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ABSTRACT

Universal Early Childhood Education and Adolescent Risky Behavior*

The evidence for the effects of early childhood education on risky behavior in adolescence is limited. This paper studies the consequences of a reform of a large-scale universal kindergarten program in Japan. Exploiting a staggered expansion of kindergartens across regions, we estimate the effects of the reform using an event study model. Our estimates indicate that the reform significantly reduced juvenile violent arrests and the rate of teenage pregnancy, but we do not find that the reform increased the high school enrollment rate. We suspect that improved non-cognitive skills can account for the reduction of risky behavior in adolescence.

JEL Classification: H52, I20, I28, J13, J24, K40

Keywords: early childhood education, crime, teenage pregnancy

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1 Introduction

US President Biden's American Families Plan envisions universal access to high-quality early childhood education. The economic case for promoting early childhood programs depends on whether and how they benefit children and society at large. Risky behavior, including criminal activity and teenage pregnancy, often leads to negative consequences, not only for the individuals themselves but also for others in society, including their children (Anderson, 1999). Given that risky behavior incurs large negative externalities, the efficacy of early childhood education in reducing risky behavior can justify a policy intervention for early childhood education accompanied by large public expenditures.

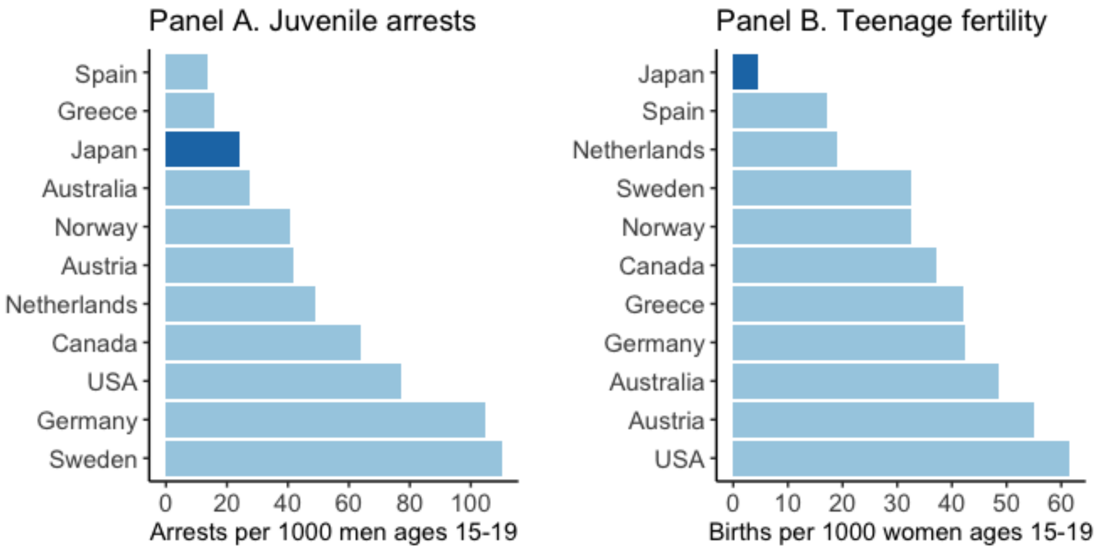
Despite their possible social benefits, the direct effects of early childhood education on risky behavior are not well understood. Small-scale randomized studies targeting severely disadvantaged children, including the Perry Preschool Project (PPP) and the Carolina Abecedarian Project (ABC), provide relatively compelling evidence for crime-reducing effects.[1] However, the efficacy of such programs may be substantially lower when they are expanded and serve a different population. In fact, the existing evidence for large-scale programs is mixed. On the one hand, Johnson and Jackson (2019) and Anders, Barr and Smith (Forthcoming) show that Head Start, a large-scale targeted program, reduced crime, although the effect sizes are smaller than those of the PPP and ABC. On the other hand, Baker, Gruber and Milligan (2019) find that the universal childcare program in Quebec had detrimental effects on children and increased crime rates later in life. The effects of early childhood education on risky behavior can vary by scale of the program, population served, and other institutions in society. Although policy makers around the world often rely on limited evidence from the US, existing evidence may not necessarily be valid in other countries with different social contexts.

We attempt to fill this gap by estimating the effects of a kindergarten reform in Japan on risky behavior in adolescence, including juvenile arrests and teenage pregnancy. There are at least two reasons why studying the Japanese case can improve understanding of the efficacy of early childhood education. First, the kindergarten program in Japan is nationwide and universal. Not only does the program accept children of all socioeconomic backgrounds, but its scale is

[1] See Heckman (2006), for example.

also much larger than that of Head Start in terms of enrollment: Approximately 1.7 million children were enrolled in kindergartens in Japan at the end of the kindergarten reform in 1971, while there were 397,500 children enrolled in Head Start.[2] Second, Japan can be characterized as having much lower rates of juvenile arrest and teenage fertility than are found in North American countries (see Figure 1). Both of these factors can be expected to weaken the efficacy of early childhood education in reducing risky behavior. If we find significant effects in such a social context, we will gain greater confidence in early childhood education, which may help policy makers in Europe and other parts of the world.

Figure 1: International comparison of juvenile arrests and teenage fertility



Notes: Panel A displays the arrest rate of men ages 15-19 in 1975 among OECD countries in the United Nations Surveys on Crime Trends and the Operations of Criminal Justice Systems (UN-CTS). Panel B compares the fertility rate of women ages 15-19 between 1970 and 1975 across these countries, based on data from the United Nations World Population Prospects.

Japanese kindergartens provide formal pre-primary education, and their curriculum focuses on fostering social-emotional skills rather than academic skills. In response to the recommendation of the United Nations Educational, Scientific, and Cultural Organization (UNESCO) in 1961, the national government promoted pre-primary education and increased the kindergarten enrollment rate from 18 to 39% during 1964-1970. Exploiting staggered rollouts of the reform across provinces[3], we estimate the effect of the reform on juvenile risky behavior using an event

[2] US Department of Health & Human Services (2022)

[3] The actual administrative term used by the government is *prefecture*, but we use province as it is

study model.

One of our main outcomes of interest is juvenile arrests because they incur large social costs, and the prevalence of criminal offenses tends to peak in the late teens. Another outcome of interest is teenage pregnancy. Although teenage pregnancy itself is not necessarily problematic, it is associated with weaker labor market outcomes and external costs, such as greater welfare dependency. Furthermore, children born to adolescent mothers are more likely to experience poor school performance, health problems, and criminal activity. Although it is not clear whether such associations are causal, to the extent that they are, reducing teenage pregnancy can improve the welfare of young women, their children, and society as a whole.

There are three reasons early childhood education may prevent risky juvenile behaviors. First, early childhood education can increase years of education, mechanically reducing people's time to engage in behaviors that occur outside of school, including criminal and sexual activities. This effect is known as the "incapacitation" or "incarceration" effect in the literature on juvenile crime (Jacob and Lefgren, 2003) and teen fertility (Black, Devereux and Salvanes, 2008), respectively. Second, by increasing years of education early childhood education can result in higher earnings. This explanation is based on human capital theory, as increased human capital raises the opportunity cost of risky behaviors after graduating from school. The third explanation is related to non-cognitive skills. Childhood externalizing behavior, including aggressive, antisocial, and rule-breaking behavior, predicts later antisocial behavior (Moffitt, 1993). Early childhood education improves non-cognitive skills and eventually reduces risky behaviors later in life (Heckman, Pinto and Savelyev, 2013).

We find that the kindergarten reform in Japan reduced the juvenile violent arrest and teenage pregnancy rates significantly. The estimated effects on juvenile non-violent arrests are also negative, but statistically insignificant with a large standard error. Our estimates indicate that the kindergarten reform decreased the juvenile violent arrest and teenage pregnancy rates by 1.052 and 1.325 per 1,000 individuals, respectively. In other words, the reform reduced these outcomes by 38% and 17% from their pre-reform averages, respectively. This result implies that a large universal early childhood education program can reduce risky behavior later in life, even

more intuitive to most readers. Japan has 47 provinces that constitute its first level of administrative and jurisdictional division.

in a country with very low rates of crime and pregnancy among adolescents, and should give policy makers around the world greater confidence in the efficacy of early childhood education.

To investigate the underlying mechanisms at work here, we also estimate the effects of the kindergarten reform on high school enrollment. High school is not part of compulsory education in Japan and 92% of the children affected by the kindergarten reform were enrolled in high school during the analysis period. We do not find that the kindergarten reform increased high school enrollment, which implies that additional schooling is unlikely to be a channel through which early childhood education reduces risky behaviors.

Although we do not have a measurement for non-cognitive skills, Yamaguchi, Asai and Kambayashi (2018*b*) find that the universal childcare program in Japan, which shares similar curriculum standards with kindergartens, reduces aggressive behavior among children with low-educated mothers. These findings might collectively suggest that a plausible explanation for the effect of preventing risky behavior is the development of non-cognitive skills.

We address possible endogeneity biases by testing pre-trends, conducting falsification tests, and estimating several alternative models. First, we do not find a significant pre-trend in the event study model. Second, we conduct a falsification test in which the outcome variables are crime and pregnancy rates of older cohorts that are not directly affected by the reform. If a labor market shock or an unmeasured policy change affects criminal behavior and pregnancy and is correlated with the reform, our estimates are biased. To the extent that it affects both adolescents and adults in a given year, our falsification test can detect the existence of a confounder. Third, we address four issues including inter-regional migration, endogenous kindergarten enrollment, and the existence of other confounders by using alternative specifications, but our main results remain essentially unchanged.

The remainder of this paper proceeds as follows. We review the related literature in Section 2. Section 3 describes the institutional background in Japan and the kindergarten reform. Section 4 explains our empirical strategies. Section 5 describes the data. Section 6 presents the results. In Section 7, we perform several specification checks. We discuss potential mechanisms in Section 8 and conclude in Section 9.

2 Related Literature

This paper contributes to the large literature on early childhood education. Previous research focuses mainly on short-term cognitive and non-cognitive outcomes[4] and long-term outcomes such as educational attainment and labor market outcomes.[5] These studies established evidence that early childhood education can improve these outcomes; however, the evidence for early childhood education reducing risky behavior is limited and concentrated in targeted programs in the US such as the PPP, ABC, and Head Start.

Heckman et al. (2010) find that participation in the PPP significantly decreased felony arrests. The Chicago Child-Parent Center (CPC) preschool program reduced felony arrests especially among children of high school dropouts (Reynolds et al., 2011). García, Heckman and Ziff (2019) also find that the ABC significantly reduced criminal activities, particularly for the least advantaged children. Recent studies on the effects of Head Start on criminal behavior (Johnson and Jackson, 2019; Anders, Barr and Smith, Forthcoming) find a significant reduction in incarceration and serious convictions. All of these programs are targeted at poor children, and hence it is not entirely clear whether the demonstrated effects can be maintained when a program serves non-poor children as well as poor children.

Indeed, the evidence for a universal program is mixed. Anders, Barr and Smith (Forthcoming) estimate the effects of the Smart Start Program, a universal program in North Carolina, and find a significant decrease in serious convictions. Baker, Gruber and Milligan (2019) study the long-term effects of the universal childcare program in Quebec, Canada. They found that cohorts with greater access to universal childcare in Quebec experienced a higher, instead of a lower, crime rate later in life. Both of these papers study programs in North America, and the evidence from these programs may or may not be applicable to other countries because

[4]See Gormley and Gayer (2005), Baker, Gruber and Milligan (2008), Datta Gupta and Simonsen (2010), Kottelenberg and Lehrer (2017), Cornelissen et al. (2018), Carta and Rizzica (2018), Felfe and Lalive (2018), Yamaguchi, Asai and Kambayashi (2018*a*,*b*), Cornelissen and Dustmann (2019), Fort, Ichino and Zanella (2019), Drange and Havnes (2019), Cascio (2020), Kose (2021), and Rege et al. (2021), for example.

[5]See, for example, Berlinski, Galiani and Manacorda (2008), Havnes and Mogstad (2011*a*,*b*, 2015), DeCicca and Smith (2013), Akabayashi and Tanaka (2013), Felfe, Nollenberger and Rodríguez-Planas (2015), Drange, Havnes and Sandsør (2016), Datta Gupta and Simonsen (2016), Herbst (2017), Gray-Lobe, Pathak and Walters (2021), Bailey, Sun and Timpe (2021), and Kawarazaki (2022).

institutions and contexts differ.

This paper is also related to the literature on the effects of education on teenage pregnancy. Several papers in the literature estimate the effects of an additional year of education on teenage pregnancy by exploiting changes in compulsory schooling laws in different countries.[6] To the extent that early childhood education improves cognitive and non-cognitive skills, it is likely to eventually prevent risky behavior such as teenage pregnancy as well as crime. However, evidence for early childhood education reducing teenage pregnancy is scarce, mixed, and limited to the US. Berrueta-Clement et al. (1984) and Campbell et al. (2002) find that the PPP and ABC reduced teenage pregnancy, respectively, but Campbell et al. (2008) find that Project CARE, a randomized study in North Carolina, had insignificant effects.

Our contribution is to offer new evidence for early childhood education preventing crime and pregnancy among adolescents in a country with different institutions and social contexts than are found in the places where the existing evidence was gathered. First, the Japanese kindergarten program was universal and far larger than programs studied in the previous research. This program accepted all children regardless of socioeconomic status. In terms of enrollment size, the Japanese program was four times greater than the US Head Start program during the analysis period. Second, the rates of juvenile arrest and teenage fertility were much lower in Japan than in North America. Both factors are expected to reduce the effectiveness of a program. Our positive result suggests that early childhood education can prevent risky behavior of adolescents even in such a context and therefore should give policy makers outside the US added confidence in the efficacy of early childhood education.

3 Institutional Background

We describe the institutional characteristics of Japanese kindergartens in Section 3.1. The national curriculum, which was developed under the strong influence of the US during the US occupation of Japan, emphasizes children's socio-emotional skills rather than their academic

[6]Examples include, but are not limited to, the US (Black, Devereux and Salvanes, 2008; McCrary and Royer, 2011), U.K. (Geruso and Royer, 2018), Germany (Cygan-Rehm and Maeder, 2013), Sweden Grönqvist and Hall (2013), Norway (Black, Devereux and Salvanes, 2008), and Canada (DeCicca and Krashinsky, 2020).

skills. We then outline the kindergarten reform of 1964-1970 in Section 3.2. This reform allowed more than 1.4 million children to enroll in kindergartens throughout the country.

3.1 Kindergartens in Japan

While most countries offer early childhood education and care (ECEC) in a unified framework, Japan provides kindergartens and center-based childcare as distinct ECEC institutions (OECD, 2015). The main objective of kindergartens is to provide formal pre-primary education to children over the age of 3 regardless of their parents' employment status. Kindergartens offer a half-day (at least 4 hour) program. In contrast, center-based childcare offers a full-day program for children ages 0-6, and both parents being employed is virtually required for its use. Neither kindergartens nor center-based childcare are means-tested.

ECEC is heavily subsidized by the government, and therefore kindergarten fees are highly affordable, although they vary according to several factors, including province of residence, household income, and number of siblings. In 1970, the average annual out-of-pocket fee for public and private kindergartens was 14,520 JPY and 37,920 JPY respectively, which amounted to about 1-3% of average household income.[7]

All public and private kindergartens are subject to quality regulations, including national curriculum standards and teacher certification requirements. The Ministry of Education first developed the national curriculum guidelines in 1947 during the American occupation of Japan with the advice of Helen Heffernan, who served as the chief of rural and elementary education for the California State Department of Education during 1926-1965 (Blackwood, 1988; Weiler, 2011). These guidelines later became the foundation of the national curriculum for kindergartens, which has been implemented since 1956 with minor revisions. This curriculum specifies key experiences in children's development and emphasizes hands-on activities among children, rather than teacher-driven instruction. Similar to the HighScope curriculum and the Head Start framework, the Japanese kindergarten program focuses on fostering social-emotional

[7]Note that 1970 is the last year of the kindergarten reform on which we focus in Section 3.2. According to the Household Survey, the average annual income of working households with two or more people was 1,355,388 JPY (i.e., 112,949 JPY per month × 12 months) in 1970. See Ministry of Health and Welfare (1971) for more details regarding kindergarten fees.

skills rather than academic skills. In addition, since 1954, all kindergarten teachers must have a national license for kindergarten teachers that requires at least two years of post-secondary education.

The maximum student-teacher ratio in each kindergarten classroom was first required to be 40 to 1 in 1956 and this requirement has been modified to 35 to 1 since 1995. In practice, the class size constraint seldom holds in kindergartens. Japan complied with the recommendation of the International Conference on Public Education in 1961 that the student-teacher ratio for pre-primary institutions should be below 25 to 1 (IBE-UNESCO, 1961). The average student-teacher ratio was 24 to 1 during 1957-1985, but this was higher than in other countries. For example, Havnes and Mogstad (2011*b*) report that the student-teacher ratio in Norwegian childcare centers was between 17 and 19 to 1 for children over 3 years of age during 1975-1981. Head Start recommends that class sizes for children ages 4-5 range between 17 and 20 (Carneiro and Ginja, 2014). Appendix A provides more details on the institution of kindergarten in Japan.

3.2 Kindergarten Reform in 1964-1970

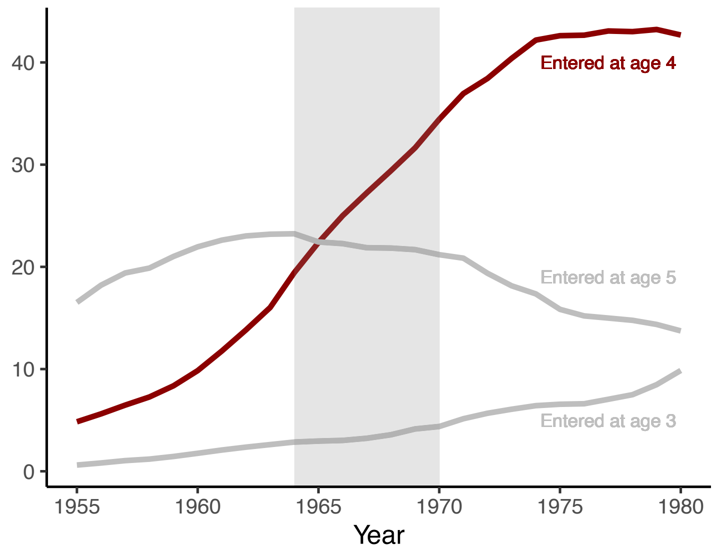
The regional gaps in kindergarten availability were substantial in the early 1960s; while the number of kindergartens gradually increased after World War II, 1,709 out of 3,388 municipalities still did not have any kindergarten before 1966 due to insufficient financial support from the national government.[8] Following the recommendations to promote pre-primary education in 1961[9], the national government reformed its pre-primary education policy by heavily subsidizing the construction and expansion of kindergartens during 1964-1970. The goal was to establish at least one kindergarten in all municipalities with more than 10,000 residents.[10]

This reform successfully increased the number of kindergartens by 45% (from 7,687 to 11,180) and the number of kindergarten teachers by 84% (from 37,041 to 68,607) between 1963 and 1971. Most of the newly created kindergarten slots were immediately filled. In fact, many kindergartens remained oversubscribed even after the reform. The median child-to-slot ratio in 1965 was 113 % and remained almost the same at 110 % in 1971.

[8] Ministry of Education (1979)
[9] IBE-UNESCO (1961)
[10] Ministry of Education (1979).

Although all children ages 3 to 5 were eligible for kindergarten, the typical kindergarten starting age was 4 during and after the reform. Figure 2 shows that the fraction of children starting kindergarten at age 4 increased dramatically during the reform, while the trends for other ages remained relatively stable. Consequently, the kindergarten enrollment rate at age 4 doubled from 18.4 % in 1963 to 41.3 % in 1971.[11]

Figure 2: Percentage of children entering kindergarten at ages 3-5, 1955-1980



Notes: This figure displays the fractions of children entering kindergarten at each age between 3 and 5. The gray-colored area corresponds to years during the kindergarten reform. To calculate the number of children entering kindergarten at age 4 in year t, we subtract the number of 3-year-old kindergarten students in year $t-1$ from the number of 4-year-old kindergarten students in year t, assuming that no children who entered kindergarten at age 3 dropped out. The number entering at age 5 is calculated in a similar manner. The number entering at age 3 is equal to the number of 3-year-old students as kindergartens only accept children who are at least 3 years old.

Despite the rapid increase in kindergarten enrollment, the reform does not appear to have compromised the quality of kindergartens. For example, the average ratio of kindergarten students to teachers changed very little from 24.9 in 1963 to 25.4 in 1971.[12] The stability of the student-to-teacher ratio suggests that the increase in enrollment during the reform did not cause substantial crowding in kindergartens.

[11] See Figure B.2 for variations in the kindergarten enrollment rate at age 4.

[12] Based on data from the School Basic Survey, we calculate an average ratio of kindergarten students to teachers in each province as the ratio of the number of kindergarten students to the number of kindergarten teachers.

4 Empirical Strategy

To identify the causal effects of the kindergarten reform, we exploit inter-provincial variations in the growth of the kindergarten enrollment rate for 4-year-old children during 1964-1970. Section 4.1 describes the growth of the kindergarten enrollment rate across provinces and discusses possible determinants of the reform rollout. Specifically, we examine the correlations between the growth of the kindergarten enrollment rate and pre-reform provincial characteristics such as the provincial GDP. In Section 4.2 we specify our econometric model based on the event study approach. Section 4.3 discusses possible threats to the identification strategy and explains how we validate the parallel trend assumption.

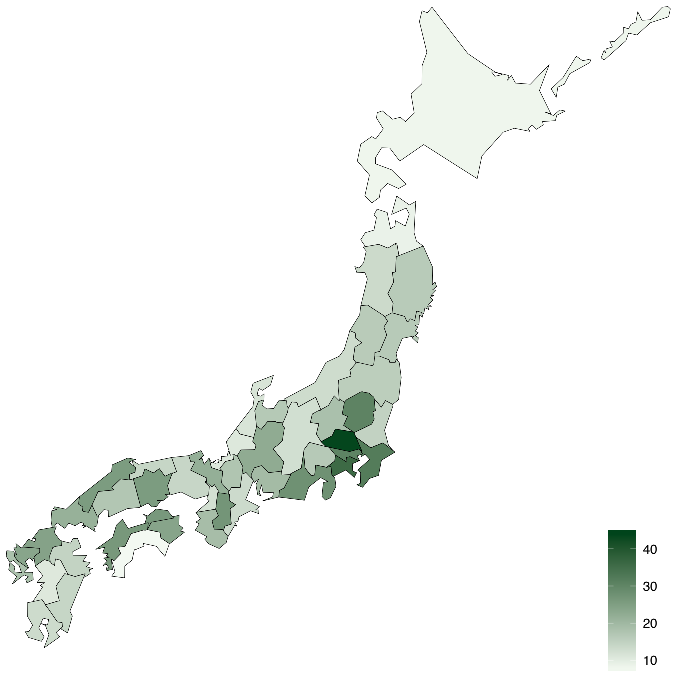
4.1 Regional Variation in the Reform Rollout

The kindergarten reform caused substantial variations in the growth of kindergarten enrollment across provinces. As Figure 3 indicates, some provinces increased the enrollment rate by more than 40 percentage points, while growth in other provinces remained as low as 8 percentage points. Provinces with high enrollment rates before the reform tended to exhibit a higher growth rate. The growth rate was notably higher in Tokyo and surrounding provinces, including Kanagawa, Chiba, and Saitama, whereas it was lower in the western region, including the provinces of Osaka, Kyoto, Hyogo, Aichi, and Fukuoka. Figure B.1 reports how the kindergarten enrollment rate varied before and after the reform in each province.

A plausible explanation for these regional variations is that provincial and municipal governments had a certain level of discretion in expanding kindergartens. In particular, while the central government pursued a reform initiative and offered subsidies to local governments, local governments were responsible for implementing the reform and determining the appropriate level of kindergartens given their local needs.

We take advantage of the differences in the growth of kindergarten enrollment across provinces in the reform period to construct a treatment variable. To shed light on the determinants of kindergarten enrollment growth during the reform, we examine the correlations between the growth of the kindergarten enrollment rate and pre-reform provincial characteristics

Figure 3: Geographical variation in the growth of the kindergarten enrollment rate during the kindergarten reform



Notes: This figure shows variations in the kindergarten enrollment rate during 1963-71 for 46 provinces. We omit the province of Okinawa due to missing data.

including real provincial GDP per capita and fiscal capacity. To measure the fiscal capacity of a province we use the amount of fiscal-equalization grants. Fiscal-equalization grants for provinces are transfers from the central to provincial governments and cover provincial fiscal needs that cannot be covered only by provincial tax revenues, so provinces with higher fiscal capacities typically receive a relatively small amount of fiscal-equalization funding from the central government.

We note three findings in Table 1. First, the growth of the kindergarten enrollment rate is positively correlated with the pre-reform kindergarten enrollment rate (see column 1). Second, the growth of the kindergarten enrollment rate is positively correlated with the real provincial GDP per capita (column 2) and negatively correlated with the amount of fiscal-equalization grants (column 3). Together, these patterns suggest that the major provinces with greater fiscal capacity experienced a higher growth in kindergarten enrollment.

These results indicate that the growth of the kindergarten enrollment rate is not random. Note, however, that randomness is not required for identification. Our identification is based

Table 1: Correlation between reform rollout and pre-reform provincial characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Kindergarten enrollment rate in 1960	0.632						0.955
	(0.145)						(0.357)
Real Provincial GDP per capita in 1960		0.012					-0.034
		(0.007)					(0.016)
Fiscal-equalization grants per capita in 1960			-0.615				-0.593
			(0.241)				(0.308)
Low-educated women's share in 1960				-0.531			-0.242
				(0.333)			(0.347)
Growth in low-educated women's share during 1950-1960				0.457			0.131
				(1.854)			(1.352)
Women's employment rate in 1960					-0.201		0.014
					(0.143)		(0.184
Growth in women's employment rate during 1955-1960					-0.146		-0.680
					(0.557)		(0.447)
Child population ratio in 1960						-0.125	-0.025
						(0.055)	(0.071)
Growth in child population ratio during 1955-1960						-0.297	-0.737
						(0.328)	(0.329)

Notes: This table shows slope estimates obtained from a simple regression of the growth in the kindergarten enrollment rate during the reform on the pre-reform provincial characteristics. See Table B.2 for definitions and data sources of the provincial characteristics. Heteroskedasticity-robust standard errors are reported in parentheses.

on the difference-in-differences design, and hence the key identifying assumption is a parallel trend assumption conditional on a set of control variables. We assess the plausibility of the parallel trend assumption by examining the existence of a pre-intervention trend using the event study model laid out in Section 4.2.

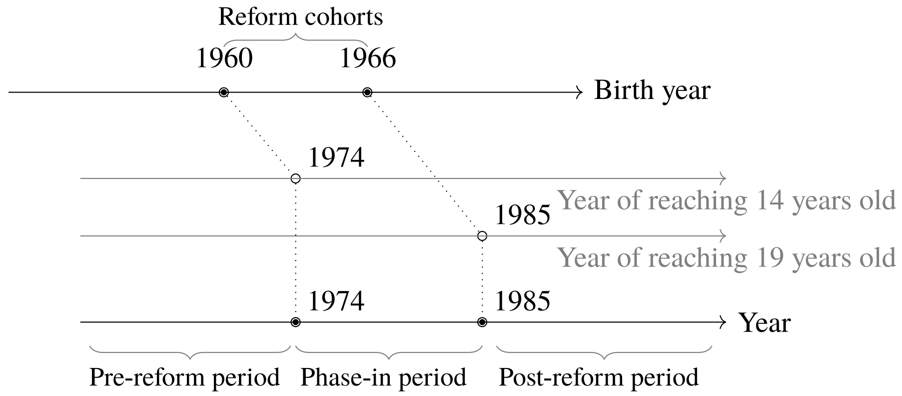
4.2 Econometric Model

We estimate the causal effects of the kindergarten reform using the following event study model:

$$Y_{it} = \sum_{h \in \{1967,\ldots,1995\}} \{\alpha_h \Delta Enroll_i \times \mathbf{1}[t = h] + \beta_h X_i \times \mathbf{1}[t = h]\} + \pi_i + \mu_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} is an adolescent outcome variable such as the juvenile arrest rate in province i in year t. The treatment variable denoted by $\Delta Enroll_i$ is the growth of the kindergarten enrollment rate during the reform period in province i. Note that the treatment variable is continuous. The indicator function $\mathbf{1}[t = h]$ takes the value of one if the condition in the square brackets

Figure 4: Pre-reform, phase-in, and post-reform periods for adolescent outcomes



Notes: This figure defines the pre-reform, phase-in, and post-reform periods for evaluating the effects of the kindergarten reform on adolescent outcomes among individuals ages 14-19. We refer to individuals born between 1960 and 1966 as "reform cohorts" as they may have entered kindergarten at age 4 during the reform. Accordingly, for adolescent outcomes we define the period of 1974-1985 as the "phase-in period" because the reform cohorts reached ages 14-19 during this period. Years before and after the phase-in period are defined as the "pre-reform period" and "post-reform period", respectively.

is satisfied and the value of zero otherwise. The coefficient α_h captures the time-varying effect of the kindergarten reform. To control for preexisting differences in outcome trends across provinces, we include X_i, the pre-reform provincial characteristics listed in Table 1. The parameters π_i and μ_t are province-specific fixed effects and year fixed effects, respectively. The variable ε_{it} is an idiosyncratic error.

For normalization, we assume that $\alpha_{1973} = \beta_{1973} = 0$ for adolescent outcomes because individuals exposed to the kindergarten reform would have begun to reach adolescence 10 years after the reform began in 1964. As illustrated in Figure 4, we refer to those born between 1960 and 1966 as the "reform cohort" because they were four years old and eligible for kindergarten during the reform period, 1964-1970. Consequently, we define the period 1974-1985 as the "phase-in" period of the reform, because the reform cohort reaches ages 14-19 during this period. In addition, we refer to the years prior to the phase-in period as the "pre-reform" period and the years after the phase-in period as the "post-reform" period. As such, the estimates α_h for the pre-reform ($h < 1973$) period capture a pre-trend, while those for the phase-in ($1974 \leq h \leq 1985$) and post-reform ($h > 1985$) periods are interpreted as the dynamic effects of the kindergarten reform.

4.3 Threats to Identification

Our key identification assumption is the parallel trend assumption. We assess the plausibility of this assumption by examining the presence of a pre-treatment trend, but there are several remaining concerns for identification.

One of the threats to our identification strategy is the possible influence of policy changes other than the kindergarten reform. If there were concurrent policy changes that were likely to influence juvenile crime and teenage pregnancy, we cannot disentangle the effects of the kindergarten reform from other policy changes. Apart from the kindergarten reform, we are not aware of any substantial changes in the criminal justice system or abortion law during the analysis period. For example, the Juvenile Act, established in 1948, stipulates nationally standardized criminal procedures for juvenile offenders. It was revised in April 2001 to allow criminal charges to be imposed on offenders ages 14-15, but this is outside the analysis period (1967-1995);[13] Similarly, the Maternity Protection Act, which legalized induced abortions among mothers until the second trimester of pregnancy (22 weeks), was enacted in 1948. During 1948-1952, performing induced abortions required a review by a regional abortion committee. The abortion law was revised in 1952 to permit designated medical doctors to perform abortion surgeries at their discretion. Since then, there has been no substantial revision to the abortion law, including within the analysis period.

Although there seem to have been no substantial policy changes, we may have missed some policy changes at the local government level. To address this concern, we perform placebo tests in which we take as outcomes the arrest and pregnancy of older cohorts who were not directly affected by the kindergarten reform. To the extent that an unobserved policy change influences not only adolescents but also older cohorts, we are able to detect such a policy change biasing our estimates. Furthermore, our placebo tests can detect other confounders as long as they affect both adolescents and older cohorts. An example of such confounders is a local economic condition or a labor demand shock. We will discuss the placebo tests extensively later in Section 7.1.

Inter-provincial migration is another threat to identification. If crime-prone youth are more

[13] Oka (2009) provides background of this revision.

likely to move to urban regions where the growth of the kindergarten enrollment rate is high, the estimates may be biased upward. Yet another concern about biased estimates arises from the fact that kindergarten enrollment is an endogenous decision by parents. Furthermore, our econometric model may not be correctly specified and some relevant variables may have been omitted from the regression model. We discuss these concerns extensively in Section 7.2.

5 Data

We construct panel data from 46 Japanese provinces, excluding the province of Okinawa because data were unavailable from this province until the 1970s. Our main outcome variables include juvenile arrest and teenage pregnancy rates. We also take the high school enrollment rate, the center-based childcare enrollment rate, and the female employment rate as additional outcome variables that facilitate the interpretation of the main results. We report the summary statistics of the key variables in Table 2 and provide the remaining details of the data in Appendix B. We describe juvenile arrest and teen pregnancy rates below, as they are our main outcomes. Other variables are briefly explained as they are introduced in the analysis.

Juvenile arrests are of primary interest to us because they involve large social costs and the prevalence of criminal offenses tends to peak in the late teens (Anderson, 1999). Juvenile suspects in Japan are released, placed on probation, sent to a juvenile correction center, or sent to prosecutors to be considered for criminal charges after receiving investigations from police and domestic court. Under the Juvenile Act, individuals ages 13 and younger are protected from being processed in the juvenile justice system. Individuals ages 14-15 years can be processed in the juvenile justice system while being exempted from criminal charges to avoid depriving them of the right to receive compulsory education. Offenders ages 16-19 are subject to criminal charges.

Our data are taken from administrative records, allowing us to avoid desirability and/or recall biases seen in self-reported surveys. Indeed, Kling, Ludwig and Katz (2005) document that young people tend to underreport their involvement in antisocial activities such as criminal arrests. We define a juvenile arrest rate as arrests per 1,000 people ages 14-19 in a given year.

Violent offenses include assault, assembling with offensive weapons, arson, murder, robbery, rape, criminal intimidation, and extortion. The non-violent arrest rate includes all other arrests including those for traffic violations. The juvenile arrest rate for violent crimes was 2.183 per 1,000 individuals per year during the analysis period from 1955 to 1995. The juvenile non-violent arrest rate was much higher at 16.875 per 1,000 individuals. To place Japan in the international context at that time, we compare the juvenile arrest rate between countries. Japan's juvenile arrest rate in 1975[14] was 24.4 per 1000 men, which amounts to 60% of that in Norway, 38% of that in Canada, and 32% of that in the US (see Figure 1). Therefore, Japan had a relatively low juvenile arrest rate.

Another main outcome variable is the teenage pregnancy rate. Unlike juvenile arrests, teenage pregnancy is not necessarily undesirable, but is associated with many long-term economic and health disadvantages for both mothers and children, such as dropping out of high school, poverty, participation in crime, low birth weight, and a higher infant mortality rate. We define the teenage pregnancy rate as the total number of childbirths and induced abortions per 1,000 women ages 15-19 in each year. Numbers of births and induced abortions are taken from administrative records. Miscarriages and stillbirths are not considered in this paper because of a lack of data for early-stage miscarriages. Induced abortion is legal in Japan and designated medical doctors can perform abortion surgeries at their discretion. Doctors are required by law to report each abortion case to the local office of the Japan Association of Obstetricians and Gynecologists. Given these circumstances, we believe that unreported cases are rare. We observe induced abortions among women ages 15-19 between 1970 and 1995, but not for the period 1967-1969. For these three years, we impute induced abortions among women ages 15-19 by multiplying the overall induced abortions in each year during this period by the share of induced abortions among women ages 15-19 in 1970. The teenage pregnancy rate during the analysis period was 8.990.

We could not find statistics for teenage pregnancy that allow international comparison in the analysis period, but teenage birth rates are available. From 1970 to 1975, the birth rate per 1,000 women ages 15-19 was 61.4 in the US, 32.5 in Norway, and 37.2 in Canada. Given that these

[14]Note that 1974 was the first year when individuals directly affected by the kindergarten reform reached age 14 and could thus be arrested as juvenile offenders.

statistics can serve as lower bounds for teenage pregnancy rates, Japan's teenage pregnancy rate was much lower at 4.6 (see Figure 1).

Table 2: Summary statistics of selected variables

	Obs. (1)	Pre-reform average (2)	Overall average (3)
Juvenile risky behavior			
Juvenile violent arrest rate (per 1,000 individuals)	1334	2.743	2.183
		[1.427]	[1.182]
Juvenile non-violent arrest rate (per 1,000 individuals)	1334	14.000	16.875
		[4.466]	[4.565]
Teenage pregnancy rate (per 1,000 women)	1334	7.857	8.990
		[2.779]	[3.064]
Other outcomes			
Enrollment rate for center-based childcare (%)	1334	11.191	19.981
		[7.411]	[12.685]
Enrollment rate for kindergartens and center-based childcare (%)	1334	23.034	56.396
		[11.247]	[25.911]
Men's high school enrollment rate (%)	1334	82.320	91.210
		[8.181]	[6.159]
Women's high school enrollment rate (%)	1334	83.373	92.982
		[8.444]	[6.401]
Employment rate of women ages 25-44 (%)	414	52.786	54.507
		[12.976]	[10.264]
Outcomes for placebo tests			
Adult violent arrest rate (per 1,000 individuals)	1334	1.440	0.711
		[0.566]	[0.541]
Adult pregnancy rate (per 1,000 women)	1334	187.116	152.300
		[21.492]	[40.329]

Notes: Data for all variables, except for the female employment rate, are available for 46 provinces for 29 years ($46 \times 29 = 1334$). The employment rate is taken from 9 waves of quinquennial census ($46 \times 9 = 414$), and we focus on women ages 25-44. Numbers in square brackets are standard deviations. See Table B.1 for definitions and data sources of each variable.

6 Results

To assess the long-term consequences of the kindergarten reform, we focus on risky behavior, including violent and nonviolent crime rates and the teenage pregnancy rate. In addition to these primary outcomes, we also examine the high school enrollment rate, the center-based childcare enrollment rate, and the female employment rate as secondary outcomes because they shed light on the underlying mechanisms for why the reform affected the main behavioral outcomes.

6.1 Crime and Pregnancy Rates

This section provides evidence for the effects of the kindergarten reform on juvenile crime and teenage pregnancy rates. We begin by graphically comparing the time trends of outcomes in two groups of provinces; the first group includes strongly affected (i.e., treatment) provinces, and the second includes less affected (i.e., comparison) provinces. Through this exercise, we illustrate how we identify the causal effects of the reform in an intuitive and transparent manner. We then show the estimates from the event study model (see Equation 1) in which we control for the pre-reform characteristics of provinces.

6.1.1 Graphical Evidence

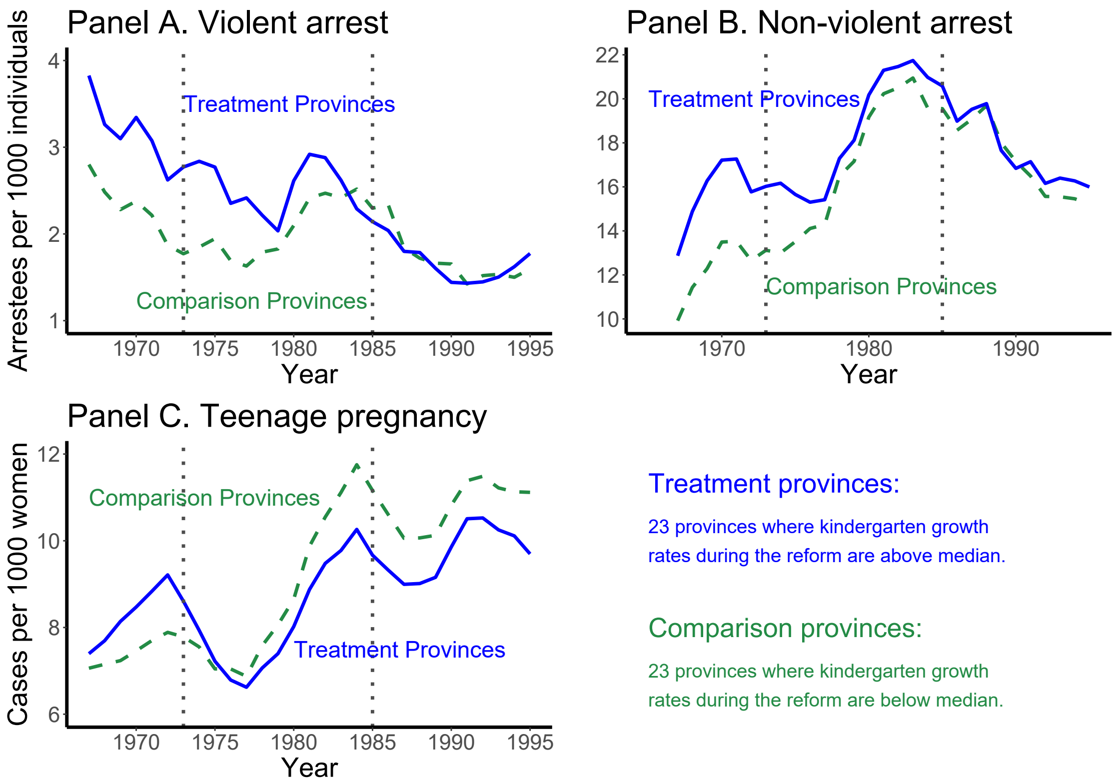
In Figure 5, we compare the trends of adolescent behavioral outcomes between two groups of provinces. One consists of provinces where the growth of the kindergarten enrollment rate during the reform was above the median. For brevity, we refer to these as treatment provinces. The other group consists of provinces with below-median growth, which are referred to as comparison provinces. Because a juvenile offender is a person between 14 and 19 years of age in Japan, we define the phase-in period according to when the treated cohorts fell in this age range. The first treated cohort reached 14 years of age in 1974, while the last treated cohort reached 19 years of age in 1985. Therefore, we define the phase-in period as beginning in 1974 and ending in 1985 (see Figure 4).

Panel A shows the trend in the juvenile arrest rate for violent crimes. In the pre-reform period, the rate was higher among the treatment provinces than among the comparison provinces. However, the gap narrowed during the phase-in period and disappeared in the post-reform period. We find similar patterns with non-violent crimes in Panel B. As shown in panel C, the rate of teenage pregnancy was higher in treatment provinces in the pre-reform period, but the gap reversed in the phase-in period and remained stable throughout the post-reform period.[15]

Our graphical evidence suggests that the kindergarten reform decreased juvenile arrests and

[15] One might wonder why the juvenile arrest rate temporarily rose during the late 1970s and early 1980s. The literature offers possible explanations including deteriorating labor market conditions (Tsushima, 1996; Ohtake and Okamura, 2000), cohort crowding (Ichikawa and Nakamura, 1988), and increased police activity (Yokoyama, 1989), but there appears to be no consensus on the mechanism.

Figure 5: Variations in juvenile arrest and teenage pregnancy rates



Notes: This figure displays variations in juvenile behavioral outcomes separately for treatment and comparison provinces. We define "treatment provinces" as provinces where the growth of the kindergarten enrollment rate during the reform exceeded the median among all provinces, while comparison provinces are those with below-median growth. The two vertical dashed lines demarcate the three periods defined in Figure 4: the pre-reform period (1967-1973), phase-in period (1974-1985), and post-reform period (1986-1995).

teenage pregnancy, but we do not control for any observed provincial characteristics prior to treatment here. We address this concern in the following section using the event study model.

6.1.2 The Event Study Model

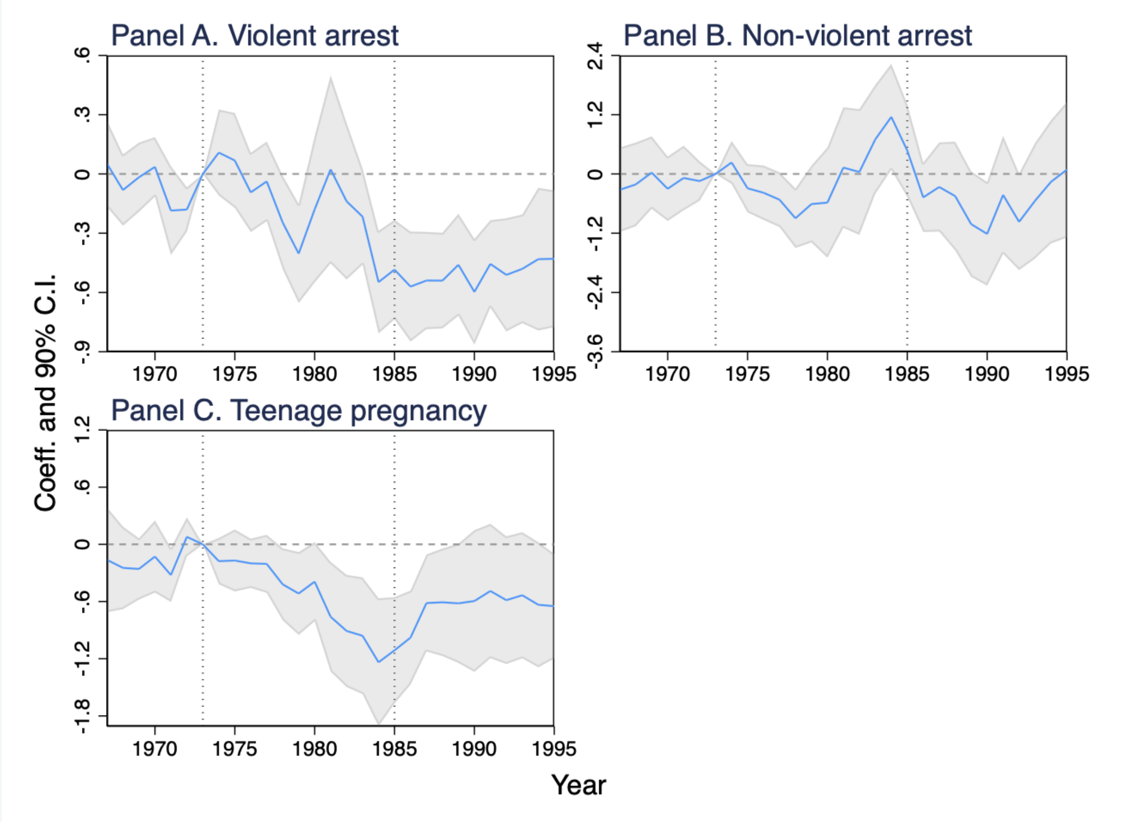
We estimate the causal effects of the kindergarten reform using the event study model (1) and present the estimates graphically in Figure 6 with standard errors clustered at the provincial level. Panel A shows the estimates for the juvenile violent arrest rate. In the pre-reform period (1967-1973), most of our estimates are close to zero and statistically insignificant, implying the absence of a pre-trend. In the phase-in period (1974-85), there is a downward trend[16]. Finally, throughout the post-reform period (1986-1995), the estimates are significantly negative, implying that the reform has reduced the juvenile violent arrest rate.

Panel B shows the estimates for the juvenile non-violent arrest rate. We find that the estimates are small and statistically insignificant in almost all years including the post-reform period. Although Figure 5 suggests that the reform may have reduced non-violent arrests, the results are not robust once we control for the pre-reform provincial characteristics. Panel C shows the estimates for the teenage pregnancy rate. We find no evidence for a pre-trend. In the phase-in period, the estimates exhibit a downward trend and are significantly negative in the last few years of the phase-in period and the first few years of the post-reform period. The estimates become insignificant later in the post-reform period.

Table 3 summarizes the estimation results by averaging the estimates over years during each of the pre-reform, phase-in, and post-reform periods. In the pre-reform period, the estimates are close to zero and insignificant for all outcomes, implying no evidence for a pre-trend. For the juvenile violent arrest rate, the effect of a 10-percentage-point increase in the kindergarten enrollment rate is -0.501 in the post-reform period and statistically significant at the 1% level. For the non-violent arrest rate, the estimated effect in the post-reform period is negative at -0.542, but its large standard error leads to an insignificant result. For the teenage pregnancy rate,

[16]One might wonder why the estimated effects on arrest rates temporarily rose in the early 1980s (see Panels A and B of Figure 6). This is largely driven by a temporary increase in juvenile arrest rates in provinces with a higher growth in the kindergarten enrollment rate (see also Figure 5 and the discussions in Section 6.1.1). Section 7 discusses how adding time-varying confounders to the baseline model (1) may affect our estimates.

Figure 6: Effects of the kindergarten reform on juvenile arrest and teenage pregnancy rates



Notes: This figure shows the estimated effects of the kindergarten reform. Specifically, we plot the estimates for $\hat{\alpha}_h$, the coefficients for the growth of the kindergarten enrollment rate during the reform, in the event study model (see Equation 1). The shades indicate the 90% confidence bands. The estimates indicate the effects of a 10-percentage-point increase in the kindergarten enrollment rate. Standard errors are clustered at the provincial level. The vertical dashed lines at 1973 and 1985 demarcate the estimates into the three periods indicated in Figure 4: the pre-reform period (1967-1973), phase-in period (1974-1985), and post-reform period (1986-1995).

the estimated effect in the phase-in period is -0.588 and statistically significant at the 1% level. The estimate in the post-reform period is slightly larger at -0.631 and statistically significant at the 10% level. Given that the reform increased kindergarten enrollment for the entire country by 21 percentage points, these estimates imply that the reform reduced the juvenile violent crime rate by 1.052 and the teenage pregnancy rate by 1.325, which are 38% and 17% reductions from the pre-reform averages, respectively.[17]

Our estimates are difficult to compare with those of previous studies because criminal

[17]The estimate for the violent crime rate is obtained by $-0.501 \times 21/10 = -1.052$. Because the pre-reform average is 2.743, the reduction rate is $-1.052/2.743 = -0.38$. Similarly, the estimate for teenage pregnancy is obtained by $-0.631 \times 21/10 = -1.325$, and the reduction rate is $-1.325/7.857 = -0.17$.

outcomes are measured differently between studies. In particular, our measurement, the juvenile violent crime rate, covers a shorter time period (six years from 14 to 19 years) and a more specific type of crime than other studies. Similarly, fertility outcomes are measured from different age ranges across studies, and therefore estimates are not directly comparable.

Table 3: Estimated effects of the kindergarten reform by period

	Pre-reform	Phase-in	Post-reform
	(1)	(2)	(3)
(A) Juvenile violent arrest	-0.063	-0.179	-0.501
	(0.073)	(0.137)	(0.150)
(B) Juvenile non-violent arrest	-0.172	-0.045	-0.542
	(0.321)	(0.441)	(0.586)
(C) Teenage pregnancy	-0.173	-0.588	-0.631
	(0.189)	(0.218)	(0.344)

Notes: This table presents the estimated effects of the kindergarten reform during the pre-reform period (1967-1973), phase-in period (1974-1985), and post-reform period (1986-1995). See Figure 4 for definitions of the three periods. To calculate the estimates, we estimate the coefficient α_h in the event study model (1) for each year, and then separately estimate its average over years during each of the three periods. The estimates indicate the effects of a 10-percentage-point increase in the kindergarten enrollment rate. Standard errors are in parentheses; they are clustered at the provincial level and obtained using the Delta method.

6.2 Secondary Outcomes

6.2.1 High School Enrollment

Next, we estimate the effects of the kindergarten reform on high-school enrollment rates for boys and girls separately. We study this outcome for two reasons. First, high school enrollment is an important educational outcome in itself because high school is not a part of compulsory education in Japan. Second, high school enrollment is a possible mediator of behavioral outcomes such as juvenile arrests and teenage pregnancy. Lochner (2011) points out the reasons additional schooling can reduce juvenile crime. First, additional schooling can lead to incapacitation effects, as opportunities for crime and pregnancy are typically more limited in schools than on the street. Second, additional schooling can promote skills among children, increasing the opportunity cost of engaging in risky behavior.

Table 4: Effects of the kindergarten reform on high school enrollment, ECEC enrollment, and female employment

	Pre-reform (1)	Phase-in (2)	Post-reform (3)
High school enrollment			
(A1) Men	-0.334	-0.042	-0.271
	(0.291)	(0.285)	(0.528)
(A2) Women	-0.249	-0.290	-0.502
	(0.260)	(0.329)	(0.489)
ECEC enrollment			
(B1) Center-based childcare	0.297	-0.458	-2.103
	(0.234)	(0.313)	(0.780)
(B2) Center-based childcare and kindergartens	0.151	4.423	9.234
	(0.407)	(0.571)	(0.846)
Female employment			
(C) Employment rate among women ages 25-44	0.047	-0.715	0.152
	(0.032)	(0.903)	(0.826)

Notes: This table presents the estimated effects of the kindergarten reform during the three periods: the pre-reform period, phase-in period, and post-reform period. The estimates indicate the effects of a 10-percentage-point increase in the kindergarten enrollment rate. We estimate the coefficient α_h in the event study model (1) for each year, and then its average over years during each of the three periods. We adjust the definitions of the three periods for each outcome as follows. The pre-reform period is defined for rows (A1)-(A2) as 1968-72, for (B1)-(B2) as 1974-81, and for (C) as 1955. The phase-in period is defined for rows (A1)-(A2) as 1974-85, for (B1)-(B2) as 1964-70, and for (C) as 1965 and 1970. The post-reform period is defined for rows (A1)-(A2) as 1982-96, for (B1)-(B2) as 1971-85, and for (C) as 1975-95. Standard errors are in parentheses; they are clustered at the provincial level and obtained using the Delta method.

Results are reported in rows (A1)-(A2) of Table 4. The estimated effects of a 10-percentage-point increase in the kindergarten enrollment rate caused by the reform are -0.271 percentage points for boys and -0.502 percentage points for girls and are statistically insignificant, suggesting that the kindergarten reform did not increase high school enrollment. This may be because there was little room for improvement as the high school enrollment rates were already high. These rates were 91% for boys and 93% for girls during the analysis period, as reported in Table 2. This null result suggests that increased schooling is unlikely to be the primary reason kindergarten reform reduced violent crime among juveniles and teen pregnancy.

6.2.2 Enrollment for Center-Based Childcare

We also examine how the kindergarten reform affected other types of childcare. This helps us interpret the estimated effects on juvenile outcomes because the effects on a juvenile outcome depend on both an observed outcome with the reform and a counterfactual outcome without the reform. In other words, estimating the effects of the reform on other childcare choices allows us to understand the counterfactual mode of care and subsequent counterfactual juvenile outcomes in the absence of the reform.

Specifically, we estimate the effects on the enrollment rate for center-based childcare other than kindergartens. As detailed in Section 3.1, these are distinct programs in the Japanese ECEC system and are likely to be imperfect substitutes. If the reform simply moved children from center-based childcare to kindergartens, the effects are from an intensive margin (i.e., from a full-day center-based childcare to a half-day kindergarten). If the reform had little effect on center-based childcare, the effects arise from an extensive margin (i.e., from informal care to kindergartens).

We find that a 10-percentage-point increase in the kindergarten enrollment rate during the reform decreased the enrollment rate of center-based childcare by 2 percentage points, while it increased the overall enrollment rate of ECEC (i.e., kindergartens and center-based childcare) by 9 percentage points in the post-reform period (see rows B1-B2 in Table 4). Note that during the analysis period, the enrollment rate for center-based childcare for 4-year-olds was 20% and that for overall ECEC (i.e., kindergartens and center-based childcare) was 56% as reported in Table 2.

These results imply that the kindergarten reform did not substantially crowd out enrollment in center-based childcare. We consider these results to be intuitive because, as described in Section 3.1, kindergartens and center-based childcare differ in how they serve families with preschool-aged children. These results also suggest that the observed effects on violent arrest and teenage pregnancy appear to be the consequences of a change at the extensive margin (from informal care to kindergartens) rather than at the intensive margin. Note that our results here are based on estimates averaged across all provinces and may not be applicable to all provinces individually. We further examine possible heterogeneity in substitution in Section 6.3.

6.2.3 Female Employment

Although kindergartens take children for only four hours a day, some women may choose to work part-time, which may eventually influence child development. For example, women working increases household income, which is likely to lead to better child outcomes. However, mothers working could also harm child development if stress and fatigue at work cause deterioration in mother-child relationships (Baker, Gruber and Milligan, 2008).

To address this issue, we estimate the effects of the kindergarten reform on the employment rate among women ages 25-44 using data from the quinquennial census from 1955 to 1995. The ideal outcome to examine would be the maternal employment rate, but it is not available for the analysis period. Furthermore, conditioning on being a mother may not be a valid statistical analysis because fertility may also be affected by the reform. The female employment rate was 54.507% during the analysis period (see Table 2). Also note that 63% of women ages 25-44 were married and had at least one child in 1960. A 10-percentage-point increase in the kindergarten enrollment rate is associated with a 0.15-percentage-point higher female employment rate in the post-reform period (see row C of Table 4). Although positive, the estimated effect is small and statistically insignificant. The null effect on the female labor supply is unsurprising because kindergartens only offer half-day services and therefore have a limited scope to affect parental time allocation.

6.3 Heterogeneity of the Treatment Effects

Finally, we examine the possible heterogeneity of the treatment effects of the kindergarten reform. Previous studies have shown that children in disadvantaged families tend to benefit more from accessing early childhood education and care (see, e.g., Havnes and Mogstad (2015), Kottelenberg and Lehrer (2017), Cornelissen et al. (2018), Felfe and Lalive (2018), and Yamaguchi, Asai and Kambayashi (2018*b*)). Unfortunately, we cannot examine heterogeneous effects in terms of the socioeconomic status of individual households because our data are aggregated at the provincial level. Instead, we group provinces in terms of the share of women with low education and examine treatment effects separately for each group. We consider the share of women with low (i.e., less than high school) education in 1960 as a proxy for the share of disadvantaged families within each province.

Table 5 reports how the estimated effects of treatment differ between the two groups of provinces with different levels of education of women. In this table, we refer to provinces with a below-median share of low-education women as high-education provinces and the rest as low-education provinces. The results of the estimations in the post-reform period suggest that the kindergarten reform led to reductions in violent juvenile arrests and teenage pregnancy only in high-education provinces (rows A1 and C1). On the other hand, corresponding post-reform estimates for low-education provinces are close to zero (rows A2 and C2). These results imply that the estimated effects in the baseline results (see Table 3) are largely driven by the high-education provinces.

One possible explanation for the null effect in low-education provinces is that the quality of the counterfactual care mode was similar to that of kindergartens. That is, while the high-education provinces include urbanized regions where more mothers were housewives in nuclear families, the low-education provinces mainly consist of rural regions where many mothers worked using center-based childcare. Specifically, the female employment rate in low-education provinces was higher than that in high-education provinces by about 14 percentage points in 1960. As pointed out by a municipal survey in Okada (1982), some kindergarten slots created by the reform may have crowded out center-based childcare in rural low-education provinces.

Our estimates in Table 6 do in fact indicate a substitution between kindergartens and center-based childcare in the low-education provinces. Among the low-education provinces, a 10-percentage-point increase in the growth of the kindergarten enrollment rate led to a 7.5-percentage-point decrease in center-based childcare enrollment (row A2). Consequently, the reform resulted in only a 4.8-percentage-point increase in the overall ECEC enrollment rate (row B2). On the other hand, counterfactual care in urbanized high-education provinces seems to be informal care provided by mothers because we do not observe a sizable crowding out of center-based child care in these provinces (row A1).

In summary, the estimated heterogeneity of the treatment effect may be explained by the fact that the main counterfactual form of care in the high-education provinces is parental care, while that in the low-education provinces includes a substantial portion of center-based childcare.

Table 5: Heterogeneity of treatment effects on adolescent outcomes

	Pre-reform	Phase-in	Post-reform
	(1)	(2)	(3)
Juvenile violent arrests			
(A1) High-education provinces	-0.057	-0.408	-0.661
	(0.125)	(0.113)	(0.193)
(A2) Low-education provinces	0.098	0.146	0.147
	(0.172)	(0.178)	(0.153)
Difference (A1-A2)	-0.155	-0.554	-0.809
	(0.210)	(0.209)	(0.244)
Juvenile non-violent arrests			
(B1) High-education provinces	0.085	-0.727	-0.523
	(0.663)	(0.413)	(0.733)
(B2) Low-education provinces	0.453	0.272	-0.586
	(0.503)	(0.668)	(0.703)
Difference (B1-B2)	-0.368	-1.000	0.063
	(0.823)	(0.777)	(1.005)
Teenage pregnancy			
(C1) High-education provinces	-0.307	-0.858	-1.387
	(0.192)	(0.344)	(0.597)
(C2) Low-education provinces	-0.285	-0.256	0.162
	(0.273)	(0.366)	(0.580)
Difference (C1-C2)	-0.023	-0.602	-1.549
	(0.331)	(0.497)	(0.823)

Notes: This table presents the averaged estimates ($\hat{\alpha}_h$, $h \in [1967, 1995]$) of the model (1) separately for two groups of provinces: high-education provinces and low-education provinces. We define high-education provinces as those with a below-median share of women with less than high school education in 1960, and the rest as low-education provinces. The sample size for both subgroups is 23. Estimates indicate the effects of a 10-percentage-point increase in the kindergarten enrollment rate, setting the year 1973 as a reference year ($h = 1973$). We average the estimates over the three periods described in Figure 4: the pre-reform period (1967-1972), phase-in period (1974-1985), and post-reform period (1986-1995). Standard errors are in parentheses; they are clustered at the provincial level and obtained using the Delta method.

Note also that average mothers in high-education provinces were still undereducated in the 1960s from the current perspective. Specifically, 67% of women ages 25-44 in high-education provinces and 79% of those in low-education provinces had less than high school education, according to the 1960 census. Hence, it is plausible that kindergartens provided better care than parental care to households in the high-education provinces during and after the reform

Table 6: Heterogeneity of treatment effects on center-based ECEC enrollment

	Pre-reform	Phase-in	Post-reform
	(1)	(2)	(3)
Center-based childcare			
(A1) High-education provinces	0.310	-0.514	-1.880
	(0.549)	(1.052)	(2.239)
(A2) Low-education provinces	0.329	-3.109	-7.486
	(0.345)	(0.700)	(1.377)
Difference (A1-A2)	-0.019	2.595	5.606
	(0.641)	(1.250)	(2.600)
Center-based childcare and Kindergartens			
(B1) High-education provinces	0.198	6.974	8.921
	(0.695)	(1.116)	(1.678)
(B2) Low-education provinces	-0.717	4.365	4.865
	(0.731)	(0.652)	(1.446)
Difference (B1-B2)	0.914	2.608	4.056
	(0.997)	(1.279)	(2.192)

Notes: This table presents the estimates ($\hat{\alpha}_h$, $h \in [1957, 1985]$) of the event study model (1) separately for two groups of provinces: high-education provinces and low-education provinces. We average the estimates over the following three periods: the pre-reform period (1957-1962), reform period (1964-1970), and post-reform period (1971-1985). High-education provinces are those with a below-median share of women with less than high school education in 1960, and the other provinces are referred to as low-education provinces. The sample size for both subgroups is 23. Estimates indicate the effects of a 10-percentage-point increase in the kindergarten enrollment rate. Standard errors are in parentheses; they are clustered at the provincial level and obtained using the Delta method.

period. Admittedly, we cannot exclude the possibility that other factors correlated with maternal education at the provincial level drive the results in this section.

7 Specification Checks

We perform a battery of specification checks for the preferred event study model. First, we conduct a set of placebo tests in which we take as outcomes the arrest and pregnancy rates of older cohorts that were not directly affected by the kindergarten reform. Second, we estimate treatment effects using alternative model specifications to address the following concerns: inter-provincial migration, endogeneity in kindergarten enrollment decisions, time-varying confounders, and the choice of control variables.

7.1 Placebo Tests

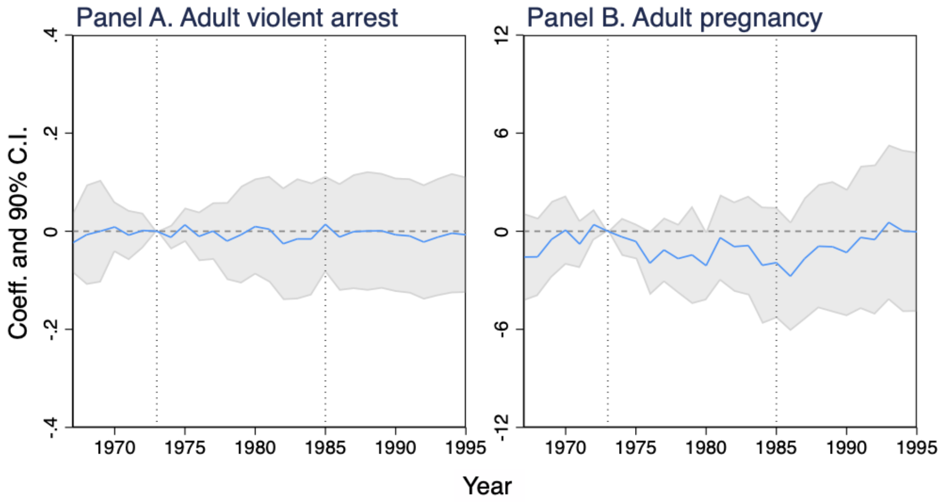
We do not find a significant difference in the pre-intervention trends of the outcome variables between provinces, but our estimates may still be biased if there exists a confounding factor that is correlated with the treatment variable and outcome trends in the phase-in and post-reform periods. An example of such a confounder is a labor demand shock. Previous studies have found that deteriorating labor market conditions are strongly associated with the crime rate (Chalfin and McCrary, 2017). If a labor demand shock is also correlated with the growth of the kindergarten enrollment rate during the reform, our estimates may reflect differential time trends in the outcomes caused by the labor demand shock, rather than a true policy effect. Such a confounder may also arise if alternative policy variations coincide with the kindergarten reform. Although we are not aware of any institutional changes that are likely to affect juvenile crime and / or teen pregnancy rates (see Section 4.3 for a discussion of this topic), there could be an unobserved provincial policy change that affects criminal and/or fertility behavior generally. If such a policy change is correlated with the treatment variable, the parallel trend assumption is violated.

To address these concerns, we perform placebo tests using outcomes from cohorts older than the reform cohort. The older cohorts are not supposed to be affected by the kindergarten reform as they had passed preschool age before the reform, but they are likely to be affected by the kind of unobserved labor demand shocks and policy changes mentioned above. Therefore, we may detect the presence of a confounder that biases our estimates if the reform is significantly associated with the placebo outcomes of older cohorts. In practice, we use the adult arrest rate for violent crime and adult pregnancy rate as placebo outcomes. Strictly speaking, the placebo outcomes during the post-reform period include the outcomes of the affected cohort, but these are negligibly small compared to the entire adult population. Even if this affects the placebo tests to some degree, it only increases the chance of a failing result. We admit that the placebo tests do not rule out the presence of a confounder that affects only teenagers, but passing the placebo tests would give us more confidence in our main results.

Figure 7 presents the results of the placebo tests. The average adult arrest rate for violent crimes was 0.710 per 1,000 adults and the adult pregnancy rate was 126 per 1,000 women (see

Table 2). For both the adult violent crime and pregnancy rates, we find insignificant effects throughout the period of analysis. The magnitude of the estimated effects during the post-reform period is less than 1% of the averages for both outcomes: about -0.007 for adult arrests and -0.796 for adult pregnancy. Null results in the falsification tests give us added confidence that our main results are driven by the kindergarten reform and not by unobserved confounders.

Figure 7: Placebo tests: estimated effects on older (unaffected) cohorts



Notes: This figure shows the estimated effects of the kindergarten reform on older cohorts that are not directly affected by it. Specifically, we plot the estimates for $\hat{\alpha}_h$, the coefficients for the growth of the kindergarten enrollment rate during the reform, in the event study model (see Equation (1)). The shades indicate the 90% confidence bands. Standard errors are clustered at the provincial level. The two vertical dashed lines at 1973 and 1985 demarcate the three periods described in Figure 4: the pre-reform period (1967-1973), phase-in period (1974-1985), and post-reform period (1986-1995).

7.2 Other Threats to Identification

This section addresses four remaining concerns about our identification strategy: (1) inter-provincial migration, (2) endogeneity in kindergarten enrollment decisions, (3) influence of time-varying confounders, and (4) choice of control variables. We discuss each of these in order and present all results at the end of this section.

Inter-Provincial Migration First, inter-provincial migration is a potential source of an endogeneity bias. Rapid economic growth during 1955-1973 in Japan attracted a substantial population from rural to urban regions (Fielding, 2017). If crime-prone youth moved disproportionately from rural to urban regions, the crime-reducing effects may be biased downward because the kindergarten enrollment rate increased faster in the metropolitan area during the kindergarten reform (see Table 1). Nevertheless, inter-provincial migration was relatively rare among teenagers and only 9% of junior high school graduates in 1970 moved to another province after finishing compulsory education at age 15. Although we suspect that inter-provincial migration does not affect our estimates substantially, we address possible biases by removing the largest destination provinces from our data and re-estimating the event study model (1) with the same set of covariates used in Section 6.1.2.[18] Specifically, we exclude observations for Tokyo and Osaka because they are the two most popular destinations for individuals moving out of their home province to take a job upon graduating junior high school. According to the 1970 School Basic Survey, 37% of job seekers moved to one of these two provinces.

Endogenous Kindergarten Enrollment Second, endogenous kindergarten enrollment decisions can bias our estimates. Parents who are enthusiastic about education are more likely to invest in their children not only by enrolling them in kindergartens, but also by spending their time and money. As a result, their children are less likely to be arrested in adolescence, even if kindergarten has no effect on the juvenile arrest and teenage pregnancy rates. In other words, unobserved parental characteristics, such as enthusiasm for education, can affect kindergarten enrollment and behavioral outcomes during adolescence. To address a potential endogeneity

[18] Kondo and Shigeoka (2013) also use panel data for Japanese provinces and adopt a similar approach to examining the potential influences of inter-provincial migrations on their estimates.

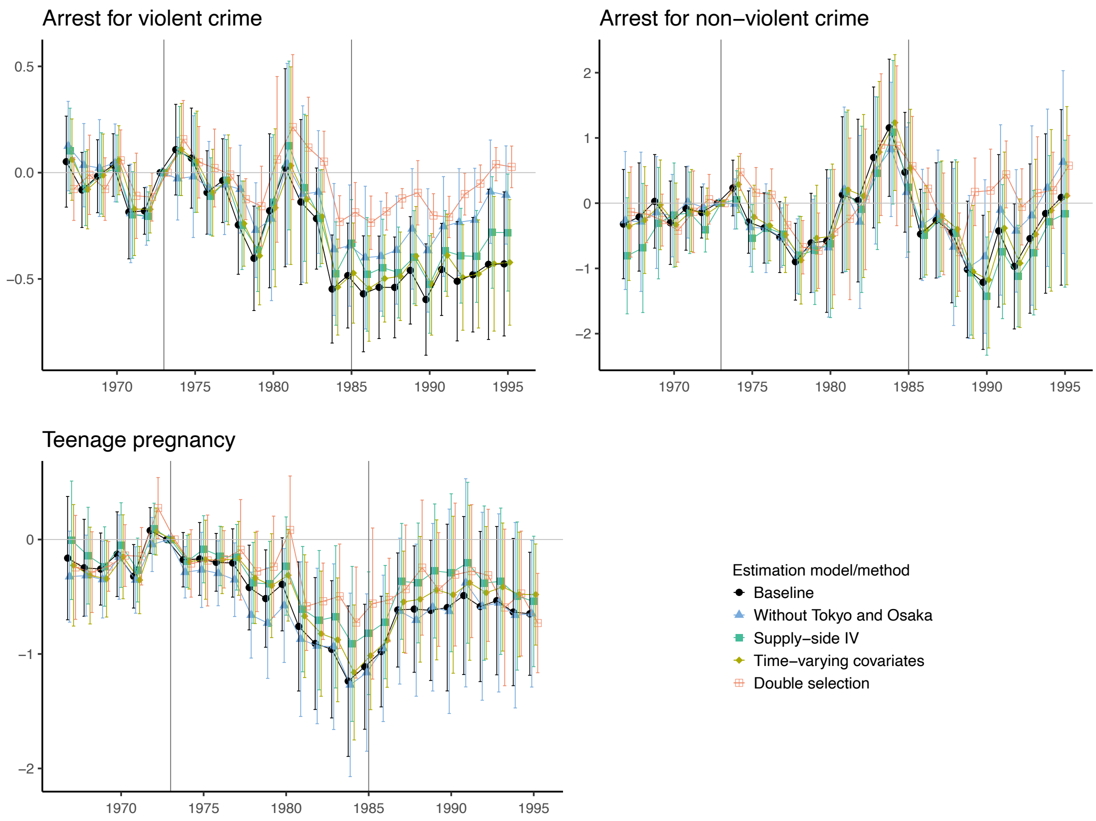
bias, we instrument the kindergarten enrollment rate by measures of the supply of kindergarten education, including the number of kindergartens, kindergarten teachers, kindergarten classrooms, and kindergarten slots per child in a province. These supply-side variables are likely to be uncorrelated with unobserved parental characteristics.

Time-Varying Confounders Third, time-varying confounding factors during and after the kindergarten reform may bias our estimates. In our preferred specification (1), we control for pre-reform provincial characteristics but exclude post-reform characteristics because the latter may be affected by the kindergarten reform. Nevertheless, one might be concerned about biases arising from not controlling for post-reform provincial characteristics correlated with both the treatment and outcome variables. To address this concern, we estimate an alternative model that includes a set of time-varying control variables in addition to the pre-reform provincial characteristics. These variables include lagged real provincial GDP per capita, real child welfare spending per capita, real fiscal-equalization grants per capita, and police officers per capita as proxies for local economic conditions, child welfare policies, and local police activities.

Alternative Set of Control Variables Fourth and finally, our model may fail to include relevant pre-reform control variables. To overcome this limitation, we substantially expand the set of pre-reform regional characteristics as control variables and apply the post double selection lasso proposed by Belloni, Chernozhukov and Hansen (2014). Specifically, we estimate the cross-sectional first-difference DID model $\Delta Y_{i\tau} = \alpha_\tau Z_i + \beta' X_i + \varepsilon_{i\tau}$ at each $t = \tau$, where $\Delta Y_{i\tau} = Y_{i\tau} - Y_{iT_0}$ and T_0 is a pre-reform reference year. In this model, the components of X_i are selected by the lasso-based procedure that selects the most relevant control variables to predict $\Delta Y_{i\tau}$ and Z_i at each $t = \tau$ from a set of high-dimensional covariates. α_τ is the parameter of interest. For the candidates for control variables X_i, we use 56 pre-reform covariates (i.e., levels and differences of 28 covariates around $t = T_0$) and their quadratic terms, resulting in 112 covariates. Appendix B lists these covariates.

Estimation Results Figure 8 presents the results of the robustness checks. The estimates labeled "without Tokyo and Osaka", "supply-side IV", " time-varying covariates" and "double selection" are based on the model specifications that address the above four concerns. For comparison, we also show estimates from the preferred baseline specification. We find that the primary findings of the baseline analysis are robust to the alternative model specifications and estimation methods. For all the outcomes, pre-reform estimates do not differ significantly from zero, suggesting that our results are not driven by pre-existing differential trends. For the phase-in and post-reform periods, the estimated effects on the arrest rate for violent crimes and the teenage pregnancy rate are largely negative and statistically significant, whereas those on the non-violent arrest rate are insignificant. In general, the results of the alternative specifications are in line with the baseline results in Section 6.1.2.

Figure 8: Estimated effects of the reform under alternative specifications



Notes: This figure displays the estimates with 90% confidence intervals regarding the effects of the kindergarten reform under alternative specifications described in Section 7.2. Except for the double selection estimates which are obtained by the cross-sectional first-difference model, standard errors are clustered at the provincial level. Two vertical lines at 1973 and 1985 correspond to the three periods described in Figure 4: the pre-reform period (1967-1973), phase-in period (1974-1985), and post-reform period (1986-1995). The first-stage F-statistic for the kindergarten-supply IV estimation is 40.790, based on a cross-sectional regression of the treatment variable on the instruments and baseline covariates.

8 Discussion

We show that the kindergarten reform decreased the juvenile arrest rate for violent crimes and the teenage pregnancy rate, but why did it have these effects? Our result in Section 6.2.1 does not support an explanation based on the incapacitation effects of additional schooling because the reform did not increase the high school enrollment rate. This finding is largely consistent with previous evidence from targeted programs. Heckman et al. (2010) find that the PPP did not increase the male high school graduation rate but nevertheless decreased the male crime rate. Carneiro and Ginja (2014) report similar results for Head Start.

A more plausible explanation for why early childhood education reduces adolescent risky behavior is improvement in children's non-cognitive skills. In criminology and psychology, it is well documented that childhood externalizing behavior predicts later antisocial behavior and arrests for violent crimes (Farrington, 1989; Moffitt, 1993). Heckman, Pinto and Savelyev (2013) show that the PPP alleviates externalizing behavior among children and that these effects account for most of the crime-reducing benefits of this program. Cultivating noncognitive skills is also likely to reduce teenage pregnancy, according to developmental psychologists. Arnold and Coyne (2020) argue that early childhood education decreases behavioral problems in schools and increases future aspirations, which potentially reduces the associated risk of teenage pregnancy.

The kindergarten curriculum in Japan is comparable with the HighScope curriculum, as both are based on a child-centered rather than teacher-centered approach. Schweinhart and Weikart (1997) find that programs based on a child-centered approach lead to fewer crimes at age 23 than a program based on a teacher-centered approach in their randomized study in Ypsilanti, Michigan. A child-centered approach is generally expected to foster non-cognitive skills rather than academic skills. Although we are unable to evaluate the effects of the kindergarten reform on children's behavior and non-cognitive skills because of a lack of data, there is evidence from Japan that early childhood education reduces children's externalizing behavior. Yamaguchi, Asai and Kambayashi (2018*b*) show that center-based childcare in Japan reduces symptoms of attention deficit hyperactivity disorder (ADHD) and aggressive behavior among children of mothers with less than high school education. Given that about 72% of young women 25-

44 years of age had less than high school education before the kindergarten reform, children affected by the kindergarten reform were likely to have benefited from better quality care.

Although in the opposite direction, studies of Quebec's universal childcare program also demonstrate that early childhood education affects the crime rate in adulthood through the mediation of behavior and non-cognitive skills in early childhood. Baker, Gruber and Milligan (2008) find that the Quebec childcare reform increased the maternal labor supply, parental stress, and low-quality parenting styles, causing deficits in non-cognitive outcomes among children. Baker, Gruber and Milligan (2019) show that negative effects on non-cognitive outcomes persisted until adolescence and that cohorts with greater access to childcare had a higher crime rate later in life.

In sum, we speculate that the kindergarten reform in Japan improved children's non-cognitive skills and reduced externalizing behavior, which eventually resulted in reducing the juvenile arrest and teenage pregnancy rate. Improved non-cognitive skills may have raised people's earnings, and hence the opportunity cost of engaging in risky behavior. Our results do not support the view that the incapacitation / incarceration effect of additional schooling plays a significant role in accounting for the effects of the kindergarten reform on criminal and fertility behavior in adolescence.

9 Conclusion

Social benefits associated with reducing risky behavior in adolescence can justify large public expenditures on early childhood education. Evidence from a small number of targeted interventions for disadvantaged children has demonstrated their effectiveness, but little is known about the efficacy of large-scale universal early childhood education programs, particularly outside of North America. We contribute to the literature by providing new evidence from Japan, where the institutions significantly differ from those in North America.

Our estimates show that the kindergarten reform in Japan has significantly reduced violent arrests and pregnancy among teenagers. Given that Japan had low arrest and teenage pregnancy rates, our findings demonstrate the potential of a universal early childhood program outside of

North America to produce long-lasting benefits for children and society at large.

An important limitation of our study is a lack of non-cognitive skill measurement. Our result shows that the kindergarten reform did not affect the high-school enrollment rate, which implies that the hypothesis of an "incarceration" effect can be rejected. We argue that improved non-cognitive skills are a likely channel through which early childhood education prevents adolescent risky behavior, but we do not observe children’s non-cognitive skills.

Another limitation is that we do not have individual-level data. As our provincial panel data provide administrative records for crime and pregnancy, they are likely to be precise, and we have very little concern about under-reporting. However, our aggregate data prevented us from implementing a detailed subgroup analysis at the individual level to uncover the heterogeneity of the treatment effect. The question of how the effects on risky teenage behavior vary with gender, family background, and other individual characteristics remains. We leave these important tasks to future research.

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A Kindergarten Curriculum Standards and Regulations

Kindergarten regulations impose strict quality standards for education in kindergartens in Japan. Kindergartens are part of the formal education system. The School Education Act (1947) stipulates the duties of kindergarten teachers, as well as the roles of kindergartens as a formal provider of pre-primary education to children over the age of 3. The Ministry of Education gradually strengthened the regulation of kindergartens after WWII by introducing national curriculum standards in 1948, imposing license requirements on kindergarten teachers in 1949, and specifying classroom regulations in 1952. In the following, we describe the national curriculum standards for kindergartens that play a central role among those regulations.

Ever since the post-WWII educational reforms during the American occupation of Japan (1945-52), national kindergarten curriculum standards have emphasized the importance of children's hands-on experiences and learning rather than teacher-centered instruction. In particular, the Ministry of Education developed new preschool curriculum guidelines in 1947 with the advice of Helen Heffernan as mentioned in Section 3.1. These guidelines were the foundation of the National Curriculum Standards for Kindergartens, which has been implemented since 1956 with minor revisions. Both the original guidelines and the national curriculum standards underscore the importance of centering kindergarten activities around children's interests and needs – physical, cognitive, psychological, and social – to promote their health and personality skills. The national curriculum standards require 4 hours of instruction and care per day on every weekday for at least 39 weeks per year. All kindergartens in Japan, regardless of whether they are public or private, must adhere to these standards.

Understanding the relative merits of various curriculum models may be relevant for interpreting the results of early childhood interventions. While there are various curriculum approaches for early childhood education, they can be broadly classified into two categories, teacher-centered approaches and child-centered approaches, depending on the degree of initiative expected of children. In teacher-centered approaches, teachers play a primary role in organizing small groups of children to teach subjects with question-and-answer lessons in a well-planned sequence. Learning priorities in these approaches are often given to academic skills that can be assessed by intelligence and achievement tests. In child-centered approaches, children have choices of learning activities based on their interests and needs, while teachers facilitate key experiences in children's development such as social relations, literacy, and creative representation. Child-centered approaches emphasize the development of social skills rather than academic skills.

The following five development goals encapsulate the educational content of the kindergarten curriculum: (1) physical development and health, (2) social relations, (3) environment and nature, (4) language and literacy, and (5) creative representations. The first national curriculum published in 1956 had six developmental goals, defining music and drawing as two separate goals. A revision of the national curriculum standards in 1988 integrated them into a single goal as creative representation. All of these goals are key components of the HighScope curriculum and the Head Start framework.

Kindergartens are also responsible for providing basic medical services to children in kindergartens. The School Health Law requires that a kindergarten employ a school physician. School physicians typically work part-time, and their main duties are to supervise school-based medical checks and to provide various health services to children and teachers. School physicians also provide first-aid treatment to students at the request of school heads.

B Details on Data

Table B.1: Definitions and data sources: main variables

Variable name	Definition	Source
Main outcomes		
Juvenile violent arrest rate	Arrests per 1000 population ages 14-19 for violent crimes	(A), (C)
Juvenile non-violent arrest rate	Arrests per 1000 population ages 14-19 for non-violent crimes	(A), (C)
Teenage pregnancy rate	Sum of abortion cases and childbirths per 1000 women ages 15-19	(A), (D)
Additional outcomes		
Center-base childcare enrollment rate at age 4	Enrollment rate in center-based childcare at age 4 divided by cohort size	(A), (B)
Overall center-based ECEC enrollment rate at age 4	Sum of kindergarten enrollment and childcare enrollment at age 4 divided by cohort size	(A), (B)
Male high school enrollment rate	Proportion of men enrolled in high schools at age 15 among those attended Grade 9	(A)
Female high school enrollment rate	Proportion of women enrolled in high schools at age 15 among those attended Grade 9	(A)
Female employment rate	Proportion of women ages 25-44 who are employed	(F)
Adult violent arrest rate	Arrests per 1000 adults for violent crimes	(A), (C)
Adult pregnancy rate	Sum of abortion cases and childbirths per 1000 women ages 20-29	(A), (D)
Treatment variables		
Variation in kindergarten enrollment during the reform	Variation in kindergarten enrollment rate at age 4 during 1963-1971	(A)
Variation in overall center-based childcare enrollment during the reform	Variation in enrollment rates in kindergartens and nurseries at age 4 during 1963-1971	(A), (B)
Kindergarten enrollment among individuals ages 14-19	Kindergarten enrollment rate at age 4 among individuals ages 14-19	(A)
Instrumental variables		
Variation in kindergartens during the reform	Variation in kindergartens per children ages 3-5 during 1963-1971	(A)
Variation in kindergarten slots during the reform	Variations in kindergarten teachers per children ages 3-5 during 1963-1971	(A)
Variation in kindergartens teachers during the reform	Variation in kindergarten slots per children ages 3-5 during 1963-1971	(A)
Variation in kindergartens classrooms during the reform	Variation in kindergarten classrooms per children ages 3-5 during 1963-1971	(A)

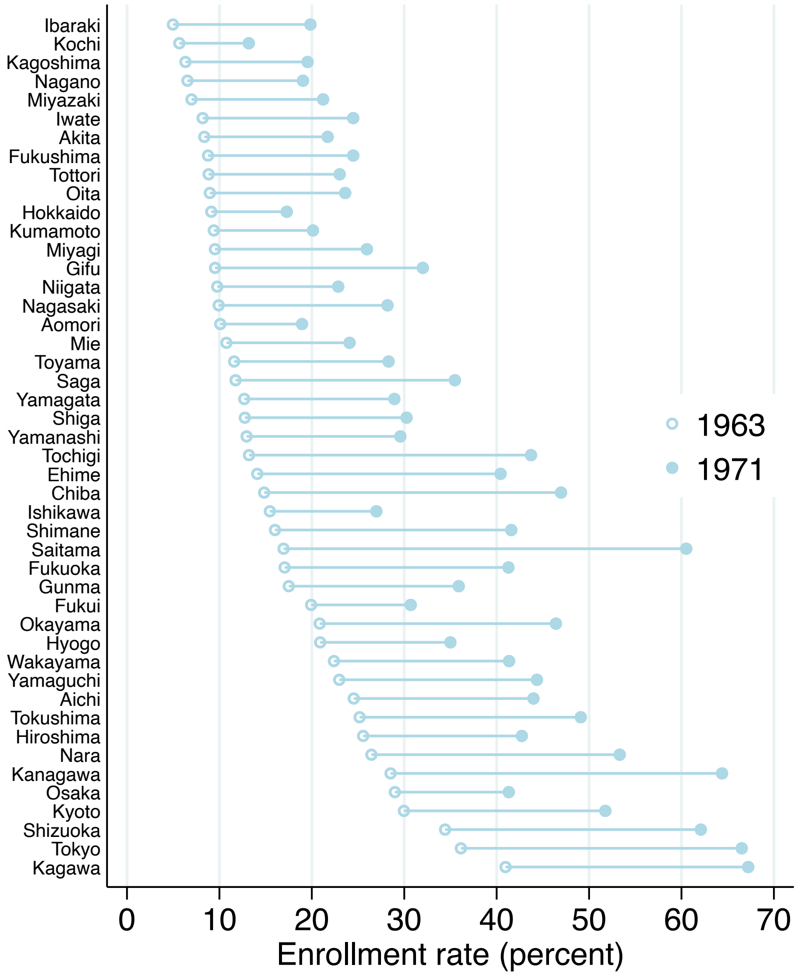
Sources: (A) School Basic Survey, (B) Survey of Social Welfare Institutions, (C) Crime Statistics, (D) Vital Statistics

Table B.2: Definitions and data sources: control variables

Variable name	Definition	Source
Baseline controls		
Kindergarten enrollment rate	Proportion of children enrolled in kindergarten at age 4	(A)
Real provincial GDP per capita	Real Gross Provincial Product per capita (in 1980 JPY)	(H)
Fiscal-equalization grants per capita	Real fiscal-equalization grants allocated to provinces divided by the provincial population	(H),(I)
Share of low-educated women	Proportion of women ages 25-44 without a high school degree	(F)
Female employment rate	Proportion of employed among the population of women ages 25-44	(F)
Child population ratio	Proportion of children in provincial population	(E)
Additional controls		
Population	Provincial population	(F)
Population density	Number of individuals per square kilometers	(E)
Monthly earnings	Average real monthly salary among workers in establishments with more than 30 employees	(J)
Male employment rate	Proportion of employed individuals among the population of men	(F)
Unemployment rate	Number of unemployed individuals among the labor force	(F)
Share of workers in secondary industries	Proportion of workers in secondary industries among the population of workers	(F)
Share of workers in third industries	Proportion of workers in third industries among the population of workers	(F)
Share of high school educated women	Proportion of women with a high school degree	(F)
Childcare enrollment rate	Enrollment rate in nurseries at age 4	(A), (B)
Kindergartens per capita	Number of kindergartens per child ages 3-5	(A)
Kindergarten teachers per capita	Number of kindergarten teachers per child ages 3-5	(A)
Elementary school teachers per student	Number of elementary school teachers per elementary school student	(A)
Junior high school teachers per student	Number of junior high school teachers per junior high school student	(A)
Provincial spending on child welfare	Per capita real provincial expenditure on child welfare services	(H), (I)
Provincial spending on education	Per capita real provincial expenditure on education	(H), (I)
Provincial spending on welfare	Per capita real provincial expenditure on welfare services	(H), (I)
Provincial spending on civil construction	Per capita real provincial expenditure on civil construction	(H), (I)
Provincial spending on public health	Per capita real provincial expenditure on public health services	(H), (I)
Provincial spending on kindergartens	Per capita real provincial expenditure on kindergartens	(H), (I)
Provincial spending on elementary schools	Per capita real provincial expenditure on elementary schools	(H), (I)
Provincial spending on junior high schools	Per capita real provincial expenditure on junior high schools	(H), (I)
Total provincial spending	Per capita real provincial expenditure	(H), (I)
Time-varying controls		
Lagged real provincial GDP per capita	Lagged real Gross Provincial Product per capita (in 1980 JPY)	(H)
Fiscal-equalization grants per capita	Real fiscal-equalization grants allocated to provinces divided by the provincial population	(H),(I)
Provincial spending on child welfare	Per capita real provincial expenditure on child welfare services	(H), (I)
Police officers per capita	Number of police officers divided by provincial population	(C)

Sources: (A) School Basic Survey, (B) Survey of Social Welfare Institutions, (C) Crime Statistics, (D) Vital Statistics, (E) Population Estimates, (F) Census, (H) Province SNA (in 68SNA format), (I) Annual Local Public Finance Statistics, (J) Monthly Labor Survey

Figure B.1: Kindergarten enrollment rate before and after the reform



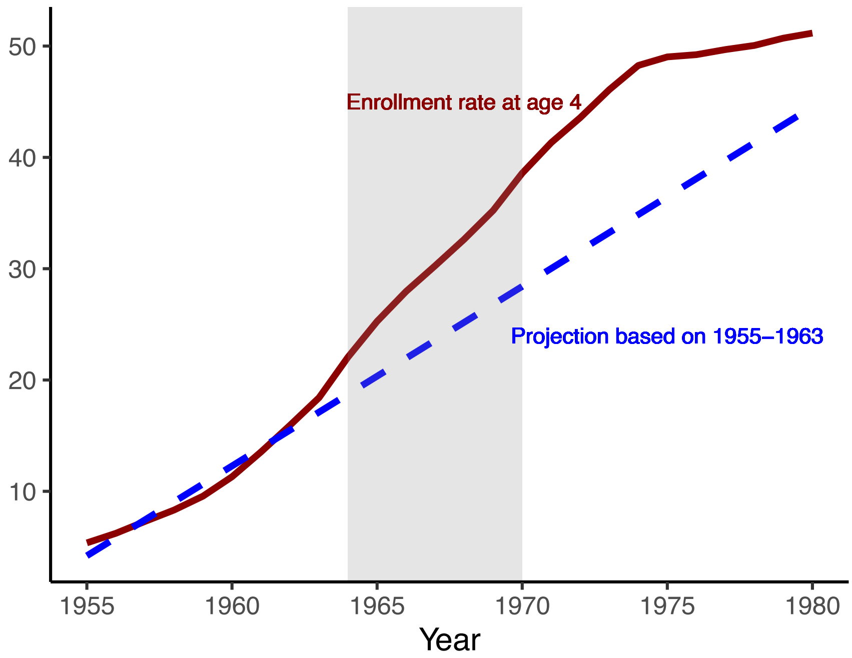
Notes: This figure displays provincial kindergarten enrollment rates in 1963 and 1971. Provinces are sorted by the enrollment rate in 1963.

C Additional Results

C.1 Estimation without Covariates

The empirical models presented in Section 4.2 include a vector of covariates X_i that consists of the pre-reform characteristics of provinces in Table 1. To the extent that these covariates capture pre-existing differences across provinces, we expect them to reduce potential noise and biases in our baseline results presented in Section 6.1.2. To understand how the covariates affect out baseline results, we show estimates without the covariates and compare them with our baseline

Figure B.2: Kindergarten enrollment rate at age 4, 1955-1980



Notes: This figure shows variations in the kindergarten enrollment rate at age 4, together with a projection line obtained from a simple regression using the pre-reform data for 1955-63. The gray-colored area corresponds to years during the kindergarten reform (1964-70).

results.

Table C.1 reports estimates based on the model (1) with and without the covariates. Estimates without covariates exhibit greater effects on juvenile violent arrests and teenage pregnancy. At the same time, we obtain substantially smaller standard errors when controlling the covariates. These results suggest that the pre-reform covariates in the baseline model play an important role in reducing noises and biases in our estimation.

Table C.1: Estimates of juvenile arrests and teenage pregnancy with and without covariates

	Pre-reform (1)	Phase-in (2)	Post-reform (3)
Juvenile violent arrest			
(A1) Baseline	-0.063	-0.179	-0.501
	(0.073)	(0.137)	(0.150)
(A2) Without covariates	-0.050	-0.226	-0.586
	(0.057)	(0.135)	(0.308)
Juvenile non-violent arrest			
(B1) Baseline	-0.172	-0.045	-0.542
	(0.321)	(0.441)	(0.586)
(B2) Without covariates	0.236	-0.222	-0.477
	(0.313)	(0.365)	(0.415)
Teenage pregnancy			
(C1) Baseline	-0.173	-0.588	-0.631
	(0.189)	(0.218)	(0.344)
(C2) Without covariates	0.116	-0.936	-1.169
	(0.209)	(0.307)	(0.424)

Notes: This table presents the estimated effects of the kindergarten reform obtained from the event study model (1) and the model without covariates. Specifically, we estimate the coefficient α_h in the event study model (1) for each year, and then take the average over the years comprising each of the three periods described in Figure 4: the pre-reform period (1967-1973), phase-in period (1974-1985), and post-reform period (1986-1995). We repeat the same procedure for a model without covariates. Standard errors are in parentheses; they are clustered at the provincial level and obtained by the Delta method.

C.2 Fixed-Effect Regression

As a robustness check, we also estimate a more conventional fixed effects regression model:

$$Y_{it} = \gamma Enroll_{it} + \delta' X_{it} + \pi_i + \mu_t + \varepsilon_{it}. \tag{C.1}$$

Here, the treatment variable $Enroll_{it}$ is the kindergarten enrollment rate in province i in year t, and hence is time-varying. The rest of the parameters are defined similarly to the event study model (1) while the parameter (γ) is time-invariant in the fixed effects model (C.1).

Unlike the event study model (1), the fixed effect model (C.1) restricts the causal effects to being time-invariant. The recent econometric literature points out that this restriction can lead to biased estimates. In addition, the fixed effect model does not allow us to test for the existence of a pre-trend.

Table C.2 reports the estimation results for a conventional fixed-effects regression model (C.1). Estimates show that both juvenile violent arrests and teenage pregnancy are negatively associated with the kindergarten enrollment rate, but non-violent arrests are not significantly correlated with the enrollment rate, conditional on the fixed effects and covariates. These results align well with the findings from the baseline model shown in Figure 6 and Table 3. This estimate implies that a 10-percentage-point increase in the kindergarten enrollment rate reduces the juvenile arrest rate for violent crimes by 0.280 individuals per thousand ages 14-19 and the teenage pregnancy rate by 0.491 cases per 1000 women ages 15-19.

Table C.2: Results of two-way fixed effects regressions

	Fixed-effects regression
	(1)
(A) Juvenile violent arrest	-0.280
	(0.099)
(B) Juvenile non-violent arrest	0.376
	(0.350)
(C) Teenage pregnancy	-0.491
	(0.264)

Notes: This table presents the estimates, $\hat{\gamma}$, of the two-way fixed effects model (C.1). These estimates indicate the effects of a 10-percentage-point increase in the kindergarten enrollment rate at age 4. Standard errors in parentheses are clustered at the provincial level.