THREE ESSAYS ON THE IMPACT OF ENVIRONMENTAL AND POPULATION POLICIES IN SOUTH KOREA

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Abstract

This dissertation investigates the health and economic consequences of environmental shocks and policy interventions in South Korea through three empirical studies grounded in applied microeconomics. Each chapter exploits a natural experiment to identify causal effects, offering evidence relevant to environmental economics, health economics, and public policy.

The first chapter examines how temporary environmental regulations during the 2018 PyeongChang Winter Olympic Games and Asian dust events affected mortality. Using a fixed-effect instrumental variable approach with these events as instruments for PM_{10} concentrations, the study finds that a 10 $\mu g/m^3$ decrease in monthly PM_{10} leads to a 7.9% reduction in cardio-cerebrovascular and respiratory mortality. The effects are most pronounced among women and older adults, while no significant impacts are found on all-cause, cancer, or injury mortality. These results underscore the substantial public health benefits of air pollution control.

The second chapter assesses the effects of prenatal exposure to severe wildfires on birth outcomes, focusing on the April 2000 wildfires in Gangwon Province. Using a difference-in-differences framework and detailed birth registry data, the analysis finds that wildfire exposure during pregnancy significantly reduces birth weight. The adverse effect is particularly pronounced when exposure occurs during the first and third trimesters. The impact is stronger for older mothers and for female infants, highlighting heterogeneity in fetal vulnerability by maternal age and fetal sex.

The third chapter evaluates the 2013 expansion of South Korea's childcare subsidy, which extended financial support from the bottom 70% of households to all families with children aged

3-4. Using a difference-in-differences design with propensity score matching, the study finds that the policy significantly reduced total household consumption and preschool expenditures. However, these savings were not redirected to non-consumption categories such as savings or insurance. Instead, higher-income households increased spending on private extracurricular education, potentially exacerbating educational inequality. The policy had no discernible effect on maternal employment, suggesting that financial support alone may not be enough to overcome structural labor market barriers.

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Chapter 1

Introduction

The dissertation explores important intersections between environmental changes, policy interventions, and their consequences for public health and economic outcomes. The overarching theme of this research is the application of natural experiments to provide causal evidence on the impacts of environmental events and policy shifts, with implications that extend beyond local contexts to broader economic and public health discourses.

Chapter 2 addresses the health consequences of air quality improvements driven by governmental policy initiatives for a major international sporting event, the PyeongChang 2018 Winter Olympic Games, and a natural environmental phenomenon,Asian dust events. Employing instrumental variable methods, the chapter presents compelling evidence that reductions in air pollution, specifically particulate matter(PM10), significantly decrease mortality rates from cardio-cerebrovascular and respiratory diseases. The finding highlights the differential impacts based on gender and age, emphasizing the increased vulnerability of the elderly population and women.

Chapter 3 examines the prenatal health impacts of severe wildfire events in South Korea. Utilizing birth records from Gangwon Province for the period 1999–2000, this analysis reveals the negative effects of wildfire exposure on birth weight. The study further explores heterogeneous impacts by maternal age, child's gender, and gestational timing of exposure, offering nuanced insights into prenatal health vulnerabilities associated with environmental shocks.

Chapter 4 evaluates the impact of South Korea's 2013 childcare subsidy expansion, which extended eligibility from households in the bottom 70% of the income distribution to all households with children aged 3-4 years old. Through a difference-in-differences approach combined with propensity score matching, this chapter reveals significant shifts in household spending patterns attributable to the subsidy. Specifically, it finds that universal childcare subsidies reduce total household consumption and preschool expenditures while showing limited effects on maternal labor market participation.

Collectively, these studies deepen our understanding of health and household economics by examining how environmental shocks and policy interventions shape individual and community outcomes. This dissertation contributes to the literatures of health economics, public policy, environmental economics, and family and labor behavior.

Chapter 2

The Effect of Air Pollution on Mortality: Evidence from Pyeongchang 2018 Winter Olympic Games and Asian Dust

2.1 Introduction

Air pollution is a critical environmental factor that significantly affects public health. It has been associated with one third of deaths from respiratory and cardiovascular diseases (World Health Organization, 2018). Although air pollution comprises various pollutants, including nitrogen oxides, sulfur dioxide, and carbon monoxide, particulate matter (PM), a localized pollutant, is particularly harmful to human health. These particles vary in size and shape and are composed of numerous chemicals emitted by power plants, industries, and vehicles. Estimating the health effects of air pollution is crucial for developing effective policies aimed at reducing pollution. Overestimating or underestimating these effects can lead to substantial societal deadweight losses in the society.

This study uses PyeongChang 2018 Winter Olympic Games (PWOG18) and Asian dust days as a natural experiment to estimate the causal effects of air pollution on mortality in South Korea. Beginning in July 2017, the South Korean government implemented extensive regulations to reduce air pollution in Olympic regions. These policies significantly improved air quality in the regulated cities, with effects persisting beyond the event itself. To estimate the causal effect of air pollution on mortality, we employ a fixed-effects instrumental variable model. The air pollution regulations and Asian dust days serve as instruments that induce changes in air quality without directly influencing mortality. Moreover, we address the concern of endogeneity and measurement error in particulate matter, a common issue in air pollution research (He et al., 2016; Deryugina et al., 2019).

Estimating the impact of air pollution on public health poses several challenges. The primary concern is the potential endogeneity of pollution levels, and the second concern is the risk of measurement error. An instrumental variable (IV) strategy effectively addresses both issues and has been widely applied in the literature. For example, Chay and Greenstone (2003) used non-attainment status as an instrument for Total Suspended Particulates (TSPs) to evaluate the Clean Air Act Amendments' effects on infant health. Similarly, Halliday et al. (2018) and Deryugina et al. (2019) utilized wind patterns and wind directions, respectively, as instruments for particulate matter to estimate air pollution's health effects. In another study, He et al. (2016) leveraged regulations and traffic control measures during the 2008 Beijing Olympics as an instrument for particulate matter.

Our empirical strategy involves two stages. In the first stage, we estimate how regulations and Asian dust, an exogenous meteorological phenomenon, affect monthly PM_{10} concentrations, controlling for city fixed effects, year-month fixed effects, and weather variables. In the second stage, we use this exogenous variation to estimate the effects of PM_{10} on monthly age-standardized mortality rates, using Korean death records from 2015 to 2019.

Using this approach, we find that a 10 $\mu g/m^3$ increase in the monthly average PM_{10} concentration results in a 7.9% increase in the mortality rates of cardio-cerebrovascular and respiratory (CVR) disease. Furthermore, our analysis reveals heterogeneous effects of air pollution: we find no impact on all-cause, cancer, or injury mortality rates. However, the elderly are the most vulnerable to air pollution exposure, and women exhibit greater susceptibility than men.

This study makes two key contributions to the literature. First, it is the first to account for both temporary policy changes and the incidence of Asian dust (also known as yellow dust) in South Korea. Asian dust originates from the deserts of Mongolia and northern China, carrying fine, dry soil particles to China, Korea, Japan, and even the western United States (Stricherz, 2007). Before Chinese industrialization, Asian dust was viewed as a natural phenomenon of minimal concern. Today, it is a significant public health issue, as it often carries industrial pollutants from China (Choi et al., 2001; Li et al., 2012). Previous studies have explored Asian dust's health impacts: Altindag et al. (2017) used public alarms about Asian dust in South Korea to estimate its effect on infant health, and Jia and Ku (2019) combined Asian dust data with China's air quality information to analyze its impact on Korean mortality. However, these studies did not consider the potential confounding effects of temporary policy changes or events influencing air quality during their study periods. By controlling for both Asian dust and air quality regulations implemented for PWOG18, our study addresses this gap.

Second, this is the first study to estimate the effects of exogenous air quality variations, created by the Winter Olympics, on mortality rates in South Korea. While studies employing natural or quasi-experimental approaches to link air pollution with health outcomes in South Korea are limited, there is a growing body of research estimating the effects of policies on air pollution levels. For example, Yi and Sung (2018) employed a difference-in-differences approach to examine the local effects of coal-fired power plant shutdowns on PM2.5 concentrations, finding reductions of 3.7–4.4 $\mu g/m^3$. In contrast, China offers a relatively rich body of quasi-experimental studies. For instance, Rich et al. (2012) analyzed daily pollutant levels and health outcomes in 125 healthy young adults before, during, and after the 2008 Beijing Olympics, observing notable health improvements during the games. Similarly, He et al. (2016) estimated that a 10 $\mu g/m^3$ decrease in PM_{10} during the Beijing Olympics led to an 8% reduction in monthly standardized all-cause mortality rates. Inspired by He et al. (2016), we investigate the PWOG18, for which no comparable study exists. This research complements existing literature by providing insights into the mortality effects of air pollution regulations in urban areas of South Korea.

The remainder of the paper is organized as follows. Section 2 provides background information and describes the data used in the analysis. Section 3 outlines the research design, followed by Section 4, which discusses the results. Section 5 concludes the study.

2.2 Background and Data

As background for our analysis, we describe the air pollution regulations introduced for PWOG18 and the characteristics of Asian dust events. Our primary dataset includes information on cause-specific mortality, air pollution levels, the incidence of Asian dust, and weather conditions. The analysis focuses on monthly variations across 10 major cities in South Korea from 2015 to 2019. Summary statistics of the main variables are presented in Table 2.1.

2.2.1 Air Pollution Regulations for the PWOG18

The 2018 PyeongChang Winter Olympics took place from February 9 to February 25, 2018, followed by the Paralympics from March 9 to March 19, 2018. PyeongChang served as the main host city, hosting most of the snow events, while Gangneung, located approximately 40 minutes away by car, functioned as a sub-host city. Gangneung hosted all ice events, including curling, ice hockey, and figure skating, as well as the media center during the PWOG18.

In this study, Gangneung is designated as the only treatment city because particulate matter measurements in PyeongChang County and Jeongseon County began only in January 2018. Moreover, the health impacts of air pollution in urban areas differ from those in rural areas, with research indicating that individuals in rural areas are more vulnerable to air pollution than their urban counterparts (Fan et al., 2020). To prepare for the PWOG18, the South Korean government promoted environmentally sustainable practices, earning ISO 2012-1 certification, a global standard for sustainable events, in 2018. This achievement marked the first instance of the Winter Olympics being awarded this certification. (Hardy, 2018). The government constructed wind farms capable of generating electricity exceeding the minimum requirements for the event and utilized solar and geothermal energy for six newly constructed Olympic venues. Additionally, four competition venues were built on reclaimed landfill sites. To facilitate transportation for visitors and athletes, the government also invested in a high-speed train connecting Seoul to PyeongChang and Gangneung.

Despite the government's efforts to promote a sustainable Olympics, the winter season in South Korea is notoriously associated with high levels of particulate matter (PM). PM_{10} concentrations during winter are significantly higher compared to other OECD countries (Choi and Myong, 2018). To address air pollution during the PWOG18, the Gangwon Province government implemented multiple regulatory measures.

First, an old coal power plant in Gangneung was suspended from January to June 2018, while another coal power plant in the city was permanently decommissioned in July 2017, as part of the national air pollution reduction plan. Additionally, nearby power plants reduced their operations by half during the event. Second, public offices and institutions introduced an "alternate no-driving day" policy, which limited public vehicles to half of their usual operations during the games. Gangneung expanded this policy to include all private vehicles with fewer than ten passengers. Approximately 81,000 out of 103,000 registered vehicles in the city were affected by the policy. To mitigate traffic disruptions, the local government provided free public transportation. Third, all chemical production plants in the region reduced their operations by half, and construction activities were restricted during this period. The government also deployed mobile air pollution monitoring units near Olympic venues to provide real-time data on air quality. As a result of these measures, the daily average PM_{10} concentration in Gangneung during the PWOG18 decreased by 15.6%, while PM2.5 levels fell by 10.7%, compared to the same period in the previous two years (Figure 2.1).

2.2.2 Asian Dust

Asian dust (also known as Yellow dust) originates from the deserts of northern China and Mongolia and carries pollutants to South Korea, Japan, and even the west coast of the United States (Galbraith, 2013). The first documented occurrence of Asian dust dates back to AD 174, highlighting its long history in Korea. During Asian dust events, the levels of major pollutants such as PM_{10} and SO2 increase significantly (Park et al., 2003; Lee et al., 2007). Choi et al. (2001) reported that elevated levels of non-crustal elements, including nickel, copper, zinc, cadmium, and lead, provide evidence that Asian dust picks up anthropogenic pollutants before reaching Korea. If the dust did not incorporate man-made pollutants, it would consist predominantly of crustal elements such as sodium, magnesium, aluminum, calcium, and iron. In this study, Asian dust is used as an instrumental variable to control for the effects of trans-boundary air pollution from China.

As illustrated in Figure 2.2, the frequency of Asian dust events varies significantly across years. Even within the same month, the incidence of Asian dust differs across cities. For example, in April 2016, all ten sampled cities experienced at least two days of Asian dust. In contrast, in April 2019, only three cities in the northwest recorded Asian dust events. In April 2015, the southeast cities—farthest from China—experienced Asian dust, along with Incheon, the closest city to China. These variations can be attributed to differences in wind speed, wind direction, and the topography of South Korea.

Asian dust follows a distinct seasonal pattern, occurring more frequently in spring and winter, while being absent in the humid summer months. Despite this seasonality, substantial year-to-year variations exist in the timing, intensity, and geographic distribution of Asian dust events (Jia and Ku, 2019; Altindag et al., 2017). These events significantly affect PM_{10} levels, as shown in Figure 2.3.In April 2016, when Asian dust occurred on two to three days across the sampled cities, the average monthly PM_{10} concentration was approximately 70 $\mu g/m^3$. By comparison, in April 2019, when only three cities experienced Asian dust, the average PM_{10} concentration dropped to approximately 50 $\mu g/m^3$.

2.2.3 Data

Mortality

Mortality data were obtained from Statistics Korea, which provides comprehensive nationwide records at the county and city levels. These data have been published since 1982 and represent the most detailed source of death information available in South Korea. Death reporting in South Korea requires a death certificate issued by a medical doctor, and causes of death are categorized using a standardized framework that includes a 103-item general mortality condensed list, a 236-item detailed list, and a 56-item list for ranking causes of death. For this study, we utilize the 103-item general mortality condensed list to identify deaths related to air pollution.

We categorize all deaths into five groups: all-cause, cardio-cerebrovascular and respiratory (CVR) diseases, cancer, and injury. According to epidemiological literature, CVR mortality and morbidity are strongly associated with air pollution levels, while cancer and injury categories serve as placebo outcomes. Summary statistics for monthly mortality are presented in Table 2.1. To account for variations in age structure across cities, we calculate age-standardized mortality rates using population age data from July 1 of each year, provided by Statistics Korea. The monthly age-standardized mortality rate is defined as the number of deaths per 100,000 people per month.

We match the mortality data with PM_{10} data at the monthly level for nine cities. Gangneung is designated as the treatment city, while the remaining nine cities serve as control cities.

The monthly standardized CVR mortality rate in the treatment city is 15.76 per 100,000 people, with a standard deviation of 2.73. In the control cities, the monthly standardized CVR mortality rate is 15.84 per 100,000 people, with a standard deviation of 2.95.

Air Pollution

Air pollution data were obtained from AIRKOREA, a system with origins tied to another international sporting event, the 2002 FIFA World Cup Korea/Japan. Since April 2002, the Korean Ministry of Environment has released real-time air quality data near World Cup stadiums. As public interest in air pollution and environmental cleanliness grew, AIRKOREA expanded its coverage to provide nationwide air quality information starting in December 2016. The dataset includes measurements of pollutants such as PM_{10}, PM2.5, O3, CO, SO2, and NO. Monthly averages were calculated from hourly measurements.

AIRKOREA primarily focuses on urban air pollution data, meaning that air quality data are only available for cities. In PyeongChang County, air pollution measurements began in January 2018. As a result, there is insufficient data to assess the impact of the air quality regulations implemented during the PWOG18 in PyeongChang. However, Gangneung, the third-largest city in Gangwon Province, provides sufficient data to evaluate the impact of these regulations. Consequently, Gangneung is designated as the treatment city in this study.

To establish a comparison, we selected major cities from each province as control cities, matching air pollution data with mortality records. These control cities account for 24.4 million people, representing nearly half of South Korea's total population of 51.7 million in 2019. Figure 2.4 illustrates the geographical distribution of the sampled cities. Gangneung, the treatment city, is the largest city in northeastern South Korea, while the control cities are distributed across the country. The closest control city is Seoul, located 168 kilometers west of Gangneung. The distance between Gangneung and the control cities minimizes the risk of spillover effects.

Asian Dust and Weather

Asian dust and weather data were obtained from the Korea Meteorological Administration (KMA). For Asian dust, we utilized the Asian Dust Warning System, which issues warnings when the hourly average PM_{10} concentration is expected to exceed 800 $\mu g/m^3$ for more than two hours. When such warnings are issued, kindergarten and elementary school students are prohibited from participating in outdoor activities, and classes are dismissed. Additionally, outdoor sports events are rescheduled. In this study, we count the number of days with Asian dust warnings for each month in each city. Weather data were sourced from the Automated Surface Observing Systems (ASOS), the primary surface weather observing network in South Korea. All ASOS stations record weather conditions simultaneously, ensuring consistent data collection. We use temperature and precipitation data from ASOS to control for confounding weather conditions in the analysis.

2.3 Research Design

Our objective is to estimate the effect of air pollution on mortality. Specifically, we compare changes in mortality in a city that experienced improvements in air quality with those in cities that did not experience such improvements. To achieve this, we employ a fixed-effects instrumental variable model. Our primary specification is represented by the following equations:

$$\mathrm{Y}_{it} = \delta \hat{\mathrm{P}}_{it} + \mathrm{X}'_{it}\gamma + \mathrm{u}_i + \mathrm{v}_t + \varepsilon_{it} \tag{2.1}$$

$$\mathrm{P}_{it} = \lambda_1 \mathrm{R}_{it} + \lambda_2 \mathrm{A}_{it} + \mathrm{X}'_{it}\theta + \tau_i + \pi_t + \xi_{it} \tag{2.2}$$

where Y_{it} is the logarithm of monthly mortality rate per 100,000 in city i at time t, P_{it} is the monthly air pollution level in city i at time t. X_{it} is a set of control variables. Both u_i and τ_i are city fixed effects, while v_t and π_t are year-month fixed effects, and ε_{it} and ξ_{it} are error terms. R_{it}

is a dummy variable representing regulations. $R_{it} = 1$ if city i is regulated at time t, otherwise it is 0. A_{it} is the number of days of Asian Dust in city i at time t.

In the first stage, we estimate Eq. 2.2 to examine how regulations and Asian dust affected air pollution during the treatment period. Since the model includes both time fixed effects and city fixed effects, the coefficient λ_1 functions as a difference-in-differences estimator. λ_1 captures the differences in changes in air pollution levels between the regulation period (July 2017 to December 2019) and the non-regulation period (January 2015 to June 2017) across regulated and unregulated cities. If the regulations were effective, we expect λ_1 to be negative. The coefficient λ_2 quantifies the increase in air pollution attributable to an additional day of Asian dust, which we expect to be positive.

In the second stage, we estimate Eq. 2.1 to evaluate the effect of PM_{10} on monthly mortality rates. If air pollution negatively impacts public health, we expect δ to be positive. To address the endogeneity of the PM_{10} variable, we employ an instrumental variable (IV) strategy. Exposure to PM_{10} is not random and is likely subject to measurement error (Deryugina et al., 2019). Therefore, we use regulations and Asian dust as instruments for air pollution.

Figure 2.5 illustrates the timeline of regulations and the trends in the monthly average PM_{10} concentrations for both the treatment and control cities during the period 2015–2019. As discussed in previous sections, the first regulation involved the closure of an old coal power plant in Gangneung in July 2017. Therefore, we designate July 2017 as the starting month of the treatment period.

During February and March 2018, several temporary regulations, such as traffic control and temporary emission reductions, were implemented but discontinued after the PWOG18. However, the closure of coal power plants is expected to have a lasting effect on air quality in the treatment city. Consequently, we extend the treatment period to December 2019 as the ending point. Thus, the regulation dummy R_{it} equals 1 for the treatment city from July 2017 to

December 2019, and 0 otherwise.

Starting in November 2017, the treatment city exhibited significant improvements in PM_{10} concentrations, particularly during the winter months. However, the difference between the treatment city and the control cities narrowed from May to September 2018 and followed a similar pattern in 2019. Both control and treatment cities demonstrated strong seasonality in air pollution trends. The overall mean PM_{10} concentration was 49 $\mu g/m^3$, compared to the Beijing Olympics study, where He et al. (2016) reported an overall mean of 97.99 $\mu g/m^3$. Consequently, we anticipate different effects compared to the Beijing Olympics case, as the baseline levels of air pollution differ significantly. Prior to the PWOG18, both the control and treatment cities displayed similar patterns in air pollution trends.

In this study, we use monthly mortality data to avoid capturing only very short-term health effects, such as the daily impact of air pollution on mortality (Cheung et al., 2020). Schwartz (2000) demonstrated that the association between air pollution and mortality is stronger at the monthly level than at the daily level. This difference is explained by the possibility that chronic exposure to particulate matter may lead to the development of more complex cardio-cerebrovascular and respiratory (CVR) diseases, with cumulative effects manifesting over time. Consequently, using monthly data allows us to capture the medium-run effects of air pollution on mortality.

Additionally, lagged effects of air pollution on mortality may exist. For example, in a medical study, Kim et al. (2003) found that air pollution levels on the day of death are more strongly associated with respiratory mortality, while air pollution levels from the previous day are more closely linked to cardiovascular mortality. However, Cheung et al. (2020) found no significant lagged effects at the monthly level. Therefore, we do not test for lagged effects in this study.

2.4 Results

2.4.1 Main results

Table 2.2 presents the results from estimating Eq. 2.2, the first-stage regression. In column (1), we estimate the effect of the two instruments on PM_{10} concentrations without including weather controls. In columns (2) and (3), we gradually introduce controls for temperature, squared temperature, and precipitation. Both temperature and precipitation may influence both air pollution and mortality. For example, Deschênes and Moretti (2009) found that extreme heat and cold significantly increase immediate mortality. Precipitation, on the other hand, may be negatively correlated with air pollution, as rainfall removes pollutants from the air. Moreover, precipitation can impact mortality by altering humidity levels and the broader disease environment (He et al., 2016).

All instruments have statistically significant effects on particulate matter. The estimated coefficients remain stable across all specifications. The regulations reduced PM_{10} concentrations by 6.4–7.2 $\mu g/m^3$, while Asian dust increased PM_{10} concentrations by 0.85–1.24 $\mu g/m^3$. Although the magnitude of the effect from Asian dust is smaller than that of the regulations, the results indicate that both air quality regulations and Asian dust have a significant impact on PM_{10} concentrations. Additionally, the F-statistics confirm that the instruments are not weak.

Table 2.3 presents the results of the effect of air pollution on all-cause mortality. Unlike the findings of He et al. (2016), there is no significant effect on all-cause death rates in this study. Several factors may explain the differing results between South Korea and China. One possible explanation is that the magnitude of air pollution reduction was much smaller in South Korea. During the PWOG18, PM_{10} concentrations decreased by 14 $\mu g/m^3$, compared to a reduction of approximately 26 $\mu g/m^3$ during the 2008 Beijing Olympics (He et al., 2016). Additionally, South Korea's baseline air quality levels were significantly better than those in China. Another potential explanation is omitted variable bias, as all-cause mortality encompasses a wide range of causes of death. Given these limitations, we focus on cardio-cerebrovascular and respiratory (CVR) mortality, which is more closely associated with air pollution.

Table 2.4 presents the results of the effect of air pollution on CVR mortality. The control variables, city fixed effects, and year-month fixed effects are consistent with those used in Table 2.3. The coefficients for PM_{10} are statistically significant at the 5 percent level and remain robust after controlling for weather. The 2SLS results indicate that a 10 $\mu g/m^3$ reduction in monthly PM_{10} concentrations leads to a 5.8–7.9 percent decrease in monthly CVR mortality. The overall models explain 75 percent of the variation in CVR mortality rates. These findings are consistent with the study of the Beijing Olympics, which reported an 8.8 percent reduction in monthly CVR mortality (He et al., 2016). This result provides causal estimates of the effect of PM_{10} on standardized CVR monthly mortality in South Korean urban areas.

2.4.2 Heterogeneous effect of air pollution

The relationship between air pollution and mortality caused by CVR diseases is well-documented in the literature. However, associations between air pollution and other causes of death are less frequently studied. To address this gap, we examine the effect of PM_{10} on cause-specific mortality rates. Table 2.7 presents the estimation results for the effects of PM_{10} on CVR mortality, all-cause mortality, cancer mortality, and injury mortality, based on the most restrictive specification in Table 2.4. The results for CVR mortality indicate that a 10 $\mu g/m^3$ reduction in monthly PM_{10} concentrations leads to a 7.9 percent decrease in the CVR mortality rate. Although the coefficients for all-cause, cancer , and injury mortality are negative, they are statistically insignificant and close to zero in magnitude. These findings highlight how particulate matter adversely impacts public health, primarily affecting CVR diseases.

Table 2.8 presents the heterogeneous impacts of air pollution on gender-specific mortality rates for all-cause and CVR mortality. We analyze males and females separately to account for gender differences. The results indicate no significant effect on all-cause or CVR mortality rates for males. However, females exhibit a relatively higher coefficient for CVR mortality, which is statistically significant at the 10 percent level. This finding aligns with the epidemiological literature, which highlights greater female susceptibility to air pollution (Oliveira et al. 2011; Chen et al. 2012).

We examine the heterogeneous effects of PM_{10} on mortality rates across different age groups. Table 2.9 presents regression results for all-cause and CVR mortality rates by age group. Consistent with findings in the literature, the elderly exhibit stronger effects on all-cause mortality rates. However, we fail to find statistical significance in most age groups, likely because the mortality rates for some groups are very low, and the monthly variations in these rates are insufficient to yield statistical significance. Nonetheless, comparing the magnitudes of the coefficients provides reasonable insights into how air pollution impacts mortality.

For the 0–4 age group, the coefficients for both CVR and all-cause mortality are close to zero. This is expected, as infants and toddlers are less likely to die from CVR diseases. For individuals younger than one year, the leading causes of death are congenital malformations, deformations, and chromosomal abnormalities. In contrast, the 65–69 age group shows the strongest effect on all-cause mortality, with statistical significance at the 5 percent level. For CVR mortality, the 70–74 age group demonstrates the strongest effect, with statistical significance at the 10 percent level. These findings suggest that for individuals aged 65 and older, the impact of air pollution on mortality may primarily occur through non-CVR diseases, except for the 70–74 age group.

2.4.3 Validity of instrumental variables

We used air pollution regulations and Asian dust days as instruments. For these instruments to be valid, they must be highly correlated with PM_{10} and must affect mortality only through their effects on PM_{10}. First, we assess the correlation between the instruments and PM_{10}.

The first-stage results in Table 2.2 show that the estimated coefficients of both instruments are statistically significant across all three specifications. The regulations reduced PM_{10} concentrations by 6.4–7.2 $\mu g/m^3$, while one additional day of Asian dust increased PM_{10} concentrations by 0.85–1.24 $\mu g/m^3$. Furthermore, the F-statistic ranges from 62.2 to 75.5, which exceeds the recommended threshold of 10 for a reliable 2SLS estimation. Additionally, weather controls have little effect on the estimated coefficients, suggesting that the two instrumental variables are not correlated with weather conditions.

Second, since we have two instruments for PM_{10}, we perform a test of overidentifying restrictions to verify the validity of the excluded instruments. The null hypothesis of the overidentification test is that the instruments are valid. The results, presented in Table 2.5, indicate that we do not reject the null hypothesis at the 1%, 5%, and 10% significance levels, supporting the validity of the instruments.

Third, we conduct an endogeneity test for PM_{10}. If PM_{10} were exogenous, 2SLS and instrumental variables would not be necessary. The results of the endogeneity test, shown in Table 2.6, reject the null hypothesis that PM_{10} is exogenous at the 5% significance level, based on both the Durbin and Wu-Hausman tests. These results validate the use of the instruments and the 2SLS approach.

2.4.4 Robustness Checks

As a robustness check, we compare the effect of regulations on mortality using a reduced-form model to demonstrate that these regulations primarily affect air-pollution-related deaths:

$$\mathrm{Y}_{it} = \lambda_1 \mathrm{R}_{it} + \mathrm{X}'_{it}\theta + \tau_i + \pi_t + \varepsilon_{it} \tag{2.3}$$

where Y_{it} represents the logarithm of cause-specific mortality in city i at time t, and R_{it} is a dummy variable that equals 1 for the treatment city starting from July 2017, capturing the interaction term in a difference-in-differences analysis. X'_{it} is a set of control variables, including weather conditions and Asian dust days. τ_i and π_t denote city fixed effects and month-year fixed effects, respectively.

One of the main concerns regarding the identification strategy is that the regulations in the treatment city might have led not only to improvements in air quality but also to other factors that could affect public health in the treatment city. For example, it is possible that more medical treatments became available in the treatment city due to hosting the Olympic Games. If the regulations significantly altered other health-influencing factors, air-pollution-irrelevant mortality rates would also decrease in the treatment city.

The empirical results, however, suggest that overall public health remained unchanged and even worsened for cancer-related mortality during the regulated period. Table 2.10 reports the results. The regulations had a negative impact on CVR mortality, indicating that fewer people died from CVR diseases in the treatment city during the regulated period compared to the unregulated period. While CVR mortality decreased, cancer mortality increased, and there was no significant effect on injury or all-cause mortality. In other words, the regulations reduced CVR mortality in the treatment city but did not affect cancer, injury, or all-cause mortality.

The coefficient estimates of Equation 2.3 can be interpreted as difference-in-differences (DID) estimates since both city fixed effects and month-year fixed effects are included. Figure 2.6 illustrates the differences in mortality rates between the treatment city and the control cities in 2016 and 2019. Although the treatment period spans the entire years of 2018 and 2019, data from 2018 are omitted as they exhibit similar patterns.

In the top-left panel, the solid line represents the difference between the treatment and control cities in 2019, while the dotted line represents the same difference in 2016. The solid line is consistently lower than the dotted line during the winter and spring months, suggesting that the treatment had a significant impact on reducing CVR mortality, particularly during periods when air pollution is severe. Conversely, the differences for non-CVR mortality are higher in 2019, implying that more deaths from non-CVR causes occurred during the regulated period. The differences in injury and cancer mortality rates between the treatment and control cities remain similar in 2016 and 2019. These patterns are consistent with our findings.

During the PWOG18, it is reasonable to argue that temporary confounding factors may have influenced the analysis (He et al., 2016). For example, traffic control measures might have allowed patients to access medical treatment more quickly, while exposure to cold weather at sporting events or the increased incidence of heart attacks due to the excitement of the events could have affected health outcomes. The PWOG18 was held in February and March, during which Figure 2.5 illustrates significant air quality improvements.

To address potential bias caused by these temporary confounding factors, we exclude these months from the estimation of Equation 2.1. If excluding February and March 2018 significantly alters the coefficient estimates for PM_{10}, it would suggest that temporary confounding factors could bias our results. Table 2.11 presents the results for CVR mortality after excluding the PWOG18 period. The revised coefficient estimates for PM_{10} range from 5.0 to 7.3 percent, which are slightly smaller than the main results (5.8 to 7.9 percent). Therefore, excluding February and March 2018 has a minimal impact on the estimated effects of air pollution, effectively ruling out the possibility of significant bias from temporary confounding factors during the PWOG18.

2.5 Conclusion

This study explores the causal impact of air pollution on cardio-cerebrovascular and respiratory (CVR) mortality rates in South Korea, leveraging the 2018 PyeongChang Winter Olympic Games (PWOG18) and Asian Dust events as natural experiments. Through a fixed-effects instrumental variable approach, we find that a 10 $\mu g/m^3$ reduction in PM_{10} leads to a

7.9% decrease in CVR mortality, reinforcing the critical role of air pollution in shaping public health outcomes.

Our results highlight several important findings. First, the impact of PM_{10} is most pronounced in CVR mortality, while we observe no significant effects on all-cause, cancer, or injury-related mortality rates. This aligns with epidemiological research emphasizing the specific susceptibility of cardiovascular and respiratory systems to particulate matter exposure. Second, our analysis of heterogeneity reveals that the elderly are disproportionately affected by air pollution, with the strongest CVR mortality effects observed in the 70–74 age group. Additionally, women exhibit greater susceptibility to air pollution than men, consistent with findings from prior studies on differential vulnerabilities.

These findings underscore the substantial public health benefits of reducing air pollution. If South Korea were to achieve a 10 $\mu g/m^3$ reduction in monthly PM_{10} levels, it could prevent more than 7,600 premature deaths annually from CVR diseases alone. Policies targeting air quality improvements, particularly during winter and spring when pollution levels are highest, could yield significant health dividends. Moreover, given the heightened vulnerability of the elderly and women, tailored interventions to protect these groups may further enhance the effectiveness of public health strategies.

Our analysis incorporates multiple robustness checks, including excluding data from the PWOG18 period to mitigate the influence of temporary confounding factors. These checks confirm the reliability of our main results. However, several limitations merit attention. First, this study does not account for behavioral adaptations to air pollution, such as increased indoor activities or mask usage, which may influence exposure levels. Second, while our findings are robust for urban areas, they may not generalize to rural regions, where the population may face different exposure dynamics and health risks. Third, the dose-response relationship between PM_{10} and health outcomes may be nonlinear, and our study does not explore potential thresholds below which particulate matter may have negligible effects.

This study contributes to the growing body of literature on air pollution and public health by providing the first causal evidence on the mortality effects of PM_{10} reductions in South Korea using a natural experimental design. It complements previous research from China and other contexts by demonstrating that even relatively smaller reductions in PM_{10} can yield meaningful health benefits, especially in regions with baseline air quality levels superior to those in heavily polluted areas such as Beijing.

Several avenues for future research emerge from this study. First, examining behavioral responses to air pollution could provide deeper insight into exposure mitigation strategies. Second, exploring the economic costs associated with air pollution-related morbidity and mortality would offer a more comprehensive understanding of its societal impacts. Finally, investigating the role of long-term exposure and possible harvesting effects could enhance the robustness of policy recommendations.

In conclusion, our findings highlight the critical role of air quality improvements in reducing CVR mortality and protecting vulnerable populations. As policymakers continue to grapple with the dual challenges of economic growth and environmental sustainability, this study underscores the urgency of implementing and maintaining effective air pollution control measures.

2.6 Tables

Table 2.1. Summary statistics of the main variables

	Treatment		Control	
	Mean	Std. Dev.	Mean	Std. Dev.
PM_{10}($\mu g/m^3$)	35.02	8.16	45.19	12.90
Standardized All- Cause Mortality Rate(per 100,000)	51.09	5.54	47.82	5.13
Standardized CVR Mortality Rate(per 100,000)	15.76	2.73	15.84	2.95
Standardized Cancer Mortality Rate(per 100,000)	13.52	1.87	12.50	1.29
Standardized Injury Mortality Rate(per 100,000)	5.26	1.34	4.42	0.78
Precipitation(10 mm)	138.64	139.02	93.57	92.16
Temperature (°C)	14.63	8.90	14.10	9.19

Notes: All variables are measured at monthly level. CVR stands for cardio - cerebrovascular and respiratory diseases. Monthly PM_{10} concentrations are calculated from hourly measures. Organization (2006) guides PM_{10} concentration level 20 $\mu g/m^3$ for annual mean and 50 $\mu g/m^3$ for 24-hour mean. We use age structure of population on July 1st for each year, provided by Statistics Korea, to calculate age-standardized mortality rates. Asian Dust measures days of public alerts for the incidence of Asian dust in a month.

Table 2.2. The effect of regulations and Asian dust on PM_{10}

	PM_{10} ($\mu g/m^3$)		
	(1)	(2)	(3)
Regulation	-7.1981***	-6.5325***	-6.3650***
	(1.6264)	(1.3980)	(1.4029)
Asian Dust(Days)	1.2428***	0.8497**	0.8575**
	(0.3568)	(0.3592)	(0.3585)
Observations	599	599	599
Temp and Sq.	N	Y	Y
Precipitation	N	N	Y
City FE	Y	Y	Y
Month-Year FE	Y	Y	Y
F-statistics	62.2145	75.4960	74.3976
R^2	0.8826	0.9029	0.9036

Notes: This table reports the first stage regression result. The dependent variable is monthly average of PM_{10}. Regulations and Asian dust are used as instruments for monthly PM_{10} concentration. These instruments have statistically significant coefficients and are robust to weather control, which is known as a confounding factor of air pollution.
*** indicate the 1 percent significance level.
** indicate the 5 percent significance level.

Table 2.3. The effect of PM_{10} on the monthly standardized mortality rates

	OLS	2SLS (All-cause mortality(log))		
	(1)	(2)	(3)	(4)
PM_{10}(10 $\mu g/m^3$)	0.0027	-0.0239	-0.0271	-0.0260
	(0.0053)	(0.0182)	(0.0216)	(0.0218)
Observations	599	599	599	599
Temp and Sq.	Y	N	Y	Y
Precipitation	Y	N	N	Y
City FE	Y	Y	Y	Y
Month-Year FE	Y	Y	Y	Y
R^2	0.8069	0.7932	0.7913	0.7922

Notes: This table reports the second stage regression result. The dependent variable is the logarithm of monthly age-standardized mortality rate per 100,000 people. Regulation, and Asian dust are used as instruments for monthly PM10 concentration. Weather variables are controlled gradually in column (3) and (4).

Table 2.4. The effect of PM_{10} on the monthly CVR mortality rates

	OLS	2SLS (CVR mortality(log))		
	(1)	(2)	(3)	(4)
PM_{10}(10 $\mu g/m^3$)	-0.0038	0.0576**	0.0774**	0.0792**
	(0.0084)	(0.0277)	(0.0332)	(0.0340)
Observations	599	599	599	599
Temp and Sq.	Y	N	Y	Y
Precipitation	Y	N	N	Y
City FE	Y	Y	Y	Y
Month-Year FE	Y	Y	Y	Y
R^2	0.7868	0.7577	0.7485	0.7474

Notes: This table reports the second stage regression result. The dependent variable is the logarithm of monthly age-standardized CVR disease mortality rate per 100,000 people. Regulations, and Asian dust are used as instruments for monthly PM10 concentration. Weather variables are controlled gradually in column (3) and (4).
** indicate the 5 percent significance level.

Table 2.5. Test of Over-Identifying Restrictions

Test of Over-Identifying Restrictions	Value	Significance(p-value)
Sargan (score) chi2(1)	2.6047	0.1065
Basmann chi2(1)	2.2972	0.1296

Notes: This table reports the result of test of over-identifying restrictions. The test model is third column of Table 2.4 that controls Temperature. Based on the Sargan and Basmann score tests, we do not reject the null hypothesis that our instruments are valid at the 1%, 5%, and 10% significance level.

Table 2.6. Test of Endogeneity

Test of Endogeneity	Value	P-value
Durbin (score) chi2(1)	6.54977	0.0105
Wu-Hausman F(1,526)	5.81514	0.0162

Notes: This table reports the result of test of endogeneity. The test model is third column of Table 2.4 that controls Temperature. Based on the Durbin and Wu-Hauman tests, we reject the null hypothesis that PM_{10} is exogenous.

Table 2.7. The effect of PM_{10} on cause-specific mortality rates

	CVR	All-cause	Cancer	Injury
PM_{10}(10 $\mu g/m^3$)	0.0792**	-1.6355	-0.0657	-0.0486
	(0.0340)	(1.3025)	(0.0460)	(0.0649)
Observations	599	599	599	599
Weather	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month-Year FE	Y	Y	Y	Y

Notes: This table reports the second stage regression results. The dependent variable is the logarithm of monthly age-standardized cause-specific mortality rates per 100,000 people. Regulation, and Asian dust are used as the instruments for monthly PM10 concentration.
** indicate the 5 percent significance level.

Table 2.8. The effect of PM_{10} on male and female mortality rates

	Male		Female	
	(1)	(2)	(3)	(4)
	All-Cause	CVR	All-Cause	CVR
PM_{10}(10 $\mu g/m^3$)	-0.0269	0.0736	0.0019	0.0862*
	(0.0238)	(0.0461)	(0.0099)	(0.0475)
Weather	Y	Y	Y	Y
City FE	Y	Y	Y	Y
Month-Year FE	Y	Y	Y	Y
Observations	599	599	599	599

Notes: This table reports second stage regression result. Regulations, and Asian dust are used as the instrument for monthly PM10 concentrations. The dependent variables are the logarithm of monthly age-standardized mortality rates per 100,000 people for all cause, CVR. Column 1-2 shows male result and column 3-4 shows female results.
* indicate the 10 percent significance level.

Table 2.9. The effect of PM_{10} on age specific mortality rates

	CVR (1)	All-Cause (2)
Age 0-4	0.0112	-0.0181
	(0.0109)	(0.1014)
Age 5-9	0.0271**	-0.0086
	(0.0126)	(0.0424)
Age 10-19	-0.0020	0.0762
	(0.0067)	(0.0637)
Age 20-39	0.0346*	0.0362
	(0.0192)	(0.0871)
Age 40-59	0.0823*	0.1095
	(0.0437)	(0.0909)
Age 60-64	0.0271	0.1234
	(0.0773)	(0.0882)
Age 65-69	0.0947	0.2912**
	(0.0943)	(0.1131)
Age 70-74	0.1704*	0.1701
	(0.0968)	(0.1063)
Age 75-79	0.0077	0.2196**
	(0.1053)	(0.0953)
Age 80 +	0.0994	0.2184**
	(0.1011)	(0.1112)

Notes: This table reports second stage regression result. Regulation, and Asian dust are used as the instruments for monthly PM10 concentration. The dependent variables are the logarithm of monthly age-standardized cause-specific mortality rates per 100,000 people per age group. This shows the heterogeneous effect of air pollution on different age groups' mortality rate.
** indicate the 5 percent significance level.
* indicate the 10 percent significance level.

Table 2.10. The effect of regulations and Asian dust on mortality rates

	CVR			Cancer	Injury	All-Cause
	(1)	(2)	(3)	(4)	(5)	(6)
Regulated (R)	-0.0578*	-0.0601*	-0.0595*	0.0598*	0.0343	0.0285
	(0.0348)	(0.0348)	(0.0349)	(0.0349)	(0.0540)	(0.0889)
Weather	N	Y	Y	Y	Y	Y
Asian Dust	N	N	Y	Y	Y	Y
City FE	Y	Y	Y	Y	Y	Y
Month-Year FE	Y	Y	Y	Y	Y	Y

Notes: This table reports the reduced-form coefficients and standard errors. The dependent variable is the logarithm of monthly age-standardized cause-specific mortality rates per 100,000 people. The average number of Asian dust days is 0.46 day with standard error 1.19.
*** indicate the 1 percent significance level.
* indicate the 10 percent significance level.

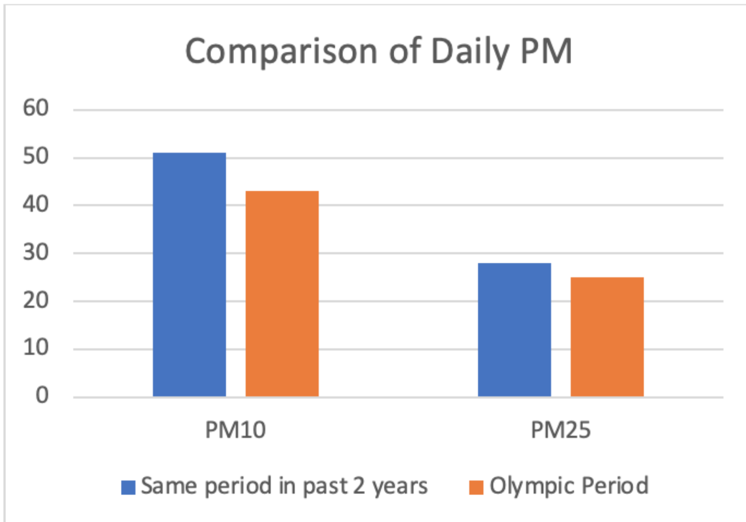
Table 2.11. Robustness check without February and March 2018

	CVR Mortality		
	(1)	(2)	(3)
PM_{10}(10 $\mu g/m^3$)	0.0499*	0.0714*	0.0731*
	(0.0298)	(0.0367)	(0.0377)
Observations	579	579	579
Temp and Sq.	N	Y	Y
Precipitation	N	N	Y
City FE	Y	Y	Y
Month-Year FE	Y	Y	Y
R^2	0.7594	0.7491	0.7482

Notes: This table reports second stage regression result. The dependent variable is the logarithm of age-standardized CVR mortality. Air regulation, and Asian dust are used as the instruments for monthly PM_{10} concentration.
* indicate the 10 percent significance level.

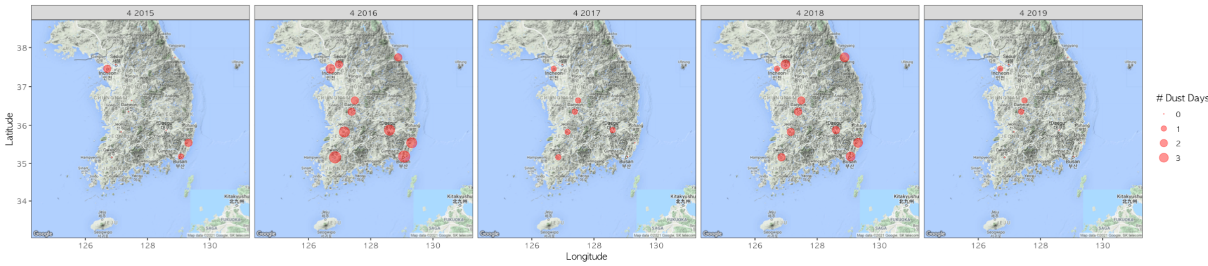
2.7 Figures

Figure 2.1. Daily PM Comparison in Gangneung



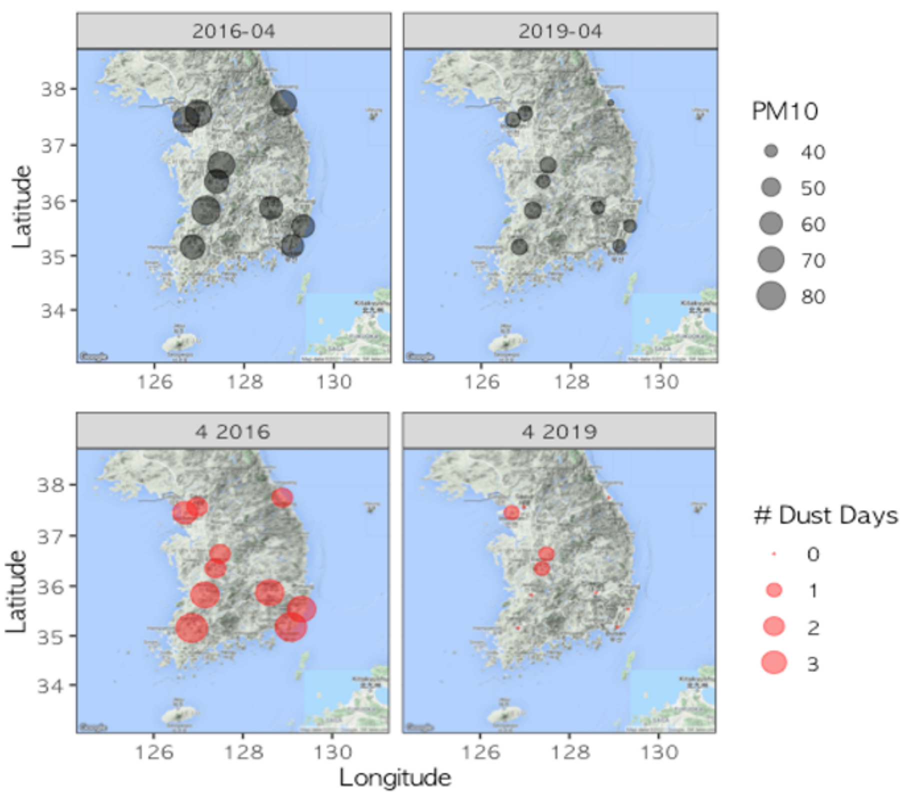
Notes: Daily PM was calculated from hourly measurements. This figure compares daily average of PM level from 9 to 25 February 2018 with the same period in 2016 and 2017. Both PM10 and PM25 are reduced during the PWOG18 in Gangneung.

Figure 2.2. Examples of Asian Dust Variations in South Korea



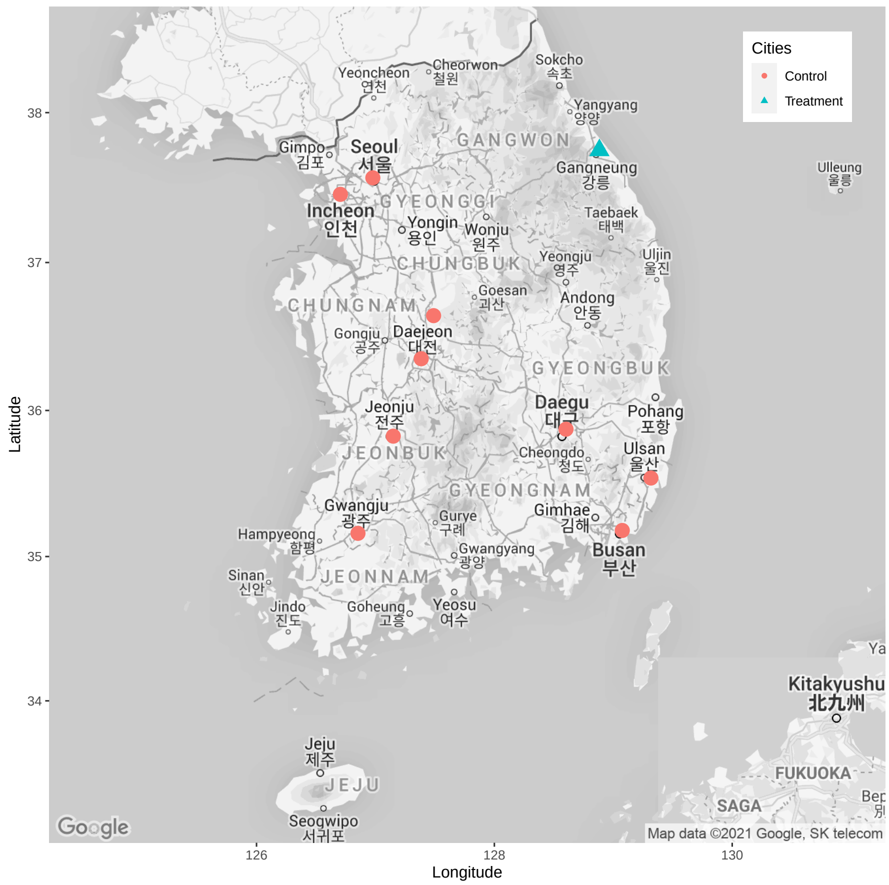
Notes: This figure plots the distribution of dust days across cities in April 2015, April 2016, April 2017, April 2018 and April 2019. This figure illustrates that the incidence of Asian dust varies significantly across regions and years

Figure 2.3. Association between Asian Dust and PM_{10}



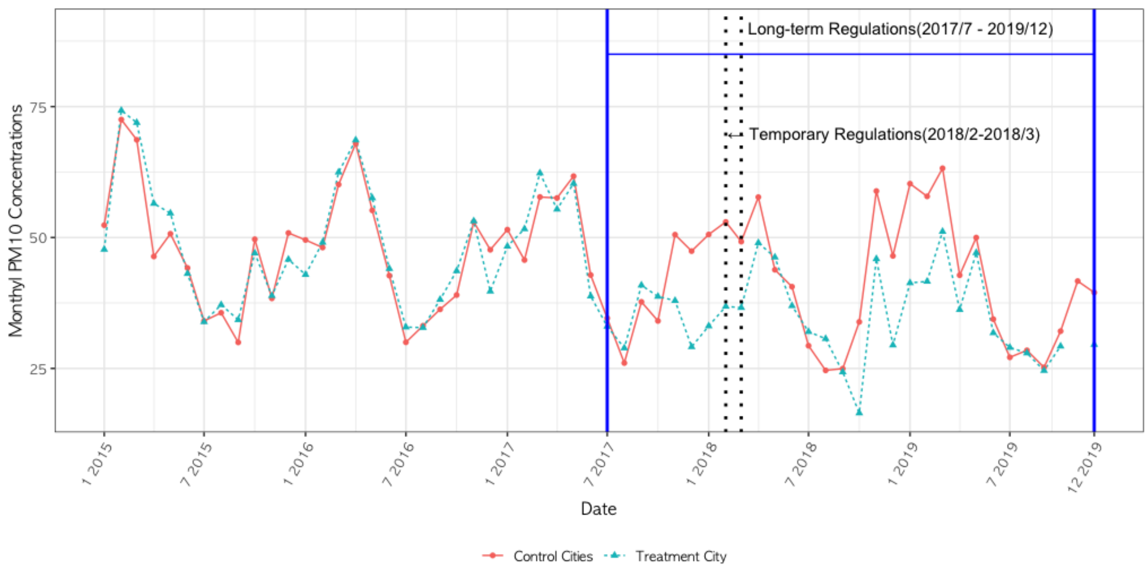
Notes: This figure plots the dust days across cities and average of PM_{10} in April 2016 and April 2019. This figure illustrates the association of the incidence of Asian dust days and the level of PM_{10}

Figure 2.4. Geographical distribution of sampled cities in South Korea



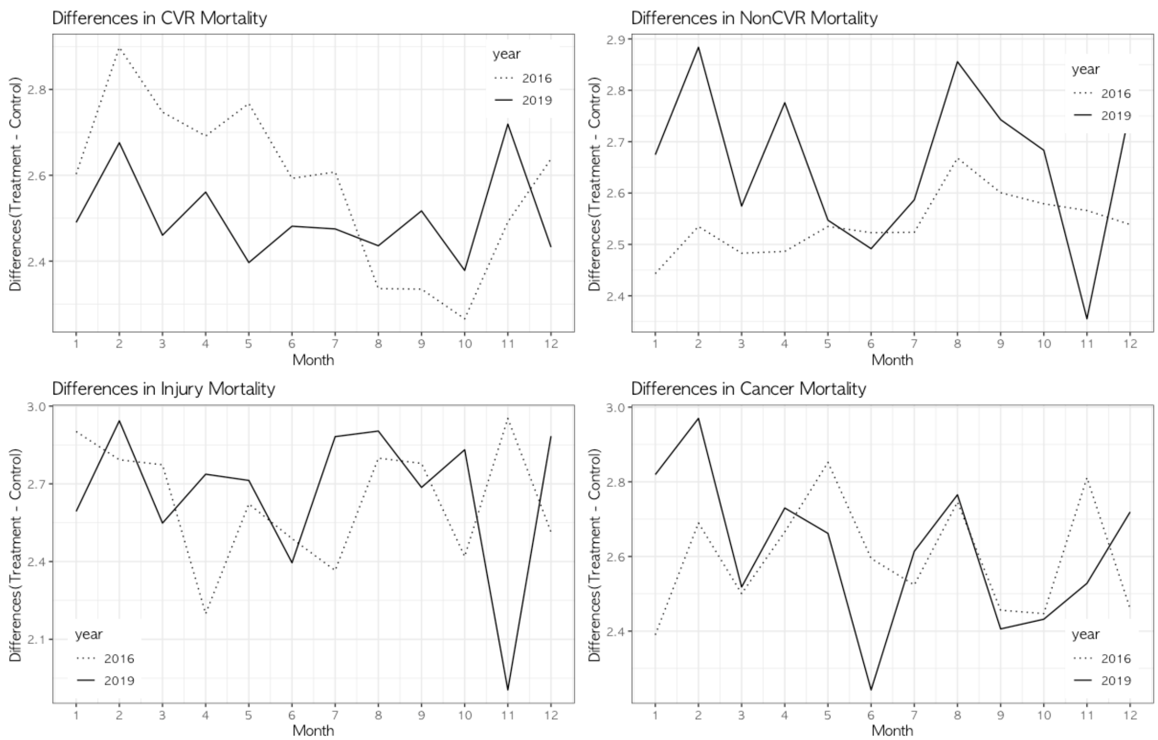
Notes: This figure provides geographical locations of the treatment and control cities in our sample. The circle represents the control cities and the triangle represents the treatment city.

Figure 2.5. Monthly PM_{10} Concentrations in treatment and control cities



Notes: This figure plots monthly average of PM_{10} in control cities and treatment city. The dashed line presents the treatment city and the solid line presents the average of control cities. From July 2017 to December 2019 is the treatment period as the treatment city has a long-term effect of closing old coal power plants. This figure shows that air quality in the treatment city was significantly improved during and even after the PWOG18, especially in winter and spring when air quality is worse.

Figure 2.6. Cause-specific mortality rate differences between the treatment city and control cities



Notes: The figure shows the mortality rate differences between treatment and control cities. The solid line represents 2019 that is in the treatment period and the dashed line represents 2016 that is in the control period. Top left panel shows that the difference of CVR mortality is lower in 2019, indicating the effect of improved air pollution on reduced CVR mortality. Top right panel shows that the difference of Non-CVR mortality rate is much bigger in 2019, indicating there was no significant improvement of medical condition in the treatment city. Bottom panels show that injury mortality and cancer mortality have no big difference before and after the treatment.

Chapter 3

Impact of Wildfires on Birth Weight: Evidence from the April 2000 in Gangwon, South Korea

3.1 Introduction

Due to climate change, wildfires are becoming more frequent and severe, posing significant risk to both the environment and human health (United Nations, 2023). While direct damages, such as property destruction and displacement, are well-documented, the indirect impacts on human health, particularly for vulnerable groups like pregnant individuals and their fetuses, remain underexplored (Amjad et al., 2021).

Pregnancy is a critical period for fetal development, and health shocks during this period can have long-term effects on infants. Poor birth outcomes, such as low birth weight, are associated with higher risks of chronic health conditions, lower educational attainment, and reduced earnings over a lifetime (Currie and Rossin-Slater, 2013; Almond and Currie, 2011; Black et al., 2007). This connection underscores the fetal origins hypothesis, which posits that early-life conditions have enduring effects on health and socioeconomic status (Barker, 1998). Consequently, wildfires may not only harm immediate health but also have intergenerational economic impacts by limiting opportunities and increasing healthcare

This study aims to estimate the causal impact of wildfire smoke on birth weight, focusing on the April 2000 wildfires in Gangwon Province, South Korea. From April 7 to 12, ten wildfires erupted simultaneously across four coastal cities, and all fires were extinguished by April 15. The region's unique geographical and climatic setting—with the Taebaek Mountains to the west and the Pacific Ocean to the east—creates a natural barrier, enabling a quasi-experimental approach to estimate the causal impact of wildfire exposure on birth outcomes. The analysis utilizes Difference-in-Difference (DID) method to compare the birth weight between counties directly affected by the wildfires and those that were not, thereby isolating the effects of wildfire exposure from other confounding factors (Currie and Rossin-Slater, 2013).

Leveraging the quasi-experimental setting created by the April 2000 wildfires in Gangwon Province, South Korea, this study employs a Difference-in-Differences (DID) approach to establish the causal impact of wildfire exposure on birth outcomes. Our analysis reveals that infants born in affected areas—Gangneung, Samcheok, Donghae, and Goseong—experience an average birth weight reduction of approximately 26 grams. The adverse effect is particularly pronounced when exposure occurs during the first and third trimesters, underscoring the critical nature of timing in fetal development. Furthermore, heterogeneity analysis indicates that the effect is more severe among mothers aged 30 and above and slightly greater for female infants compared to males. In addition to birth weight, we examine the incidence of low birth weight (LBW) and gestational duration: while wildfire exposure shifts birth weights downward, it does not significantly increase the probability of LBW nor does it affect gestational age. Multiple robustness checks—including coastal-only regressions, analyses incorporating parental education as a socioeconomic proxy, and placebo tests using 1999 data—corroborate these findings and reinforce the interpretation that wildfire exposure causally diminishes neonatal health outcomes.

This study makes three key contributions to the literature. First, while much of the research has examined the effects of general air pollution on birth outcomes, less is known about how acute pollution events, such as wildfires, impact fetal development—particularly in regions with unique geographic characteristics. In South Korea, for instance, the Taebaek Mountains act as a natural barrier, influencing how pollution spreads, yet causal studies on wildfire exposure and birth weight remain scarce. Second, this research is the first to assess the causal impact of wildfire exposure on birth weight in South Korea, moving beyond correlation to identify cause and effect. By examining how wildfire-driven pollution affects birth outcomes, this study fills an important gap and adds valuable context to the wider environmental health literature, showing how acute environmental shocks can distinctly shape birth outcomes in different local settings. Lastly, by analyzing trimester-specific impacts of wildfire exposure, this study identifies vulnerable stages in pregnancy. Despite mixed findings in the literature, these insights underscore the need for interventions to protect mothers and infants in wildfire-prone areas.

The remainder of the paper is organized as follows. Section 2 provides background on the wildfires and the link between wildfires and birth weight. Section 3 describes the data and methods. Section 4 presents the results, and Section 5 concludes.

3.2 Background

3.2.1 Description of the Wildfires in East Cost of Korea in 2000

In April 2000, ten wildfires - the largest wildfire in Korean history at that time simultaneously broke out in four cities along the eastern coast of Gangwon Province. The wildfires began between April 7 and April 12, and all were fully extinguished in every affected area by April 15. The wildfires affected an area of 23,448 hectares, forced the evacuation of over 100,000 residents, representing more than 20 percent of the population in the affected cities. Figure 3.1 shows the satellite images of Gangwon province, confirming the wildfire outbreak across these coastal cities on April 7, 2000, and on April 11, 2000.

The damage from the wildfires in the eastern coastal region of Gangwon Province was exacerbated by the region's topological features (Jeon and Chae, 2016; Lee, 2001; Ryu et al., 2018). The Taebaek Mountains, which separate the eastern coast of Korea from the inland, play a significant role in this phenomenon. Warm winds originating from the Chinese mainland cool as they rise over the windward side of the mountains, leading to precipitation. However, as these winds descend on the leeward side, they become hot and dry, creating a favorable environment for wildfires along the coastal areas. The wind speeds can be higher than those of typhoons, contributing to the rapid spread of large-scale wildfires. This phenomenon is known as the Foehn effect and is called the 'Chinook Wind' in the United States, where it occurs along the Rocky Mountains.

Due to the Foehn effect and the Taebaek Mountains, air pollution caused by the wildfires did not spread to inland areas. As shown in Figures 3.2 and 3.3, the carbon monoxide (CO) and ozone levels in Wonju, an inland city, in April 2000 were not significantly different from those of the previous year. In contrast, in Gangneung, where the fires occurred, ozone levels were high in April 2000, and although CO levels for April are missing, they remained elevated even in May. Unfortunately, data on air pollution, especially PM10 and PM2.5 levels, is not available, and the CO level data for April is also not publicly available. Therefore, this study did not include air pollution variables in the regression analysis.

3.2.2 Link Between Wildfires and Birth Weight

Wildfires pose considerable threats to human health (Centers for Disease Control and Prevention, 2024). Beyond causing direct injuries related to the fires, wildfires also expose individuals to smoke and induce stress. They release various harmful substances, including carbon monoxide, nitrogen oxide, particulate matter, hydrocarbons, and volatile organic compounds into the atmosphere (Amjad et al., 2021). Moreover, individuals who lose their homes or are forced to evacuate often experience significant stress levels. Pregnant individuals are especially vulnerable to health problems caused by wildfires (Centers for Disease Control and Prevention, 2024). It has been proposed that air pollutants released during wildfire events can cross the placental barrier, disrupting the circulation between the mother and fetus and directly affecting fetal development (Malley et al., 2017; Proietti et al., 2013). Pregnant women exposed to wildfire smoke are associated with increased stress and depression (Belleville et al., 2019), which may, in turn, relate to adverse birth outcomes, such as preterm birth and low birth weight. While these associations highlight potential risks, the specific physiological pathways linking maternal wildfire exposure to adverse birth outcomes have yet to be fully understood.

3.3 Data and Methods

3.3.1 Data on Birth Weight

We utilized birth registration data from January 1999 to December 2000, provided by the Korean National Statistical Office. Due to legal requirements, this birth registration encompasses nearly all births. From this data, we obtained information on birth weight, parity, parents' educational attainment, maternal age categorized into ranges, birth months, birth years, and parents' address at the county level.

Based on birth registration data from 1999 and 2000, a total of 38,625 infants were born in Gangwon Province. Our analysis excluded cases of preterm births (gestational age $<$ 36 weeks), post-term births (gestational age $>$ 42 weeks), birth weights below 1 kg or above 6 kg, and multiple births (e.g., twins). As a result, we excluded 2,280 observations from the dataset. Additionally, cases born in April 2000(1,541 observations) were excluded from the study because the birth date information was missing, making it impossible to determine whether they were born before or after the wildfire. Table 3.1 provides descriptive statistics comparing key demographic and birth-related characteristics between treatment and control groups. It includes information on gender distribution, birth weight, low birth weight prevalence, gestational age distribution, maternal age, maternal education, and exposure status.

3.3.2 Data on Weather and Air Pollution

Research has shown that weather factors, such as temperature and precipitation, are associated with birth weight in addition to air pollution (Currie and Neidell, 2005; Ha et al., 2017). Monthly weather data are obtained from the Korea Meteorological Administration's Open Weather Data Portal, which provides detailed climate metrics across various scales. We used monthly data to match the birth data, which only provide monthly information for 1999 and 2000. A summary of the weather data is presented in Table 3.2.

Table 3.2 presents a descriptive comparison of weather conditions between the control and treatment cities. The average maximum temperature is identical in both groups (17°C), showing no statistically significant difference ($p = 0.3$). However, the average minimum temperature is significantly lower in the control cities (5°C) compared to the treatment cities (8°C) ($p < 0.001$). Monthly precipitation is slightly lower in the control cities (115 mm) than in the treatment cities (117 mm), but this difference is not significant ($p = 0.4$). The distribution of the maximum gust direction differs significantly ($p < 0.001$): although west is the predominant gust direction in both groups, the treatment cities have a higher proportion of gusts from the west (49% vs. 42%) and a much lower proportion from the east (1.1% vs. 6.3%). This suggests that wildfire-related air pollution was less likely to be transported to inland cities due to the prevailing wind patterns.

While some air pollution data were available for 1999 and 2000, there were too many missing values for consistent use. Air pollution data were especially lacking for small towns and rural areas, with records only available for major cities like Wonju (Control) and Gangneung (Treatment). Therefore, we excluded air pollution data from the regression analysis in this study. In Figure 3.2 and Figure 3.3, we included carbon monoxide (CO) and ozone (O3) levels from Gangneung and Wonju to demonstrate the absence of a spillover effect of air pollution from wildfires. CO levels showed a consistent trend from 1999 up until before March 2000, with data missing for March and April 2000. The May data revealed that Gangneung, where the wildfire occurred, had higher CO levels than both Wonju, the control city, and Gangneung's own levels from the previous year. CO could have remained elevated in the atmosphere for one to three months after a wildfire, as wildfires reduced the levels of hydroxyl radicals (OH), which were essential for breaking down CO (Wang and Prinn, 1999; Zheng et al., 2019). For ozone (O3), the data from April 2000—the month of the wildfire—showed a significant increase in Gangneung compared to April 1999, while Wonju's levels remained relatively unchanged from the previous year. Wildfires emitted nitrogen oxides and volatile organic compounds (VOCs), which contributed to the formation of ground-level ozone (Xu et al., 2021).

3.3.3 Methods

To estimate the impact of wildfires on birth weight, we conduct the DID regression analysis. Our baseline empirical specification is represented as follows:

$$y_{ict} = \beta \cdot \text{Treatment}_{ict} \times \text{Post}_{ict} + Z_i'\omega_1 + G_{ct}'\omega_2 + \text{County}_i + \text{Year-Month}_t + \varepsilon_{ict} \tag{3.1}$$

where y_{ict} is the birth weight of infant i from county c during year-month t. Post_{ict} equals 1 if the birth occurred after April 2000, and 0 otherwise. Treatment_{ict} equals 1 if the county experienced a wildfire, and 0 otherwise. Figure 3.4 displays a map illustrating the geographical locations of both the treatment and control cities. The variable of interest is the interaction term, $\text{Treatment}_{ict} \times \text{Post}_{ict}$. The interaction term measures the impact of wildfires on birth weight in counties that experienced wildfires relative to those that did not. Z_i' is a vector of demographic controls, including mother's education level, age groups, and parity. G_{ct}' is a vector of weather controls, including average maximum temperature, average minimum temperature, precipitation, and the 16 compass directions of wind. We include county fixed effects to control for time-invariant, unobserved characteristics specific to each county that might independently affect birth weight. Additionally, year-month fixed effects are included to control for time-specific factors, such as seasonal weather patterns, that may affect all counties simultaneously but are unrelated to wildfire exposure.

We also decompose Post_{itc} into three trimesters of pregnancy for analysis of how wildfire exposure at different stages of gestation affects birth weight, following Currie and Rossin-Slater (2013):

$$
\begin{aligned}
y_{ict} = & \sum_{k=1}^{3} \left(\alpha_k \cdot \text{Trimester}_{k,ict} + \beta_k \cdot \text{Treatment}_{ict} \times \text{Trimester}_{k,ict} \right) \\
& + Z_i' \omega_1 + G_{ct}' \omega_2 + \text{County}_i + \text{Year-Month}_t + \varepsilon_{ict} \qquad (3.2)
\end{aligned}
$$

$\{\text{Trimester}_{k,ict}\}_{k=1}^{3}$ are binary variables indicating the specific trimester during which each infant was exposed to wildfire while in utero. The coefficients of interest are $\{\beta_k\}_{k=1}^{3}$, which measure how birth weights differ for infants exposed to wildfires during trimester k, compared to those who were not exposed. By estimating separate coefficients for each trimester, we can show that the effect of wildfire exposure varies across different stages of fetal development, reflecting potential differences in vulnerability at each trimester.

3.4 Results

In this section we present the estimated impacts of wildfire exposure on birth weight using DID regressions. We analyze the main effects and conduct robustness checks across different model specifications.

3.4.1 Main Results

Table 3.3 presents the result from estimating the baseline DID model assessing the impact of wildfire exposure on birth weight. The key coefficient of interest is the interaction term (Post $\cdot$ Treatment), which remains consistently negative and statistically significant across all four specifications. The coefficient for Post $\cdot$ Treatment across different specifications ranges between -26.28 and -26.77 grams. The negative coefficients highlight that infants born in wildfire-exposed counties experienced an average reduction in birth weight of approximately 26 grams. The magnitude and significance of this effect remain robust across different model specifications, indicating that wildfire exposure has a substantial negative impact on neonatal health as measured by birth weight.

Table 3.4 presents the estimated effects of wildfire exposure on birth weight across different trimesters of pregnancy, allowing us to examine how the timing of exposure influences neonatal health outcomes. Each column corresponds to a different model specification that includes year-month and county fixed effects. In all specifications, the dependent variable is birth weight.

The coefficients for each trimester interaction term—First Trimester $\times$ Treatment, Second Trimester $\times$ Treatment, and Third Trimester $\times$ Treatment—consistently exhibit negative coefficients, although the magnitude and levels of statistical significance differ across trimesters.

The coefficients for the interaction term in the first trimester are consistently negative and statistically significant across all three models. The wildfire exposure reduced birth weight from -29 grams to -32 grams in the affected area after the wildfires compared to the unaffected area. These findings align with previous research(Currie and Rossin-Slater, 2013; Ritz and Wilhelm, 2008) suggesting that the first trimester is a critical window of fetal development and therefore highly susceptible to environmental stressors.

The coefficients for the second trimester are also negative across the models but are smaller in magnitude and not statistically significant. The lack of statistical significance implies that wildfire exposure during the second trimester may have a less consistent impact on birth weight, possibly indicating that this period is less sensitive to the adverse effects of wildfire smoke compared to the first and third trimesters. This finding aligns with the public health literature, which suggests that the second trimester is considered less vulnerable to certain environmental hazards compared to the first and third trimesters (Ritz and Wilhelm, 2008; Amjad et al., 2021).

Lastly, the third trimester shows the largest negative and statistically significant coefficients, indicating a stronger association between third-trimester wildfire exposure and reduced birth weight. The coefficients range from -44 grams to -46 grams. These results indicate that exposure to wildfire late in pregnancy may have the most substantial impact on reducing birth weight. One possible explanation is that the fetus experiences accelerated growth demands during the final trimester, making this period particularly sensitive to environmental hazards (Gray et al., 2010; Parker et al., 2005).

3.4.2 Heterogeneity of Effect by Maternal Age Groups and by Gender

Table 3.5 reports the estimated effects of wildfire exposure on birth weight by maternal age groups: under 30, and 30 and over. Each column shows the effect of the Post $\times$ Treatment interaction term on birth weight for mothers in these specific age ranges. The coefficient for the interaction term is negative and statistically significant in both columns, indicating that wildfire exposure is associated with a reduction in birth weight across maternal age groups. Specifically, the estimated effect is a decrease of 27.118 grams for younger mothers and 39.964 grams for older mothers. The results suggest that wildfire exposure has a more pronounced adverse effect on birth weight among older mothers.

Table 3.6 reports the estimated effects of wildfire exposure on birth weight by infant's gender. The interaction term, Post $\times$ Treatment, captures the differential effect of wildfire exposure on birth weight. The results show negative and statistically significant coefficients for both genders, with wildfire exposure reducing birth weight by 22.814 grams for boys and 33.366 grams for girls. These findings suggest that wildfire exposure has an adverse impact on newborn health regardless of gender, although the magnitude of the effect appears slightly greater for girls.

One possible explanation for this gender difference is the role of placental function and fetal adaptation mechanisms. Research suggests that the placenta responds differently depending on fetal sex, with male placentas exhibiting greater efficiency in nutrient transport and adaptation to stressors, whereas female placentas may be more vulnerable to harmful exposures (Clifton, 2010). This is consistent with findings from broader research on air pollution and fetal health, which indicate that female fetuses may have less physiological reserve to compensate for in-utero stressors (Currie and Schwandt, 2016). The mechanism behind this disparity remains an area of active research, but potential explanations include hormonal differences, oxidative stress response, and epigenetic modifications that affect fetal growth trajectories.

3.4.3 The Impact of Wildfire Exposure on Low Birth Weight

Table 3.7 presents the estimated effects of wildfire exposure on low birth weight (LBW), defined as birth weight below 2,500 grams. LBW is a widely recognized indicator of neonatal health and is associated with increased risk of infant morbidity, mortality, and long-term developmental challenges. The analysis of LBW complements the findings from the birth weight regressions by examining whether wildfire exposure not only reduces birth weight on average but also increases the likelihood of newborns falling below the LBW threshold, which has critical clinical and public health implications.

The coefficient of interest, Post $\times$ Treatment, captures the effect of wildfire exposure on the likelihood of LBW across different model specifications. The results across all four columns suggest that wildfire exposure is associated with a positive, albeit small and statistically insignificant, increase in the probability of LBW. This finding suggests that while wildfire exposure leads to reductions in birth weight, the overall shift may not be large enough to significantly increase the proportion of newborns classified as LBW.

Prior research suggests that exposure to air pollution and other environmental hazards during pregnancy increases the risk of LBW (Parker et al., 2005; Currie and Rossin-Slater, 2013). However, the lack of statistical significance in Table 3.7 suggests that, in this specific context, wildfire exposure may primarily shift birth weights downward within the normal range rather than substantially increasing the probability of LBW.

3.4.4 The Impact of Wildfire Exposure on Gestational Age

Table 3.8 presents the estimated effects of wildfire exposure on gestational age, measured in weeks, to examine whether the observed reductions in birth weight are driven by shortened pregnancy duration. The coefficient of interest, Post $\times$ Treatment, remains small and statistically insignificant across all model specifications, indicating that wildfire exposure does not significantly alter gestational age. These findings suggest that infants in both the treatment and control groups were born at similar gestational ages, implying that wildfire exposure primarily affects intrauterine growth rather than pregnancy duration.

These findings are consistent with our main results on birth weight (Table 3.3 through Table 3.7), where wildfire exposure leads to a statistically significant reduction in birth weight without affecting gestational age. If wildfire exposure had led to shortened pregnancy durations, we would expect to see significant reductions in gestational age, which could partially explain the observed declines in birth weight. However, since gestational age remains unaffected, the observed reductions in birth weight must be attributed to fetal growth restrictions rather than preterm birth. This finding aligns with the literature suggesting that air pollution and wildfire smoke primarily impact fetal development through intrauterine growth restriction rather than by triggering preterm labor (Parker et al., 2005; Ritz and Wilhelm, 2008; Currie and Rossin-Slater, 2013).

By confirming that wildfire exposure does not significantly affect gestational age, these results strengthen the interpretation that the observed reductions in birth weight are a direct consequence of fetal exposure to wildfire-related pollutants rather than an indirect effect mediated through preterm birth. The distinction is crucial for understanding the underlying biological mechanisms.

3.4.5 Robustness Check

To ensure the robustness of the estimated effects of wildfire exposure on birth weight, additional analyses were conducted, as shown in Tables 3.9, 3.10, and 3.11.

Table 3.9 presents the results of regressions limited to coastal areas, which were directly affected by wildfires. The purpose of isolating coastal areas is that among these counties, some were directly exposed to wildfire while neighboring counties were not. These coastal regions share similar geographical and climatic characteristics, making them ideal for testing potential spillover effects of wildfire exposure. The Post $\times$ Treatment coefficient is consistently negative and statistically significant across all models, with values ranging from -29.3 to -30.3 grams. The result indicates that the observed reduction in birth weight is specific to those areas that directly experienced wildfires, with no evidence of spillover effects impacting neighboring coastal counties. This finding supports the validity of the causal interpretation, confirming that the negative impact on birth weight is attributable specifically to direct wildfire exposure in the affected counties.

To address the potential concern that wealthier families might have migrated after the wildfires, we use parents' educational attainment as a proxy for income. Table 3.10 examines whether wildfire exposure influenced the educational attainment of parents. The Post $\times$ Treatment coefficients for both mother's and father's education are not significantly different from zero, with values of 0.016 and 0.008, respectively. These results suggest that there was no significant shift in the socioeconomic composition of the affected areas after the wildfires, alleviating concerns that selective migration based on income or education could have biased the estimated impact on birth weight.

Placebo tests were conducted to further assess the robustness of the main results. For these tests, we used data from 1999 only, the year prior to the wildfires. In each month from February to November 1999, we hypothetically assumed that wildfires occurred in the treatment counties and ran separate DID regressions for each month. Table 3.11 presents the estimated effects of these hypothetical wildfire exposures. None of the Post $\times$ Treatment coefficients for these placebo tests are consistently negative, and several coefficients are positive and statistically significant. This indicates that if the observed reduction in birth weight after the wildfires were due to random variation or other unrelated factors, we would expect to see similar patterns in the placebo tests. The absence of such patterns strengthens the argument that the negative impact on birth weight is indeed due to wildfire exposure in 2000

3.5 Discussion and Conclusion

This study provides causal evidence that acute exposure to wildfire smoke can adversely affect fetal development, as measured by reductions in birth weight. By exploiting the quasi-experimental setting created by the April 2000 wildfires in Gangwon Province, South Korea, and applying a Difference-in-Differences (DID) method, the analysis isolates the impact of wildfire exposure from other potential confounders. The findings show that infants born in wildfire-exposed counties weighed, on average, 26 grams less than those in unaffected areas. This effect was robust across various specifications and robustness checks, including analyses restricted to coastal regions, placebo tests, and assessments of potential compositional changes in the sample.

The timing of exposure emerged as a critical determinant. In line with prior research, exposure during the first and third trimesters was associated with significant declines in birth weight, whereas second trimester exposure did not yield statistically significant effects. These results are consistent with the fetal origins hypothesis and reinforce the notion that certain stages of gestation are more vulnerable to environmental insults. Particularly, the third trimester—when fetal growth is most rapid—appears highly sensitive to external shocks such as smoke-related pollution or stress caused by disaster displacement.

Heterogeneity in treatment effects was also observed across maternal age groups and by infant sex. While both younger and older mothers experienced reductions in birth weight, the magnitude of the effect was greater among mothers aged 30 and above. Similarly, both male and female infants were adversely affected, though the reduction was more pronounced for females. These patterns are consistent with existing biological literature suggesting that fetal susceptibility to environmental stressors may vary by sex and maternal age, possibly due to differences in placental function and hormonal pathways.

Notably, wildfire exposure did not significantly affect gestational age, nor did it substantially increase the likelihood of LBW. This distinction is important: it implies that the observed reductions in birth weight are driven primarily by intrauterine growth restriction rather than preterm birth. This interpretation aligns with broader evidence on the health effects of air pollution and wildfire smoke, which are often linked to growth deficits rather than shortened pregnancies.

The validity of the causal claims is supported by multiple robustness checks. The use of placebo tests with pre-treatment data showed no spurious effects. Additionally, limiting the sample to coastal counties with similar geographical and climatic conditions strengthened the identification strategy. Finally, analyses of parental educational attainment showed no significant changes before and after the wildfires, reducing concerns about selective migration or compositional shifts in the population.

While the results contribute meaningful insights to the literature on environmental health, some limitations remain. The lack of fine-grained pollution data, such as PM2.5 levels during the wildfire period, restricts a more nuanced understanding of the exposure pathway. Moreover, individual-level maternal health data, such as pre-existing conditions or stress measures, were not available. These limitations point to directions for future research that could more precisely identify biological and behavioral mechanisms linking environmental disasters to birth outcomes.

Taken together, the findings suggest that even short-term, localized environmental shocks can have measurable impacts on human capital at its earliest stage. Understanding these dynamics is particularly relevant as climate change increases the frequency and intensity of wildfires worldwide. While this study does not focus on proposing specific policy solutions, it raises important considerations for how societies evaluate the broader costs of natural disasters—beyond immediate damage to property and infrastructure—and how vulnerable populations, including pregnant individuals and infants, may bear long-term consequences.

3.6 Tables

Table 3.1. Summary Table

Variables	Control Cities (n = 24,996)	Treatment Cities (n = 9,808)	p-value
Gender			0.12
Male	13,096 (52%)	5,047 (51%)	
Female	11,900 (48%)	4,761 (49%)	
Birth weight(g)	3,262 (403)	3,267 (398)	0.3
Low Birth Weigth(<2500g)	471 (1.9%)	162 (1.7%)	0.14
Gestational age(week)			<0.001
37	1,121 (4.5%)	353 (3.6%)	
38	4,384 (18%)	1,354 (14%)	
39	5,334 (21%)	1,647 (17%)	
40	11,303 (45%)	5,671 (58%)	
41	2,426 (9.7%)	634 (6.5%)	
42	428 (1.7%)	149 (1.5%)	
Maternal Age			0.2
Under 30	17,079 (68%)	6,768 (69%)	
30 and over	7,875 (32%)	3,011 (31%)	
Unknown	42	29	
Maternal Education			0.2
Less than high school	1,973 (7.9%)	809 (8.2%)	
High school	15,438 (62%)	5,953 (61%)	
College and more	7,585 (30%)	3,046 (31%)	
Parity			0.043
1	11,952 (48%)	4,822 (49%)	
2	10,276 (41%)	3,965 (40%)	
3+	2,768 (11%)	1,021 (10%)	
Exposure by Trimester			
First	3,071 (12%)	1,236 (13%)	0.4
Second	3,103 (12%)	1,257 (13%)	0.3
Third	2,098 (8.4%)	839 (8.6%)	0.6

Table 3.2. Descriptive Comparison of Weather Conditions

Variables	Control Cities (n = 24,996)	Treatment Cities (n = 9,808)	p-value
Avg. Max. Temp.(°C)	17 (10)	17 (9)	0.3
Avg. Min. Temp.(°C)	5 (11)	8 (9)	<0.001
Monthly Precipitation(mm)	115 (136)	117 (136)	0.4
Max. Gust Direction			<0.001
East	1,572 (6.3%)	112 (1.1%)	
North	5,717 (23%)	2,053 (21%)	
South	7,305 (29%)	2,856 (29%)	
West	10,402 (42%)	4,787 (49%)	

Table 3.3. The Impact of Wildfire Exposure on Birth Weight

	Dependent variable:			
	Birth weight			
	(1)	(2)	(3)	(4)
Post	10.996**	9.550*		
	(5.399)	(5.391)		
Treatment	13.895**	14.402**	14.522**	
	(5.878)	(5.868)	(5.870)	
Post x Treatment	−26.275***	−26.399***	−26.517***	−26.767***
	(10.123)	(10.103)	(10.105)	(10.100)
Constant	3,258.526***	3,401.440***	3,409.465***	3,360.075***
	(3.106)	(112.457)	(112.909)	(118.310)
Weather Control	N	Y	Y	Y
Mother Control	N	Y	Y	Y
Year-Month FE	N	N	Y	Y
County FE	N	N	N	Y
Observations	34,804	34,804	34,804	34,804
F Statistic	2.817**	7.601***	4.837***	4.665***
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 3.4. The Impact of Wildfire Exposure on Birth Weight by Pregnancy Trimester

	Dependent variable:		
	Birth weight		
	(1)	(2)	(3)
First * Treatment	-31.758^{**}	-31.778^{**}	-28.870^{*}
	(15.214)	(15.089)	(16.235)
Second * Treatment	-21.172	-21.239	-21.185
	(15.064)	(15.030)	(12.992)
Third * Treatment	-45.734^{***}	-46.184^{***}	-44.004^{**}
	(17.192)	(17.219)	(18.690)
Year-Month FE	X	O	O
County FE	X	X	O
Observations	34,804	34,804	34,804
F Statistic	5.109^{***}	5.041^{***}	4.726^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 3.5. The Impact of Wildfire Exposure on Birth Weight by Maternal Age Group

	Dependent variable:	
	Birth Weight	
	Under 30	30 and over
	(1)	(2)
Post x Treatment	-27.118^{**}	-39.964^{**}
	(12.798)	(19.869)
Constant	$3{,}229.496^{***}$	$3{,}347.642^{***}$
	(57.863)	(92.855)
Observations	23,847	10,886
F Statistic	2.302^{***}	1.575^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 3.6. The Impact of Wildfire Exposure on Birth Weight by Gender

	Dependent variable:	
	Birth weight	
	Boy	Girl
	(1)	(2)
Post x Treatment	−22.814*	−33.366*
	(9.897)	(13.484)
Constant	3,466.090***	3,400.335***
	(224.713)	(144.165)
Observations	18,143	16,661
Adjusted R^2	0.006	0.008
F Statistic	2.303***	2.697***
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 3.7. The Impact of Wildfire Exposure on Low Birth Weigth

	Dependent variable:			
	Low Birth Weight(<2500g)			
	(1)	(2)	(3)	(4)
Post	-0.0002	-0.0001		
	(0.002)	(0.002)		
Treatment	-0.003	-0.004^{*}	-0.004^{*}	
	(0.002)	(0.002)	(0.002)	
Post x Treatment	0.004	0.005	0.005	0.005
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.035^{***}	0.035^{**}	0.070	0.059
	(0.009)	(0.014)	(0.081)	(0.082)
Weather Control	N	Y	Y	Y
Mother Control	N	Y	Y	Y
Year-Month FE	N	N	Y	Y
County FE	N	N	N	Y
Observations	34,804	34,804	34,804	34,804

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.8. The Impact of Wildfire Exposure on Gestational Age

	Dependent variable:		
	Gestational Age(week)		
	(1)	(2)	(3)
Post	-0.033		
	(0.031)		
Treatment	0.132^{**}	0.196^{***}	
	(0.053)	(0.045)	
Post x Treatment	-0.062	-0.070	-0.076
	(0.059)	(0.044)	(0.050)
Constant	39.444^{***}	39.472^{***}	39.747^{***}
	(0.042)	(0.281)	(0.272)
Year-Month FE	X	O	O
Couny FE	X	X	O
Observations	34,804	34,804	34,804
F Statistic	33.807^{***}	37.477^{***}	24.048^{***}
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 3.9. The Impact of Wildfire Exposure on Birth Weight for Coastal Areas

	Dependent variable:		
	Birth weight		
	(1)	(2)	(3)
Post x Treatment	-29.306^{**}	-29.930^{**}	-30.307^{**}
	(10.811)	(10.610)	(10.590)
Constant	$3{,}264.475^{***}$	$3{,}156.367^{***}$	$3{,}175.398^{***}$
	(5.159)	(56.419)	(52.873)
Year-Month FE	X	O	O
County FE	X	X	O
Observations	12,535	12,535	12,535
F Statistic	3.246^{***}	2.706^{***}	2.566^{***}
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3.10. Regression on Parental Education

	Dependent variable:	
	Mother's Education (Binary)	Father's Education (Binary)
	(1)	(2)
Post x Treatment	0.016	0.008
	(0.012)	(0.018)
Constant	0.294^{***}	0.396^{***}
	(0.019)	(0.024)
Observations	34,804	34,804
F Statistic	14.083^{***}	20.848^{***}
Note:		*p<0.1; **p<0.05; ***p<0.01

Table 3.11. Placebo Test Results: Wildfire Exposure by Month in 1999

	Dependent variable:									
	Birth weight									
	Feb Fire	Mar Fire	Apr Fire	May Fire	Jun Fire	Jul Fire	Aug Fire	Sep Fire	Oct Fire	Nov Fire
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post x Treatment	42.365^{**}	45.026^{***}	38.369^{**}	20.545	7.984	18.047	20.503	8.224	11.421	7.654
	(13.923)	(12.626)	(16.119)	(17.873)	(16.714)	(14.625)	(18.245)	(21.711)	(23.950)	(30.664)
Observations	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160	17,160

Note: *p<0.1; **p<0.05; ***p<0.01

3.7 Figures

Figure 3.1. NASA Worldview Satellite Images(April 7,2000 and April 11,2000)

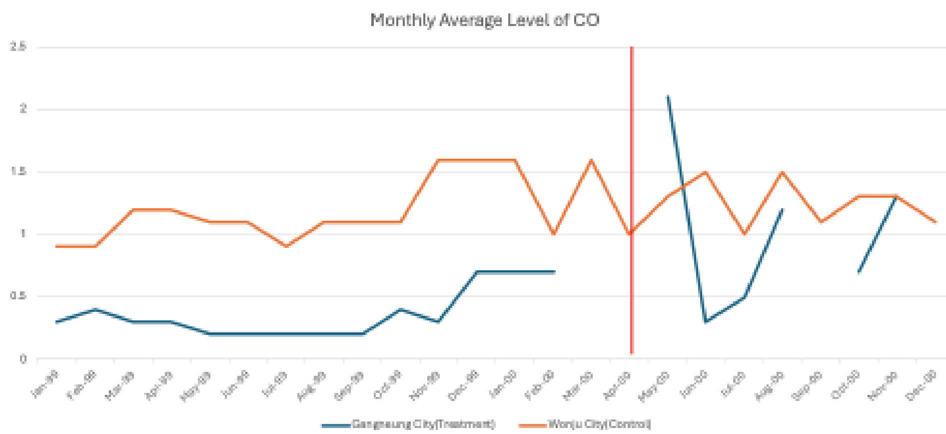


Figure 3.2. Monthly CO Level of Wonju and Gangneung

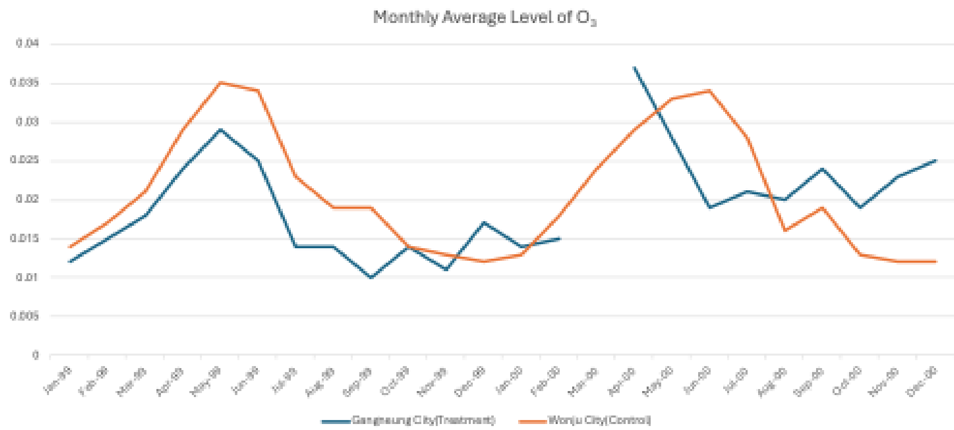


Figure 3.3. Monthly Ozone Level of Wonju and Gangneung

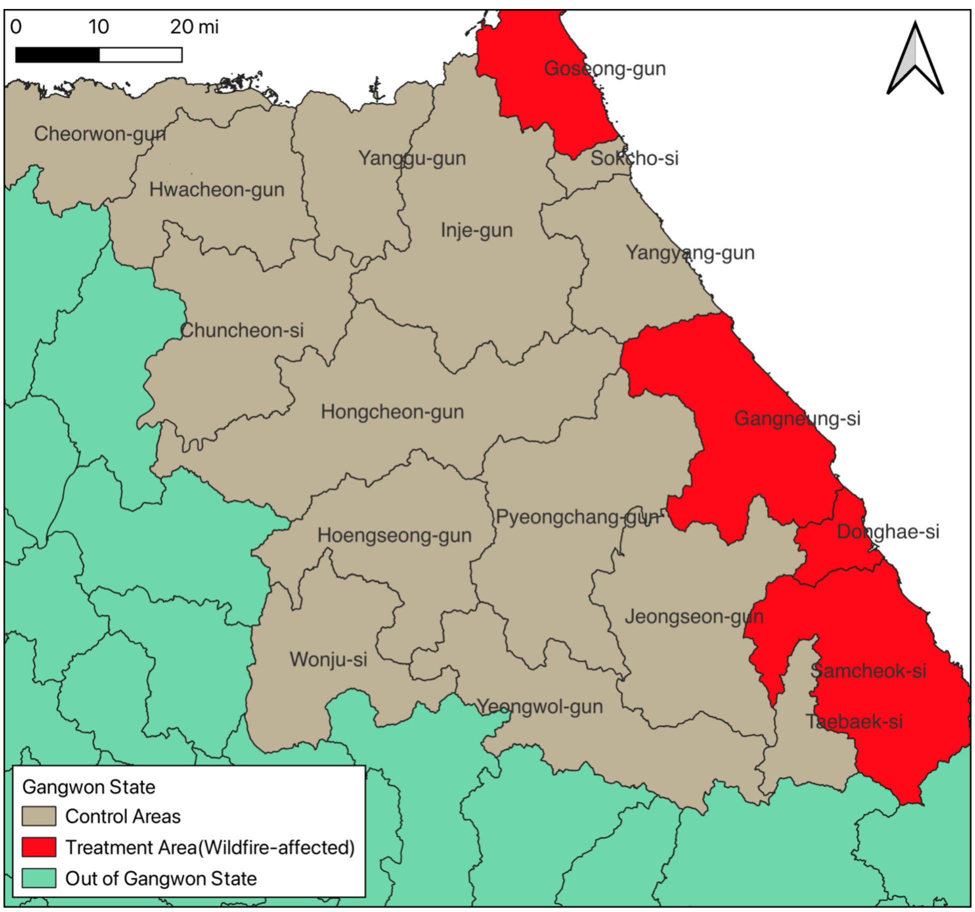


Figure 3.4. Map of Gangwon Province

Chapter 4

Universal Childcare Subsidy and Household Spending: Evidence from South Korea

4.1 Introduction

The South Korean government has executed several programs to respond to the rapid decline in the fertility rate in South Korea (Korea hereafter). Universal childcare, a major subsidy helping households with childcare, is a representative government program alleviating the burden of paying childcare, promoting women's labor force participation, and boosting birth rate. However, there is considerable political controversy on the effectiveness of the universal childcare program, and there is limited empirical evidence on the topic. By focusing on the 2013 childcare expansion, this study attempts to make a causal inference between the policy and the responses of households.

Korea provides a unique and interesting setting. The total fertility rate (TFR) of Korea declined rapidly from 4.53 in 1970 to 1.15 in 2009. Since then, it has fluctuated around 1.00, until it dropped even further in recent years. The TFR was 0.72 in 2023, the lowest in the world. Meanwhile, the female labor force participation rate at ages 15-64 has been increasing from 49.4% in 1991 to 54.7% in 2010, but still lower than the OECD average of 61.3% in 2010.

There is substantial parental demand in early childhood education in South Korea; from an early age in South Korea; as of 2024, 47.6% of children under age six attended private educational institutions. To boost fertility and to promote the labor force participation of women, the Korean government has started to subsidize the childcare system for lower income households since 1992. The government gradually expanded the income eligibility. In 2006, households in the bottom 50% of income distribution were eligible to receive the childcare subsidy, and in 2009, the threshold was raised to the bottom 70%. Since 2012, the government has supported all children aged 0 to 2 years and 5 years, regardless of their family background. Since 2013, the government has extended the subsidy to all children aged 3-4. The subsidy for children aged 3-4 is KRW 220,000 (about USD 220). However, the effectiveness of the program became quite controversial, especially in political circles, and there is little empirical evidence that the subsidy program alleviates the burden of paying childcare and promoting women's labor force participation.

There is a growing body of literature on large-scale or universal childcare programs. Most of these studies focus on European countries and Quebec, Canada (Baker et al., 2008; Havnes and Mogstad, 2011; Libertad González, 2013). The literature focused on maternal labor supply and cognitive development of children in western countries. However, there is limited literature on the effect of universal childcare on children's private education. Early education is important because the impact of receiving any form of early education has lasting positive effects on cognitive and social outcomes during school years and even into adulthood (Goodman and Sianesi, 2005; Barnett, 2011). If universal childcare caused an expansion of private education at early ages, it is possible that the subsidy would increase the educational gap between the rich and the poor.

In contrast to the western studies, there are a limited number of Asian studies related to universal childcare. In Taiwan, Wang et al. (2021) assessed the impact of the universal child allowance program implemented in Taoyuan County in 2015 on family expenditure. They found that the universal child allowance increased health expenditure and decreased expenditure on food at home. However, this is a county-level experiment, not a large-scale experiment. In South Korea, Jung et al. (2016) showed the correlation between the expansion of childcare subsidy and household education expenses. Shin (2020) analyzed the expansion in childcare subsidy on household expenditure but did not investigate its impact on alleviating childcare cost or on women's labor force supply.

Using data from the Panel Study on Korean Children, we examine the impact of extending childcare benefit eligibility—from the lowest 70% of income households to all households—on households' consumption, educational expenditure, and maternal labor supply. In general, it is challenging to estimate the effect of childcare subsidy since the eligibility often depends on income, assets of households, or mother's employment. For example, reverse causation may bias the estimated effects of childcare subsidy if it is unclear whether mothers choose to work to receive the childcare subsidy, or whether the subsidy itself enables mothers to work. However, the expansion of eligibility for the childcare subsidy provides a unique context in which to estimate the effect of this policy.

This study employs a difference-in-differences approach with propensity score matching, where matching is based on the likelihood of getting subsidy, conditional on observed household characteristics before the policy reform in 2013. The treatment and control groups are defined by the benefit recipient status before and after the policy reform. To address possible endogeneity issues, we use propensity score matching to create the counterfactual group, then compare households' consumption, non-consumption expenditures, education costs, mothers' employment status, working hours, and birth plans in the treatment group with the control group. The matching tries to mimic randomization on observed characteristics, such that we account for selection on observables and unobservable time-invariant household heterogeneity and for confounding factors that affect both treatment and control groups (Brucal et al., 2019).

The results indicate that the 2013 childcare expansion significantly reduced household consumption, expenditures for children, and preschool costs for the treatment group compared to the control group. However, the savings from reduced childcare costs were not redirected toward non-consumption expenditures or extracurricular activities. Additionally, the expansion had no effect on mothers' employment, working hours, or their birth plans. In big cities, the savings from reduced childcare costs increased expenditures on extracurricular activities among the treatment group, but this was not observed in small cities or rural areas. For boys, household consumption expenditures decreased by USD 208, whereas no statistically significant decrease was observed for households with girls.

The rest of the paper is organized as follows. Section 2 provides background information on childcare benefit expansion in South Korea. Section 3 presents the data and identification strategy. Section 4 presents the results, and Section 5 concludes.

4.2 Background on Childcare Subsidy Policy in Korea

The Korean government began to consider childcare as a social responsibility in the 1990s when urbanization and industrialization accelerated (Cha et al., 2018). In 1991, the Infant and Child Care Act was legislated to benefit low-income families and families with children with special needs. While the number of eligible children was about 34,000 children, it was the very first legislation that the government viewed childcare provision as a public matter and a social responsibility (Park et al., 2013).

Starting from 2007, free childcare has become a political issue as presidential candidates announced free childcare as a campaign promise. The income eligibility increased from the bottom 50% to the bottom 70% in 2010. In 2012, the government increased the income eligibility from the bottom 70% to all households for ages of 0-2 and 5-year-old childcare. Then, in 2013, the government extended the free childcare program to 3-year-old children.

As a result of the introduction of free childcare, the childcare market saw substantial growth despite the declining fertility rate. The number of childcare centers, including both public and private facilities, increased from approximately 1,900 in 1990 to approximately 43,000 in 2012. During the period 1993-2012, the number of children attending childcare rose from 153,270 children to 14,871,361, a 75-fold increase (Park et al., 2013). The childcare budget increased from USD 300 million in 2003 to USD 4.14 billion in 2013. The government aimed to improve the quality of childcare and increase its supply as an investment in human capital and to boost fertility rates. However, the free childcare policy faced criticism, as it deviates from the approach taken by OECD countries, which subsidize childcare based on households' income (Yoon and Kim, 2013).

Despite the implementation of South Korea's universal childcare subsidy, the financial burden of raising children remains exceptionally high. Various factors—including intense competition in private education, rising housing costs, and limited public support for after-school care—continue to drive up the overall cost of child rearing. According to a recent report, South Korea is currently the most expensive country in the world to raise children, suggesting that government subsidies have not fully alleviated the economic pressures faced by parents (DW, 2023).

4.3 Methods

4.3.1 Data

Our primary data set for this analysis is the Panel Study of Korean Children (PSKC), a nationally representative longitudinal survey conducted by the Korean Institute of Child Care and Education. The study follows a cohort of Korean children born from April to July 2008 and their parents. The survey is conducted annually, from June to November, by trained interviewers at the home of participants, with 2,078 households included in the first wave of data collection in 2008. 77.3% of the original sample was still participating in the sixth wave in 2013.

The age composition of the sample children in the PSKC provides a distinctive setting for analysis. In Korea, eligibility for childcare subsidies is determined based on a child's birth year. In 2012, the government expanded the subsidy to cover all households with children aged 0-2 and 5, regardless of income level. However, for 3- and 4-year-old children, the subsidy remained restricted to households in the bottom 70% of the income distribution. As a result, children in the PSKC cohort, who were 3 years old in 2012, were not eligible unless their household met the income requirement, which means that those in the top 30% of the income distribution were excluded from receiving the subsidy. In 2013, the government further extended the subsidy to include all 3- and 4-year-old children, regardless of income. From that point onward, all households in the PSKC became eligible for the childcare benefit upon registration. Table 4.1 summarizes the childcare eligibility criteria for the Panel Study of Korean Children cohort.

Table 4.2 and Figure 4.1 jointly illustrate the effectiveness of the matching procedure in balancing key covariates between the treatment and control groups. The treatment group consists of households that became eligible for the childcare subsidy in 2013 but were ineligible in 2012, whereas the control group includes households that received the subsidy in both years.

Table 4.2 presents descriptive statistics for both unmatched and matched groups, showing that, before matching, the treatment group had higher household income, higher levels of maternal education, fewer children, and a lower likelihood of residing in big cities compared to the control group. These imbalances highlight the need for matching to ensure comparability. After matching, the rightmost columns of Table 4.2 indicate that differences between the groups are substantially reduced, suggesting that the matching process successfully balances the covariates.

Figure 4.1 further visualizes this improvement by displaying the mean differences in covariates before and after matching. The red dots represent the unadjusted differences, showing considerable imbalances, particularly in household income and parental education. In contrast, the blue dots, representing the adjusted differences after matching, are much closer to zero, indicating that the matching procedure effectively minimizes differences across key covariates. Together, these results confirm that the matched treatment and control groups are well-balanced, enabling a more reliable causal analysis.

4.3.2 Empirical Strategy

We employ the difference-in-differences approach with propensity score matching (PSM-DID) to identify the causal effects of the universal childcare subsidy on household expenditures and maternal labor market outcomes. The empirical specification is given by the following equation:

$$y_{it} = \beta_0 + \beta_1(\text{Post}_{2013,2014} \times \text{Treatment}_i) + \beta \mathbf{X}'_{it} + \delta_i + \gamma_t + \mu_{it} \tag{4.1}$$

where y_{it} is the outcome measure for household i at time t. $\text{Post}_{2013,2014}$ is a dummy variable that equals 1 if the observation comes from the years 2013 or 2014, and 0 otherwise. Treatment_i equals 1 for households that started receiving the subsidy in 2013, and 0 for households that received the subsidy in both 2012 and 2013. $\mathbf{X}'_{it}$ is a vector of household characteristics that may be related to outcomes, including parents' educational attainment, birth order, urban residence (big city), gender, and monthly household income. The main coefficient of interest, β_1, captures the effect of the childcare subsidy on the outcome of interest. The term δ_i represents household fixed effects, which control for time-invariant, unobserved household characteristics that may be related to the outcomes. By including household fixed effects in the model, we compare each household to itself over time, isolating within-household variation caused by the introduction of the childcare subsidy. This approach accounts for household-specific factors, such as persistent differences in preferences for education spending or maternal labor force participation. The term γ_t denotes year fixed effects, which control for time-specific factors that are constant across households but vary over time. By including year fixed effects, we ensure that our estimates are not biased by time-varying external factors such as macroeconomic conditions, policy changes, or other external shocks.

The DID approach enables us to compare the outcomes of the treatment group with those of the control group. This method eliminates the influence of time-invariant unobserved characteristics that might be correlated with households' outcomes. The main identifying assumption of the DID method is the *parallel trends assumption*, which requires both treatment and control groups to have parallel outcome trends in the absence of the extended childcare subsidy.

There are challenges in estimating the causal effect of the extended childcare subsidy eligibility. First, there could be self-selection bias as households must decide whether to register for the childcare subsidy. Second, it is difficult to satisfy the parallel trends assumption of DID as the treatment and control groups differ, as shown in Table 4.2. To mitigate self-selection bias and better satisfy the parallel trends assumption, we use the PSM-DID method as proposed by Heckman et al. (1997). We apply 1:1 nearest-neighbor matching to calculate the propensity score. To ensure the robustness of our findings, we also employ alternative matching techniques and propensity score calculations for robustness checks.

4.3.3 Validation of the Parallel Trends Assumption

In order to validate the use of the DID approach, it is essential to establish that the treatment and control groups followed similar trends in household expenditures prior to the 2013 childcare subsidy expansion. Figure 4.2 provides a graphical depiction of trends in household consumption, non-consumption, child-related expenditures, educational costs, maternal employment status, working hours, and birth plans for both the treatment and control groups.

Prior to the 2013 childcare subsidy expansion, the trends in key household economic indicators appear largely parallel between the treatment and control groups. Across most categories—including total consumption, non-consumption expenditures, child-specific consumption, and working hours—both groups exhibit similar trajectories before policy implementation, supporting the plausibility of the parallel trends assumption. However, employment status shows a noticeable spike in 2012, particularly for the control group, before stabilizing in 2013. While this deviation does not completely undermine the parallel trends assumption, it suggests that factors other than the childcare subsidy may have influenced short-term employment patterns in the pre-treatment period.

After 2013, notable differences emerge between the treatment and control groups, particularly in preschool costs and extracurricular expenditures. The sharp reduction in preschool costs for the treatment group in 2013, followed by a convergence in 2014, provides preliminary visual evidence of the subsidy's effect. Additionally, extracurricular expenditures exhibit a delayed increase in 2014 for the treatment group, suggesting that households may have reallocated financial resources toward supplementary education after adjusting to the policy change.

Employment status, after the 2012 fluctuation, does not exhibit a strong post-treatment divergence, indicating that the subsidy did not induce a persistent shift in maternal labor market participation. Moreover, working hours remain relatively stable, reinforcing the view that the policy's immediate effects were concentrated in household expenditure patterns rather than labor supply decisions.

4.4 Result

This section presents the estimated impact of the 2013 childcare subsidy expansion on household economic behavior, focusing on consumption, education expenditures, and maternal labor market outcomes. The analysis first outlines the overall results, followed by heterogenous effects by geographic location and by child gender.

4.4.1 Main Results

Tables 4.3, 4.4, and 4.5 summarize the policy's effects on household consumption, education costs, and maternal labor market participation. The results indicate that the subsidy led to significant reductions in total household consumption and preschool education expenses but had limited effects on maternal employment and birth plans.

Table 4.3 shows that households in the treatment group reduced their overall consumption expenditures by an average of USD 118.4, with statistical significance at the 10% level. More notably, consumption allocated specifically to children saw a sharper decline of USD 132.1, significant at the 1% level, suggesting that households adjusted their child-related spending in response to reduced preschool costs. This decline is likely driven by the fact that consumption expenditures include education costs, which significantly decreased due to the subsidy. However, non-consumption expenditures, including housing and durable goods, did not exhibit a statistically significant change.

Table 4.4 further breaks down the effects on education expenditures. The subsidy substantially reduced preschool costs by USD 152.6, significant at the 1% level, implying that the policy effectively lowered direct childcare expenses. Interestingly, there was a modest but significant increase (USD 10.33, significant at the 10% level) in extracurricular activity expenditures, suggesting that the treatment group reallocated financial resources toward supplementary educational investments. Given that the treatment group comprises households in the top 30% income bracket, this finding raises concerns about potential disparities in educational opportunities. Higher-income households, benefiting from reduced preschool costs, may be able to reinvest these savings into additional educational activities, thereby reinforcing pre-existing advantages.

This pattern suggests that while the policy provided financial relief, it may have also contributed to the early formation of educational inequalities by enabling wealthier families to further invest in their children's development. However, the effect cannot be confidently attributed to the subsidy based solely on the data. The increase in extracurricular costs is worth noting, especially considering the treatment group's higher baseline extracurricular spending (Figure 4.2). This indicates that treated households were already investing more in extracurricular activities prior to the subsidy, potentially signaling a pre-existing preference for enhancing their children's human capital through non-academic programs, such as art, English lessons, Taekwondo, or sports.

The policy, while alleviating preschool costs, does not appear to have substantially altered this behavior in a statistically significant manner. This trend is aligned with the broader literature on human capital investment, such as Baker et al. (2008), which suggests that households may view early childhood development as critical to long-term educational and social outcomes. While the subsidy may reduce direct educational costs, families that prioritize extracurricular activities may continue to invest in these programs as part of their strategy for human capital accumulation. Further research is needed to investigate whether these trends could lead to widening educational gaps, as early investments in extracurricular activities have long-term impacts on children's cognitive and social development.

Table 4.5 reveals no statistically significant impact on maternal employment status or working hours. The coefficient for employment status (-0.0029) and working hours (0.0035) are both close to zero, indicating that the subsidy did not meaningfully alter mothers' labor supply decisions. Maternal employment status is coded as 1 if the mother is employed, including those on maternity leave. This definition ensures that temporary absences due to childbirth do not influence employment status estimates, reinforcing the conclusion that the subsidy had no substantial effect on maternal labor supply. Additionally, there is weak evidence that the policy affected fertility decisions, as indicated by the negative but insignificant coefficient (-0.0190) on birth planning. These findings suggest that while the childcare subsidy directly reduced household financial burdens, it did not lead to increased maternal labor force participation.

4.4.2 Heterogeneous Effects by Region

Tables 4.6 through 4.11 examine whether the policy's impact differs across geographic locations, distinguishing between big cities and small cities/rural areas. The findings suggest that while the subsidy reduced preschool costs in both regions, its effects on household consumption and education cost varied.

In big cities (Tables 4.6–4.8), the reduction in total household consumption was more pronounced, with a larger decrease of USD 156.7, though not statistically significant. Consumption for child declined significantly by USD 127.7 at the 5% level. A stronger reduction in preschool costs (USD 177.9, significant at 1%) was observed, alongside a greater increase in extracurricular activity expenditures (USD 10.95, significant at 5%). These findings suggest that urban households benefited more from the policy in terms of financial relief and had greater flexibility to reallocate savings toward additional education investments.

Notably, Table 4.8 shows that birth planning decisions were significantly affected in big cities. The coefficient on birth planning is -0.0730, indicating a 7.3 percentage-point reduction in the likelihood of planning additional children. This result suggests that families in the treatment group living in big cities may have responded to the policy by delaying or reconsidering childbirth. Maternal employment status and working hours remained unaffected, consistent with the overall findings.

In contrast, in small cities and rural areas (Tables 4.9-4.11), the reduction in household consumption was smaller and not statistically significant (USD 30.64). However, child-specific consumption still declined significantly (USD 108.5, $p < 0.01$). The decrease in preschool costs was also substantial (USD 135.3, $p < 0.01$). Unlike in big cities, there was no significant increase in extracurricular expenditures, which may reflect differences in the availability of extracurricular opportunities. Families in the treatment group living in small cities or rural areas may have had fewer options for extracurricular activities, limiting their ability to reallocate financial savings toward such expenditures. Furthermore, the policy had no significant effect on birth planning in these areas, as the coefficient remained close to zero (-0.0124).

4.4.3 Heterogeneous Effects by Child's Gender

Table 4.12 examines whether the policy's effects vary based on the gender of the child. The results suggest that while the childcare subsidy reduced household consumption and preschool expenses for both boys and girls, the magnitude of the effects was slightly larger for households with daughters.

For households with boys, total consumption decreased significantly by USD 208.4 ($p < 0.05$), and child-specific consumption declined by USD 94.89 ($p < 0.05$). The reduction in preschool costs was particularly strong, amounting to USD 139.2 ($p < 0.01$).

For households with girls, the decline in total consumption was not statistically significant (USD 20.34, $p > 0.1$), but child-specific consumption exhibited a larger reduction (USD 180.9, $p < 0.01$). Notably, preschool costs also saw a greater decrease (USD 168.6, $p < 0.01$) compared to boys, suggesting that families with daughters benefited slightly more in terms of direct cost savings.

4.5 Discussion

This study examines the impact of South Korea's 2013 childcare subsidy expansion, which extended eligibility from the bottom 70% of the income distribution to all households with children aged 3–4. Using a Difference-in-Differences (DID) approach with Propensity Score Matching (PSM-DID), this study evaluates the effects of the policy on household consumption, education expenditures, and maternal labor market participation. The findings contribute to the broader literature on universal childcare by providing empirical evidence in a low-fertility, high-education-expenditure context, offering insights distinct from studies in Western economies where similar policies primarily aimed to increase maternal labor supply.

The results indicate that the subsidy significantly reduced household consumption, with a more pronounced decline in child-specific consumption, primarily driven by a reduction in preschool costs. However, the savings from lower childcare costs were not reallocated toward non-consumption expenditures, which in this study include various taxes, savings deposits, insurance premiums, allowances for other household members, and monthly repayments. Instead, the findings suggest that the subsidy functioned more as a financial relief mechanism rather than as an income shock leading to increased discretionary spending.

The effects on education expenditures provide further insight into household responses, as the reduction in preschool costs did not uniformly lead to decreased overall education spending. Rather, there was a modest but significant increase in extracurricular expenditures, suggesting that some families reinvested the subsidy savings into supplementary educational activities. Given that the treatment group consists of higher-income households, this pattern raises concerns that the policy may have widened disparities in early childhood educational investment, as wealthier families were better positioned to allocate additional resources to private education. This finding aligns with existing research indicating that households with greater financial flexibility often reinvest public subsidies into additional human capital accumulation rather than reducing total spending (Baker et al., 2008; Fitzpatrick, 2012).

Despite the expectation that universal childcare would promote female labor force participation, this study finds no significant effect on maternal employment or working hours. This suggests that, at least in the short run, childcare availability alone is insufficient to influence mothers' workforce participation in Korea. Structural labor market constraints, rigid workplace norms, and the absence of complementary policies such as flexible working arrangements may contribute to this result. Unlike findings from European and North American studies, where subsidized childcare has been associated with increased female labor supply (Lefebvre and

Merrigan, 2008; Bauernschuster and Schlotter, 2015), the absence of a labor market response in Korea suggests that cultural and institutional barriers may play a more significant role in shaping maternal employment decisions.

The policy's effects vary by geographic location, with more pronounced changes observed in big cities than in small cities and rural areas. In urban areas, the subsidy resulted in a larger reduction in preschool costs and a significant increase in extracurricular expenditures, indicating that urban households were more likely to reinvest savings into additional education for their children. Moreover, birth planning behaviors were significantly affected in big cities, where the likelihood of planning additional children declined following the subsidy expansion. This suggests that in high-cost urban environments, families may have perceived the subsidy as insufficient to offset the broader financial burdens associated with raising children. In contrast, in small cities and rural areas, there was no significant increase in extracurricular expenditures, possibly due to limited availability of private educational services. The absence of a birth planning response in these areas further underscores the differential constraints and opportunities faced by households across regions, highlighting that a uniform policy may produce heterogeneous effects depending on local economic conditions and educational infrastructure.

A further dimension of heterogeneity is observed in differences in household responses based on child gender. The results indicate that for households with boys, total consumption declined significantly, whereas for households with girls, the reduction in total consumption was not statistically significant, but child-specific consumption exhibited a sharper decline. Notably, preschool cost reductions were larger for girls than for boys, suggesting that families may have allocated childcare savings differently based on the child's gender. This finding aligns with prior research indicating that Korean parents have traditionally allocated more educational resources to sons, but recent shifts in gender preferences and rising female educational attainment may be influencing spending decisions (Choi and Hwang, 2015).

While this study provides empirical evidence on the short-term effects of universal childcare subsidies, several avenues for future research remain. First, since the analysis covers only 2011 to 2014, investigating the long-term effects on children's educational attainment, maternal labor force participation, and fertility decisions would provide a more comprehensive assessment of the policy's impact. Second, although PSM-DID helps mitigate selection bias, households that prioritize early education may self-select into receiving the subsidy, potentially affecting the estimated effects. Third, the absence of a labor market response suggests that subsidized childcare alone may not be sufficient to increase female employment, necessitating further examination of how childcare subsidies interact with other labor policies, such as paid parental leave, workplace flexibility, and tax incentives for working mothers. Additionally, as higher-income households appear to benefit more from childcare cost savings by investing in extracurricular activities, further research should explore whether universal childcare reinforces or mitigates existing disparities in private education investment. Lastly, given that fertility responses varied by region, future studies should investigate how childcare subsidies interact with other economic constraints—such as housing costs and career stability—in shaping fertility outcomes, particularly in low-fertility, high-education-investment contexts.

This study contributes to the understanding of universal childcare policies in a low-fertility, high-education-investment setting, highlighting how households respond to subsidized childcare beyond traditional labor supply considerations. While the policy successfully reduced preschool costs and led to a significant decline in household consumption, it did not significantly influence maternal employment. The findings suggest that rather than serving as a catalyst for increased labor force participation, the subsidy primarily alleviated direct childcare costs while reinforcing existing disparities in private educational investments. Moreover, the heterogeneous effects by region and child gender indicate that families respond to childcare subsidies in ways shaped by economic circumstances, educational priorities, and cultural norms. Future research should examine the long-term implications of universal childcare policies on labor market dynamics, educational mobility, and fertility decisions, particularly in societies where private education plays a significant role in shaping human capital accumulation.

4.6 Tables

Table 4.1. Childcare Eligibility

Year	Childcare Subsidy Eligibility for PSKC Cohort
2008	Bottom 70% of income distribution
2012	Bottom 70% of income distribution
2013	All households

Table 4.2. Descriptive Statistics: Unmatched vs. Matched

	Unmatched		Matched	
Variable	Control[n=6148]	Treatment[n=2452]	Control[n=2379]	Treatment[n=2379]
Matching Control Variables				
Household Income	4357.76 (5335.11)	5219.54 (4328.75)	4855.25 (4971.39)	5211.84 (4344.18)
Maternal Education(1=College Degree)	0.36 (0.48)	0.56 (0.5)	0.55 (0.5)	0.56 (0.5)
Paternal Education(1=College Degree)	0.45 (0.5)	0.68 (0.47)	0.62 (0.48)	0.68 (0.47)
Birth Order	1.72 (0.76)	1.57 (0.61)	1.57 (0.67)	1.57 (0.61)
Gender(1=Male,2=Female)	1.5 (0.5)	1.47 (0.5)	1.47 (0.5)	1.47 (0.5)
Living in Big Cities	0.43 (0.5)	0.36 (0.48)	0.35 (0.48)	0.36 (0.48)
Dependent Variables				
Consumption	2130.34 (1084.97)	2487.26 (1259.6)	2257.78 (1188.73)	2486.8 (1260.59)
Non-consumption	1254.34 (998.37)	1583.79 (1168.69)	1378.22 (1136.5)	1583.29 (1169.21)
Consumption for Kid	889.43 (581.67)	1019.79 (639.31)	908.17 (605.66)	1021.01 (638.71)
Non-consumption for Kid	276.79 (286.14)	305.85 (295.1)	289.25 (332.82)	305.86 (295.66)
Preschool Cost	171.63 (177.53)	256.68 (217.19)	194.31 (201.93)	257.57 (217.65)
Extracurricular cost	64.6 (44.13)	67.44 (49.65)	65.22 (42.55)	67.3 (49.71)
Maternal Employment Status(1=employed)	0.42 (0.49)	0.4 (0.49)	0.45 (0.5)	0.4 (0.49)
Maternal Working Hours	7.67 (1.98)	7.84 (1.9)	7.6 (1.94)	7.84 (1.89)
Birth Plan(1=Plans to have an extra child)	0.08 (0.28)	0.11 (0.31)	0.11 (0.32)	0.11 (0.31)

Table 4.3. Household Expenditure Analysis (Unit = KRW 1,000 $\sim$ USD 1)

Dependent Variables:	Consumption	Non-consumption	Consumption for Child	Non-consumption for Child
Model:	(1)	(2)	(3)	(4)
Variables				
Post $\times$ Treatment	-118.4*	-19.49	-132.1***	-1.538
	(67.06)	(56.14)	(32.12)	(21.59)
Fixed-effects				
Household ID	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	4,252	4,247	4,275	4,260
R^2	0.64018	0.66697	0.67949	0.52771

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.4. Household Education Cost Analysis (Unit = KRW 1,000 $\sim$ USD 1)

Dependent Variables:	Preschool Cost	Extracurricular Cost
Model:	(1)	(2)
Variables		
Post $\times$ Treatment	-152.6***	10.33*
	(11.35)	(6.187)
Fixed-effects		
Household ID	Yes	Yes
Year	Yes	Yes
Observations	3,990	4,280
R^2	0.71236	0.52739

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.5. Effect of the Childcare Subsidy on Mothers' Employment, Working Hours, and Birth Plans

Dependent Variables:	Employment Status	Working Hours	Birth Plan
Model:	(1)	(2)	(3)
Variables			
Post × Treatment	-0.0029	0.0035	-0.0190
	(0.0234)	(0.1606)	(0.0217)
Fixed-effects			
Household ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Observations	4,280	1,588	4,242
R^2	0.75337	0.80407	0.50465

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.6. Household Expenditure Analysis in Big Cities(Unit = KRW 1,000 $\sim$ USD 1)

Dependent Variables:	Consumption	Non-consumption	Consumption for Child	Non-consumption for Child
Model:	(1)	(2)	(3)	(4)
Variables				
Post × Treatment	-156.7	-14.40	-127.7**	25.31
	(105.1)	(91.16)	(55.64)	(23.43)
Fixed-effects				
Household ID	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	1,576	1,578	1,581	1,578
R^2	0.65171	0.71123	0.72417	0.64958

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.7. Household Education Cost Analysis in Big Cities (Unit = KRW 1,000 ∼ USD 1)

Dependent Variables:	Preschool Cost	Extracurricular Cost
Model:	(1)	(2)
Variables		
Post × Treatment	-177.9***	10.95**
	(18.46)	(5.099)
Fixed-effects		
Household ID	Yes	Yes
Year	Yes	Yes
Observations	1,471	1,583
R^2	0.80080	0.56946

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.8. Effect of the Childcare Subsidy on Mothers' Employment, Working Hours, and Birth Plans in Big Cities

Dependent Variables:	Employment Status	Working Hours	Birth Plan
Model:	(1)	(2)	(3)
Variables			
Post × Treatment	0.0137	-0.2265	-0.0730*
	(0.0383)	(0.3467)	(0.0389)
Fixed-effects			
Household ID	Yes	Yes	Yes
year	Yes	Yes	Yes
Observations	1,583	1,583	1,566
R^2	0.79930	0.45306	0.58807

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.9. Household Expenditure Analysis in Small Cities or Rural Areas(Unit = KRW 1,000 ∼ USD 1)

Dependent Variables:	Consumption	Non-consumption	Consumption for Child	Non-consumption for Child
Model:	(1)	(2)	(3)	(4)
Variables				
Post × Treatment	-30.64	-76.09	-108.5^{***}	-9.598
	(93.82)	(75.01)	(40.60)	(33.19)
Fixed-effects				
Household ID	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Observations	2,692	2,685	2,711	2,698
R^2	0.63536	0.65925	0.66110	0.49181

Clustered (N_ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.10. Household Education Cost Analysis in Small Cities or Rural Areas (Unit = KRW 1,000 ∼ USD 1)

Dependent Variables:	Preschool Cost	Extracurricular Cost
Model:	(1)	(2)
Variables		
Post × Treatment	-135.3^{***}	3.167
	(13.56)	(3.243)
Fixed-effects		
Household ID	Yes	Yes
Year	Yes	Yes
Observations	2,532	2,713
R^2	0.69800	0.53911

Clustered (N_ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.11. Effect of the Childcare Subsidy on Mothers' Employment, Working Hours, and Birth Plans in Small Cities or Rural Areas

Dependent Variables:	Employment Status	Working Hours	Birth Plans
Model:	(1)	(2)	(3)
Variables			
Post × Treatment	-0.0065	0.1273	-0.0124
	(0.0300)	(0.2314)	(0.0267)
Fixed-effects			
Household ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Observations	2,713	2,713	2,688
R^2	0.74990	0.76769	0.48817

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.12. Effects of Childcare Subsidy on Household Consumption, Consumption for Kids, and Preschool Costs: A Gender Comparison

	Boy			**Girl**		
Dependent Variables:	Consumption	Consumption for Kids	Preschool Cost	Consumption	Consumption for Child	Preschool Cost
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Post × Treatment	-208.4**	-94.89**	-139.2***	-20.34	-180.9***	-168.6***
	(88.02)	(40.63)	(13.71)	(101.9)	(50.17)	(18.81)
Fixed-effects						
Household ID	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,279	2,288	2,143	1,973	1,987	1,847
R^2	0.67980	0.74969	0.74061	0.60129	0.59887	0.69106

Clustered (Household ID) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

4.7 Figures

Figure 4.1. Covariate Balance Before and After Matching

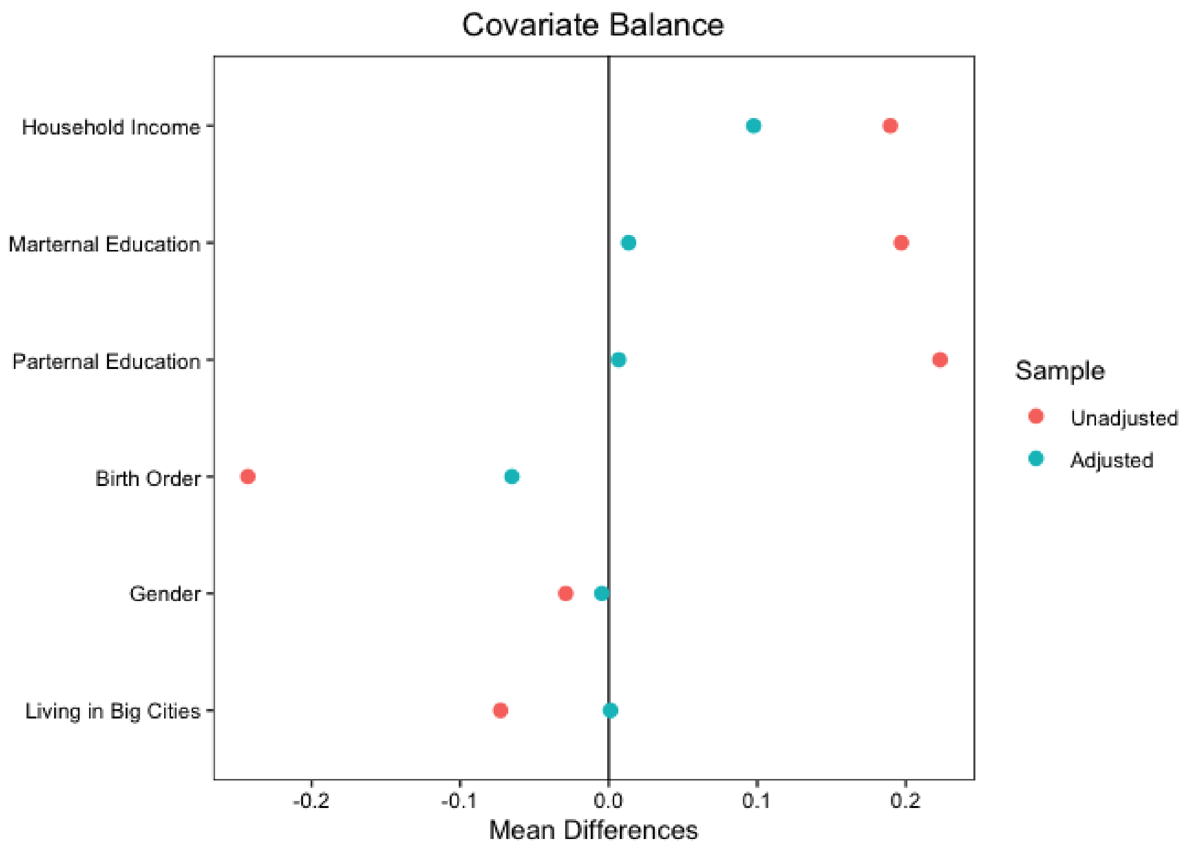
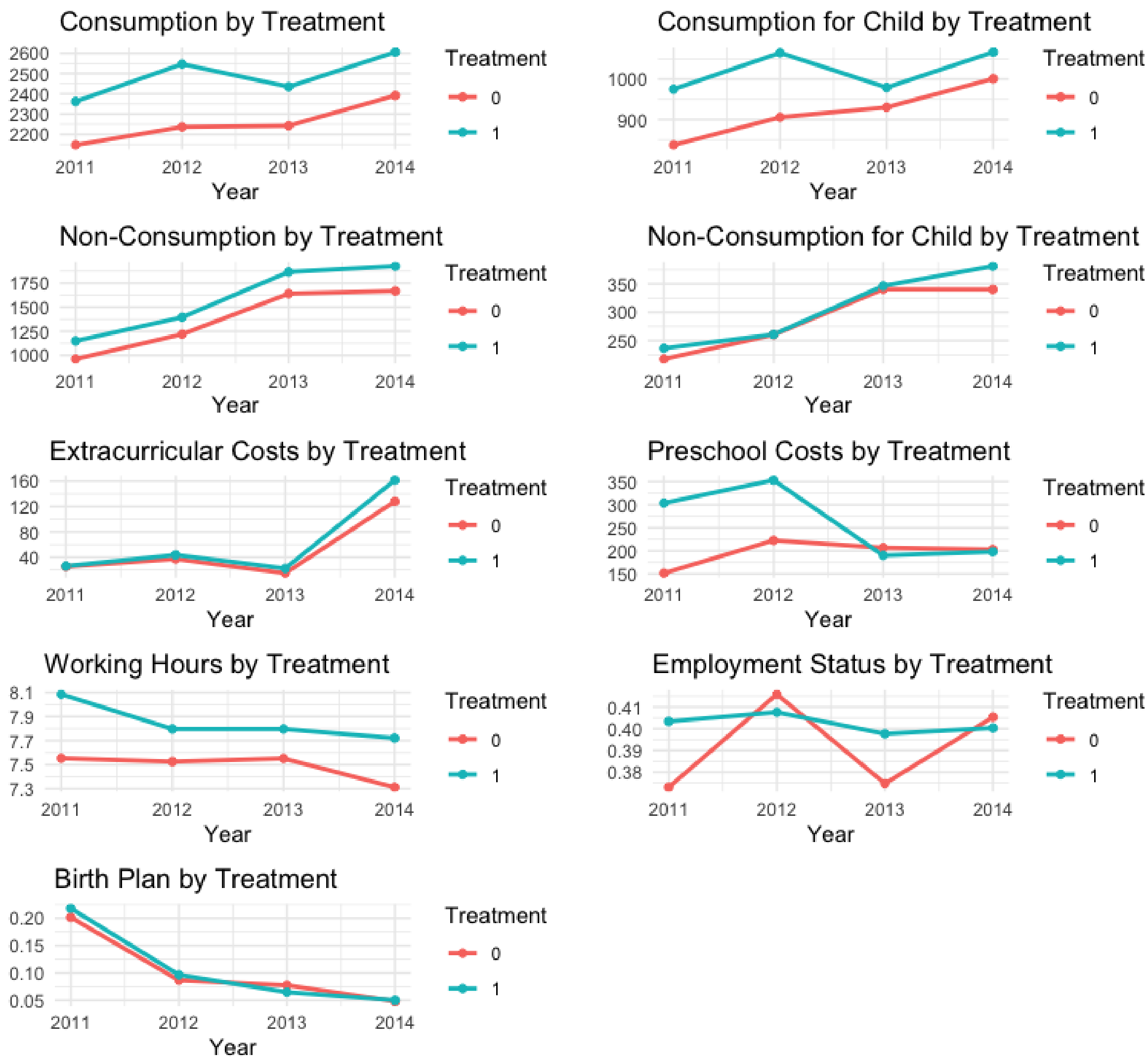


Figure 4.2. Trends by Treatment



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