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Indoor Smoking Bans and Children's Health Outcomes in Japan*

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Abstract

Passive smoking has long been recognized as a public health threat that imposes negative externalities on non- smokers. To address this issue, Japan implemented a nationwide indoor smoking ban in April 2020, prohibiting smoking in public spaces. We hypothesize that the ban has a more direct impact on families with at least one smoker, as they are more likely to visit public areas where smoking was allowed. Consequently, the policy reduces opportunities for public smoking among these individuals, thereby lowering their children's exposure to second-hand smoke. We examine whether this policy improved the health outcomes of children from smoking households by analyzing the probability of asthma diagnoses among children under two years old in Japan. Using JMDC Claims Database monthly data from 2018 to 2023, we find that children in smoking households have a higher probability of being diagnosed with asthma compared to those in non-smoking households. This gap gradually narrowed after the implementation of the smoking ban. An event study analysis that accounts for staggered policy exposure based on children's birth time shows that the probability of asthma diagnosis among children in smoking households decreased significantly one year after the intervention. An intensity-of-treatment analysis that examines the policy's effect based on time elapsed since the intervention also reveals a significant reduction in asthma diagnoses among the treated group in 1 to 1.5 years following the smoking ban. These results are robust to environmental factors, such as the COVID-19 pandemic, under the assumption that treated and control groups were similarly affected by the pandemic.

Keywords: indoor smoking ban, asthma, secondhand smoke, public policy, child health, difference-in-differences

JEL classification: I18, I12, H75, D64

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1. Introduction

In recent years, the health risks associated with passive smoking have drawn increasing attention worldwide, prompting many countries to implement indoor smoking bans. Japan is no exception. In April 2020, the government implemented a nationwide indoor smoking ban, which prohibits smoking in public facilities for the first time in the nation's history. This paper studies the health effect of this policy on newborns. A growing body of literature has demonstrated the health benefits of public smoking bans, particularly for children, who are especially vulnerable to secondhand smoke and have little control over their living environments. For example, Millet et al.(2013)[8] find that the implementation of comprehensive smoke-free legislation in England led to a significant reduction in hospital admissions for childhood asthma across various demographic and geographic subgroups. Been et al. (2014)[2] conduct a meta-analysis and reported a 10.1% reduction in asthma-related hospital visits among children following the introduction of such legislation. However, these studies largely focus on average effects across the entire population and do not account for possible heterogeneous effects across household smoking environments. This leaves open the question of whether children in smoking households, who are exposed to second-hand smoke both publicly and privately, could experience more beneficial health effects from the policy than the children in the non-smoking families.

Several previous studies suggest that smoking bans may alter smoking behavior and location rather than eliminate smoking altogether. Zeng et al. (2019)[10] find that a partial smoking ban in Japan increased secondhand smoke exposure in households and workplaces among non-smokers, suggesting that the policy displaced smokers from public to private spaces. Boes et al. (2015)[3] show delayed reductions in smoking rates due to enforcement challenges and behavioral inertia. Ko (2020)[7] finds that although outdoor smoking bans increased quit attempts, they did not reduce overall smoking prevalence. Anger et al. (2011)[1] and Jones et al. (2015)[6] also find that public smoking bans have only limited effects on smoking prevalence or intensity in the short term, especially among subgroups with entrenched smoking behavior. These findings hint at possible displacement effects: that smokers may simply shift their behavior to less regulated spaces such as the home.

Cooper and Pesko (2017)[4] provide more direct evidence of behavioral substitution: indoor e-cigarette bans led to a 2% increase in prenatal smoking, possibly due to reduced convenience and changed risk perception. Friedman (2020)[5] shows that smoking may serve as a coping response to distress. This reinforces the concern that smoking-related policies can lead to behavioral adaptations with unintended adverse effects on children at home. For example, Renner et al. (2025)[9] highlight that disruptions in family dynamics, such as increased child-related absence from work, can intensify smoking behavior over the long term, pointing to complex interactions between stress, caregiving, and addiction.

Taken together, while existing studies provide strong evidence that public smoking bans can improve health at the population level, they also suggest the potential for negative spillovers in specific subpopulations—namely, children in smoking households. If smoking shifts indoors due to public bans, children in these environments may face increased exposure to second-hand smoke, potentially offsetting the policy's intended health benefits and even worsening outcomes like asthma incidence. However, if the exposure to indoor smoking remain unchanged, while the exposure to public smoking This study was reduced, the policy may have a positive impact on children's health. As the effects could be both positive and negative, while previous studies do not explore this possibility, this paper extends the previous research by examining whether the implementation of a public indoor smoking ban in Japan led to increased or decreased asthma diagnoses among children under age two in smoking households.

This paper proceeds as follows: the next section discusses the background and details of this policy. Section 3 describes the data and the summary statistics of the sample used in this paper. Section 4 presents the empirical framework. Section 5 reports the preliminary findings. Section 6 concludes.

2. Institutional Background

The Health Promotion Act, enacted in 2002, was the first national legislation in Japan to address passive smoking. However, its provisions were non-binding, merely recommending the establishment of designated smoking areas in public facilities such as schools, hospitals, and offices. Compliance relied largely on the discretion of individuals and businesses.

A turning point came in 2004 when Japan ratified the World Health Organization Framework Convention on Tobacco Control (WHO FCTC), committing to stronger legal measures to protect the public from tobacco smoke in enclosed environments. In response, policymakers began shifting from a "manners-based" to a "rules-based" approach to tobacco regulation. This shift culminated in a 2018 revision of the Health Promotion Act, which introduced legally enforceable restrictions on indoor smoking. Under the revised law—fully implemented in April 2020—facilities were classified into two categories. Category I facilities, including schools, hospitals, and government offices, were subject to a total indoor smoking ban with no exceptions. Category II facilities, such as restaurants, bars, and private offices, were permitted to install designated smoking rooms that met specific technical standards for ventilation and containment. Smoking in main customer-facing areas was strictly prohibited.

The policy introduced administrative penalties, including fines for non-compliance, and was supported by public awareness campaigns and standardized signage. Although certain exemptions were granted to small-scale establishments, the reform marked Japan's most comprehensive effort to reduce public exposure to secondhand smoke. Its timing also coincided with preparations for the 2020 Tokyo Olympics and growing international pressure to meet public health standards under the WHO FCTC.

Despite Japan's long-standing political and economic ties to the domestic tobacco industry, the government proceeded with the reform, signaling a shift toward prioritizing public health. According to the Ministry of Health, Labour and Welfare, the policy was associated with a modest decline in adult smoking rates, as shown in Figure 1. However, its broader effects—particularly on vulnerable populations such as young children—remain underexplored.

This study leverages the 2020 policy change as a quasi-natural experiment to estimate the short-run health effects of Japan's indoor smoking ban on children. By focusing on asthma diagnoses among infants and stratifying the analysis by household smoking status, we offer novel insights into how the benefits—or potential unintended consequences—of public smoking bans may vary across demographic groups. Our findings contribute to the broader understanding of the equity and effectiveness of smoke-free legislation in developed countries where household-level smoking remains prevalent.

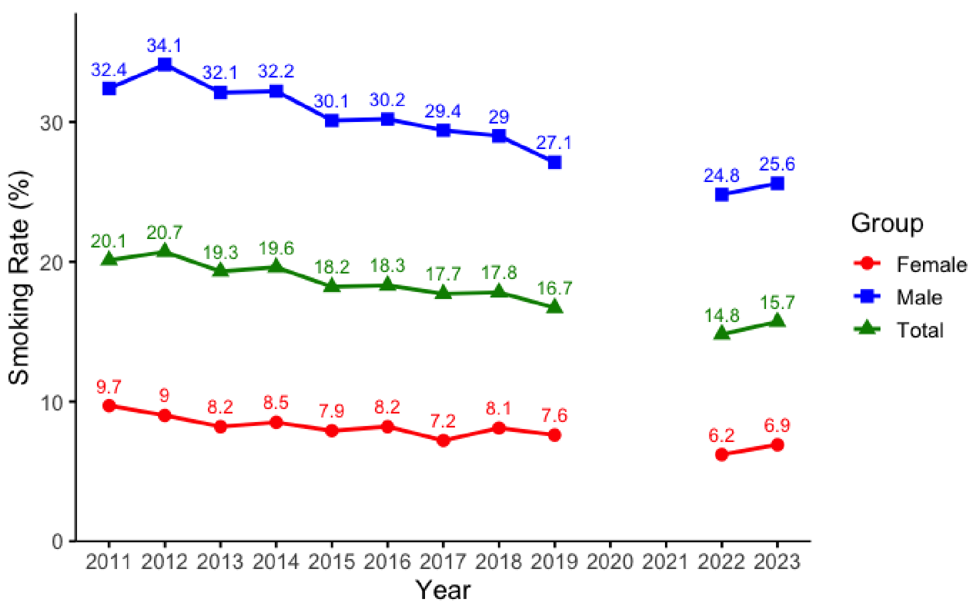


Figure 1: Smoking rate in Japan by group (2011–2023)

The data are obtained from the 2023 National Health and Nutrition Survey conducted by the Ministry of Health, Labour and Welfare. Data were not collected between 2020 and 2021, resulting in missing values for those years.

3. Data

This study utilizes data from the Japan Medical Data Center (JMDC), which provides the largest medical claims database available for academic and industrial use in Japan. The JMDC Claims Database includes detailed records of inpatient, outpatient, and prescription claims, as well as health checkup data collected from multiple employer-based health insurance societies since 2005. Individuals are uniquely identified across institutions, allowing researchers to track patients even when they switch hospitals or visit multiple facilities. Importantly, the database does not include individuals enrolled in community-based insurance schemes, such as retirees. As of August 2023, the cumulative number of individuals in the database is approximately 1.8 billion.

Based on the availability of detailed ICD-10 disease codes in this database and comprehensive family relationship information provided through employer-based health insurance coverage, we were able to link insured individuals with their family members. This allowed us to obtain precise birth dates (to the month) for children covered by the insurance. Additionally, mandatory routine health checks for insured individuals provided information on their smoking status. In this study, we focused on families in which at least one member underwent a health check and reported their smoking status. We excluded families in which any member reported quitting smoking after the implementation of the indoor smoking law (Before the indoor smoking ban, the proportion of smoking households was 25.47%, which decreased slightly to 24.37% after the policy was implemented.). Furthermore, our analysis specifically examined the incidence rate of asthma among children younger than two years of age, born between 2018 and 2023, and the research period is between January 2018 to May 2023.

Table 1 and Table 2 present summary statistics for the main variables across children from smoking and non-smoking families, based on the full sample period (January 2018 to May 2023) and the baseline period (March 2020), respectively. In the full sample, the number of children in non-smoking households is approximately 2.5 times larger than that in smoking households. Across both samples, children in smoking families exhibit a significantly higher incidence and frequency of asthma. For example, the average asthma diagnosis rate is 7.5% among children from smoking households, compared to 6.7% among those from non-smoking households. Similarly, the average number of asthma diagnoses per child is also higher in the smoking group. While these differences are statistically significant, other characteristics—such as gender distribution and average age—are only slightly different between the two groups. The overall pattern holds across both the full and baseline samples. These findings suggest that, even prior to the policy intervention, children in smoking households were consistently at greater risk of asthma.

Table 1: Descriptive Statistics by Treatment Group (Total)

	Smoking		Non-smoking		Pairwise t-test	
Variable	N	Mean/(Var)	N	Mean/(Var)	N	P-value
asthma_dummy	2919464	0.075 (0.069)	7290928	0.067 (0.062)	10210392	0.000***
asthma_num	2919464	0.084 (0.098)	7290928	0.075 (0.089)	10210392	0.000***
gender_dummy	2919464	0.514 (0.250)	7290928	0.512 (0.250)	10210392	0.000***
age_month	2919464	10.601 (46.833)	7290928	10.463 (46.593)	10210392	0.000***

Main descriptive statistics for the full study period (January 2018 – May 2023).

Table 2: Descriptive Statistics by Treatment Group (Baseline)

	Smoking		Non-smoking		Pairwise t-test	
Variable	N	Mean/(Var)	N	Mean/(Var)	N	P-value
asthma_dummy	72,071	0.072 (0.067)	169,503	0.063 (0.059)	241,574	0.000***
asthma_num	72,071	0.080 (0.091)	169,503	0.070 (0.081)	241,574	0.000***
gender_dummy	72,071	0.513 (0.250)	169,503	0.512 (0.250)	241,574	0.552
age_month	72,071	11.726 (47.894)	169,503	11.425 (47.821)	241,574	0.000***

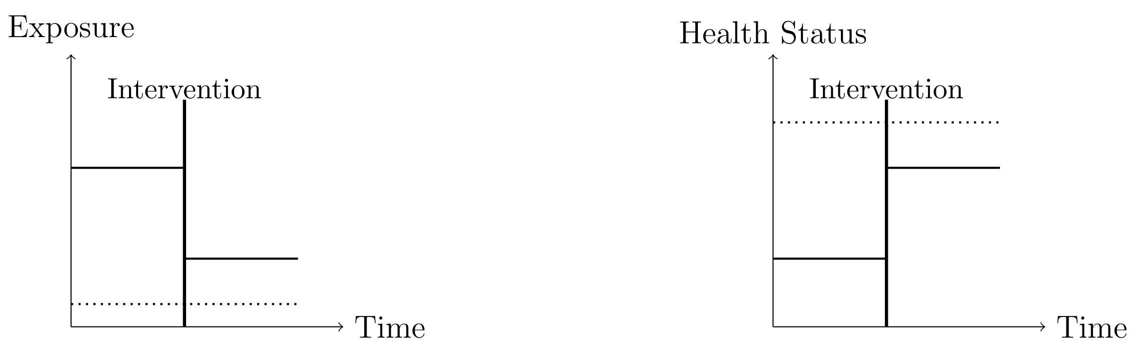
Main descriptive statistics for the baseline study period (March 2020).

4. Empirical Framework

4.1. The Basic Model and Assumptions

To evaluate the effect of the smoking indoor bans implemented in April 2020 in Japan on children's health outcomes, we consider that there are mainly two types of families in society, based on whether a family has at least one smoker. For the family without a smoker, the non-smoking family, we consider the parents to be alerted about the children's exposure to secondhand smoke. Before the policy, their children would only suffer secondhand smoke when in public places such as restaurants. But they would minimize the possibility for their children to be exposed to secondhand smoke, especially for newborns. Accordingly, we expect the policy to have minimal effect on these children's exposure, thus their health outcomes. These children are considered the control group in our settings.

The parents of a family with smokers, the smoking family, are assumed to be less concerned about their children's exposure to secondhand smoke. They would take children to public places where smoking was allowed, and would smoke at home when children are around. After the ban on indoor smoking was implemented in 2020, these children's exposure to secondhand smoke would be reduced. The policy may even raise the parents' awareness of secondhand smoke to children. Accordingly, these children in smoking families are expected to have less exposure to secondhand smoke after the ban. If the ban improves the health outcomes to these children because of the reduced exposure, we would observe this change in comparison to the control group. Figure 2 illustrates our idea.



(a) Level of Exposure

(b) Health Status

—— Treatment: Children in Smoking Families
·········· Control: Children in Non-smoking Families

Figure 2: Impact of Policy on Exposure and Health Status

Our assumption that the children's health status changes after the intervention is fully attributed to the intervention based on the following assumptions: First, the parallel trend assumption that the trends of health outcomes of children in smoking and non-smoking families are parallel before the intervention. This assumption is testable since we observe the health outcomes of the children from both families. Second, the no anticipation assumption that children's health outcomes in the treated families are similar in the treated and control states. While this assumption is not testable, it is reasonable to assume the intervention is not expected by the smoking families. Third, the pandemic happened around the same time as the policy. As the pandemic also reduces the exposure to secondhand smoke in public, we expect the pandemic would only reinforce the policy effect. The pandemic might change people's healthcare utilization because of the avoidance of public places. But it is reasonable to assume that, on average, the change of behavior are similar between the smoking and non-smoking family. Besides, the seriousness of pandemic would also affect people's healthcare utilization differently. We will deal with this possibility later after the initial analysis. Fourth, it is possible that a smoker quits smoking after the ban because he or she does not want to smoke at home, especially during the pandemic. In such case, we expect the children's health would be improved more than the children whose family members did not quit smoking after the policy. We will test this hypothesis in the later analysis.

Fifth, there are some children who would be exposed to secondhand smoke when they were in the womb. To simplify the initial analysis, we consider the control group of children who are not influenced by the smoking bans entirely, even when in the uterus. Accordingly, the children who were born before April 2020 are included in our sample, including the children of smoking and non-smoking families. These samples are the pre-treatment sample. We then include only the children born after January 2021, ie., born in February 2021 or later, as the post-treatment group because they were affected by the bans entirely even when in the uterus. This excludes children born between April 2020 to January 2021 as these children were partially affected by the policy when in the uterus. We deal with these newborns later We begin from the simplest setting as follows:

$$y_{it} = \alpha + \beta_1 Smoking + \beta_2 After + \beta_3 Smoking * After + X'_{it}\Gamma + \gamma_i + \lambda_t + \varepsilon_{it}, \qquad (1)$$

where y_{it} is the health outcome of a child i who have a doctor's visit in month t. $Smoking$ labels our treatment group that includes the children of smoking families. $After$ indicates the time period after the policy implementation in April 2020. β_3 is our focus that measures the difference of the health outcomes between children in smoking and non-smoking families after the policy. X'_{it} are the covariates related to the health outcomes of children, including parents' and children's characteristics. γ_i controls for individual fixed effects that remove the potential genetic or unobserved, time-invariant influences on children's health. λ_t is the visit-month fixed effect.

4.2. Accounting for Staggered Effects

Relative to the intervention in April 2020, each child was exposed to the policy for different lengths of time given their birthdays. Eq. (1) ignores this exposure intensity and compares only the health outcomes of children before and after the intervention. However, as pointed out by Goodman-Bacon (2021) and Wooldridge (2021), the estimates might be compromised by the comparison of treated group to already treated group. The parallel trend assumption can be violated and invalidate the estimation (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Goodman-Bacon, 2021; Baker et al., 2022). This section proposes an event study design that incorporates this staggered intervention.

Our sample consists of repeated monthly observations of each newborn's health outcomes from age 0 to 2, who were born during 2018 to 2023. Relative to the intervention timing in April 2020, the newborns can be grouped as the never treated (NT), born before May 2018; the partially treated (PT), born between May 2018 and March 2020; the in-utero treated group (UT), born between April 2020 to February 2021; and the always treated group (AT), born after January 2021. As we temporarily ruled out the UT group, this leaves us the comparison between the NT, PT, and AT groups. For these groups, we further consider their age under exposure.

Formally, denote $d^{AT}_{ym,i} = 1$ if individual i was born in year i and month m, and 0 otherwise. The AT group includes 11 cohorts born in 2021, whose health outcomes are observed up to their age 2 in 2023. Let $f_{T,t} = 1$ when the calendar year-month $T = t$, and 0 otherwise. The cohorts of the AT group with k month exposure is then

$$\sum_{ym=2102}^{2112} d^{AT}_{ym,i} \times f_{ym+k,t} \tag{2}$$

where $t = \{2102, 2103, ...2112, 2201, 2202, ...2212, 2301, 2302, ...2312\}$ for valid calender months and $k = \{1, 2, ..., 24\}$.

For comparison, we use the cohorts in the NT and PT groups to control for the parallel trends that are unrelated with the smoking ban. Starting from the NT group, including the cohorts with birthdays during January to April 2018, we denote $d^{NT}_{ym,i} = 1$ if individual i was born in year i and month m, and 0 otherwise. There are four cohorts in this group, and their growth in each calender month can be denoted as

$$\sum_{ym=1801}^{1804} d^{NT}_{ym,i} \times f_{ym+k,t} \tag{3}$$

where $t = \{1801, 1802, ...1812, 1901, 1902, ...1912, 2001, 2002, ...2012\}$ for valid calender months and $k = \{1, 2, ..., 24\}$.

For the cohorts in the PT group, we further separate them into non-exposure and under-exposure age-months. For example, the cohort born in May 2018 would be exposed to the policy for a month in April 2020 when their age is 24 months. Therefore, this group has 23 months without exposure and 1 month under exposure. Formally, we denote the cohort-period in the PT group without exposure as follows:

$$\sum_{ym=1805}^{2003} \sum_{k=1}^{ne_{ym}} d_{ym,i}^{PT} \times f_{ym+k-1,t}, \tag{4}$$

where ne_{ym} is the number of non-exposure months between birth month ym and April 2020, so that $ym+k < 202004$. The calendar months $t = \{1805, 1806, ...1812, 1901, 1902, ...19 12, 2001, 2003\}$ for valid calendar months. Relatively, we denote the cohort-period in the PT group under exposure as follows:

$$\sum_{ym=1805}^{2003} \sum_{k=ne_{ym}+1}^{24} d_{ym,i}^{PT} \times f_{ym+k-1,t}. \tag{5}$$

We are now ready to estimate the staggered treatment effect of the smoking ban on newborns' health. We first estimate the intensity of treatment under k years of exposure, which is defined as

$$\tau_{ym,k} = E[y_{ik}(d_{ym,k}^{AT}, d_{ym,24-ne_{ym}}^{PT}) - y_{ik}(d_{ym,k}^{NT}, d_{ym,ne_{ym}}^{PT})|Smoking_{ym,k} = 1], \tag{6}$$

where $y_{ik}(d_{ym,k}^{AT}, d_{ym,24-ne_{ym}}^{PT})$ is the health outcome of the groups under exposure, and $y_{ik}(d_{ym,k}^{NT}, d_{ym,ne_{ym}}^{PT})$ is the health outcome of the non-exposure groups. We estimate 24 $\tau_{ym,k}$ for each birth month. $Smoking_{ym,k}$ indicates the newborns of age k in smoking families of the cohort ym. This treatment effect can be estimated by the following specification:

$$\begin{aligned} y_{ikt} = \sum_{k=1}^{24} \tau_k \cdot \Bigg(& \sum_{ym=2102}^{2112} d_{ym,i}^{AT} \times f_{ym+k,t} \\ & + \sum_{ym=1805}^{2003} \sum_{k=ne_{ym}+1}^{24} d_{ym,i}^{PT} \times f_{ym+k-1,t} \Bigg) \times Smoking_i \\ & + \alpha_i + \lambda_t + \varepsilon_{ikt} \end{aligned} \tag{7}$$

where α_i is individual fixed effects and λ_t is calender year-month fixed effects. τ_k estimates the intensity of treatment on the treated under k years of exposure since birth.

We then estimate the treatment effect for each group ym at time t, denoted as

$$\tau_{ym,t} = E[y_{it}(d_{ym,t}^{PT}(1)) - y_{it}(d_{ym,t}^{PT}(0))|Smoking_{ym,t} = 1]. \tag{8}$$

$\tau_{ym,t}$ is the treatment effect in t that compares the health outcomes of the treated and control groups under exposure. We denote $d_{ym,t}^{PT}(1)$ and $d_{ym,t}^{PT}(0)$ as the potential outcomes of the treated in the treated and control states, respectively. Only the PT group is included because we want to test the parallel trend assumption using pre-intervention observations when $t < 202004$. We then estimate the average treatment effect on the treated by the following specification

$$y_{it} = \beta_0 + \sum_{t=1805}^{2202} \theta_t f_t + \sum_{ym=1805}^{2003} \lambda_{ym} d_{ym,i}^{PT} \times Smoking_i + \sum_{ym=1805}^{2003} \sum_{t=1805}^{2202} \tau_{ym,t}(d_{ym,i}^{PT} \times f_t \times Smoking_i) + \epsilon_{it} \quad (9)$$

The regression includes the identification of 23 partially-exposed cohorts with their health outcomes in 24 periods up to age 2. The parallel trend assumption can be tested by

$$\sum_{ym=1805}^{2003} \sum_{t<202004} \tau_{ym,t}(d_{ym,i}^{PT} \times f_t \times Smoking_i), \quad (10)$$

with the hypothesis that $H_0 : \tau_{ym,t} = 0$. Since the treatment effect could vary with newborns' time-variant characteristics such their age, we also examine Eq. (9) by adding covariates as the doubly-robust covariate-adjusted approach proposed by Wooldridge (2021) as follows:

$$\sum_{ym=1805}^{2003} d_{ym,i}^{PT} \times Smoking_i \times X_{it} + \sum_{t=1805}^{2202} f_t \times X_{it} + \sum_{t=1805}^{2202} (d_{ym,i}^{PT} \times f_t \times Smoking_i \times \dot{X}_{it}), \quad (11)$$

where $\dot{X}_{it} = X_{it} - E(X_{it} | d_{ym,i}^{PT} \times Smoking_i = 1)$.

4.3. Empirical Results

Table 3 reports the estimation results of Eq. (1). The DID estimate indicates that, relative to the children in non-smoking family, the children in smoking family have increased their probability of a visit for asthma after the smoking ban was implemented. As discussed in the empirical framework, the estimate is a result of weighted result from comparing treatment and control group in various time periods. Thus, we are more interested in the results considering the staggered effects proposed in Eq. (7). Figure 3 presents the estimates of τ_k in Eq. (7), which shows the intensity of treatment on the treated under k period of time under exposure. The results show that the policy effect is insignificant in the first 6 months after the birth of the newborns on average. The children in smoking family are then found to have about .02 more visits than the other children after 15 months of exposure, which is likely the main driver behind the DID estimates reported in Table 3. Figure 3 then reveals a negative trend in a longer exposure, where the children in the smoking family are found to have lower incidence of asthma than the other children. Accordingly, our results shows the smoking ban significantly mitigates the health gap between children in the smoking and non-smoking family after 21 months of exposure.

We report the individual estimates in Figure 3 in Table 4. Columns (2) report the results by interacting the severity of pandemic with the DID estimates. The estimates are mostly insignificant, suggesting our estimates are unrelated to pandemic.

Figure 4 reports the estimate of Eq. (9), which shows the staggered event analysis of each birth cohort. Similar to the intensity of treatment analysis, the staggered event analysis reveal the positive effects for the birth cohorts under age 1. The significant differences are not found for the older cohorts, which shows the incidence of asthma becomes similar between children of smoking and non-smoking families. Our preliminary results thus indicate the policy reduce the health gap of the children when they reach age 2.

Table 5 reports the detailed estimates shown in Figure 4. The parallel trend assumption is not satisfied before the intervention, suggesting our estimate could be biased. We plan to address this concern by adding covariates such as age. We also plan to extend the method to consider the in-utero effect by considering the exposure to children in the womb, which are currently excluded in our analysis.

Table 3: Effects of Smoking indoor ban on Asthma Incidence

	(1)	(2)	(3)
`DID`	0.006***	0.003***	0.003***
	(0.0007)	(0.0008)	(0.0008)
Control	No	No	Yes
Individual FE	No	Yes	Yes
Record Date FE	No	Yes	Yes
Observations	10,210,392	10,210,392	10,210,392
R-squared	0.0006	0.293	0.294

Clustered standard errors at the individual level in parentheses. *** $p < 0.01$.

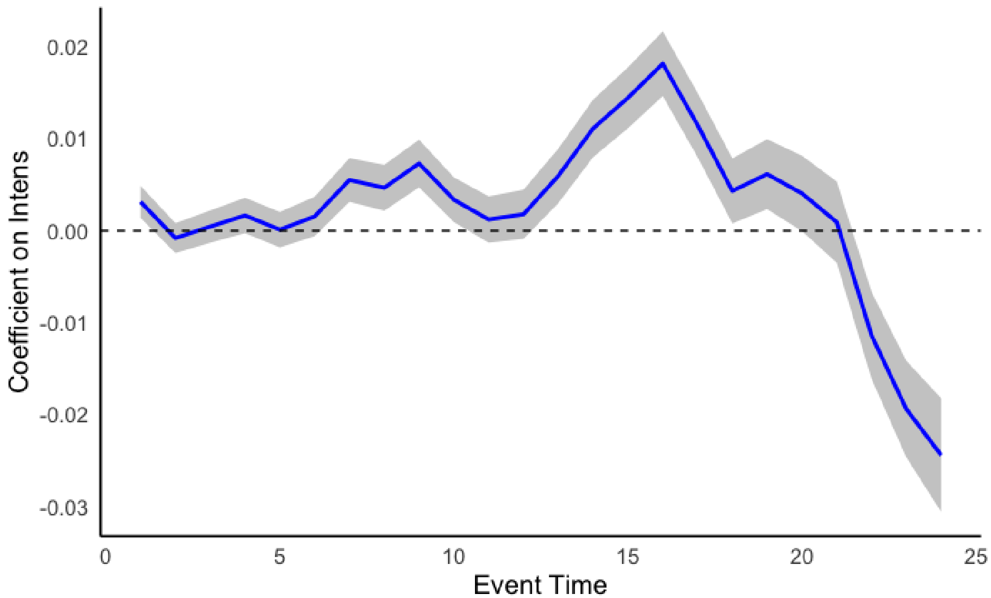


Figure 3: Exposure duration and asthma incidence among children under two years old.

Coefficients are obtained from Table 4, column (1), and the confidence interval is 95%

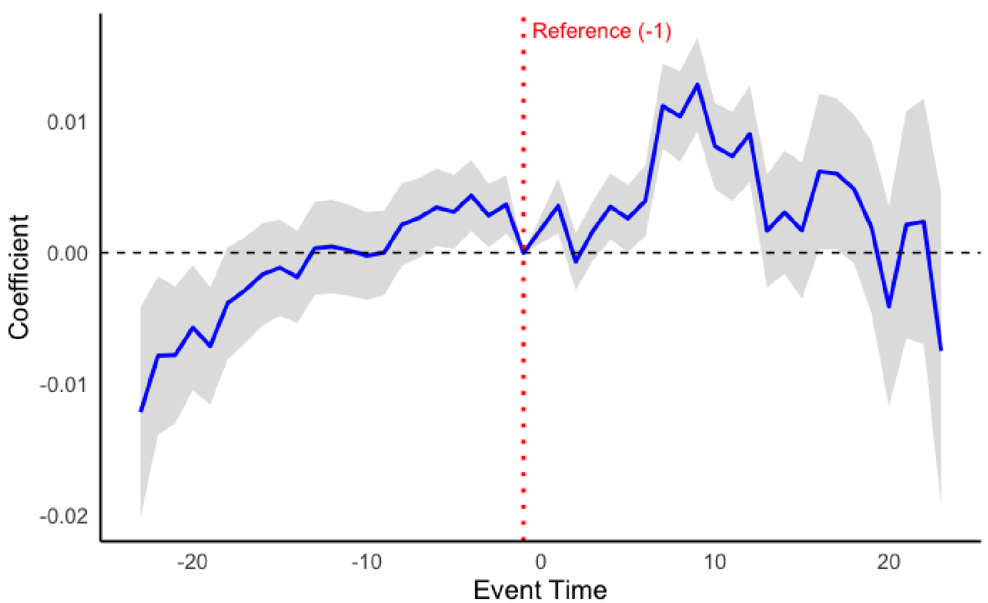


Figure 4: Parallel Trend

Estimates are based on Table 5, with month -1 as the reference. Confidence intervals are at the 95% level.

Table 4: Event Study: Effects of Smoking Indoor Ban Intensity on Asthma Incidence

Months of Exposure	(1)	(2)
One	0.0031***	0.00032***
	(0.0009)	(0.00009)
Two	-0.0008	-0.00003
	(0.0008)	(0.00009)
Three	0.0005	0.00009
	(0.0009)	(0.00009)
Four	0.0017*	0.0002
	(0.0009)	(0.00009)
Five	0.0001	0.00005
	(0.0009)	(0.00009)
Six	-0.0016*	0.00016
	(0.0011)	(0.00009)
Seven	-0.0055***	0.00047***
	(0.0012)	(0.00011)
Eight	-0.0047***	0.00038
	(0.0013)	(0.00011)
Nine	-0.0073***	0.00057
	(0.0013)	(0.00011)
Ten	0.0034**	0.00025
	(0.0012)	(0.0001)
Eleven	0.0012	0.0001
	(0.0013)	(0.00011)
Twelve	0.0018*	0.00015
	(0.0014)	(0.00011)
Thirteen	0.0059***	0.00051
	(0.0015)	(0.00012)
Fourteen	0.0111***	0.00089
	(0.0016)	(0.00012)
Fifteen	0.0145***	0.00115
	(0.0017)	(0.00013)
Sixteen	0.0182***	0.00135
	(0.0018)	(0.00014)
Seventeen	0.0115***	0.00087

Table 4 continued

	(1)	(2)
	(0.0018)	(0.00013)
Eighteen	0.0043***	0.00039
	(0.0018)	(0.00014)
Nineteen	0.0062***	0.00056
	(0.0019)	(0.00016)
Twenty	0.0041**	0.00034
	(0.0021)	(0.00017)
Twenty One	0.0009	0.00009
	(0.0023)	(0.00018)
Twenty Two	-0.0114***	-0.00078
	(0.0024)	(0.00018)
Twenty Three	-0.0194***	-0.00141
	(0.0027)	(0.0002)
Twenty Four	-0.0244***	-0.00182
	(0.0032)	(0.00025)
Covariate-adjust	No	log of COVID cases
Individual FE	Yes	Yes
Record Date FE	Yes	Yes
Observations	10,210,392	10,210,392
R-squared	0.2927	0.2927

Notes: Each row reports the coefficient on the interaction between the exposure time intensity. All models include individual and record month fixed effects. column (2) adds log-transformed COVID intensity. Standard errors clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Event Study: Parallel Trend

Event Time	Coef.	Std. Err.	95% CI
Month +1	0.0036***	0.0011	[0.0015, 0.0057]
Month +2	-0.0007	0.0011	[-0.0028, 0.0015]
Month +3	0.0017	0.0012	[-0.0006, 0.0039]

Table 5 continued

Event Time	Coef.	Std. Err.	95% CI
Month +4	0.0035**	0.0013	[0.001, 0.006]
Month +5	0.0026*	0.0013	[0.0001, 0.0052]
Month +6	0.0039**	0.0014	[0.0012, 0.0067]
Month +7	0.0112***	0.0016	[0.0079, 0.0144]
Month +8	0.0104***	0.0018	[0.0069, 0.0138]
Month +9	0.0128***	0.0018	[0.0093, 0.0164]
Month +10	0.0081***	0.0017	[0.0049, 0.0114]
Month +11	0.0074***	0.0017	[0.0039, 0.0108]
Month +12	0.0091***	0.0019	[0.0054, 0.0128]
Month +13	0.0017	0.0022	[-0.0026, 0.0059]
Month +14	0.0031	0.0024	[-0.0016, 0.0078]
Month +15	0.0017	0.0027	[-0.0035, 0.0069]
Month +16	0.0062**	0.003	[0.0003, 0.0121]
Month +17	0.006**	0.0029	[0.0003, 0.0118]
Month +18	0.0049*	0.0029	[-0.0008, 0.0105]
Month +19	0.0019	0.0034	[-0.0047, 0.0085]
Month +20	-0.0041	0.0039	[-0.0117, 0.0036]
Month +21	0.0021	0.0044	[-0.0065, 0.0108]
Month +22	0.0024	0.0048	[-0.0069, 0.0118]
Month +23	-0.0075	0.0061	[-0.0194, 0.0044]
Month -2	0.0037***	0.0011	[0.0015, 0.0059]
Month -3	0.0028**	0.0012	[0.0004, 0.0053]
Month -4	0.0044***	0.0014	[0.0017, 0.0071]
Month -5	0.0031**	0.0014	[0.0003, 0.0059]
Month -6	0.0035**	0.0015	[0.0005, 0.0064]
Month -7	0.0027*	0.0016	[-0.0004, 0.0057]
Month -8	0.0021	0.0016	[-0.0009, 0.0053]
Month -9	0.00002	0.0016	[-0.0032, 0.0032]
Month -10	-0.0002	0.0017	[-0.0036, 0.0031]
Month -11	0.0002	0.0018	[-0.0033, 0.0036]
Month -12	0.0005	0.0018	[-0.0031, 0.0041]
Month -13	0.0004	0.0018	[-0.0032, 0.0039]
Month -14	-0.0018	0.0018	[-0.0054, 0.0017]

Table 5 continued

Event Time	Coef.	Std. Err.	95% CI
Month -15	-0.0011	0.0019	[-0.0048, 0.0025]
Month -16	-0.0016	0.0019	[-0.0056, 0.0023]
Month -17	-0.0029	0.0021	[-0.0069, 0.0012]
Month -18	-0.0038*	0.0022	[-0.0081, 0.0004]
Month -19	-0.0071***	0.0023	[-0.0116, -0.0026]
Month -20	-0.0057**	0.0024	[-0.0105, -0.0009]
Month -21	-0.0078***	0.0027	[-0.0129, -0.0026]
Month -22	-0.0078***	0.0031	[-0.0139, -0.0018]
Month -23	-0.0121***	0.0041	[-0.0202, -0.0041]
Control	No	-	-
Individual FE	Yes	-	-
Record Date FE	Yes	-	-
Observations	5,563,768	-	-
R-squared	0.285	-	-

Standard errors clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5. Conclusions

Passive smoking has long been recognized as a public health threat that imposes negative externalities on non-smokers. This paper examines the health effects of Japan's nationwide indoor smoking ban implemented in April 2020, with a particular focus on asthma incidence among children under two years old. Utilizing the JMDC Claims Database, we employ both canonical and staggered DID approaches to evaluate the policy's impact. While the canonical DID model shows a significant increase of 0.3% in the probability of asthma diagnosis among children in smoking households following the policy's implementation, the staggered DID analysis reveals more nuanced dynamics. Specifically, asthma incidence appears to rise during the initial months of exposure but subsequently declines over time. The most substantial health improvements are observed after 21 months after the ban, with the effect peaking at 24 months, showing an approximate 0.2% reduction in asthma diagnoses. These findings are robust to controls for COVID-19 and other potential confounders. We also find the parallel trend assumption holds about 18 years before the policy. The overall pattern indicates that the smoking ban helped reduce health disparities between children in smoking and non-smoking households.

In future work, we plan to include additional covariates in the analysis. For instance, the health conditions of family members may play an important role in explaining asthma incidence, as asthma is not solely determined by environmental exposure but is also strongly influenced by genetic and hereditary factors shared among family members. By incorporating information on parents' and siblings' medical histories, such as the presence of asthma, allergies, or other chronic respiratory diseases, we can better isolate the causal impact of the smoking ban from family-level predispositions. In addition, we plan to incorporate hospital-level information to further refine our estimates. Although our JMDC data do not contain the exact geographic locations of medical institutions, they provide unique institution codes that can be used to construct institution-level fixed effects. By doing so, by identifying the institutions most frequently visited by each child and using those codes to define the child's primary medical provider, we can mitigate the potential omitted variable bias arising from differential healthcare-seeking behavior, even in the absence of spatial information. This approach will allow us to account for institutional heterogeneity while acknowledging the inherent limitation of not having precise location data.

Furthermore, because the policy was implemented almost simultaneously with the onset of the COVID-19 pandemic, it is crucial to disentangle the effects of the smoking ban from pandemic-induced changes in healthcare utilization and parental behavior. To achieve this, we will examine the interaction between the policy and local COVID-19 infection rates. An insignificant interaction term would indicate that our main estimates are robust to pandemic-related confounding. Beyond this, we plan to expand the scope of our study to investigate potential in-utero effects of the smoking ban. It is possible that fetuses were indirectly affected by second-hand smoke exposure when mothers were pregnant, particularly in households with smokers. By linking children's birth timing with the months of policy exposure during gestation, we can test whether reductions in prenatal exposure contributed to improved respiratory health outcomes after birth.

We also plan to further investigate heterogeneity across different family backgrounds. In particular, we will distinguish between families with a single smoking parent and those where both parents smoke, as the intensity and persistence of second-hand smoke exposure may differ substantially between these groups. Although the JMDC data do not record the smoking status of every adult for every year, we can still identify families in which one or both parents reported quitting smoking after the implementation of the indoor smoking ban. Examining these "quit-smoking families" will provide insight into how behavioral adjustments in response to the policy translate into improvements in children's health. Additionally, we intend to incorporate key perinatal characteristics such as preterm birth and low birth weight, which are known to be strong predictors of later respiratory vulnerability. By doing so, we will be able to separately examine the policy's effects among the most vulnerable children within smoking households, thereby offering a more nuanced understanding of differential policy benefits across risk groups.

Based on our findings that Japan's indoor smoking ban had a nonlinear impact on children's health in smoking households, several important policy implications emerge. First, evaluations of smoking bans should avoid focusing solely on short-term effects, as this may lead to an underestimation of their true impact. Our study shows that the policy did not lead to immediate health improvements; instead, children's health appeared to deteriorate in the medium term following implementation. However, over time, significant health gains were observed. This pattern suggests that the ban may have initially shifted smoking behavior from public spaces to private settings—particularly the home—thereby increasing children's exposure to secondhand smoke in the short run. Over the longer term, however, the policy likely prompted behavioral changes such as reduced smoking frequency or smoking cessation, ultimately improving child health outcomes. These dynamics underscore the importance of evaluating public health policies over a longer time horizon to fully capture their benefits. Second, given the temporary deterioration in health outcomes, it is essential to accompany indoor smoking bans with complementary measures aimed at preventing increased smoking in private environments. Public education campaigns on the dangers of secondhand smoke, promotion of smoke-free homes, and access to cessation support services could help mitigate these unintended short-term effects. Implementing such supportive policies may enhance the overall effectiveness of smoking bans and protect vulnerable populations during the transition period.

In sum, our findings suggest that the indoor smoking ban was effective in reducing second-hand smoke exposure for vulnerable populations and contributed to narrowing health gaps across household types. These results support the importance of sustained tobacco control policies for improving child health outcomes and provide empirical evidence relevant to similar interventions globally.

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