housework	28.22	22.72	28.57	25.20	28.08	22.21
Others	16.75	18.29	12.09	18.40	18.49	18.27
Students	8.87	9.73	6.59	6.28	9.73	10.45

Notes: Standard deviation for continuous variables are reported in parentheses.

Data source: National Health and Nutrition Survey (2010, 2013, 2016)

Table 2: The impact of a legislative smoking ban on nonsmokers' exposure to SHS (Multinomial logit model)

Passive Smoking	Household (1)	Workplace (2)	Restaurant (4)
Frequent exposure	0.098***	0.143**	0.039
	(0.036)	(0.067)	(0.030)
Occasional exposure	-0.009	-0.043	-0.130*
	(0.031)	(0.054)	(0.076)
No exposure	-0.089**	-0.100	0.091
	(0.045)	(0.077)	(0.077)
Pseudo R^2	0.067	0.095	0.068
Chi-Square	2105.36	2851.30	2322.48
N	30244	17843	20657

Notes: Column (1)-(3) correspond to average marginal effects derived from regression for passive smoking in household, workplace, and restaurant respectively. Base group is "No exposure". Controlled covariates include age, age square, gender, household size, employment status, occupation type, year fixed effects, prefecture fixed effects, and linear form prefecture-specific time trend. Delta-method standard errors are reported in parentheses. *Inference: * p<0.1; ** p<0.05; *** p<0.01

Table 3: The impact of a legislative smoking ban on smoking behaviors (Multinomial Logit Model)

(A)	Smoking Status			
	Nonsmoker	Daily smoker	Occasional Smoker	Quitter
	(1)	(2)	(3)	(4)
DID	-0.007	0.010	-0.001	-0.002
	(0.007)	(0.006)	(0.002)	(0.003)
Pseudo R^2	0.153			
Chi-Square	546777.65			
N	2366896			

(B)	Smoking Intensity (cigarettes / day)			
	1~10	10~20	20~30	>=31
	(1)	(2)	(3)	(4)
DID	-0.003	0.003	0.003	-0.003
	(0.014)	(0.017)	(0.013)	(0.008)
Pseudo R^2	0.059			
Chi-Square	80541.80			
N	592551			

Notes: Column (1)-(4) in panel (A) correspond to average marginal effects derived from regression for smoking status, base group is “nonsmoker”. Column (1)-(4) in panel (B) correspond to average marginal effects derived from regression for smoking intensity, base group is “smoke 1~10 cigarettes per day”. Controlled covariates include age, age square, gender, household size, household expenditure, marital status, self-rated health, employment status, occupation type, year fixed effects, prefecture fixed effects, and linear form prefecture-specific time trend. Delta-method standard errors are reported in parentheses. *Inference: * p<0.1; ** p<0.05; *** p<0.01

Appendix

Appendix A

Table A1 The impact of a legislative smoking ban on whether people do not go there (Linear Probability Model)

	Workplace	School	Restaurant	Game hall
	(1)	(2)	(3)	(4)
DID	-0.080	-0.081	0.088	-0.035
	(0.052)	(0.057)	(0.062)	(0.059)
Adjusted R^2	0.499	0.046	0.117	0.035
F-statistics	344.687	12.738	42.048	10.781
N	30244	30244	30244	30244

Notes: Column (1)-(4) correspond to coefficients estimated from linear probability models for whether respondents do not go to workplace, school, restaurant and game hall respectively. Controlled covariates include age, age square, gender, household size, employment status, occupation type, year fixed effects, prefecture fixed effects, and linear form prefecture-specific time trend. Heteroskedasticity-robust standard errors are reported in parentheses. *Inference: * p<0.1; ** p<0.05; *** p<0.01

Table A2 The impact of a legislative smoking ban on nonsmokers' exposure to SHS and smokers' smoking intensity
(Ordered Logit Model)

	Passive smoking			Smoking intensity
	Household	Workplace	Restaurant	
	(1)	(2)	(3)	(4)
DID	-0.876**	-0.646	0.202	-0.004
	(0.341)	(0.403)	(0.337)	(0.067)
Pseudo R^2	0.061	0.089	0.058	0.056
Chi-Square	1787.76	2136.49	1804.18	68538.06
N	30244	17843	20657	592551
Brant test (Chi-Square)	254.0***	280.2***	317.5***	4124.0***

Notes: Column (1)-(3) correspond to coefficients estimated from ordered logit models for passive smoking in household, workplace, and restaurant respectively. Controlled covariates include age, age square, gender, household size, employment status, occupation type, year fixed effects, prefecture fixed effects, and linear form prefecture-specific time trend. Column (4) corresponds to coefficient estimated from an ordered logit model for smokers' smoking intensity. Controlled covariates include age, age square, gender, household size, household expenditure, marital status, self-rated health, employment status, occupation type, year fixed effects, prefecture fixed effects, and linear form prefecture-specific time trend. Heteroskedasticity-robust standard errors are reported in parentheses. A Brant test is to examine the parallel regression assumption (proportional odds assumption) in ordered models, the null hypothesis is that there is no difference in the coefficients between models (several binary response models), a significant test statistic provides evidence that the parallel regression assumption has been violated. *Inference: * p<0.1; ** p<0.05; *** p<0.01