Journal of Population Economics (2023) 36:2725–2797
https://doi.org/10.1007/s00148-022-00899-w

ORIGINAL PAPER

Early childhood education and care: effects after half a century and their mechanisms

Hikaru Kawarazaki[1,2]

Received: 1 April 2020 / Accepted: 8 April 2022 / Published online: 9 June 2022

Abstract
The effects of early childhood education and care (ECEC) have been widely researched, but most studies focus on targeted or relatively short-term programmes. This paper investigates the long-term effects of a universal ECEC programme and underlying mechanisms. By exploiting differences in expansion rates of childcare institutions across Japan from the 1960s to the 1980s, I find a positive effect of ECEC on income at up to age 50. The overall effect is driven by a significant impact among women, who were disadvantaged at that time, while there are no adverse effects on others. Mediation analysis shows that an increase in wages leads to an increase in income, which is triggered by improved educational attainment and not an increase in labour supply. The results imply that a universal childcare system has the potential to reduce income inequality.

Keywords Early childhood education and care · Inequality · Preschool · Mediation analysis · Return to education

JEL Classification I24 · I26 · I38

1 Introduction

Early childhood education and care (ECEC) is widely considered an important channel to help children reach their full potential; however, a consensus is yet to emerge on whether all children benefit and the causal mechanism if any. As the period from

Responsible editor: Shuaizhang Feng

This paper was previously circulated under the title "The Hidden Mechanism: Revealing the Effects of Early Childhood Education and Care After Half a Century."

✉ Hikaru Kawarazaki
hikaru.kawarazaki.20@ucl.ac.uk

1 Department of Economics, University College London, Drayton House, 30 Gordon St, WC1H 0AX London, UK

2 Institute for Fiscal Studies, 7 Ridgmount St, London WC1E 7AE, UK

birth to the age of five is the most crucial time for children's overall development, or "crucial period," ECEC attracts worldwide attention from policymakers. Following the 1965 introduction of the Head Start programme in the USA, many Organisation for Economic Co-operation and Development (OECD) and middle-income countries have established or expanded their ECEC programmes. The United Nations has also included universal ECEC among its Sustainable Development Goals (SDGs) (United Nations 2015).[1] In Japan, early childcare was improved as a pillar of postwar reconstruction and economic development, and its continued importance is reflected by the government's decision to introduce free ECEC in 2020.[2]

Besides the political realm, ECEC has long been of interest to researchers in several academic fields (Phillips and Shonkoff 2000; Knudsen et al. 2006; Barnett 2011; Currie and Almond 2011; Campbell et al. 2014). Economics studies focus on relatively short-run effects or effects of targeted programmes (e.g., Garces et al. (2002)).[3] Other recent studies discuss the effects of universal ECEC programmes using a difference-in-differences (DID) approach (Havnes and Mogstad 2011b; Herbst 2017). Some investigate which aspects of children's ability developed during an ECEC programme affect later outcomes (Heckman et al. 2013). In Japan, high-quality ECEC facilities have attracted many scholars' attention (Matsushima 2015; Yamaguchi et al. 2018b).

Despite numerous ECEC studies, little is known about universal ECEC's (local) average treatment effects in the long run or the observable paths through which such programmes affect children. These issues are important for three reasons: first, DID estimates the effects on all children, but cannot separate the effects of those who change their behaviours based on the treatment from overall effects.[4] In addition to what we can learn about overall benefits from the DID approach, it is important to consider who is a potential beneficiary and how large the effect is on them when formulating policy. Second, if the aim is to identify the optimal ECEC design for maximum social benefit, it is necessary to understand the effects of a universal programme rather than a targeted programme. The latter excludes many potentially eligible people, and the effects might differ between the disadvantaged and advantaged. Finally, if we formulate a new policy, which may affect a later life stage, we need to consider long-term effects and their mechanisms. In Japan, high-quality ECEC facilities have existed since World War II, which are suitable for analysing long-term effects, but few analyses have exploited this long history because of a lack of data.

These concerns motivate the present analysis of ECEC's long-term local average treatment effects (LATE) on children's future outcomes, such as income and

[1] See Myers (1995); Berlinski et al. (2009); OECD (2016) as well as conclusions of the Barcelona European Council in 2002 (http://www.bollettinoadapt.it/old/files/document/12563Barcelona_summit.pdf) and plan of the former U.S. President Obama (https://www.acf.hhs.gov/sites/default/files/occ/fact_sheet_president_obama_508.pdf. Last access: 1st April 2022).

[2] Details are discussed in Council for Designing 100-Year Life Society (2018).

[3] The growing literature includes Berlinski et al. (2009); Heckman (2013); Heckman et al. (2013); Felfe et al. (2015); Kottelenberg and Lehrer (2017); Yamaguchi et al. (2018b).

[4] The effects estimated with the DID approach are often different from those with the IV approach because they focus on different groups, but both are of interest.

education attainment. Besides these socioeconomic outcomes, this study analyses the effects on individuals' psychological outcomes, such as risk preference and the "Big Five" indicators, as non-cognitive abilities may affect socioeconomic outcomes (Heckman and Masterov 2007; Heckman et al. 2013; Havnes and Mogstad 2015).[5]

One challenge in analysing ECEC's effects is the existence of a potential endogeneity problem, as a parent's choice to enrol a child in ECEC is likely to be correlated with unobservable household characteristics. Therefore, we need to exploit an exogenous shock to reveal the causal effects of enrolling in the childcare system. To overcome this problem, this study uses the universal ECEC expansion in Japan from the 1960s to the 1980s as a quasi-random shock. During this post-war recovery period of rapid economic growth, a larger labour force was needed to support growth and the government and companies urged women to work (Matsushima 2015). To support women's labour participation, the government opened more ECEC institutions, such as kindergartens and nursery schools. ECEC institutions were opened to mitigate regional imbalances in the number of facilities. Therefore, jointly controlling for area, cohort fixed effects, and their interactions, the intensity of the expansion can be seen as quasi-random.[6,7,8] Another challenge is related to data quality. To investigate the long-term (local) average treatment effect of enrolling in ECEC, we need data on whether children went to ECEC facilities and their outcomes in adulthood. Information about both is rarely available for the same person. However, this study uses unique Japanese survey data containing both types of information. Using this data set and quasi-random variation, this study estimates ECEC's long-term local average treatment effects.

The results show that enrolling in a universal childcare system increases the probability of college completion and leads to higher future wages and annual income. However, it does not increase working status nor working hours, which indicates that income increases because of a rise in wages. Further subsample analyses suggest that the effects of universal ECEC are concentrated among females. This

[5] The Big Five is a set of five psychological indexes created by Goldberg (1990, 1992) based on the language vocabulary theory and models introduced by Allport and Odbert (1936); it measures extraversion, agreeableness, conscientiousness, emotional stability, and openness to experience. See Appendix A for the Big Five questionnaire.

[6] This identification strategy is similar to Herbst (2017). This study considers the expansion of childcare after World War II. The government urged mothers to send their children to childcare facilities, so they could work to support the war. A similar strategy is adopted by Blanden et al. (2016), which exploits the expansion of preschools in the 1990s in the UK. The excess demand drives this expansion: women wanted to return to work. The government also tried to promote gender equality. Duflo (2001) adopts a similar strategy. She uses the variation in timing on the school construction, which is based on the proportion to the number of children of primary school age not enrolled in school. Cornelissen et al. (2018) use a similar quasi-random shock. They use the expansion in publicly provided child care in Germany. They show that the expansion was driven by the initial coverage rate but not by other seemingly important variables, such as the fraction of female labour supply. In this paper, I conduct a similar analysis and confirm that the expansion was based on initial coverage, not on other factors. See Table 5 for details.

[7] The rate of expansion differed by region over time. Moreover, supply-side constraints were generally binding. These are important for identification.

[8] This is commonly used in DID frameworks, whereas my analysis is based on instrumental variable estimation because the data contains the enrolment status in ECEC of each individual.

reflects the gender inequality at that time in Japan, and that women were generally disadvantaged. However, there are no effects on psychological abilities measured by risk preference and the Big Five indicators. These estimates are robust to other specifications. I also conduct a control experiment where I examine the “effects” of ECEC attendance for people old enough not to have been benefited by the expansion of ECEC, following Duflo (2001). I do not find any effects for them, which suggests the exclusion restriction holds.

Next, I conduct mediation analysis to identify the mechanism driving the above results. Most effects of a childcare system on future income can be explained by an increase in the likelihood of college completion owing to childcare enrolment. Together with the finding of a positive effect on wage but neither on working status nor working hours, the results imply that ECEC increases future income through education, leading to higher wages, which is consistent with theories on human capital development (Mincer 1974; Becker 1994; Kane and Rouse 1995; Thomas 2003). This study’s findings also imply that most of the effects of ECEC can be explained by an increase in the likelihood of college completion. No other channels are uncovered from ECEC to future income. This is consistent with not finding any long-run psychological effects.

This research contributes to the literature in the following aspects. First, as discussed above, few studies discuss long-term effects of universal ECEC. In this analysis, using individual choices on whether children enrolled in ECEC, their current outcomes observed in the unique data set, and quasi-random variation on expanding universal ECEC by the Japanese government, I estimate the LATE of a universal ECEC programme.[9]

Second, this study addresses a gap in the literature by revealing the mechanism behind ECEC effects. The mediation analysis distinguishes the direct effects of ECEC from its observable indirect effects through educational attainment. I find that ECEC increases the likelihood of college graduation, which, in turn, increases future income.

This study has implications for policymakers by showing the long-term positive effects of early-stage educational and childcare intervention on education attainment and income. ECEC reduces inequalities between the advantaged and disadvantaged, especially among genders given the cultural situation in Japan at that time. This implication provides governments with a good reason to expand ECEC, lower fees, and eliminate barriers to enrolment, particularly for disadvantaged children, given the finding of no negative effect for advantaged children based on the analysis of the marginal treatment effects. Although the results do not seem directly applicable to the current situation in Japan because the availability of ECEC is substantially different from that of Japan in the 1960s to the 1980s, the knowledge can be useful for countries with a gender culture similar to Japan’s, or where the government is investing in ECEC, including many developing countries, following the SDGs (United Nations 2015).

[9] More precisely, I use the propensity score as an instrumental variable, so the LATE is the average user’s LATE over the marginal treatment effects.

The remainder of the paper is organised as follows. Section 2 reviews the background and literature related to this research, while Section 3 explains Japan's ECEC system. Section 4 describes the data, Section 5 explains the econometric framework, and Section 6 shows the identification strategy. Section 7 presents the results of the long-term LATE analysis. Further analysis of the mechanism is provided in Section 8. Section 9 concludes the study.

2 Prior research

Many studies have discussed ECEC's effects in the literature of economics and education. However, most examined the effects of targeted programmes or relatively short- or medium-term effects of universal ECEC. Some examined the long-term ITT effects, but could not capture the effect on those who are treated and react to the change in universal ECEC availability. In this study, I use Japanese data to study the long-term LATE of universal ECEC on children's income and educational attainment, in addition to investigating its mechanism.

While many studies have focused on the short- or medium-term effects of ECEC, several have also paid attention to the long-term effects.[10][11] Many studies on the long-term effects on educational outcomes or earnings have analysed Head Start, a federal matching grant programme to improve poor children's academic and social skills and health status in the USA.[12][13] Currie and Thomas (1995) find Head Start positively impacts children's test scores and reduces the likelihood of repeating the same grade. Similarly, Garces et al. (2002) find positive effects on children's long-term outcomes, such as educational level and earnings when in their early 20s.

Regarding targeted programmes other than Head Start, Lavy (2018) examines a free school choice programme for disadvantaged primary school students in Israel. Although the focus is not on a preschool, he shows the long-term effects of schooling in relatively early childhood: he finds that the programme increases post-secondary education (university or college training) and the future income of programme participants when aged around 30. Heckman et al. (2013) discuss ECEC's long-term effects on income using data from the Perry Preschool Project, an early intervention

[10] See Dietrichson et al. (2020) for a review.

[11] Studies investigating the short- or medium-run effects include (Garces et al. 2002; Berlinski et al. 2009; Blanden et al. 2016; Conti et al. 2016; Heckman 2013; Heckman et al. 2013; Felfe et al. 2015; Kottelenberg and Lehrer 2017; Cornelissen et al. 2018; Yamaguchi et al. 2018b; Griffen 2018; Chan and Liu 2018; Drange and Havnes 2019).

[12] Some studies have examined the effects on health or other behavioural changes. Carneiro and Ginja (2014) examine the ITT effects of Head Start, showing that it reduces both commitment of crime and obesity. Conti et al. (2016) examine the effects of the Perry Preschool Project and the Carolina Abecedarian Project on health outcomes in adulthood. They find that the lifestyle is healthier for people who were in these programmes. Based on the dynamic mediation analysis, they also find that the traits in childhood have an effect on the behaviour in adulthood.

[13] Several studies have found the long-run effects last throughout children's lives, and many have focused on human capital accumulation from schooling in general or later schooling, rather than on the specific childcare system (Sanders and Taber 2012; Todd and Wolpin 2003, 2007).

programme for disadvantaged U.S. African American youths. Although the sample size is relatively small, the results show that ECEC reduces the number of crimes, increases the probability of employment for men, and increases the duration of marriage for women. The results further show that the source of these effects is persistent, with personality skills cited as the underlying mechanism.[14] Furthermore, Heckman et al. (2010) reveal that the Perry Preschool Project's internal rate of return is higher than the historical rate of return on standard equity of approximately 5.8% considering participants' income, crime status, and other socioeconomic outcomes.[15] These targeted programmes are shown to have long-term positive effects on various outcomes.

However, universal childcare systems may have different effects from those of targeted programmes. Baker (2011) urges caution when using targeted ECEC programmes' results to assess universal ECEC's effects because of significant differences in household backgrounds. Especially, the effects of targeted programmes might be shown to be much higher than those of universal programmes. Cornelissen et al. (2018) also points out the potentially higher treatment effects observed in targeted programmes.[16] A targeted programme is often used to fill the gap between the advantaged and the disadvantaged by offering the programme to the latter. For example, Head Start in the U.S. accepts children whose family's income is lower than $26,500 per year for a four-person family in most states.[17] Given that the quality of education of Head Start can be regarded as generally high, the effects of attending Head Start will be high for these disadvantaged children (Currie and Thomas 1995; Garces et al. 2002). However, children from wealthy families can go to a private school with a better environment, which tends to be too expensive for children from poor families to enrol in. Therefore, the effects of attending Head Start would not be high, or even be negative because they might already be going to the ideal school. If Head Start became a universal programme, the effects would be averaged out, and we would not see any positive effects of it. On the other hand, since Head Start's programme is unique and different from other programmes, it may be beneficial even for those from rich families. In this case, the effects of universal Head Start would be positive on average, and one could conclude that expanding this programme to everyone would be effective. These comparisons are feasible only when we examine a universal programme and a targeted programme. Although what I

[14] This mechanism is discussed in Section 8.1 in detail.

[15] Banerjee et al. (2017) and Bold et al. (2018) discuss the need for caution when scaling a small experimental intervention.

[16] Cornelissen et al. (2018) state that the heterogeneity, observed and unobserved, plays an important role in the size of the effects. They show that children from disadvantaged homes are less likely to select into child care, but they will benefit a great deal, while children from advantaged families are more likely to select into child care, but they will benefit less. In this paper, I show that the heterogeneity in gender is one of the most important factors driving the effects size differences.

[17] In addition to these, Head Start accepts those who experienced foster care, homelessness, and those from families receiving public assistance. See the office of Head Start (https://www.acf.hhs.gov/ohs/apply-services. Last access: 1st April 2022) for details.

analyse in this paper is not an existing targeted programme, examining the effects of a universal programme seems important for policymaking.[18]

Therefore, extending the analysis to universal systems is crucial. Some studies focus on universal childcare systems. Felfe et al. (2015) find positive effects on the cognitive ability of the children attending publicly subsidised childcare in Spain using the DID framework. They observe enhanced effects for girls and disadvantaged children. Herbst (2017) examines the ITT and treatment-on-the-treated (TT) effects of near-universal childcare in the USA. Herbst (2017) finds positive effects on college completion. Havnes and Mogstad (2011b) estimate the ITT effects of subsidised childcare on children's long-run outcomes using Norwegian data. They find positive effects on children's educational attainment and labour market participation and decreases in welfare dependency. The policy is most beneficial for girls and children with less-educated mothers. On the other hand, Baker et al. (2008) discuss the universal childcare system's negative persistent effects in Quebec on family well-being after approximately ten years. Haeck et al. (2018) show that these negative effects last in the medium-term, but disappear when children become teens. Furthermore, Baker et al. (2019) report the negative effect on non-cognitive outcomes, health outcomes, and self-satisfaction. Moreover, in Bologna, Fort et al. (2020) find that the daycare system reduces the participants' intelligence quotient. In Japan, the focus of this study, Yamaguchi et al. (2018b) examine the effects of childcare enrolment on short-term childhood outcomes by exploiting staggered childcare expansion across regions. They find that childcare improves language development among boys and reduces aggression and the symptoms of attention deficit hyperactivity disorder among children with less-educated mothers. The effects are stronger for poorer families. Although there have been many studies, as seen above, the conclusion on the effects of universal ECEC is still mixed, and estimates do not capture the long-term effects on those who changed their behaviour because of the change in universal ECEC's availability, which is quite informative in policymaking. Furthermore, regarding the discussion on effective policies, the life-long effects must be discussed. Therefore, examining the effects about half a century after enrolling in ECEC seems important.

Before ending the literature review, it is worth reviewing (Duflo 2001) because I will follow some of her methodology, such as the control experiment. She examines the effects of school construction in Indonesia. She adopts a similar strategy to mine: she exploits the differences in the timing of construction and uses it as an instrumental variable for the attending status. She finds positive effects on the years of education and wage. What is important in the relationship between her study and mine is the way to conduct a control experiment. She investigates the "effects" of school construction for those who were older than the school-age. This means that they would not have been benefited from the school construction, so the estimation should be close to zero, if there is no other path affecting the construction of schools

[18] This discussion seems similar to that between a means-tested cash transfer programme and a universal basic income programme. See Hanna and Olken (2018) for the latter discussion in developing countries. I thank an anonymous referee for proposing this comparison.

and educational and economic outcomes. This is useful for examining whether the exclusion restriction holds or not. I adopt this method and discuss the result in Section 7.2 in the results section.

3 Background and education system in Japan

Before explaining the ECEC system, I briefly review Japan's entire education system. In Japan during the 1950s–1980s, which covers the focus of this study, children started compulsory schooling at six years, at an elementary school with a six-year curriculum. After graduation, children attended a three-year junior high school. Compulsory education finished at this level, and children could choose to enrol in a high school for three years, followed by a college or university for two to four years. Before this compulsory education, some children enrolled in preschools, which were of roughly two types: full-time nursery school and part-time kindergarten. Nursery schools accepted children aged from zero to five, while kindergartens were for children aged three to five.[19] However, because of the capacity constraint discussed later, many parents could not choose the facility at which they wanted to enrol their children, and many children could not even enrol in ECEC, as discussed below. In the last subsection of this section, I briefly review the gender differences in the education environment, since this paper investigates gender heterogeneity in the effects of ECEC, and knowing the difference in circumstances helps us understand the mechanism behind the effects.

3.1 Preschool systems and roles

Here, I describe Japan's nursery school and kindergarten system and their roles.[20] I also discuss ECEC's history after World War II. Since I use the expansion of ECEC from the 1960s to the 1980s for my analysis, I specifically focus on the 1950s to 1980s in this review.

The purposes of part-time kindergartens and full-time nursery schools differ. Kindergartens aim to develop children's minds and physical strengths by providing a sound educational environment, while nursery schools provide care for children whose parents cannot care for them because of work or other commitments (Akabayashi and Tanaka 2013).

Despite different purposes and administration, their functions overlap in many ways. Both provide education to children, and, therefore, many consider them roughly equivalent (Akabayashi and Tanaka 2013).[21] Because of the imbalance in locations of kindergartens and nursery schools, some parents who might have

[19] The same system is used now.

[20] See Shwalb et al. (1992) for details.

[21] Akabayashi and Tanaka (2013) pointed out some differences in the results. Thus, I also examine the effects of kindergartens and nursery schools, but the results are quite similar. They are shown in Appendix C of Kawarazaki (2022) and discussed in Footnote 35.

wanted to send their children to nursery schools actually had to send them to kindergartens and vice versa, thereby reducing the differences (Matsushima 2015). Furthermore, both institutions are accredited by the government.[22]

For accreditation, both institutions must satisfy some quality requirements set by the Child Welfare Act and School Education Act in 1947 (Ministry of Education 1979; Shakai Fukushi Jigyo Shinko Kai 1963; Yamaguchi et al. 2018b). There are also educational requirements for kindergarten teachers and caregivers in nursery schools. Typically, the minimum required educational attainment for a kindergarten teacher's licence is two years of college or university education, and for a nursery schoolteacher, two years of high school education and one year of experience (Ministry of Education 1979; Yoshimi 2001; Yamaguchi et al. 2018b). These requirements indicate that teachers and caregivers are skilled, and that the quality of preschools can be considered high.

The teacher–student ratio seems similar between the two types of facilities. Zen-Nihon Shi You Ren Soumu Iinkai (2017) showed that a kindergarten's average teacher-student ratio was about 25 in the 1960s to 1980s. The nursery school teacher-student ratio was similar. The Minimum Standards for Child Welfare Facilities required at least one childcare worker in a nursery school for every three infants, at least one for every six children under the age of one to three, at least one for every 20 children under the age of three to four, and at least one for every 30 children aged four or more. Together with the fact that older children were prioritised for enrolment in a nursery school, as discussed by Matsushima (2015), and the capacity was binding as discussed later, the average ratio is around 25–30.[23] Figures 1 and 2 show the relationship between capacity and number of children enrolled in kindergartens and nursery schools. The figures illustrate the lack of space for additional children in both kindergartens and nursery schools.[24][25]

[22] Ministry of Education (1979) and Yamaguchi et al. (2018b) provide more detailed explanations, although the latter focused more on a slightly later period.

[23] ECEC prioritised five-year-old children the most, followed by four-year-old and younger children (Matsushima 2015). Because the capacity itself was not large, small slots were available for four-year-old children. Therefore, the expansion policy affected the enrolment of children aged four, thus I use the variable of the age of four. Additionally, the selection for nursery schools was based on their need for ECEC: single-father households, disaster faced by the households, and other criteria (Matsushima 2015). Accordingly, each municipality's mayor decided whom to enrol (Matsushima 2015). However, this rule was applied not strictly, and many children were enrolled whose situation did not satisfy this condition (Matsushima 2015).

[24] The government recognised the problem of the lack of nursery schools. On the discussion in a plenary session of the House of Councillors on March 18, 1967, Hideo Bo, the Minister of State at that time, stated that the number of nursery schools was not enough and had a plan to increase it House of Councillors, The National Diet of Japan (1967b). Besides, Akira Ono, a committee member of the Committee of Education in the House of Councillors, described that the lack of nursery schools happened all over Japan at the committee on July 11, 1967 (House of Councillors, The National Diet of Japan 1967a).

[25] In some cases, kindergartens seemed to have accepted slightly more children than their capacity. On the other hand, in several cases, the capacity was larger than the enrolment, but the capacity was actually binding (House of Councillors, The National Diet of Japan 1967a, b; Motoki and Yamanishi 2009; Matsushima 2015). I provide a robustness check in Section 7.2.

Regarding the ECEC fee, the average price of a nursery school was around 7,000 JPY per month in 1979 (Takayama 1982), although it changed over time across municipalities and was subsidised for low-income families.[26][27][28] Meanwhile, the fee of a kindergarten was around 8,800 JPY in 1979 (Minister's Secretariat, Ministry of Education, Culture, Sports, Science and Technology 1980).[29] This evidence supports the outcome of similar qualities among nursery schools and kindergartens.

3.2 Expansion of ECEC

After World War II, Japan became westernised in many aspects, including ECEC (Kawai 1979). Kindergartens were established according to the 1947 School Education Law under the Ministry of Education, and nursery schools were constructed following the 1948 Child Welfare Law under the Ministry of Welfare (Tobin et al. 1991). Behind this movement lay the evolution of Japanese families from traditional extended families to nuclear families (Shwalb et al. 1992).[30] This decline in the traditional form gave mothers and ECEC more responsibility for childcare, although there was still a slight increase in the number of three-generation families in which grandparents could assume responsibility for childcare (Miyake 1989). The average number of family members decreased in this period: 5.0 in 1920–1955, 4.5 in 1960, 4.0 in 1965, and 3.1 in 1988 (Shwalb et al. 1992). The decline in the birth rate also contributed to this fall. Furthermore, given the shrinking of neighbourhood communities, Japanese children had fewer playmates around their homes, which increased the importance of childcare facilities for peer interactions (Shwalb et al. 1992; Miyake 1989). Actually, the expansion of ECEC does not seem to have replaced maternal care: in the literature, Asai et al. (2015, 2016); Yamaguchi (2017); Yamaguchi et al. (2018) report no effects of ECEC on maternal labour supply. Moreover, Asai et al. (2016) and Yamaguchi et al. (2018) find that ECEC crowded out the informal childcare provided by grandparents. Although their analyses focus on the 1990s to 2010s, given the situation where the number of family members is declining, the same seems to have occurred.[31]

When the post-war fallout stabilised, the number of births increased dramatically (i.e., a baby boom) and this increased people's interest in ECEC. Simultaneously,

[26] The prices written here are original at the time, and not converted to the current value.

[27] Takayama (1982) presents a price schedule ranging from 3,000 to 13,000 JPY, but most of the prices were around 6,000 to 10,000 JPY.

[28] In 1979, the average basic monthly salary of an employed worker in Japan was 162,400 JPY, based on a Basic Survey on Wage Structure collected by Ministry of Health, Labour and Welfare.

[29] The author calculates the weighted average of tuition fees based on the tuition fees of private and public kindergartens and their enrolment rate.

[30] In an extended family, grandparents, parents, children, and their aunts and uncles, in addition to their children if any, live together. This is larger than a three-generation family, where only grandparents, parents, and children live together. In a nuclear family, only parents and children live together.

[31] Zhang and Managi (2021) examine the effects of ECEC on maternal employment in the latest period using the data from 2015 to 2017. Contrary to the previous period, they find that ECEC increases maternal employment, while the increase is mostly explained by non-regular employment of less-educated mothers.

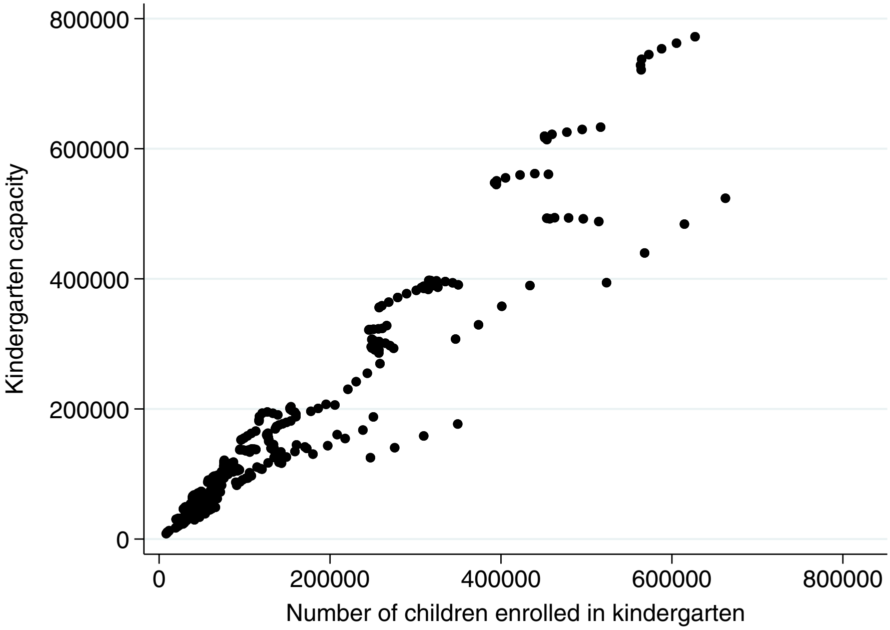

Fig. 1 Capacity and number of children enrolled in kindergartens. Each point represents a pair of capacity and the number of children enrolled in a year in a prefecture

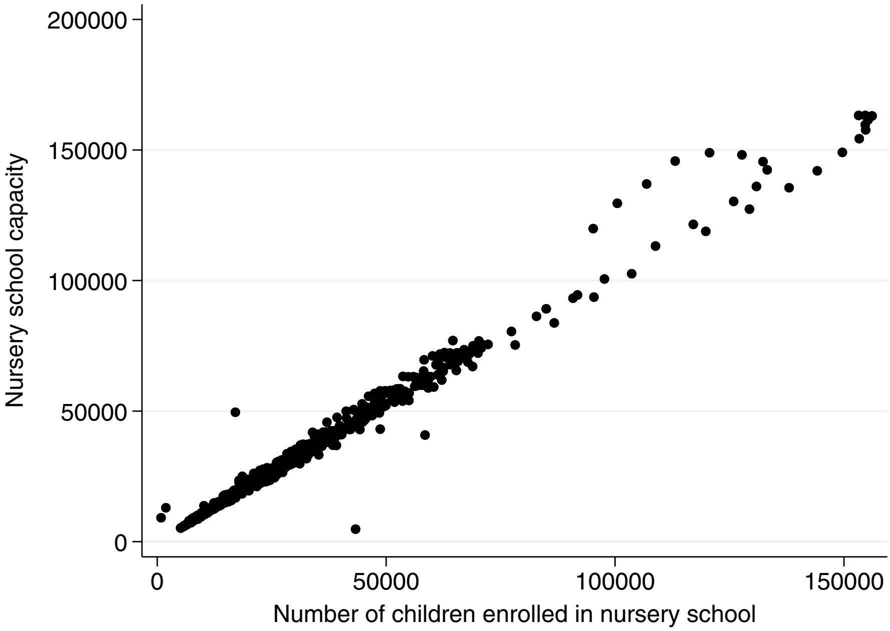

Fig. 2 Capacity and number of children enrolled in kindergartens. Each point represents a pair of capacity and the number of children enrolled in a year in a prefecture

women's social status was rising. Although the Japanese people have traditionally valued mothers' participation in childcare, more Japanese women entered the workforce, particularly after World War II, thus driving the demand for childcare systems (Shwalb et al. 1992). In the middle of the 1950s, Japan's economic growth surged, and female labour demand increased dramatically (Matsushima 2015). At this time, there were much fewer capacities of ECEC facilities than the number of children whose parents wanted them to enrol in an ECEC facility. This increase in the demand for ECEC happened all over Japan.[32] The government tried to increase the ECEC facilities, but the problem was not solved, and the size of the budget and the numbers of teachers and facilities were still too low over the 1950s, although the number of facilities themselves increased over this period. Therefore, the government prioritised areas where the number of facilities was relatively smaller compared to the population. In 1956, the Kindergarten Establishment Standards was promulgated and it said the expansion of kindergartens should be based on the imbalance of the number of facilities. In 1957, the government decided to offer subsidies for constructing nursery schools in areas where the number was not yet sufficient (Kousei Shou Jidou Kyoku 1959; Matsushima 2015). The government used the measure of the number of nursery schools per 1,000 people and that of kindergartens per 10,000 people. They tried to capture the imbalance in ECEC and prioritise the area where the resources were scarce (Matsushima 2015). I examine the determinants of the ECEC expansion quantitatively, following the analysis by Cornelissen et al. (2018).[33]

As a result of prioritisation, there were large differences in the number of ECEC facilities (e.g., nursery schools and kindergartens) at that time among areas (Matsushima 2015). Moreover, the supply of ECEC was binding—even in areas with more ECEC facilities—and government policy was necessary to meet future demand (Matsushima 2015). Thus, the Japanese government increased the number of kindergartens and nursery schools. First, the government increased the number of kindergartens under the First Kindergarten Education Revival Plan in 1963. Then, in 1966, it decided to expand nursery schools by adopting the First Nursery School Emergency Maintenance Five-year Plan. However, the expansion was insufficient to meet the demand. Therefore, the government adopted new plans, including the Second Kindergarten Education Revival Plan and the Second Nursery School Emergency Maintenance Five-year Plan in 1971. Because of these expansions, the childcare system's enrolment rate increased dramatically (Matsushima 2015). However, there were large regional differences in the expansion rate and, consequently, the enrolment rate differed, as shown in Figs. 3 and 4. Figure 3 shows the mean of the sum of the rate of enrolment in kindergartens and the ratio of capacity to the population in nursery schools at age four across all Japanese regions from 1960 to 1989. Besides the difference in initial values, there are differences in the slopes of the lines, indicating that the rates of increase differed regionally. Figure 4 shows the changes in

[32] Note that, however, regional differences in demand, if any, did not increase the supply of ECEC specific to the area. See Section 6. This means that demand rose similarly across Japan.

[33] Table 5 reports the result. I will discuss it later, but the table confirms this prioritisation.

the rate of enrolment in ECEC at age four from 1961 to 1984.[34] There is no systematic pattern in the expansion rate. Therefore, I exploit these variations. The ministries in question tried separating kindergartens from nursery schools based on their purposes, namely, education for kindergartens and social welfare for nursery schools (Takada 2014; Okada 2014). However, as their functions overlapped in many ways, the two entities' roles gradually became similar (Akabayashi and Tanaka 2013; Shwalb et al. 1992). Therefore, I analyse the effects of ECEC rather than those of kindergartens and nursery schools separately.[35]

The variations in ECEC expansion look plausible based on the above discussion. However, this would fail to work well if exclusion restriction did not hold. One of the largest concerns would be the case where some specific areas were eager to invest in education. If this were the case, expansion of ECEC and that of higher education, including universities and colleges, would be highly correlated. Also, the quality of education in these areas might be higher. These violate the assumption of exclusion restrictions. However, in my data, this is not likely to happen: the correlation between the expansion in ECEC and that of universities and colleges is 0.061. Hence, this potential threat seems minimal. I also check whether exclusion restriction holds by conducting a control experiment. See Section 7.2 in the result section for details. Furthermore, I examine the determinant of ECEC expansion quantitatively. Although the government prioritised the areas where the capacity was scarce, the exclusion restriction would fail if there were a correlation between the growth in the ECEC availability and important labour market characteristics, such as the fraction of female labour supply. As discussed in Section 6, I follow Cornelissen et al. (2018) and investigate the factors and confirm that the government increased the availability of ECEC based on the initial situation.

3.3 Gender inequality in the education system

Since this paper examines the heterogeneous effects of ECEC concerning gender, it is worth noting briefly the gender difference in the education system in the 1960s to 1980s in Japan. In Japan, at that time, there existed considerable gender inequality. Although ECEC availability was similar between boys and girls, as shown in Fig. 5, other situations differed significantly. Regarding higher education, men's enrolment rates were much higher than those of women in the 1950s to 1980s, as shown in

[34] The data set does not contain data for 1960 and from 1985 to 1990, but no major events affected the enrolment rate in those years. The numbers should be close to capacity, given the capacity constraint was binding.

[35] There may be questions about whether those who attended kindergartens and nursery schools differ. Therefore, I conduct additional analyses by treating them separately. See Appendix C of Kawarazaki (2022) for the results. Although the estimates are less accurate, the results are similar. Particularly, there is a difference in risk preference. One possible explanation for this finding is that nursery schools tended to accept a greater variety of ages (age zero to five) than kindergartens (age three to five). Therefore, children at age four, the focus of this study, might have been exposed to younger children more in nursery schools than in kindergartens. Therefore, children at nursery schools may exhibit greater maturity, leading to greater risk adverseness.

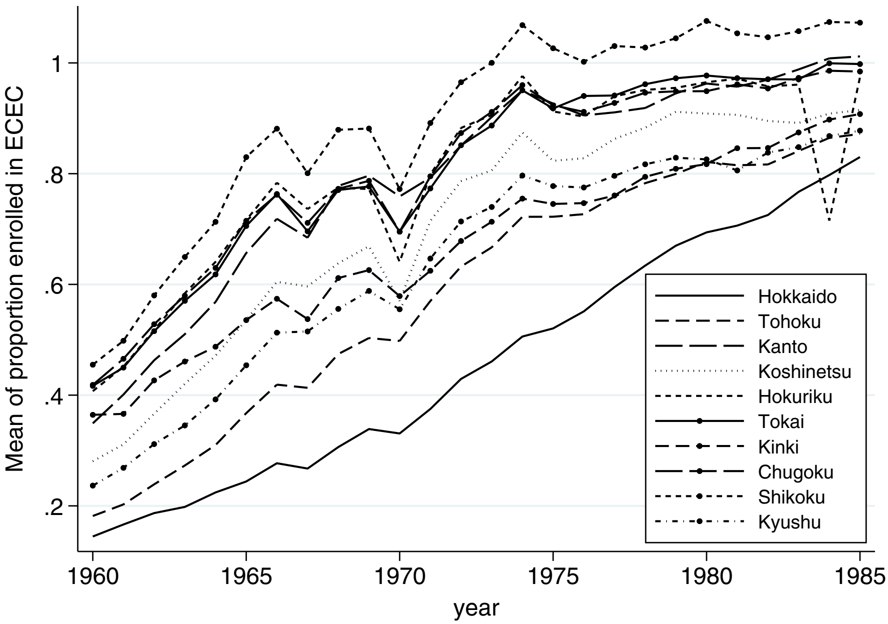

Fig. 3 Mean of the proportion of children enrolled in ECEC at age four across regions over time. The variables are weighted by the population of the cohort in each prefecture. There are variations across regions over time. Some values exceed one because I calculate the proportion of capacity in nursery schools at each age using the capacities in nursery schools across ages and the proportion of each age at the national level. Regarding area divisions, see Table 13 and Fig. 10 in Appendix A

Fig. 6. This partly reflected the atmosphere at that time—that, as women should stay home to raise the children and do housework, they do not necessarily require higher education. This culture was also reflected in the working environment. Women's employment rate at around 50% was far lower than that of men at about 80%, according to Census. Furthermore, as Fig. 7 illustrates, wages are much lower for women than men. Based on the Basic Survey on Wage Structure collected by the Ministry of Health, Labour and Welfare, the income ratio of women to men was approximately 0.6 in the mid-1980s. This indicates large gender inequality in both the educational and working environments.

4 Data

To examine the long-term effects of ECEC expansion, I use two types of data: individual-level survey data and prefecture-level administrative data sets.[36]

[36] A prefecture is the first level of jurisdiction and administrative division. There are 47 prefectures in Japan.

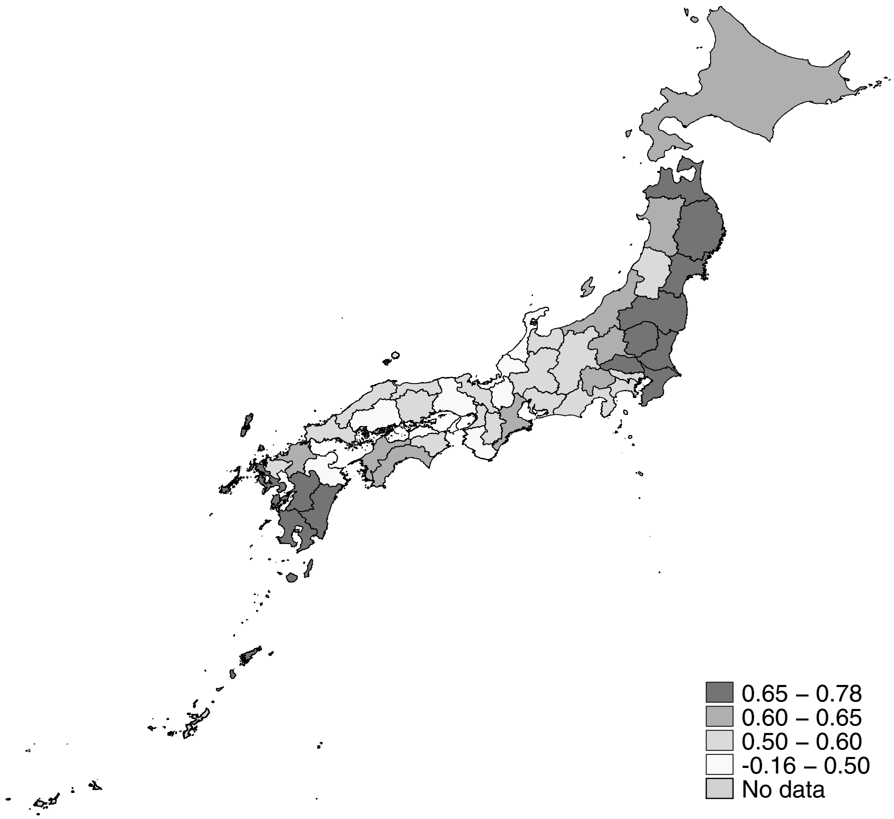

Fig. 4 Change in the proportion of children enrolled in ECEC at age four from 1961 to 1984. I use Stata command `maptile` with the option `jpn_pref` created by Chigusa Okamoto. See http://www.crepe.e.u-tokyo.ac.jp/en/materials/maptile.html for more details (Last access: 1st June 2022). This is a preliminary use, and I thank her for permitting its application

I use survey data from the Preference Parameters Study of Osaka University, which are collected annually since 2003, to create a panel data set.[37] In 2004, 2006, and 2009, Osaka University added new observations to the original panel data to compensate for the small sample size owing to the survey's attrition. This data set contains samples of individuals aged 20 to 69 years; the questionnaire's response rate is 60–70% for new samples and 70–95% for repeated surveys. I use data collected in 2009 and 2012 and focus on people born in 1960 to 1989. The survey in 2009 includes a retrospective question on whether respondents went to a kindergarten or nursery school when they were at each age from zero to five. It elicits information on their socioeconomic outcomes, including annual income, working status, working hours, wages, health-related behaviours

[37] These data have been used by many researchers, for example, to examine changes in personal characteristics (Hanaoka et al. 2018).

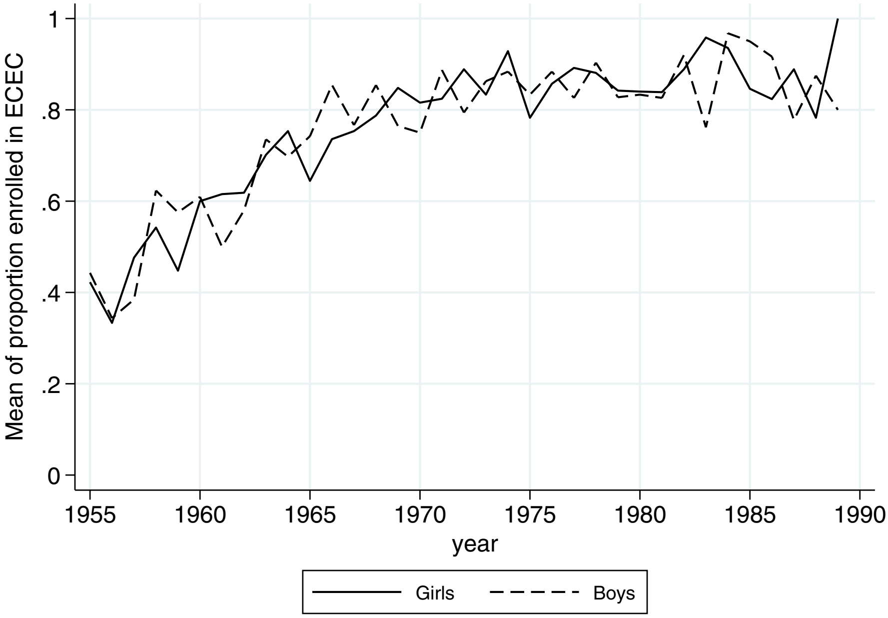

Fig. 5 Proportion of children enrolled in ECEC at age four by gender. The proportion is calculated by taking the average of the dummy variable indicating whether a child went to a kindergarten or nursery school at the age of four (1 if enrolled) in the main data

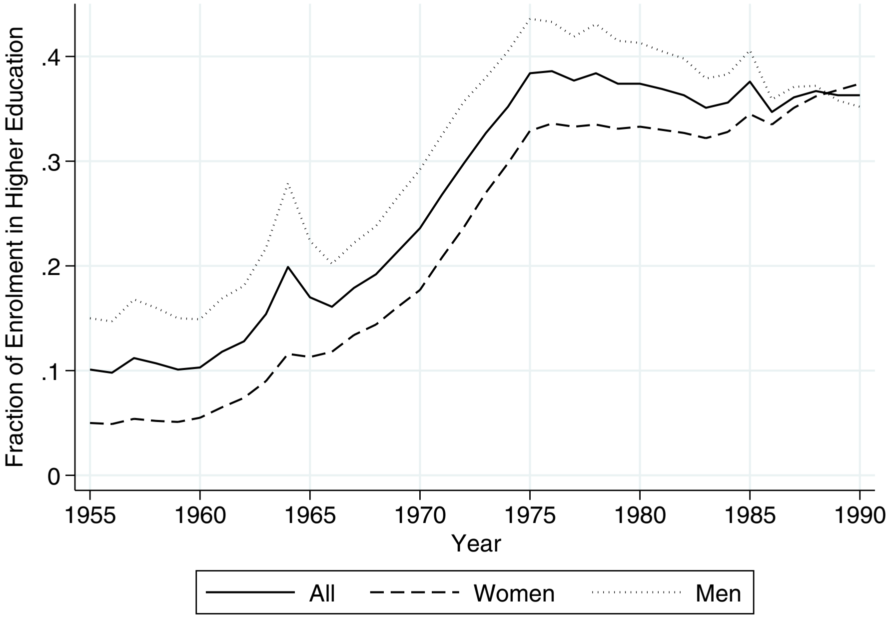

Fig. 6 Proportion of young adults enrolled in higher education over time. This number includes people who attended two-year colleges. The data source is the School Basic Survey collected by the Ministry of Education, Culture, Sports, Science, and Technology (MEXT). This includes all age groups

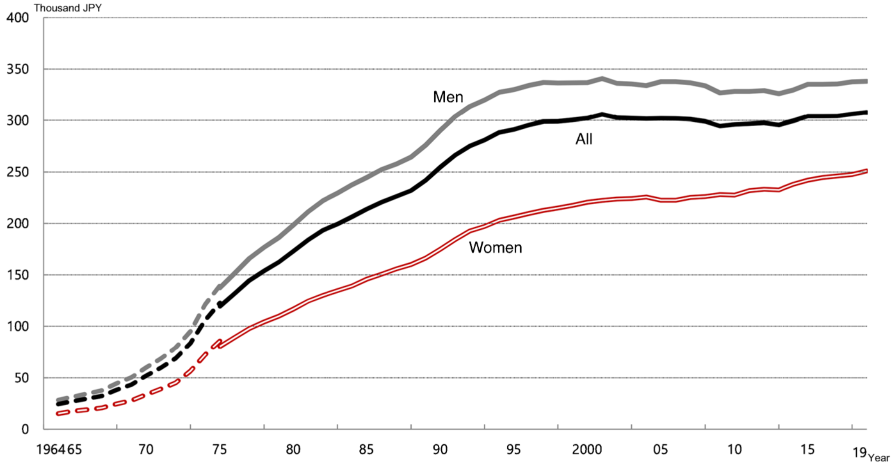

Fig. 7 Nominal wages of men and women over time in Japan. This figure is from The Japan Institute for Labour Policy and Training (2020), with the labels translated by the author from Japanese into English

such as smoking and drinking, education histories, parents' education levels, and parents' ages when they became adults; their subjective family financial background when they were young; and the prefectures in which they lived when they were aged 15 years.[38]

I omit observations if they do not have data on the ECEC enrolment at age four and its propensity score. Then, we have 2,647 observations remaining, which is 91% of the total observations in the data set of those born between 1960 and 1989. To maximise the sample size, if there are missing data in the control variables, I substitute them with 0 and make the dummy variable, indicating the substitution. In all regressions, I control for the dummy variables. Therefore, there is no contamination owing to missing data. While all remaining observations have educational information, some do not have data on income, and the number of remaining samples is 1,915. Therefore, I examine if there is any systematic tendency between ECEC enrolment and missing data in income; if there is, the main analysis would be biased. However, based on Table 15 in Appendix A, I cannot observe such a tendency for the age four analysis. This implies the estimation is not biased owing to attrition.

[38] The variables for income and subjective family financial background are categorised. For the former, respondents were asked to choose the appropriate income category (see Appendix A). For the latter, they were asked to choose a number indicating their family financial background when they were aged 15 years, from zero (poorest) to 10 (wealthiest). I implicitly assume that respondents remember their family status and that their family background did not change significantly. Moreover, individuals from poor households who are now rich may exaggerate their poorness. If this occurs, the results might be overestimated. However, I use this variable only to separate the whole sample into a relatively poor subsample (categories 1–5), that is, respondents need not remember specific numbers, for example, their parents' income. Therefore, this assumption does not seem strong.

The 2012 data contain information on socio-emotional outcomes, such as the Big Five.[39] Since the collection years differ between the main data set (in 2009) and the data set for Big Five (in 2012), the problem of attrition arises. Table 15 shows the result of examining whether there are systematic differences between children who answered Big Five outcomes and those who did not in terms of ECEC enrolment. In the main analysis, I focus on the analysis of ECEC enrolment at age four. Hence, this result implies there is no attrition problem. As the ECEC enrolment data are retrospective, it is necessary to discuss the extent to which people could remember their enrolment status accurately. To address this problem, I compare the enrolment rate of the national data with that of the survey data. Table 16 in Appendix A presents the analysis results. These numbers are similar, confirming that using retrospective data is a suitable approach.

Tables 1 and 2 show summary statistics for enrolment rates across ages, participants' socioeconomic outcomes and characteristics, and the Big Five indexes. Table 1 shows that the ECEC enrolment rate is increasing as the age increases, which is consistent with the fact that the government prioritised the enrolment of children at an older age. In the analysis, I focus on ECEC enrolling status at age four because of its wide variation.[40,41] 78% of the observations went to ECEC in my sample. Table 2 shows the average of outcomes and other characteristics. I also show the difference between the values of the control group (children without ECEC) and the treatment group (children with ECEC) with the t-test results. We can see some differences between them in their education levels, for example.

Potential caveats to the data are worth noting. Although the questionnaire used is unique, the sample size is smaller than that of recent empirical works. Therefore, estimated standard errors are larger, and, consequently, the estimations are less accurate, although the data are nationally representative. Moreover, the data are not panel data and contain subjective records, which could reduce accuracy, although I discuss these problems later.[42]

I also use administrative data sets collected by some Japanese ministries to construct the instrumental variables (IV) for the main and mediation analyses, and to check validity. First, I use Census for 1960–1990 collected by the Ministry of Internal Affairs and Communications every five years. Particularly, I use the information on the population of each cohort born from 1960 to 1985 to create the variable on the fraction of people enrolled in ECEC, in addition to that of a college for the mediation analysis. Because the data are for only every five years, I interpolate

[39] The original questionnaire was converted into an index of the Big Five. See Table 12 in Appendix A for further information.

[40] The results for other ages are similar, although less accurate.

[41] Some might think that the longer the enrolment, the better the outcome. However, Kuehnle and Oberfichtner (2020) report that longer enrolment in full-time childcare does not necessarily lead to a better outcome, based on analysis using German data for the late 1990s.

[42] The original data are constructed as panel data. However, the key variable of this analysis is whether children enrolled in ECEC, which is not time-varying. Therefore, I treat this data set as cross-sectional data.

the in-between data to create a panel data set.[43][44] I also use data on the population of women aged 20–35 years and the number of employed women in the same age range, when they are likely to quit their job and give birth. These data are used in robustness checks to test whether demand for ECEC led to its supply. Moreover, I use the information of each prefecture on population, the number of middle-educated and high-educated people, and the fraction of female labour force for the analysis on the determinant of the ECEC expansion. This analysis is done together with the data on the the mean wages from the data set called the Historical Statistics of Japan, collected by the Ministry of Internal Affairs and Communications.

Second, I use the Survey of Social Welfare Institutions collected by the Ministry of Health, Labour and Welfare. This data set describes nursery schools' capacity in each prefecture each year. I use data for 1960–1989. However, these data do not include full information on the children's age. Thus, I cannot separate their ages completely. I use the data set containing the ratio of the ages of children enrolled in a nursery school at the country level. I assume that the ratio is the same across prefectures, and I use it to calculate the capacity for each age in each prefecture.

Third, the analysis is based on the School Basic Survey data set collected by the Ministry of Education, Culture, Sports, Science, and Technology (MEXT). These data contain information on Japanese schools, including the number of children in kindergartens at each age in each prefecture. I use the data for 1960–1989. This survey is also used to calculate the college enrolment rate. I use the number of children enrolled instead of capacity because data are available for the former but not the latter. This variable can be used as an IV, and its validity is discussed in the section on the identification strategy.

Fourth, to check the validity of the IV, the discussion on identification is partly based on the National Survey on Migration in the Annual Population and Social Security Surveys collected by the National Institute of Population and Social Security Research. I use data on the migration rate and reasons for migration collected in 1996.

Finally, for mediation analysis, I use the list of colleges collected by MEXT, which contains data on the capacity of national, public, and private colleges. I use the data for 1980–2005, which corresponds to people at the age of 18 who were born between 1962 and 1987. I use these data to create the college capacity variable for the IV for college completion.

5 Empirical model

To examine the effect of ECEC on people's outcomes in adulthood, this study uses the following model to regress these outcomes on enrolment in the childcare system:

$$y_{ijT} = d_{ijt}\gamma + \boldsymbol{x}'_{it}\boldsymbol{\beta} + f(\mu_j, \nu_t) + \varepsilon_{ijT}, \tag{1}$$

[43] I interpolate the data by a linear interpolation of each observed data point.

[44] Data on Okinawa is available only from 1975.

Table 1 Summary statistics in enrolment status

	Enrolment					
	Age 0	Age 1	Age 2	Age 3	Age 4	Age 5
	(1)	(2)	(3)	(4)	(5)	(6)
ECEC (Enrol = 1)	0.026	0.043	0.074	0.368	0.784	0.919
	(0.159)	(0.202)	(0.261)	(0.482)	(0.412)	(0.273)
Nursery School (Enrol = 1)	0.026	0.043	0.074	0.219	0.313	0.305
	(0.159)	(0.202)	(0.261)	(0.414)	(0.464)	(0.461)
Kindergarten (Enrol = 1)				0.149	0.471	0.614
				(0.356)	(0.499)	(0.487)
N	2647	2647	2647	2647	2647	2647

Standard deviations are in parentheses

where y_{ijT} is the outcome of individual i who lived in prefecture j at age 15, which is measured in year T; d_{ijt} is the ECEC enrolling status dummy (enrolled = 1) of individual i in prefecture j at year t (at age four); $\boldsymbol{x}_{it}$ is individual i's characteristics at year t (at age four), including time-invariant variables, such as gender; μ_j is the prefecture fixed effects; ν_t is the cohort fixed effects; $f(\mu_j, \nu_t)$ is a function of fixed effects; and ε_{ijT} is an error term.[45]

The controls can include other variables, such as parents' education levels, parents' ages, number of siblings, and gender dummy depending on the specification. As stated above, if any variable is missing, I substitute it with 0 and include the dummy variable indicating this substitution as another control. I assume that people did not move from where they were born until age 15. I discuss this point in detail in the subsection on the identification strategy. If y_{ijT} is a binary variable, such as an indicator of college completion, the model is a linear probability model.[46]

However, there may be omitted variable bias because parents' decisions to enrol their children in ECEC could be highly correlated with their unobservable household characteristics. Therefore, I adopt an IV analysis and obtain a two-stage least squares estimator.

The IV here is the propensity score of enrolment in ECEC (i.e., probability of enrolment in ECEC), constructed as follows. The validity of this analysis using the number of children enrolled is discussed in the next section. First, define the fraction of children enrolled in ECEC, r_{jt} as

[45] Since variations of the IV are at prefecture-year level, I cannot include all of the prefecture fixed effects, the cohort fixed effects, and interactions simultaneously in the function f. Instead, to capture most of the fixed effects and their trends, I control for area fixed effects using ten area divisions based on the Japanese government's standard, cohort fixed effects in every five-year unit, and their interactions. See Fig. 10 and Table 13 for the definition of areas. As the robustness check, I further include the age, which controls for the cohort fixed effects more precisely. The estimation is shown in Column (5) in Table 8, but the result does not change.

[46] This estimator is consistent once endogeneity problems are solved. See Angrist (2001) for details.

Table 2 Summary statistics for socioeconomic outcomes and big five components

	All Sample			Enrolled in ECEC at Age 4			Not Enrolled in ECEC at Age 4			Difference (4) – (7)	p-value
	mean	sd	N	mean	sd	N	mean	sd	N		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Socioeconomic Outcomes											
Income	0.915	(0.940)	1915	0.911	(0.945)	1517	0.931	(0.923)	398	−0.020	0.707
Wage	−1.976	(0.574)	1510	−1.967	(0.573)	1198	−2.010	(0.575)	312	0.043	0.234
Working Status	0.804	(0.397)	2647	0.803	(0.398)	2075	0.804	(0.397)	572	−0.001	0.965
(Work = 1)											
Working Hours per Week	38.37	(13.23)	1496	38.43	(13.25)	1194	38.11	(13.15)	302	0.317	0.710
High School Completion	0.957	(0.202)	2647	0.963	(0.188)	2075	0.935	(0.246)	572	0.028	0.003
(Complete = 1)											
College Completion	0.503	(0.500)	2647	0.531	(0.499)	2075	0.400	(0.490)	572	0.131	0.000
(Complete = 1)											
Gender	0.551	(0.498)	2647	0.553	(0.497)	2075	0.542	(0.499)	572	0.011	0.631
(Female = 1)											
Age	38.81	(6.874)	2647	38.07	(6.822)	2075	41.48	(6.388)	572	−3.407	0.000
Number of older siblings	0.716	(0.816)	2647	0.690	(0.772)	2075	0.808	(0.955)	572	−0.118	0.002
Number of younger siblings	0.714	(0.784)	2647	0.717	(0.772)	2075	0.701	(0.826)	572	0.016	0.665
Low Father's Education	0.722	(0.448)	2647	0.708	(0.455)	2075	0.773	(0.419)	572	−0.064	0.002
(Less than High School = 1)											
Low Mother's Education	0.801	(0.399)	2647	0.789	(0.408)	2075	0.844	(0.363)	572	−0.055	0.003
(Less than High School = 1)											
Father's Age	30.80	(11.62)	2647	30.72	(11.65)	2075	31.11	(11.51)	572	-0.389	0.478
When Child is at Age 4											
Mother's Age	28.40	(10.12)	2647	28.29	(10.15)	2075	28.83	(9.987)	572	−0.544	0.255

Table 2 (continued)

	All Sample			Enrolled in ECEC at Age 4			Not Enrolled in ECEC at Age 4			Difference (4) – (7)	*p*-value
	mean	sd	N	mean	sd	N	mean	sd	N		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
When Child is at Age 4											
Subjective Wealth	0.636	(0.481)	2647	0.624	(0.484)	2075	0.680	(0.467)	572	−0.056	0.014
At the Age of 15											
Risk Preference	0.685	(0.696)	2554	0.683	(0.705)	2007	0.691	(0.666)	547	−0.008	0.812
Big Five											
Extraversion	4.154	(1.310)	1958	4.169	(1.286)	1534	4.104	(1.393)	424	0.065	0.368
Agreeableness	4.890	(0.923)	1961	4.884	(0.925)	1537	4.912	(0.919)	424	−0.028	0.580
Conscientiousness	3.844	(1.088)	1958	3.834	(1.086)	1536	3.879	(1.096)	422	−0.045	0.450
Emotional stability	3.953	(1.046)	1953	3.946	(1.049)	1532	3.979	(1.036)	421	−0.032	0.573
Openness to Experience	3.946	(1.064)	1955	3.963	(1.072)	1534	3.881	(1.032)	421	0.082	0.162
Big Five Index	4.156	(0.631)	1936	4.157	(0.631)	1519	4.151	(0.630)	417	0.007	0.845

Standard deviations are in parentheses. Socioeconomic outcomes are measured in 2009, while Big Five components are measured in 2012. In Column (11), p-value is for testing the mean of those who enrolled in ECEC at age 4 is equal to that of those who did not. The income is in the unit of a million Japanese yen (US \$1 ≈ ¥110) and the wage is in the unit of ten thousand Japanese yen. I convert the original categorical outcome into the mean of their range. See Appendix A. As to the wage, I focus only on those who report monthly wage, excluding people reporting hourly wage. I exclude observations with zero income and zero wage from the sample respectively, to be consistent with the main analysis using logarithm. The data on subjective wealth contains categorized subjective wealth at age 15. 0 is poorest and 10 is richest. Here, I formulate a dummy variable for whether the individual's subjective wealth was above or below average. This equals to 1 if below median. Risk Preference is calculated following Hanaoka et al. (2018). I use as a measure of risk preference. Note that if transformed price is larger, s/he is more risk averse. Big Five Index is made by adding all the components of Big Five and dividing it by five

$$r_{jt} := \frac{n_{jt}}{N_{jt}}, \tag{2}$$

where n_{jt} is the number of children enrolled in ECEC, a nursery school or a kindergarten, in prefecture j in cohort t, which is almost the same as ECEC capacity when the capacity is binding, and N_{jt} is the population of prefecture j in cohort t. One potential problem is that data on the capacity of kindergartens and nursery schools for children at age four are not available. However, regarding kindergartens, the number of children enrolled at each age is available. Therefore, given that the capacity constraints are binding based on Fig. 1, in the main analysis, I use the number of children enrolled in kindergartens at the age of four. For nursery schools, since age-level data on both the capacity and number of children enrolled are not available, I construct the capacity ratio of children at age four by multiplying the total capacity by the ratio of the number of children enrolled at age four with the total number of children enrolled in a nursery school. I assume that this ratio is the same across Japan over time. However, this assumption is not crucial because I also control for prefecture fixed effects and cohort fixed effects in the probit model below. Hence, any measurement error among prefectures and cohorts is absorbed by them.[47]

Given this proportion r_{jt}, I estimate the propensity score using a probit model:

$$\begin{aligned} d^*_{ijt} &= \boldsymbol{x}'_{0i}\lambda + r_{jt}\phi + g(\eta_j, \psi_t) + e_{ijt}, \\ d_{ijt} &= \begin{cases} 1 & \text{if } d^*_{ijt} > 0 \\ 0 & \text{otherwise,} \end{cases} \end{aligned}$$

where d_{ijt} is defined above, that is, the ECEC enrolment status dummy (enrolled = 1) of individual i in prefecture j at year t (at age four); d^*_{ijt} is its continuous latent variable; $\boldsymbol{x}_{0i}$ is individual i's gender; η_j is the prefecture fixed effects; ψ_t is the cohort fixed effects; and e_{ijt} is an error term, which is assumed to be independent and identically distributed (i.i.d.) over a normal distribution with mean 0 and variance 1. $g(\eta_j, \psi_t)$ is a function of fixed effects.[48] Define $\hat{z}_{ijt}$ as the probability of enrolment in ECEC for individual i in prefecture j in cohort t:

$$\begin{aligned} \hat{z}_{ijt} &\equiv \Pr\left(d_{ijt} = 1 | \boldsymbol{x}_{0i}, r_{jt}, \eta_j, \psi_t\right) \\ &= \Phi\left(\boldsymbol{x}'_{0i}\hat{\lambda} + r_{jt}\hat{\phi} + g(\hat{\eta}_j, \hat{\psi}_t)\right), \end{aligned} \tag{3}$$

[47] As a robustness check, because children at age five were prioritised for enrolment in kindergartens, I substitute the number of children enrolled at age five from the entire capacity. However, the results from the latter method are not statistically different from those of the former method.

[48] There are many ways to construct this propensity score. Therefore, I do so in various ways and show the result of the outcome in Table 8 and of the propensity score estimation in Table 28. However, the result does not depend on the model specification. Here, I remove area fixed effects and prefecture fixed effects because of potential large heterogeneity across and inside locations. Different specifications with the cohort and other fixed effects yield a similar result and they are not statistically different.

where $\Phi(\cdot)$ is the standard normal cumulative distribution function, and the hats over the variables show their estimates.

Although r_{jt} itself can be used as an IV, I use $\hat{z}_{ijt}$ as the IV in my main analysis instead, because it provides a more efficient estimation (Wooldridge 2010). Accordingly, in this study, the LATE is the average user's LATE over the marginal treatment effects.[49][50]

Based on the above models, I consider the first- and second-stage regressions as

$$d_{ijt} = \hat{z}_{ijt}\theta + \boldsymbol{x}'_{it}\boldsymbol{\kappa} + f(\mu_j, \nu_t) + \epsilon_{ijt}, \tag{4}$$

$$y_{ijT} = \hat{d}_{ijt}\tilde{\gamma} + \boldsymbol{x}'_{it}\tilde{\boldsymbol{\beta}} + f(\mu_j, \nu_t) + u_{ijT}, \tag{5}$$

where ϵ_{ijt} and u_{iT} are error terms independent of the other controls, ω_{jt} is prefecture-specific time trends, and $\hat{d}_{ijt}$ is the fitted value of $\hat{d}_{ijt}$ based on Eq. (4). The parameter of interest, $\tilde{\gamma}$, is estimated using two-stage least squares (2SLS) based on Eqs. (1), (4), and (5).[51] If ECEC has a positive effect on outcome Y, coefficient $\tilde{\gamma}$ is positive.[52]

Moreover, I show the results based on the reduced-form analysis, which is based on the following regression model:

$$y_{ijT} = \hat{z}_{ijt}\delta + \boldsymbol{x}'_{it}\boldsymbol{\varphi} + f(\mu_j, \nu_t) + \zeta_{ijT}, \tag{6}$$

where ζ_{ijT} is an error term independent of the other controls.

6 Identification issues

Here, I discuss this study's empirical strategy. If I examine the causal effect of ECEC using ordinary least squares (OLS) as Eq. (1), the estimator is likely to be biased because there may be a correlation between unobserved household characteristics and parents' decisions on children's ECEC enrolment. Therefore, an IV estimation is a possible way to estimate the parameters of interest consistently.

As an IV, I exploit the change in the expansion of capacity and number of children enrolled in nursery schools and kindergartens (see Section 3) at the prefecture level during the 1960s to 1980s. I then construct the propensity score for whether a

49 As the robustness check, I use $\hat{z}_{ijt}$ and examine the LATE in the traditional sense. According to Table 8, the result does not change qualitatively.

50 I additionally investigate the marginal treatment effects, which is discussed by Cornelissen et al. (2018), for example. I discuss the result in Section 7.

51 The standard errors are estimated based on the 2SLS procedure.

52 In Eqs. (4) and (5), I use the area fixed effects instead of prefecture fixed effects. In Japan, here are ten areas based on Japanese governmental division, such as Hokkaido and Tohoku. As to the year fixed effects, I use the cohort fixed effects where I divide the entire sample into five-year groups. I use them because I also controlled their interactions and I need to avoid multicollinearity.

respondent went to an ECEC facility. Therefore, the key assumption of my identification is that variations in expansion after controlling for households' observable characteristics are quasi-random.

A potential problem is that the extension of ECEC is not quasi-random. One possible scenario is that expansion of the number of spaces in the childcare system is correlated with some unobservable prefectural characteristics and some time trends. To address this problem, I control for area fixed effects, cohort fixed effects, and their interactions in the regression. Moreover, I use a prefecture-level variation in ECEC availability so the estimate would be biased if there were household heterogeneity within the same prefecture. To eliminate this potential bias, I control for observable household characteristics, such as parents' education levels, parents' ages, and the number of siblings. Parents' education levels are often used to identify disadvantaged children. For mothers with a low education level, childcare use reduces stress and increases well-being (Yamaguchi et al. 2018b). Therefore, parents' education level may matter and should be controlled for. Similarly, if parents are young, they might lack knowledge about raising their children; thus, their children might be disadvantaged. Hence, it may be important to control for this factor. Finally, the effect may differ if a child has siblings: If there are many siblings, parents might be unable to spend sufficient time with each child and choose to use ECEC. Therefore, I control for these variables. Household income may matter; however, data on household income when children were aged four years are unavailable. Instead, I use data on categorised subjective wealth at age 15 years. However, this control may be of low quality, since this variable may be affected by the choice of preschool. Therefore, in the main analysis, I do not control for subjective wealth. However, I do so in the robustness check in Section 7.2. I make a dummy variable for whether the individual's subjective wealth was above or below average, assuming that respondents remember their family status at least roughly and that their family background did not change significantly.[53] The robustness check shows that the results do not change.

I also check whether the supply constraints were likely to be binding. As discussed above, Figs. 1 and 2 illustrate the lack of space for additional children in both kindergartens and nursery schools.[54][55] Furthermore, Table 3 shows the regression results for growth in the number of children enrolled in ECEC on the change in the number of births in the previous period at the prefecture level. This shows that, although the government might have increased ECEC availability when demand was high, coefficients of the explanatory variables are less than

[53] This dummy variable equals 1 if subjective wealth is below the median.

[54] Although data for the capacity of kindergartens and nursery schools for a specific age are unavailable, Figs. 1 and 2 indicate no further ECEC capacity for children of any age.

[55] As discussed earlier, as a robustness check, I drop cases where capacity might not be binding but the results do not change, according to Table 8.

one, implying that ECEC was not available to all children (i.e., the supply constraint was binding).[56]

Furthermore, Niimi (2002) states that local governments have difficulty finding private companies to operate a childcare system owing to the high initial costs. Moreover, many children did not attend kindergartens because of binding capacity or lack of nearby kindergartens (Minister of Ministry of Education's Secretariat Survey Division 1972; Motoki and Yamanishi 2009; Matsushima 2015). The government recognised the lack of ECEC in Japan in their discussion in the Diet and a committee (House of Councillors, The National Diet of Japan 1967b, a).

These facts suggest that the supply-side constraints were binding, and, therefore, the increase in enrolment rate can be a good candidate of an instrumental variable.

I further examine whether increases in demand for ECEC drove its supply at the prefecture level. If this were true, the expansion of ECEC could be endogenous, which would lead to a bias in the estimation. First, as I explained above, the government expanded ECEC facilities to support the female labour supply in Japan. However, the places where the government increased ECEC facilities do not seem to be chosen because of the demand. According to Matsushima (2015), the expansion had been done to mitigate the imbalances in the supply of ECEC facilities. More concretely, the government tried to fill this imbalance and prioritise areas based on the ratio of the capacity of ECEC facilities to the population of children, or the number of facilities per population (Matsushima 2015).[57] Together with the fact that I control for area and cohort fixed effects in the estimation, although this is not perfectly done because of the collinearity, this largely mitigates the potential endogeneity problems. Besides, this hypothesis is partially testable, so I analyse whether the demand of ECEC drove its supply. The most important factor behind the ECEC expansion is changes in the female labour supply. An increase in female labour supply at age 20–35, which corresponds to a woman's typical childbearing age, might increase demand for ECEC, thus inducing ECEC expansion. Therefore, I check whether an increase in female labour force participation in each prefecture can predict a future increase in ECEC capacity and the number of children enrolling. If so, demand would have driven supply. Specifically, I run the regression of the difference in ECEC availability on the difference in the female employment rate in the previous periods. Table 4 shows the results. I vary the time difference between the ECEC expansion and measurement timing of female labour supply, as it may take time to open a new ECEC facility and the supply may take time to reflect demand. However, the coefficients are not significant. This finding implies that an increase in female labour supply cannot predict future ECEC expansion. Thus, it is likely that demand did not drive supply.[58]

[56] As I discussed in Footnote 47, the government tried to let children at age five enrol in ECEC as much as possible, so they had priority over the others (Matsushima 2015). Therefore, children at age five could enrol as their parents wanted. This is reflected by the number here.

[57] As I explained in Footnote 6, this identification strategy is similar to Duflo (2001).

[58] The possible story behind the result is as follows: the Japanese government opened ECEC facilities, and then parents sent children to ECEC. The situation here is similar to Herbst (2017), who focuses on when the government urged women to work during World War II, which encouraged ECEC to expand further. Japan's situation seems similar because the Japanese government also tried to expand ECEC under rapid economic growth, and the government and firms urged women to work (Matsushima 2015). Hence, ECEC expanded. Given the above discussion, it is likely that ECEC expanded first, and then parents sent their children to ECEC based on its availability.

Table 3 Correlation between change in enrolment rate and change in birth rate in previous period

	Growth in Enrolment Rate from (t + Age) to (t + 1 + Age)					
	Age 0	Age 1	Age 2	Age 3	Age 4	Age 5
	(1)	(2)	(3)	(4)	(5)	(6)
Increase in Births	−0.007***	−0.014**	0.128	−0.017	0.456***	0.867***
from (t-1) to (t)	(0.002)	(0.006)	(0.117)	(0.036)	(0.101)	(0.083)
p-value for Testing	0.000	0.000	0.000	0.000	0.000	0.115
if the Coefficient is Different from 1						
R-squared	0.408	0.269	0.146	0.299	0.490	0.471
N	1117	1164	1165	1166	1167	1121

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. This analysis uses data from 1960 to 1985. There is some attrition mainly because Okinawa, a prefecture in the south of Japan, became a part of Japan in 1972, and data are not available before then. When I take the 6th difference, some variables could not be defined because of the data range. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Besides, as mentioned in Section 3, I check quantitatively whether the ECEC expansion was based on the initial coverage rate of the ECEC, i.e. whether the government prioritised the construction of ECEC facilities where the coverage rate was low. If there were a correlation between the growth in the ECEC availability and important labour market characteristics, such as the fraction of female labour supply, the exclusion restriction would fail. Following the analysis of Cornelissen et al. (2018), I regress the growth in the ECEC enrolment rate of the age of four from 1960 to 1985 on the coverage rate of ECEC of the age of four in 1960 and other covariates in 1960. Table 5 shows the result.[59] It confirms that ECEC expantion was based on the initial coverage rate and shows that other seemingly important factors, including the fraction of female labour supply, education level, population, and the mean wage, did not affect the expansion.[60][61]

Another potential threat to the violation of the exclusion restriction would be a correlation between a prefecture's investment in ECEC and that in higher education.

[59] One difference from Cornelissen et al. (2018) to be noted is that I do not include the variable on the fraction of foreign labour force participation. There are two reasons. First, in the First Employment Measures Basic Plan in 1967, the Japanese government clarified that they did not accept foreign people to work, and this attitude had not changed until 1985 (Shimizu 2008). Therefore, the effects must be minimal, and it is very implausible that the foreign labour force participation affected ECEC expansion. Second, there is no data on the foreign people labour force. Therefore, I omit the variable on the foreign labour force participation.

[60] Note that the determinant of construction of the ECEC facilities is similar to that of Duflo (2001), who discusses that the number of schools constructed was proportional to the number of children of primary school ages who were not enrolled in school.

[61] The sample size is not large because there were only 46 prefectures in Japan in 1960 and some might think the estimation would be imprecise. However, the coefficient of the initial coverage is precisely estimated and those of other variables are much smaller in absolute value. Furthermore, including additional controls does not change the estimated coefficients of the initial coverage rate, which implies that the coverage in 1960 drove the expansion and others did not.

Table 4 Exogeneity check

	Growth in the Number of Enrolments in ECEC from (t-1) to (t)					
	j = 1	j = = 2	j = 3	j = 4	j = 5	j = 6
	(1)	(2)	(3)	(4)	(5)	(6)
Growth in the Number of Enrolments Percentage	−0.267	0.149	−0.070	0.012	0.398	0.149
Change of Female Employment from (t - j) to (t - j - 1)	(0.219)	(0.111)	(0.115)	(0.072)	(0.273)	(0.189)
R-squared	0.178	0.177	0.176	0.176	0.179	0.178
N	1164	1163	1162	1161	1160	1113

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. This analysis uses data from 1960 to 1985. There is some attrition mainly because Okinawa, a prefecture in the south of Japan, became a part of Japan in 1972, and data are not available before then. When I take the sixth difference, some variables could not be defined because of the data range. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

As discussed in Section 3, if a prefecture is eager to invest in education, its quality of education might be high. That is, the expansions of ECEC and later education, such as universities and colleges, might be correlated. If so, the assumption of the exclusion restriction would be violated. However, in my data, the correlation between the ECEC expansion in enrolment/capacity and college expansion in capacity is 0.061. Hence, this potential threat seems minimal.[62]

Moreover, I check whether the differences in education levels across areas could explain the speed of ECEC expansion. As discussed earlier, the education level of parents, especially mothers, might matter for children's enrolling in ECEC. Figure 8 from Ueyama (2011) shows the fraction of women who enrolled in a university: there are almost no differences in the enrolment rate and its expansion speed before the 1990s, implying that education level does not drive ECEC supply.

In the model section, I assume that people did not move from their place of birth until age 15. This assumption might seem extreme: I discuss its validity and the possible bias if the assumption were to be violated (i.e., if many people moved between the ages of 4 and 15). There are two types of movements: across prefectures and within the same prefecture. If there are many observations of the first type, the IV regression estimator would be biased. If movement occurred randomly, there would be attenuation bias. Suppose that these movements were not random. Note that the IV for enrolling status is the propensity score of ECEC enrolling, implying that its coefficient in the first stage should be positive. In this case, the bias might be upward if people moved from a prefecture with a high enrolment rate to a prefecture with a low enrolment rate, or vice versa. However, according to the National Survey on Migration in the Annual Population and Social Security Survey collected in 1996, over 80% of people remained in their birth prefecture for 15 years. Although this does not correspond exactly to the

[62] As discussed above, I conduct a control experiment to further examine the potential violation of the exclusion restriction. See Section 7.2 in the result section for details.

Table 5 Determinants of the ECEC Expansion

	Dependent Variable: Growth in ECEC Enrolment Rate at Age 4 from 1960 to 1985	
	(1)	(2)
Initial Coverage Rate	−0.465***	−0.471***
	(0.057)	(0.086)
Mean Wage		−0.000
		(0.000)
Share of High Educated		0.605
		(1.218)
Share of Medium Educated		−0.346
		(0.392)
Population (in 1,000s)		0.000
		(0.000)
Share of Women in Labour Force		−0.051
		(0.392)
Constant	0.780***	0.951***
	(0.022)	(0.263)
p-value for Joint Significance of Other Covariates		0.880
(Excluding Initial Coverage Rate)		
R-squared	0.445	0.465
N	46	46

The procedure follows Cornelissen et al. (2018). The information of the covariates are the records in 1960. Bootstrapped standard errors with 1,000 replications are reported in parentheses. Okinawa prefecture is excluded from this analysis because some information in 1960 is not available. The middle educated people graduated from a junior high school, a youth training school (in an old style), a middle school (in an old style), or a senior high school. The high educated people finished a high school (in an old style), a junior high school, a university, or a post graduate course. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

main survey's data years and does not include only data on children at the aged four, the overall data imply that this problem is not severe enough to invalidate my estimation.

The second case matters if people moved to seek ECEC availability. As discussed earlier, the supply constraint of enrolment/capacity in the childcare system seems to have been binding. Therefore, it is unlikely that people migrated to seek ECEC availability. Accordingly, the assumption on non-migration is likely to hold.

As I use the propensity score, it is necessary to check whether the assumption of common support holds. Figure 9 shows the probability density distribution of the propensity score for people with and without enrolling in ECEC, and the assumption is likely to hold.[63] I also assume the monotonicity assumption for the IVs, as I use the propensity score as an IV.

[63] Even after omitting observations outside the common support, the point estimations are similar.

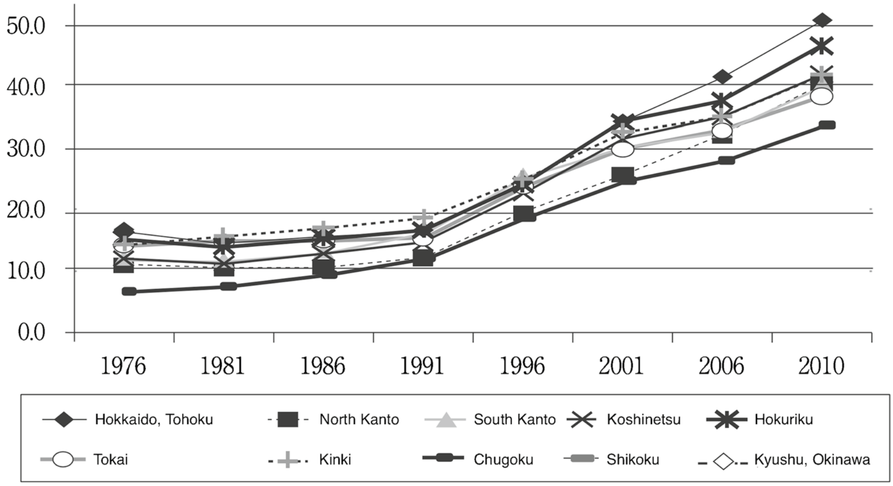

Fig. 8 Female enrolment rate in a university across regions in Japan. This figure is from Ueyama (2011) with labels translated by the author from Japanese into English. The table does not include women who attended a two-year college

7 Results

7.1 Main results

Here, I discuss the estimation results of the OLS regressions, reduced-form analysis, and IV analysis discussed in Section 5. As stated, I focus on enrolment at age four and show the results of the effects of enrolment in the entire childcare system rather than nursery schools and kindergartens separately.[64] I drop observations that do not answer questions of interest. See Appendix A for the sample selection. However, there are no systematic attrition patterns.

As shown in Table 23 in Appendix D, the first stage results are sufficiently strong for IV regressions, including those of the subsample analyses, regardless of the specification. As stated in Section 6, the assumption of the exclusion restriction is likely to hold. Therefore, I use this IV.

Table 6 shows the OLS results based on Eq. (1); those of the reduced form based on Eq. (6); and those of the two-stage least squares estimators based on Equations (1), (4), and (5). As shown in the table, enrolling in ECEC increases children's future income by approximately 44% and raises college completion rate by 38 percentage points (both statistically significant).[65][66]

[64] See Appendix C of Kawarazaki (2022) for the separate results.

[65] I conduct more analyses with other outcomes as a dependent variable, such as wage, working status, working hours, personal characteristics such as risk preference and Big Five index, and health-related behaviours such as smoking and drinking. See Appendix B. The analysis on health behaviours is comparable to Conti et al. (2016). See the discussion on potential mechanisms in Section 8.

[66] Unfortunately, I do not have data on maternal employment when their children were of ECEC age. Such an analysis would have been beneficial. However, I analyse the effect of ECEC expansion on change in female labour supply with reduced-form specification in the section of potential mechanisms, Section 8. See the discussion on the effects of ECEC on maternal employment.

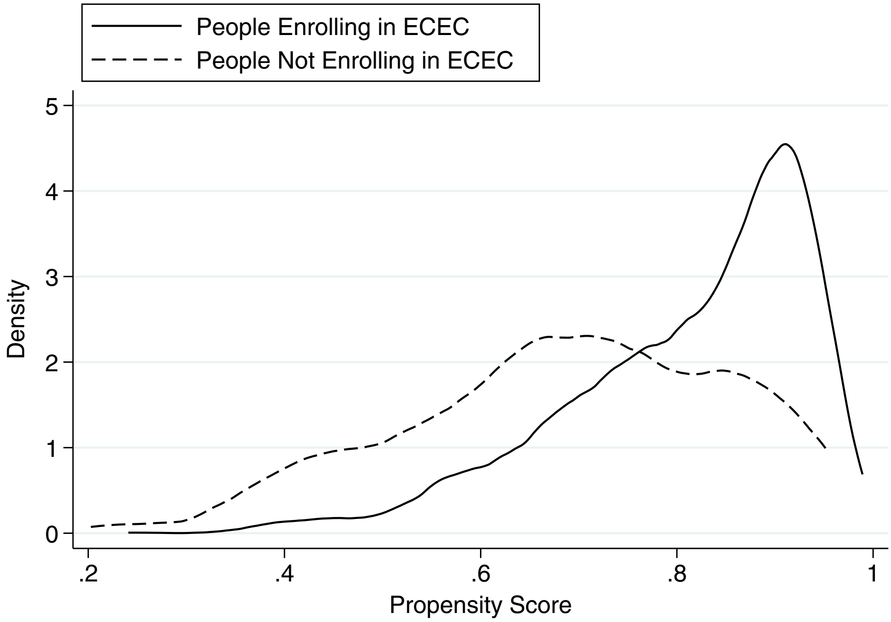

Fig. 9 Propensity score of enrolling in ECEC at age four

The value of 44% may seem excessive. However, compared with studies showing the short- and long-term effects of ECEC, the estimate is quite reasonable. According to Yamaguchi et al. (2018b), ECEC develops children's language skills by 0.7 standard deviations and reduces the tendency of inattention and hyperactivity by 0.4. I conduct the same analysis with the normalised measure of income with mean 0 and standard deviation 1, and I find that ECEC increases the income by 0.5 standard deviations. Furthermore, Heckman et al. (2013) report that the Perry Preschool Project increases the income of the enrolled children 1.5 times by age 27 compared with those who did not enrol. My estimation is quite comparable to theirs. Moreover, Havnes and Mogstad (2011b) report the long-term effects of subsidised childcare on years of schooling; their estimated TT effect shows an additional 0.35 years of education per childcare place. This is smaller than my estimation. One possible explanation is that their expansion of ECEC was from 10% to 28% of the enrolment rate, which is smaller than my variation; therefore, mine might include more disadvantaged children who benefit more from ECEC. An alternative explanation is that they estimate TT effects, while I estimate LATE. The former includes those who did not go to ECEC in the treatment group and vice versa. Therefore, given that ECEC has positive effects, the estimates may be smaller.

The OLS estimators are smaller than IV estimators. This implies that unobserved household characteristics are negatively correlated with enrolling in ECEC. One

Table 6 Effects of ECEC at Age 4

	log(Income) (in million yen)				College Completion (Complete = 1)			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ECEC Enrolment at Age 4	−0.027	−0.029	0.259**	0.440**	0.103***	0.087***	0.248***	0.375***
(Enrol = 1)	(0.063)	(0.060)	(0.116)	(0.199)	(0.032)	(0.031)	(0.078)	(0.137)
Less Educated Mothers		0.067	0.065	0.091		0.045	0.043	0.051
(Less Than High School Completion = 1)		(0.079)	(0.078)	(0.077)		(0.042)	(0.043)	(0.040)
Less Educated Fathers		−0.088	−0.079	−0.068		−0.056	−0.052	−0.051
(Less Than High School Completion = 1)		(0.102)	(0.101)	(0.102)		(0.066)	(0.067)	(0.066)
Less Educated Parents		−0.092	−0.094	−0.108		−0.245***	−0.245***	−0.245***
(Less Than High School Completion = 1)		(0.123)	(0.122)	(0.118)		(0.073)	(0.074)	(0.071)
Gender		−0.971***	−0.971***	−0.973***		0.029	0.029	0.030
(Female = 1)		(0.084)	(0.084)	(0.084)		(0.031)	(0.032)	(0.031)
Other Controls		x	x	x		x	x	x
Fixed Effects	x	x	x	x	x	x	x	x
R-squared	0.076	0.358	0.359	0.323	0.065	0.132	0.131	0.086
N	1915	1915	1915	1915	2647	2647	2647	2647

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable.If any control is missing, I substitute a zero for the missing value zero and include a dummy variable to control for the substitution. I exclude the sample who lived outside of Japan or whose living place is unknown at age 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the OLS are estimated based on Eq. (1), those of the reduced form are based on Eq. (6) and those of the IV are based on Eqs. (1), (4), and (5). Columns (1), (2), (5), and (6) show the result of the OLS, (3) and (7) show those of the reduced-form analysis, and (4) and (8) show those of the IV approach. As to the income, US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. For the further controls, I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. Fixed effects include the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

possible explanation is that relatively disadvantaged families rely on ECEC because these parents are willing to work to earn. Moreover, ECEC facilities' environment may be better than the home environment because of the minimum requirements required to operate ECEC. As discussed later in the subsection of the robustness check, the result of reduced form shows a similar tendency, although the estimation is smaller because of the specification of the reduced form Angrist and Pischke (2008), and the point estimation is comparable to the result of Havnes and Mogstad (2011b).

I further conduct analyses of the effects of ECEC on children's working status (i.e., whether they work), working hours, and wages. The result is in Table 17 in Appendix B.[67] The table shows that there is no effect on the working status and working hours, but there is on the wage. According to this, the interpretation of the effects of ECEC is that if a child enrols in ECEC, that child is more likely to finish college and earn 44% more through an increase in wages. This result is consistent with human capital accumulation theory (Mincer 1974; Becker 1994; Kane and Rouse 1995; Thomas 2003).[68][69]

Table 7 shows the results of the subsample analyses focusing on gender heterogeneity. This table shows no effects on income and college completion likelihood for men. On the contrary, there is a large and statistically significant effect of enrolment in the childcare system on future income and college completion probability for women. This means that most of the effects in Table 6 come from those on women's outcomes. As discussed earlier, women's enrolment rate in higher education is much lower than that of men (Fig. 6). Moreover, the wage gap is very large (Fig. 7). This implies that there was considerable room for increasing the enrolment rate and wages.

As discussed in Section 3, although the enrolment rates in ECEC were similar in both genders, women were disadvantaged in enrolling in higher education and wages in adulthood. It seems that these disadvantages at least partially reflected the cultural perspective that a woman should stay at home and care for her family. Given this situation, the result can be interpreted as that the effects are stronger for disadvantaged children. This is consistent with the analysis of the short-term effects for disadvantaged children by Yamaguchi et al. (2018b), although they define

[67] I also conduct analyses of the effects on Big Five components and risk prefences. However, there are no long-term effects on psychological skills measured by risk preference and the Big Five (Tables 18 and 19) in Appendix B.

[68] I also examine whether there are age effects by including the age variable and the interaction of age and ECEC enrolment in the regression model. However, the estimates are imprecise.

[69] The two OLS specifications' coefficients are not different, but that of IV is different from them, maybe because of the way of constructing the propensity score. When I created the probability of enrolling in ECEC, the IV in the main analysis, I used the parents' education level, which is also controlled in the main IV regression. I used this specification because the probability of enrolment can be defined as both the quasi-random variation (expansion of ECEC) and parental education level. After making this IV, not controlling parental education levels caused bias by construction (upward because parental education level and probability seem positively correlated), and so I controlled for it in the main IV regression. This is the source of the difference in the two specifications.

Table 7 Heterogenous effects of ECEC at age 4

	log(Income) (in million yen)		College Completion (Complete = 1)	
	Female Sample	Male Sample	Female Sample	Male Sample
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
ECEC Enrolment at Age 4	0.729**	0.172	0.433**	0.320
(Enrol = 1)	(0.346)	(0.189)	(0.174)	(0.199)
Less Educated Mothers	-0.060	0.123	0.041	0.050
(Less Than High School Completion = 1)	(0.137)	(0.089)	(0.040)	(0.068)
Less Educated Fathers	−0.016	−0.065	0.029	−0.109
(Less Than High School Completion = 1)	(0.185)	(0.088)	(0.065)	(0.112)
Less Educated Parents	−0.120	−0.102	−0.345***	−0.166
(Less Than High School Completion = 1)	(0.223)	(0.119)	(0.081)	(0.119)
				x
Other Controls	x	x	x	x
Fixed Effects	x	x	x	x
R-squared	0.022	0.306	0.096	0.143
N	893	1022	1458	1189

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute a zero for the missing value zero and include a dummy variable to control for the substitution. I exclude the sample who lived outside of Japan or whose living place is unknown at age 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of IV are based on Eqs. (1), (4), and (5). All columns show the result of the IV approach. As to the income, US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. For the further controls, I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. Fixed effects include the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

disadvantaged children as those with less-educated mothers.[70] These results imply that ECEC can reduce the inequality between the advantaged and disadvantaged by raising the outcomes for disadvantaged children to a greater extent. This also implies that ECEC enrolment could break the intergenerational poverty chain because this early childhood intervention reduces the inequality in their adulthood. The finding that the effects for women are larger is consistent with the results of the shrinking wage gap owing to ECEC reported by Havnes and Mogstad (2011b).

Furthermore, I investigate the marginal treatment effects. Figure 11 in Appendix D shows that the children who are likely to enrol in ECEC in terms of the

[70] Additional analysis shows the same result as that of Yamaguchi et al. (2018b): Children with less-educated parents benefit more from enrolling in ECEC, as shown in Table 27 in Appendix D.

unobserved heterogeneity benefit from ECEC more. The effect is significantly positive for around 40% of the children, while there is no negative impact uncovered for other children.

These results are robust based on the robustness check discussed in the next subsection.

7.2 Robustness checks

Here, I examine whether the results shown in the previous subsection are robust, by conducting further analyses. The analyses include another type of estimation, other specifications of the propensity score estimation, a slightly different sample, and other specifications of the main regression equations. The results are summarised in Table 8.[71]

First, I conduct an analysis based on the reduced-form approach, which requires fewer assumptions.[72] If one assumption for the LATE identification used in the main analysis, which is the variation in ECEC availability, is exogenous, the reduced-form approach is also valid. This is the case even if any additional assumptions on the LATE identification were to fail, such as any correlations between parental characteristics and prefecture-level exogenous variations in the rate of expansion. Column (1) in Table 8 presents the results from the reduced-form approach. The results show a lower point estimation, as discussed by Angrist and Pischke (2008) and predicted in Section 1, but have a similar result and consistent with the main results.[73]

Next, I use two additional specifications of the propensity score estimation to estimate the same effects. In the main analysis, I use a model with controls for gender, prefecture, area fixed effects, and a standard error based on the prefecture level. Another specification is based on the Probit model, but the controls are different. Since the IV must be a function of exogenous variables, there is an infinite number of candidates. Here, I control for the cohort (every five years) fixed effects, area fixed effects and their interactions, whereas I use cohort fixed effects and prefecture fixed effects in the main analysis. Another specification is based on the linear probability model instead of the Probit model. Column (2) of Table 8 shows the result with different fixed effects, and the results from the linear probability model are in Column (3). Table 28 reports coefficients of the regressions and predictions of the propensity scores under different specifications (see Appendix A). Based on these analyses, the results are robust.

Third, I omit observations in the domain when either of the propensity scores is not positive. This ensures that the analysis is based on a common support. The result shown in Column (4) of Table 8 is also robust.

[71] The results on other dependent variables are in Table D5 and D6 in Appendix D of Kawarazaki (2022).

[72] The estimation is the same in the tables of the main result.

[73] The point estimation of the reduced form is not statistically different from that of IV, based on their standard errors.

Table 8 Robustness Checks

	Dependent Variable: Log(Income)											
	Reduced Form Form	IV	IV	IV	IV	IV	IV	IV	IV	Interval Regression	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Different Specifications												
Fraction at Age 4	0.256**											
	(0.115)											
ECEC Enrolment at Age 4		0.625**	0.581**	0.382*	0.543**	0.327	0.263	0.567***	0.441**	0.503***		
(Enrol = 1)		(0.283)	(0.258)	(0.210)	(0.228)	(0.208)	(0.175)	(0.208)	(0.201)	(0.181)		
Controls	x	x	x	x	x	x	x	x	x	x	x	x
Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
Different Propensity Score												
Different Fixed Effects		x										
Linear Probability			x									
Common Support				x								
More Controls					x							
Initial Capacity						x						
Selective Samples												
Drop ECEC at Age 3							x					
Drop ECEC at Age 2								x				
Different Cluster									x			
Panel B: Restriction on Kindergarten Capacity Binding												
ECEC Enrolment at Age 4											0.216	0.500**
(Enrol = 1)											(0.191)	(0.246)
Omit Obs. with Kindergartens											x	
Where Capacity is less than												
the # of the Enrolled												
Omit Obs. with Kindergartens												x

Table 8 (continued)

	Dependent Variable: Log(Income)											
	Reduced Form Form	IV	IV	IV	IV	IV	IV	IV	IV	Interval Regression	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Accepting More than Capacity												
p-value of Testing the Coefficient of ECEC is different from the Value of the main analysis											0.224	0.831
R-squared	0.358	0.286	0.297	0.332	0.306	0.340	0.394	0.298	0.321	–	0.365	0.302
N	1915	1851	1915	1872	1915	1890	1194	1766	1915	1915	1180	1767

All the regressions include the controls and the fixed effects explained below. Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses except for Column (8), where I include the initial enrolments/capacities of ECEC measured in 1968 and the interaction term of this and cohort fixed effects. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute a zero for the missing value zero and include a dummy variable to control for the substitution. I exclude the sample who lived outside of Japan or whose living place is unknown at age 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the OLS are estimated based on Eq. (1), those of the reduced form are based on Eq. (6) and those of the IV approach are based on Eqs. (1), (4), and (5). As to the income, US \$1 $\approx$ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. In Column (10), the interval regression analysis with the control function approach is conducted. See Appendix C of Kawarazaki (2022) for the detail. The command for interval regression does not offer the standard error here. In Panel A, the variable of "Fraction at Age 4" is the fraction of the number of enrolments/capacities in ECEC in each prefecture in each cohort to the population of the the cohort in the prefecture made by Eq. (2). In all regressions, I control for the number of younger siblings, the number of older siblings, parents' ages, parents' education levels, and the gender dummy. For the more controls, I further include the dummy for big cities, the age and its polynomials of degree up to three, and the dummy for the subjective wealth at the age 15. The definition of big cities is based on the definition of Metropolitan Area (called Daitoshi-ken) by the Ministry of Internal Affairs and Communications. This includes the following cities (prefectures covering the areas): Sapporo (Hokkaido), Sendai (Miyagi), Tokyo, Yokohama, Kawasaki, Chiba, Saitama, Sagamihara (Tokyo, Kanagawa, Chiba, Saitama), Niigata (Niigata), Shizuoka, Hamamatsu (Shizuoka), Nagoya (Aichi), Kyoto, Osaka, Kobe, Sakai (Kyoto, Osaka, Hyogo), Okayama (Okayama), Hiroshima (Hiroshima), Kitakyushu, Fukuoka (Fukuoka), and Kumamoto (Kumamoto). The variable on the subjective happiness at the age 15 has 11 categories (0 (poorest) to 10 richest). The dummy is equal to 1 if the index reported is equal to or below five. All regression control for the cohort fixed effects, area fixed effects, and their interactions. In Column (2), the propensity score is estimated using the same fixed effects used above. In Column (3), the propensity score is estimated using the linear probability model. In Column (4), I omit the observations outside of the support where both the treated and the untreated are exist. In Column (7), I exclude the observations who enrolled in ECEC at age three. In Column (8), I exclude the observations who enrolled in ECEC at age two and endogenise the choice of enrolling in ECEC at age three, using a similar instrumental variable to that for the enrolment at age four. In Column (9), I use the prefecture-level cluster. R-squared is not reported for the interval regression model, which is based on a binary choice model. *$p <$ 0.1; **$p < 0.05$; ***$p < 0.01$

Fourth, I add more controls to the main regression equation. In the main analysis, I already include parental education levels, number of siblings, and parental age. However, other variables can also represent household background, such as the retrospective subjective wealth index (0 is the poorest, and 10 is the wealthiest) at age 15. While this categorisation can change depending on the treatment status, it may be an informative indicator of household wealth. Therefore, I create a dummy variable equal to one if the index is less than five, and zero otherwise. This is because it is less likely to change depending on the treatment status than the original 11-scale index. Other variables could be considered, such as prefectures in which the individuals lived at age 15 and the age of children. However, the inclusion of these variables in the regression may cause a problem because, as noted in Section 6, the identification relies on the variation across prefectures over time. Therefore, it is not possible to control for both age and prefecture simultaneously. Given that the cohort fixed effects and area fixed effects are already controlled for, additionally controlling for age (and its polynomial) and prefecture might dramatically reduce the sample size in each age-prefecture cell (some of them have only one observation), markedly deteriorating the estimator's accuracy. Therefore, I show the regression results including age only. I conduct an analysis with the variables on age and its polynomial up to three, where each cell has more observations than cells with variables on prefectures. The result in Column (5) of Table 8 is robust.

Fifth, similar to Duflo (2001) discussed in Section 2, I control for ECEC intensity, which is the initial capacity/enrolment of ECEC of each prefecture, and the interaction of this and cohort fixed effects. I do so because a potential threat of the identification is that children with initially more availability in ECEC could enrol in ECEC, as the prefecture was eager to invest in ECEC, which is correlated with other unobservable characteristics. I check this by controlling the initial availability and interactions between these and cohort fixed effects. However, as shown in Column (6), the coefficient is comparable, implying that this type of eagerness may not matter much.[74]

Sixth, I omit samples who enrolled in ECEC at age three to examine the marginal effects of ECEC at age four. In addition, I omit samples who went to a nursery school or kindergarten at age two and endogenise the choice of enrolment at age three. I then estimate the effects of enrolment at ages three and four separately using a similar IV for enrolment at age three. Columns (7) and (8) of Table 8 report results of the former and latter analyses, respectively, which support the results' robustness.

Seventh, I use different clusters from those in the main analysis. In the main analysis, I use clusters at the level of the area, cohort, and their interaction. Column (9) of Table 8 presents results based on the prefecture-level standard error, which is only slightly higher than the standard error in the main analysis.

Eighth, as discussed earlier, the data on income are categorised, and I use class values of each category in the main analysis.[75] In the analysis, I use each category's

[74] I use the initial availability measured in 1968, just before the expansion. This is because the data for Okinawa are available from this year. I also check this check's robustness by using the values measured in 1960, excluding the observations of Okinawa, but the result is quantitatively similar.

[75] See Table 14 in Appendix A for details.

median as the income variable. I assume that the difference between individuals' original income and reported value (median value of the category) is i.i.d. Although the standard errors become larger, as shown in Table 6, the estimator remains positive and significant, and enrolment in the childcare system is likely to positively affect income. As a robustness check, I also conduct an interval analysis, which considers this data-coding problem (Cameron and Trivedi 2005). Since the objective function is the IV regression, not the OLS, I adopt the control function approach to eliminate potential endogeneity (Cameron and Trivedi 2005). The result in Column (10) of Table 8 supports the main results' robustness.

Ninth, in some cases, kindergartens might accept more students than the capacity (Fig. 1. So I conducted an analysis by dropping such observations for children who went to a kindergarten. The result shown in Column (11) implies that the magnitude of the effects is comparable, although it becomes insignificant partly because of the smaller sample size. On the other hand, there were several cases where the capacity was larger than the enrolment, while the capacity was actually binding (Matsushima 2015). This likely happened because the timings of the report of enrolment and capacity are different owing to the construction of statistics, which is the basis of my data. In some cases, children left before the time of counting the number of children enrolled. I omitted samples who went to a kindergarten whose capacity exceeded the number of children enrolled. The result is in Column (12). This made the sample size very small and the estimation imprecise, leading to an insignificant estimation. However, I test whether the estimate is different from the value of the point estimate of the main analysis and cannot reject the hypothesis that they are different, which supports that this problem does not seem to matter.

Finally, I conduct a control experiment following Duflo (2001), which I discussed in Section 2. Particularly, I conduct a reduced-form analysis wherein the main independent variable is the ECEC expansion three years after the relevant period for the cohort. Therefore, this expansion was not beneficial for those cohorts, and the coefficient should be zero.[76] Table 9 reports the results, showing that the coefficients are not statistically different from zero. This result supports the exclusion restriction and the validity of the main result in this section.

8 Potential mechanisms

8.1 Mediation analysis

Here, I analyse the extent to which the effects on future income can be explained by the increase in college completion induced by enrolment in childcare systems. I conduct a mediation analysis for this purpose. This analysis is common in the psychological

[76] I adopt the reduced-form analysis because if availability in the previous period does not matter for parents' choice on ECEC, the first stage must be weak, and we cannot interpret the second stage results well. If we look at the reduced form, we can discuss the same but more clearly.

literature and some social science fields (Baron and Kenny 1986; Imai et al. 2010; Hayes 2018). In the basic model of mediation analysis, if my treatment is endogenous, two IVs would be needed: for the treatment variable and the mediating variable. Therefore, I create an additional IV. The regression in the simple mediation analysis is as follows:

$$y_{ijT} = \boldsymbol{x}'_{it}\boldsymbol{\beta} + d_{ijt}\gamma + m_{ijt'}\alpha + f(\mu_j, \nu_t) + \varepsilon_{ijT}, \tag{7}$$

where y_{ijT} is the outcome at T, such as logarithm of income; d_{ijt} is a dummy for the treatment, such as childcare enrolment; and $m_{ijt'}$ is a mediating variable, such as college completion, where $t' = t + 14$.[77][78][79] I denote d_{ijt} as childcare enrolment (enrolled = 1) and $m_{ijt'}$ as college completion (completed = 1). γ in Eq. (7) captures the direct effects. Neither d_{ijt} nor $m_{ijt'}$ is exogenous, and, therefore, I need two IVs to estimate the coefficients consistently. I assume that the effect is homogeneous. That is, I do not permit treatment–mediator interaction effects and, thus, heterogeneity in direct and indirect effects. This follows Burgess et al. (2014). As discussed in Footnote 78, I find no such effect. However, I loosen this assumption later by partly using a reduced-form approach.[80]

If most ECEC effects on income could be explained by college completion owing to childcare when young, the coefficient of d_{ijt}, γ, would become insignificant once $m_{ijt'}$ is inserted into the model. Otherwise, there would be channels from childcare to income other than the path of college completion. This might include the effects on non-cognitive abilities. However, when comparing coefficients, this method requires caution. The coefficients in an IV analysis are often interpreted as LATE. If I were to use two IVs, the support could be different. Here, I focus on people who change their behaviours based on the IVs.

As in the previous analysis, I assume that people did not move to another prefecture after age 15, which is also asked about in the survey. However, according to the National Survey on Migration in the Annual Population and Social Security Surveys collected in 1996, around 85% of people did not move from their prefecture after age 15. Therefore, this assumption is unlikely to be violated.

[77] Definitions of other controls are the same as in the previous analysis.

[78] Here, I assume that the treatment effect of ECEC, given that the college completion status is fixed, is not dependent on the college completion status, that is, $E[Y|D = 1, M = m] - E[Y|D = 0, M = m]$ is not a function of m, where capital letters indicate a random variable. I conduct an additional analysis that relaxes this assumption by adding $D \cdot M$ into Eq. (7). Although the estimation may be inaccurate, its coefficient is not statistically significant, indicating no heterogeneity with respect to m. The coefficient of D is also not statistically significant. Therefore, the result is consistent with respect to these specifications.

[79] As discussed in Section 3 and below, in Japan, most people enter college at age 18. Therefore, I assume that people start college at 18 years.

[80] In other words, I assume constant treatment effects after controlling, for example, for gender and parents' education. This may seem a slightly strong assumption; however, I allow heterogeneity in the effects with respect to the controls. Instead of using the constant treatment effect assumption, I conduct a robustness check by including a fraction of capacity of ECEC to the population in the prediction of propensity score of enrolling in a college. This is to consider the endogeneity of the enrolment of ECEC in the enrolment in colleges and universities. However, the result does not change.

Table 9 Control experiment

	log(Income) (in million yen)		College Completion (Complete = 1)	
	Reduced Form	Reduced Form	Reduced Form	Reduced Form
	(1)	(2)	(3)	(4)
Fraction of ECEC Enrolment Three Years	−0.078	−0.079	0.006	−0.040
Before for Age 4	(0.105)	(0.076)	(0.044)	(0.045)
Gender		−1.007***		0.022
(Female = 1)		(0.089)		(0.037)
Less Educated Mothers		0.082		0.000
(Less Than High School Completion = 1)		(0.087)		(0.049)
Less Educated Fathers		0.017		−0.046
(Less Than High School Completion = 1)		(0.112)		(0.068)
Less Educated Parents		−0.224*		−0.270***
(Less Than High School Completion = 1)		(0.127)		(0.076)
Other Controls		x		x
Fixed Effects	x	x	x	x
R-squared	0.085	0.384	0.065	0.154
N	1401	1401	1951	1951

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. This first stage is for the regression where the log(income) is the outcome variable. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute a zero for the missing value zero and include a dummy variable to control for the substitution. I exclude the sample who lived outside of Japan or whose living place is unknown at age 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression is based on Eq. (4). All columns show the result of the reduced-form analysis. US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. Colleges include both a four-year university/college and a two-year college. In all the regressions, I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and dummy variables indicating missings for each control. Furthermore, the area (10 areas over Japan) fixed effects, cohort (5 year unit) fixed effects, and their interactions are controlled. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

First, I discuss how I develop the additional IV. As in the main analysis, I construct the IV as the ratio of the capacity of colleges to the population in cohort t in prefecture j, r^c_{jt}:

$$r^c_{jt} = \frac{n^c_{jt}}{N^c_{jt}}, \tag{8}$$

where n^c_{jt} is the capacity of colleges in prefecture j in cohort t, and N^c_{jt} is the population in prefecture j in cohort t at age 18.

Similar to the construction of the IV for childcare enrolment, I estimate the propensity score using a probit model:

$$
\begin{aligned}
m^*_{ijt} =& \boldsymbol{x}'_{0i}\lambda^c + r^c_{jt}\phi^c + h(\eta^c_j, \psi^c_t) + e^c_{ijt}, \\
m_{ijt} =& \begin{cases} 1 \text{ if } m^*_{ijt} > 0 \\ 0 \text{ otherwise,} \end{cases}
\end{aligned}
$$

eqnarray where $\boldsymbol{x}_{0i}$ is individual i's gender; η_j is the prefecture fixed effect; ψ_t is the cohort fixed effect; h is a function of the fixed effects; and e^c_{ijt} is an error term, which is assumed to be i.i.d. over the normal distribution with mean 0 and variance 1. $h(\eta^c_j, \psi^c_t)$ is a function of fixed effects. $\boldsymbol{x}_{0i}$ can include other variables, such as mothers' and fathers' education levels and subjective wealth level at age 15, depending on the specification. Define $\hat{m}_{ijt}$ as the probability of enrolment in ECEC for individual i in prefecture j in cohort t:

$$
\begin{aligned}
\hat{m}_{ijt} \equiv& \Pr\left(m_{ijt} = 1 | \boldsymbol{x}_{0i}, r^c_{jt}, \eta^c_j, \psi^c_t\right) \\
=& \Phi\left(\boldsymbol{x}'_{0i}\hat{\lambda}^c + r^c_{jt}\hat{\phi}^c + h\left(\hat{\eta}^c_j, \hat{\psi}^c_t\right)\right),
\end{aligned}
\tag{9}
$$

where $\Phi(\cdot)$ is the standard normal cumulative distribution function.

Following the discussion on the IV for childcare enrolment, although r^c_{jt} can be an IV, I use $\hat{m}_{ijt}$ as the IV in my main analysis, because it provides a more efficient estimation (Wooldridge 2010).

8.2 Identification and result of the analysis of the mechanisms of ECEC's long-term effects

For the mediation analysis, I construct a new IV: ratio of college capacity to the cohort's population at age 18. Because of data restrictions, I use data on the cohorts born between 1962 and 1985. In Japan, most people enter college at age 18. Thus, I focus on the period from 1980 to 2003. Before the Private Schools Act was reformed in 1974, private universities required prior approval from the government to increase college capacity (Kikuchi 2017). In addition, before 2003, the School Education Act required that all universities obtain government approval before opening and closing departments, with private universities also needing approval to change capacity at the department level. The permission process included a one- or two-year screening and investigative process, such as interviews by the MEXT based on the Standards for Establishment of Universities. Hence, changing capacity was difficult (Kikuchi 2017). Therefore, I use the proportion of the cohort's population capacity as the other IV.

As Tables 24 and 25 in Appendix D show, IVs for the first stage result are not jointly weak regardless of the specification. Based on these valid IVs, Table 10 shows the second stage results. Columns (1) and (3) show the results of the same analysis in Table 6, except that I omit samples without information on the propensity score for college enrolment.[81] As presented in Table 10, the effect of college completion is positive

[81] There is no systematic difference in the estimates between Tables 6 and 10.

Table 10 Effects of ECEC at Age 4: Mediation analysis

	Dependent Variable: log(Income) (in million yen)			
	IV	IV	IV	IV
	(1)	(2)	(3)	(4)
ECEC Enrolment at Age 4	0.607**	0.416	0.430*	0.215
(Enrol = 1)	(0.248)	(0.262)	(0.261)	(0.268)
College Completion		0.459**		0.846***
(Complete = 1)		(0.209)		(0.261)
Gender			−0.935***	−0.979***
(Female = 1)			(0.087)	(0.091)
Poorly Educated Mothers			0.076	0.066
(Less Than High School Completion = 1)			(0.077)	(0.087)
Poorly Educated Fathers			−0.084	0.036
(Less Than High School Completion = 1)			(0.101)	(0.106)
Poorly Educated Parents			−0.084	0.029
(Less Than High School Completion = 1)			(0.117)	(0.128)
Other Controls	x	x	x	x
Fixed Effects	x	x	x	x
R-squared	0.016	0.033	0.313	0.240
N	1720	1720	1720	1720

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. If any control is missing, I substitute a zero for the missing value zero and include a dummy variable to control for the substitution. I exclude the sample who lived outside of Japan or whose living place is unknown at age 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients are estimated based on Eq. (7). All columns show the result of the IV approach. US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. In all the regressions, I include the number of younger siblings, the number of older siblings, mothers' age and fathers' age. Furthermore, the cohort fixed effects, area fixed effects, and their interactions are controlled. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$.

and statistically significant, which is consistent with the literature (Mincer 1974; Becker 1994; Kane and Rouse 1995; Thomas 2003). However, once I control for the effect of college completion, ECEC's effect on income disappears, although the estimates remain positive in the specification without controls.[82] This is true for the heterogeneous effects, as shown in Table 11. As discussed earlier, women were disadvantaged compared to men in terms of the likelihood of college completion and wage. Because there is room for them to improve, most effects partly come from those of women.

These results imply that most of the effects of childcare systems on future annual income can be explained by the increase in college completion, especially for female children. There are also no other channels from ECEC enrolment to future income.

[82] A potential reason the estimate without control seems very different is as follows: ECEC's expansion is more effective for female children. Moreover, the fraction of women's college enrolment is lower than that of men (Fig. 6), while women's income was lower than that of men (see Fig. 7). Excluding the gender dummy could result in the omitted variable bias that would lead the coefficient of ECEC enrolment upward and that of college completion downward, although the mechanism could be more complicated.

Table 11 Heterogeneous effects of ECEC at Age 4: Mediation analysis

	Dependent Variable: log(Income) (in million yen)			
	Female Sample		Male Sample	
	(1)	(2)	(3)	(4)
ECEC Enrolment at Age 4	0.696*	0.389	0.212	0.063
(Enrol = 1)	(0.417)	(0.421)	(0.253)	(0.288)
College Completion		1.179**		0.591**
(Complete = 1)		(0.483)		(0.243)
Poorly Educated Mothers	−0.091	−0.115	0.109	0.110
(Less Than High School Completion = 1)	(0.140)	(0.150)	(0.086)	(0.094)
Poorly Educated Fathers	0.001	0.056	−0.096	0.013
(Less Than High School Completion = 1)	(0.184)	(0.147)	(0.086)	(0.107)
Poorly Educated Parents	−0.082	0.209	−0.081	−0.041
(Less Than High School Completion = 1)	(0.225)	(0.261)	(0.117)	(0.138)
Other Controls	x	x	x	x
Fixed Effects	x	x	x	x
R-squared	0.030	0.032	0.305	0.241
N	806	806	914	914

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute a zero for the missing value zero and include a dummy variable to control for the substitution. I exclude the sample who lived outside of Japan or whose living place is unknown at age 15. In the Appendix, I show the table where I use hourly wage and calculate the monthly income for those who do not report it. The regression coefficients of the IV are based on Eqs. (1), (4), (5), (7), and (9). Attrition status is reported in Appendix A. See Table 15 for details. The R-squared in Column (2) is uncentered. US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. As to wage, I mainly use the variable on monthly income. I omit those who did not report it. In Online Appendix, I show the table where I use hourly wage and calculate the monthly income for those who do not report it. In all the regressions, I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and dummy variables indicating missing values for each control. Furthermore, the area (10 areas over Japan) fixed effects, cohort (5 year unit) fixed effects, and their interactions are controlled. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

That is, increases in non-cognitive abilities by ECEC are unlikely to increase future income directly, as the discussed literature shows. This seems consistent with the finding that there are no long-term effects on psychological skills measured by risk preference and the Big Five (Tables 18 and 19) in Appendix B. Together with the finding on working hours, the mechanism behind the increase in annual income likely is as follows: enrolment in ECEC increases the college completion rate, and children who participated in ECEC accumulate human capital at colleges or universities, which increases their wages, which leads to an increase in their annual income.

As a robustness check, I conduct an additional analysis wherein I do not impose the assumption of homogeneity. Instead, I use the propensity score for attending college as a control as follows:

$$y_{ijT} = \boldsymbol{x}'_{it}\boldsymbol{\beta} + d_{ijt}\gamma + \hat{m}_{ijt'}\alpha + f(\mu_j, \nu_t) + \varepsilon_{ijT}. \quad (10)$$

This partial reduced-form approach does not require assumptions on the IV for college graduation except for its exogeneity, as discussed before. The result in Table 26 in Appendix D is similar to the result shown above.

One important point that might violate the story above is that ECEC can increase mothers' labour force participation and income significantly enough to allow children to go to college. To discuss this possibility, I conduct the following analyses.

First, I discuss briefly again whether there is any pre-trend on labour force participation Second, I examine whether there is any difference in labour force participation after introducing ECEC. Finally, I discuss whether there is any effect on their income based on subjective recall data.

If there is any systematic pre-trend in the female labour force supply before ECEC expanded, this could capture the potential supply after ECEC expanded. However, we do not observe this based on Table 4, and this is not likely to have happened.

Then, I examine whether there is an effect of ECEC expansion on the change in female labour force participation, using a similar method of examining the pre-trend of female labour force participation: I run a regression of percentage change in female employment at time $t + j - 1$ to $t + j$ ($j = 1, ..., 6$) on the growth in the number of four-year-old children enrolled in ECEC at time $t - 1$ to t. Given that the expansion of ECEC can be seen as quasi-random, the change in female employment after ECEC expansion can be considered causal.[83]

The result is shown in Table 21 in Appendix B. Although I find a positive effect, that is, parents were likely to work around one to four percentage points more using ECEC, the coefficient seems too small to have an economic interpretation in this study's context: a coefficient of 0.04 implies that if ECEC availability increased by 100 percentage points, female labour force participation would increase by four percentage points. Note that, based on the two figures on capacity and enrolment, the situation was that if ECEC was available, parents used it. Given this, an overall 4% increase in income is not likely to generate a large enough difference in disposable income to explain the income effect of the ECEC effect on children's outcomes. Even if we assume the effect is cumulative for six years, it is less than 10% of all parents. This still seems too small to explain that the effects are coming from the income effect.

The literature stated that the effect of ECEC on parents' (especially maternal) working choice is mixed: Childcare increased maternal labour supply in some studies in the USA in the 1980s (Gelbach 2002), Argentina in the 1990s (Berlinski and Galiani 2007), Canada in the 1990s to the early 2000s (Lefebvre and Merrigan 2008; Baker et al. 2008), Spain in the 1980s to the 1990s (Nollenberger and Rodríguez-Planas 2015), and Germany in the 2000s to 2010s (Busse and Gathmann 2020). However, some reported no effect in the USA from 1950 to 1990 (Cascio 2009), the 1990s (Fitzpatrick 2010), and 1980 to 2000 (Fitzpatrick

[83] Please note that there is no pre-trend based on Table 4.

Table 12 Big five questionnaire (Ten-Item Personality Inventory, TIPI)

Disagree strongly	Disagree moderately	Disagree a little	Neither agree nor disagree	Agree a little	Agree moderately	Agree strongly
1	2	3	4	5	6	7

I see myself as:

A: Extraverted, enthusiastic.

B: Critical, quarrelsome.

C: Dependable, self-disciplined.

D: Anxious, easily upset.

E: Open to new experiences, complex.

F: Reserved, quiet.

G: Sympathetic, warm.

H: Disorganized, careless.

I: Calm, emotionally stable.

J: Conventional, uncreative.

This table is based on Gosling et al. (2003) and the original questionnaire in the survey is a Japanese translation translated by Oshio et al. (2012). Ten-Item Personality Inventory can be converted into Big-Five Inventory as follows: The score of Extraversion $= A + (8 - F)$, the score of Agreeableness $= B + (8 - G)$, the score of Conscientiousness $= C + (8 - H)$, the score of Emotional Stability $= D + (8 - I)$, and the score of Openness to Experience $= E + (8 - J)$, where each capital letter represents the score of each component in the table. Therefore, the maximum score of each component is seven, the minimum is one, and the interval is 0.5

2012); France in the 1990s (Goux and Maurin 2010); and Norway in the 1960s to 2000s (Havnes and Mogstad 2011a). In Japan, on which this study focuses, Asai et al. (2015, 2016) and Yamaguchi (2017) investigate it in the 1990s to 2010s and Yamaguchi et al. (2018) investigate it in the 2010s, but they did not find effects on maternal labour supply. Based on the literature, the effect seems small, and the role of parents' working status and income effect induced by this seems limited.[84]

Finally, I analysed the effect of ECEC on children's subjective wealth at age 15, which are the only available data on income when they were young.[85] As the results show in Table 22 in Appendix B, we do not see any effects of ECEC on their

[84] Based on these studies, ECEC seems to have replaced other types of care. Asai et al. (2016); Yamaguchi et al. (2018) find that ECEC expansion crowded out informal care provided by grandparents, based on an analysis of Japan during the 1990s–2010s and the 2010s, respectively. Although their timings of focus are different from the current study, given that the family form had become smaller (Shwalb et al. 1992), the same mechanism seems to have occurred. Note that the discussion of the change in the family size is presented in the section of ECEC expansion.

[85] This can be noisy in the following ways: (i) these are subjective recall data asked after they became adults, so it can include measurement errors in both positive and negative ways; (ii) this is answered on a range of zero to ten—zero is feeling poorest and ten is richest; and (iii) the timing of the data focuses on the age of 15, which is ten years different from the timing in this study. Regarding the first two points, if the error is random, the estimate will suffer from an attenuation bias. Considering the last point, we examine the income effect if we are interested in the cumulative income effect, that is, the effect of difference in income from when they were at the age corresponding to the ECEC age when they were in (junior) high school, the timing when the preparation for colleges/universities becomes important.

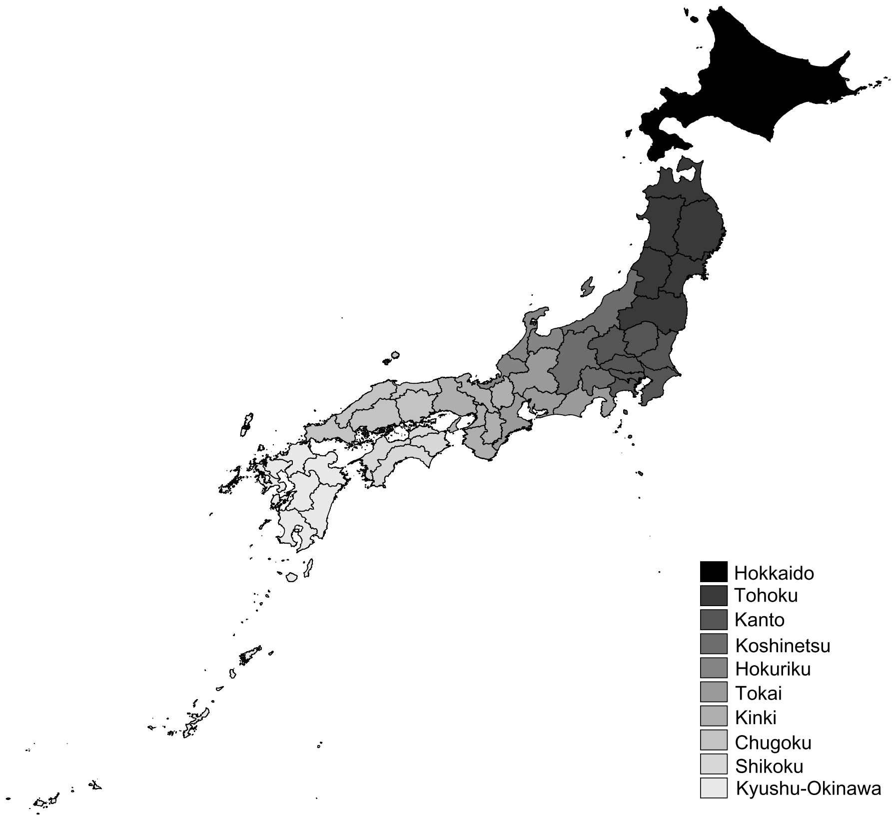

Fig. 10 Regional division of Japanese prefectures. I use the Stata command `maptile` with the option `jpn_pref` created by Chigusa Okamoto. See http://www.crepe.e.u-tokyo.ac.jp/en/materials/maptile.html for more details (Last access: 1st April 2022). This is a preliminary use, and I thank her for permitting its application

households' income, although we need to be cautious because of the noise in the measure. This seems to be consistent with the finding that there is little change in female labour force participation: the increase in income seems too small to conclude that there is an income effect.

8.3 Mechanisms behind the long-term effects of ECEC

Here, I discuss potential mechanisms that drive the results presented in the previous sections based on the literature, although some cited studies considered different timings than mine, and so we must be cautious. First, in the short run, Yamaguchi et al. (2018b) find that ECEC positively affects language development and reduces symptoms of inattention, hyperactivity, and aggression among disadvantaged children.[86] This implies that disadvantaged children receiving ECEC in my sample are likely to have improved these abilities during enrolment.

[86] Yamaguchi et al. (2018b) discuss the effects on children of mothers with low education level, particularly below high school completion.

Table 13 Regional division of Japanese prefectures

Area	Prefectures in the area
Hokkaido	Hokkaido
Tohoku	Aomori, Iwate, Miyagi, Akita, Yamagata, and Fukushima
Kanto	Ibaraki, Tochigi, Gumma, Saitama, Chiba, Tokyo, and Kanagawa
Koshinetsu	Niigata, Yamanashi, and Nagano
Hokuriku	Toyama, Ishikawa, and Fukui
Tokai	Gifu, Shizuoka, Aichi, and Mie
Kinki	Shiga, Kyoto, Osaka, Hyogo, Nara, and Wakayama
Chugoku	Tottori, Shimane, Okayama, Hiroshima, and Yamaguchi
Shikoku	Tokushima, Kagawa, Ehime, and Kochi
Kyushu-Okinawa	Fukuoka, Saga, Nagasaki, Kumamoto, Oita, Miyazaki, Kagoshima, and Okinawa

Table 14 Income category in the survey

Category	Range	Class Value
1	income <1 million yen	0.5 million yen
2	1 million yen $\leq$ income $<$ 2 million yen	1.5 million yen
3	2 million yen $\leq$ income $<$ 4 million yen	3 million yen
4	4 million yen $\leq$ income $<$ 6 million yen	5 million yen
5	6 million yen $\leq$ income $<$ 8 million yen	7 million yen
6	8 million yen $\leq$ income $<$ 10 million yen	9 million yen
7	10 million yen $\leq$ income $<$ 12 million yen	11 million yen
8	12 million yen $\leq$ income $<$ 14 million yen	13 million yen
9	14 million yen $\leq$ income	15 million yen

This income means gross income before taxes of the respondent, including bonuses, business income. If the respondent is a student, it contains his/her salary of part-time jobs, remittances from his/her home, and scholarships

Chetty et al. (2011) contend that students taught by more experienced teachers in kindergarten earn more in the future. They found that licensed childcare with more experienced teachers in terms of childcare raises children's abilities. Berlinski et al. (2009) state that attending preschool increases test scores in elementary school and improves students' self-control (e.g., better school attendance and discipline, and improved attention and efforts). This improvement can be seen in ECEC in Japan too. Although many studies found that the effects on cognitive abilities fade quickly (Heckman and Masterov 2007; Heckman et al. 2013; Havnes and Mogstad 2015), these effects could last during the early stages of elementary schools and might be sufficient to push children into better school cohorts. Children's improved non-cognitive abilities can also make a difference, and the effects on non-cognitive abilities are likely to last longer (Heckman and Masterov 2007; Heckman et al. 2013; Havnes and Mogstad 2015), despite some fluctuations (Roberts et al. 2006; Borghans et al. 2008; Roberts and DelVecchio

Table 15 Attrition status

	Independent Variable is Dummy variable of ECEC Enrolment at the Following Age					
	Age 0	Age 1	Age 2	Age 3	Age 4	Age 5
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Dep. Var. is Attrition Dummy of Income (1 if Attritor)						
ECEC Enrolment At Each Age	−0.006	0.014	−0.003	−0.021	−0.028	−0.060*
(Enrol = 1)	(0.051)	(0.041)	(0.032)	(0.017)	(0.020)	(0.031)
R-squared	0.000	0.000	0.000	0.001	0.001	0.001
N	2847	2847	2847	2847	2847	2847
Panel B: Dep. Var. is Attrition Dummy of Big Five Questions (1 if Attritor)						
ECEC Enrolment At Each Age	0.121**	0.054	0.011	0.036**	0.002	−0.035
(Enrol = 1)	(0.051)	(0.041)	(0.032)	(0.017)	(0.020)	(0.031)
R-squared	0.002	0.001	0.000	0.001	0.000	0.000
N	2847	2847	2847	2847	2847	2847

Estimated standard errors are in parentheses. The samples used in this analysis were born between 1960 and 1989 and stayed in Japan at the age of 15. In Panel A, the analysis is based on the regression of ECEC enrolment at each age corresponding to each column (1 if enrolled) on the attrition dummy on the income variable (1 if an attritor) and the constant term. In Panel B, the analysis is the same except that the independent variable is the attrition dummy on the Big Five variables (1 if an attritor). I exclude observations from the entire population if I cannot merge them with the administrative data and they do not have information on ECEC enrolment. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

2000). The traits fostered in childhood matter to the outcomes in adulthood (Conti et al. 2016). Improvements in self-control could lead to college completion. Consequently, a higher education level leads to higher wages (Mincer 1974; Becker 1994; Kane and Rouse 1995; Thomas 2003), which boosts income.

9 Conclusion

This study estimated the long-term effects of ECEC participation on college completion, adult income, and personality traits. The main analysis was based on an IV regression because there is a potential threat of endogeneity; parental choice to enrol children in ECEC may be correlated with unobservable household characteristics. To address this issue, I use a policy change that affected childcare enrolment. Specifically, the analysis used the quasi-random expansion of ECEC from the 1960s to 1980s in Japan owing to the spike in demand under Japan's high rate of economic growth. This rate of expansion differed regionally, and supply-side constraints were generally binding, allowing me to treat this variation as quasi-random.

The results show that enrolment in Japan's childcare system at age four increases children's future income and wages by age 50 as well as the likelihood of college completion. The subsample analysis reveals that the long-term effects of ECEC are mainly triggered by changes in females' college completion rates and adult income. In other words, female children benefited more from ECEC over the long run. Based

Table 16 Enrolment rate in ECEC based on the survey data and administrative record

	Rate based on the Survey	Rate based on the Administrative Data
Nursery Schools		
Age 0	0.026	0.006
	(0.159)	
Age 1	0.043	0.030
	(0.202)	
Age 2	0.074	0.072
	(0.261)	
Age 3	0.219	0.159
	(0.414)	
Age 4	0.313	0.273
	(0.464)	
Age 5	0.305	0.319
	(0.461)	
Kindergartens		
Age 3	0.149	0.078
	(0.356)	
Age 4	0.471	0.447
	(0.499)	
Age 5	0.614	0.595

Standard deviations are in parentheses. The numbers of rate based on the survey are the mean of dummy variable on enrolment in nursery schools or kindergartens (enrol = 1) which are used in the analysis. There numbers are the same as ones in Table 1. The numbers of Rate based on the Administrative Data for kindergartens are based on School Basic Survey, an administrative data collected by the Ministry of Education, Culture, Sports, Science, and Technology (MEXT). The numbers of Rate based on the Administrative Data for nursery schools are based on Survey of Social Welfare Institutions, which is collected by the Ministry of Health, Labour and Welfare. This data does not include the full information on the age of the children and I cannot separate their age correctly. Therefore I use the data set containing the ratio on the ages of children enrolling in a nursery school at the country level. I assume that the ratio is the same across the prefectures and use this to calculate the number of enrolment of each age in each prefecture

on the analysis of the marginal treatment effects, there is no negative effect uncovered for any children in terms of unobserved heterogeneity. There seem to be no long-term effects on non-cognitive abilities measured using the Big Five indexes. Furthermore, I examined the mechanism behind the effects by conducting mediation analysis. As a result, I find that most of the effects on income can be explained by the rise in the likelihood of college completion, which in turn leads to higher wages, and, therefore, leads to higher income.

This research contributes to the literature as well as policymaking. Few previous studies have discussed the long-term LATE of universal childcare systems as opposed to the shorter-term effects, or effects of targeted ECEC programmes. Besides, few studies have discussed the mechanisms behind this phenomenon. This paper contributes to the literature by showing the long-term effects of a universal ECEC programme and its mechanism. Furthermore, knowing the long-term positive effects of an early-stage intervention on income is useful for policymaking. If a government allocates a fixed budget for one cohort over its lifetime, my results might imply that it may be more efficient to limit the allocation of funds to them when they are young rather than when they become adults, only because doing so would boost skills, such as their cognitive and non-cognitive abilities in childhood, and increase their future income. Spending resources on older cohorts may lead only to consumption without any economic growth. This research exemplifies the usefulness of ECEC by showing its effects. ECEC reduces inequalities between the advantaged and disadvantaged by benefiting the latter and not imposing a direct non-monetary cost on the former. I also find that ECEC increases the likelihood of college completion and raises future income for women. These results imply that ECEC can reduce inequalities and encourage governments to eliminate any barriers to enrolment, such as childcare fees, particularly for disadvantaged children, given the finding that there is no effect and cost for advantaged children. The results also imply that ECEC enrolment could break the intergenerational poverty chain because the early childhood intervention reduces the inequality in their adulthood. Regarding gender inequality, these results are informative for countries or communities where women have culturally and historically been relegated to home production or part-time work. This research is also useful for discussing the expansion of ECEC in developing countries, as described in Target 4.2 of the United Nation's SDGs (United Nations 2015).[87] This is because this analysis is based on the universal (i.e., not targeted) ECEC expansion of 1960–1989 in Japan, a period during which the country experienced rapid development. Some might think that an alternative policy can be more efficient, such as parental leave when parents, and especially mothers, are supposed to spend more time with their children. However, as Yamaguchi et al. (2018b) discuss, using childcare is beneficial for improving parenting quality and mothers' subjective well-being, in addition to reducing stress among low-educated mothers, since the parenting quality of low-educated mothers is reported as low. Therefore, a childcare system is beneficial for both parents and children from disadvantaged households.

The main caveat of this research comes from the data. Although the questionnaire used is unique, the number of observations is smaller than that of recent empirical papers. Therefore, the estimated standard errors are larger, leading to less accurate estimations than otherwise. The data are also not panel data and contain retrospective or subjective records, which could reduce accuracy. Overcoming these caveats is left for future research. Another potential caveat is that the result cannot be

[87] This target states that "[b]y 2030, all girls and boys [must] have access to quality early childhood development, care and pre-primary education so that they are ready for primary education."

directly used to improve the current situation in Japan, because the availability of ECEC is substantially different. However, the implication stated here can be useful in developing countries where the government is investing in ECEC under the SDGs (United Nations 2015).

Appendix A. Data

In this section, I provide additional tables on the Big Five components (Table 12), a figure on the areas in Japan based on the governmental standard (Fig. 10 and Table 13), and describe my data. The data description includes tables on income category (Table 14), attrition rate (Table 15), and retrospective records (Table 16). As for attrition, there are no systematic attrition patterns in the observable characteristics.

Appendix B. Results of other outcome variables

This appendix presents the analyses with other outcome variables such as working status and hours, wage, Big Five index, risk preference index, and health-related behaviours such as drinking and smoking. Table 17 shows the results of the OLS, reduced form, and IV analysis of the effects of ECEC on working status, working hours, and wages in the future. According to this table, I do not find any effects on the working status and working hours, but do find positive effects on the wage. These results imply that ECEC increases future income not by increasing the probability of working or extending working hours, but by increasing wages. Together with the finding of mediation analysis, this finding is consistent with the explanation based on human capital accumulation theory (Mincer 1974; Becker 1994; Kane and Rouse 1995; Thomas 2003). On the other hand, Tables 18 and 19 show that none of the Big Five components or psychological measures are affected by enrolment in the childcare system.[88] There are a few potential interpretations. First, many psychological indexes evolve over the lifecycle at the mean level (Roberts et al. 2006; Borghans et al. 2008), and they are not consistent before age 50 years in the order level (Roberts and DelVecchio 2000). They might change after a significant life event (Hanaoka et al. 2018). Enrolment in the childcare system might have some short-term effects that disappeared by the time of the survey (i.e., when respondents were aged 35–50 years). Second, the sample size in my data set might be too small to have sufficient power to detect the differences. Third, attrition might matter, although there is no systematic attrition problem.[89] Table 20 shows the result of health-related behaviour such as smoking and drinking: ECEC reduces the probability of smoking, while

[88] I conduct an additional analysis to estimate the seemingly unrelated equations of the Big Five components because they might be correlated. However, there are no effects. Thus, I cannot reject the hypothesis that the estimates are statistically different from zero both in the separate hypotheses for the Big Five components and in a joint hypothesis where all the coefficients are zero.

[89] The potential attrition problem is discussed in Appendix A.

Table 17 Effects of ECEC at Age 4 on other socioeconomic outcomes

	Working Status (Work = 1)				Working Hours				log(Wage) (in ten thousand yen)			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ECEC Enrolment at Age 4	0.011	0.008	−0.058	−0.069	0.096	0.144	−0.879	1.628	0.047	0.050	0.420***	0.553***
(Enrol = 1)	(0.021)	(0.018)	(0.061)	(0.087)	(0.999)	(0.955)	(2.478)	(3.560)	(0.048)	(0.045)	(0.101)	(0.199)
Gender		−0.270***	−0.271***	−0.271***		−11.58***	−11.59***	−11.60***		−0.375***	−0.372***	−0.379***
(Female = 1)		(0.018)	(0.018)	(0.018)		(0.965)	(0.961)	(0.942)		(0.047)	(0.047)	(0.045)
Less Educated Mothers		−0.003	−0.003	−0.004		−2.231**	−2.231**	−2.158*		0.076*	0.069*	0.103**
(Less Than High School Completion = 1)		(0.039)	(0.039)	(0.037)		(1.096)	(1.095)	(1.116)		(0.041)	(0.041)	(0.044)
Less Educated Fathers		0.012	0.010	0.011		−3.489**	−3.510**	−3.444**		−0.040	−0.031	−0.000
(Less Than High School Completion = 1)		(0.041)	(0.041)	(0.039)		(1.552)	(1.563)	(1.506)		(0.071)	(0.072)	(0.075)
Less Educated Parents		−0.009	−0.009	−0.009		3.569**	3.570**	3.510**		−0.096	−0.093	−0.130*
(Less Than High School Completion = 1)		(0.051)	(0.050)	(0.049)		(1.456)	(1.449)	(1.445)		(0.073)	(0.074)	(0.069)
Other Controls		x	x	x		x	x	x		x	x	x

Table 17 (continued)

	Working Status (Work = 1)				Working Hours				log(Wage) (in ten thousand yen)			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x
R-squared	0.060	0.173	0.173	0.168	0.037	0.230	0.230	0.229	0.096	0.213	0.219	0.105
N	2647	2647	2647	2647	1496	1496	1496	1496	1238	1238	1238	1238

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the OLS are estimated based on Eq. (1) and those of the IV are based on Eqs. (1), (4), and (5). Columns (1), (2), (5), (6), (9), and (10) show the result of the OLS, (3), (7), and (11) show the result of the reduced-form approach, and (4), (8), and (12) show the result of the IV approach. In Columns (5) to (8), I exclude the observations answering that they do not work. Therefore, I focus on the internal margin here. In Columns (9) to (12), I mainly use the variable on monthly income. I omit those who did not report it. For "Other Controls," I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. In all the regressions, the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions are controlled. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

Table 18 Outcomes on personal characteristics

	Measured in 2009				Measured in 2012			
	Risk Preference				Big Five Index			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ECEC Enrolment at Age 4	−0.015	−0.009	−0.054	−0.284	0.055	0.047	0.054	0.294
(Enrol = 1)	(0.040)	(0.040)	(0.112)	(0.239)	(0.047)	(0.046)	(0.089)	(0.276)
ECEC Enrolment at Age 4				0.037				−0.155
(Enrol = 1)				(0.081)				(0.203)
× The Number of								
Older Siblings								
ECEC Enrolment at Age 4				0.305**				−0.037
(Enrol = 1)				(0.147)				(0.176)
× The Number of								
Younger Siblings								
The Number of		0.028	0.028	0.006		−0.031	−0.032	0.086
Older Siblings		(0.019)	(0.020)	(0.064)		(0.027)	(0.027)	(0.150)
The Number of		0.003	0.002	−0.229**		0.016	0.016	0.040
Younger Siblings		(0.017)	(0.017)	(0.114)		(0.025)	(0.025)	(0.125)
Gender		0.173***	0.173***	0.185***		−0.044	−0.044	−0.047
(Female = 1)		(0.033)	(0.033)	(0.035)		(0.034)	(0.034)	(0.032)
Less Educated Mothers		−0.003	−0.003	−0.021		−0.060	−0.061	−0.039
(Less Than High School Completion = 1)		(0.046)	(0.046)	(0.050)		(0.072)	(0.071)	(0.071)
Less Educated Fathers		−0.110	−0.112	−0.129		−0.207**	−0.209**	−0.184**
(Less Than High School Completion = 1)		(0.084)	(0.082)	(0.081)		(0.085)	(0.085)	(0.086)
Less Educated Parents		0.069	0.069	0.098		0.134	0.136	0.113

Table 18 (continued)

	Measured in 2009				Measured in 2012			
	Risk Preference				Big Five Index			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Less Than High School Completion = 1)		(0.089)	(0.089)	(0.087)		(0.086)	(0.086)	(0.084)
Other Controls		x	x	x		x	x	x
Fixed Effects	x	x	x	x	x	x	x	x
Chi-squared				0.239				0.096
R-squared	0.019	0.039	0.039	0.033	0.034	0.049	0.049	0.044
N	2554	2554	2554	2503	1936	1936	1936	1899

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the OLS are estimated based on Eq. (1), those of the reduced form are based on Eq. (6) and those of the IV are based on Eqs. (1), (4), and (5). Columns (1), (2), (5), and (6) show the result of the OLS, (3) and (7) show the result of the reduced-form analysis, and (4) and (8) show the result of the IV analysis. For Column (1) to (4), the risk Preference is calculated by following Hanaoka et al. (2018). I use transformed price as a measure of risk preference. Note that if transformed price is larger, s/he is more risk averse. For Column (5) to (8), the sample is selected by omitting all observations with missing values in any one of Big Five components. In "Other Controls," I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. In all the regressions, the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions are controlled. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

Table 19 Big five outcomes

	Measured in 2012																			
	Extraversion				Agreeableness				Conscientiousness				Emotional Stability				Openness to Experience			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced	IV	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
ECEC Enrolment at Age 4	0.109	0.099	0.310	0.561	−0.008	−0.013	−0.151	0.368	0.022***	0.013	0.085	0.302	0.004	−0.001	−0.001	0.355	0.154	0.139*	0.014	−0.253
(Enrol = 1)	(0.104)	(0.103)	(0.201)	(0.582)	(0.064)	(0.064)	(0.161)	(0.368)	(0.060)	(0.060)	(0.185)	(0.425)	(0.073)	(0.070)	(0.213)	(0.506)	(0.077)	(0.075)	(0.202)	(0.441)
ECEC Enrolment at Age 4				−0.243				−0.204				−0.388				−0.262				0.278
(Enrol = 1)				(0.437)				(0.227)				(0.309)				(0.242)				(0.300)
× The Number of																				
Older Siblings																				
ECEC Enrolment at Age 4				−0.080				−0.087				−0.039				−0.027				−0.002
(Enrol = 1)				(0.354)				(0.240)				(0.310)				(0.339)				(0.224)
× The Number of																				
Younger Siblings																				
The Number of		−0.007	−0.009	0.171		−0.031	−0.032	0.116		−0.047	−0.047	0.232		−0.005	−0.005	0.191		−0.055	−0.059	−0.253
Older Siblings		(0.057)	(0.056)	(0.318)		(0.041)	(0.041)	(0.160)		(0.048)	(0.048)	(0.205)		(0.038)	(0.039)	(0.183)		(0.043)	(0.043)	(0.217)
The Number of		0.042	0.042	0.096		−0.005	−0.005	0.053		0.007	0.007	0.016		0.041	0.042	0.055		0.014	0.012	0.031
Younger Siblings		(0.047)	(0.046)	(0.260)		(0.039)	(0.038)	(0.168)		(0.045)	(0.045)	(0.211)		(0.051)	(0.051)	(0.238)		(0.039)	(0.038)	(0.163)
Gender		0.289***	0.290***	0.278***		0.134**	0.134**	0.133**		−0.109**	−0.109**	−0.106**		−0.254***	−0.254***	−0.259***		−0.259***	−0.259***	−0.257***
(Female = 1)		(0.076)	(0.077)	(0.073)		(0.058)	(0.058)	(0.058)		(0.049)	(0.049)	(0.049)		(0.053)	(0.053)	(0.053)		(0.065)	(0.065)	(0.065)
Less Educated Mothers		0.003	0.001	0.023		−0.145	−0.145	−0.152		−0.068	−0.069	−0.027		0.018	0.018	0.048		−0.088	−0.092	−0.075

Table 19 (continued)

	Measured in 2012																			
	Extraversion				Agreeableness				Conscientiousness				Emotional Stability				Openness to Experience			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced	IV	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
(Less Than High School Completion = 1)		(0.135)	(0.136)	(0.140)		(0.107)	(0.106)	(0.100)		(0.110)	(0.110)	(0.109)		(0.142)	(0.142)	(0.129)		(0.102)	(0.102)	(0.104)
Less Educated Fathers		−0.172	−0.174	−0.139		−0.030	−0.031	−0.018		−0.366***	−0.365***	−0.345***		−0.176	−0.176	−0.140		−0.234	−0.241	−0.230
(Less Than High School Comple-tion = 1)		(0.206)	(0.208)	(0.200)		(0.128)	(0.128)	(0.132)		(0.135)	(0.135)	(0.133)		(0.133)	(0.132)	(0.141)		(0.166)	(0.166)	(0.168)
Less Educated Parents		0.042	0.050	0.027		0.119	0.116	0.134		0.356**	0.358**	0.317**		0.011	0.012	−0.018		0.092	0.098	0.069
(Less Than High School Completion = 1)		(0.188)	(0.191)	(0.180)		(0.135)	(0.136)	(0.138)		(0.151)	(0.150)	(0.147)		(0.136)	(0.136)	(0.134)		(0.193)	(0.195)	(0.198)
Other Controls		x	x	x		x	x	x		x	x	x		x	x	x		x	x	x
Fixed Effects	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x	x
Chi−squared				0.244				0.086				0.134				0.058				0.004
R−squared	0.041	0.066	0.066	0.060	0.045	0.056	0.057	0.050	0.049	0.059	0.059	0.056	0.040	0.061	0.061	0.055	0.032	0.057	0.055	0.046
N	1958	1958	1958	1921	1961	1961	1961	1924	1958	1958	1958	1921	1953	1953	1953	1916	1955	1955	1955	1918

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the OLS are estimated based on Eq. (1), those of the reduced form are based on Eq. (6) and those of the IV are based on Eqs. (1), (4), and (5). Columns (1), (2), (5), (6), (9), (10), (13), (14), (17), and (18) show the result of the OLS, (3), (7), (11), (15), and (19) show the result of the reduced-form analysis, and (4), (8), (12), (16), and (20) show the result of the IV analysis. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$. In "Other Controls," I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. In all the regressions, the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions are controlled

Table 20 Effects on health-related outcomes

	Measured in 2009							
	Smoking Dummy				Drinking Dummy			
	(Smoking = 1)				(Drinking = 1)			
	OLS	OLS	Reduced Form	IV	OLS	OLS	Reduced Form	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ECEC Enrolment at Age 4	−0.017	−0.012	−0.216***	−0.260**	−0.015	−0.016	0.038	−0.005
(Enrol = 1)	(0.033)	(0.030)	(0.080)	(0.128)	(0.016)	(0.017)	(0.078)	(0.096)
Gender		−0.367***	−0.368***	−0.368***		−0.119***	−0.119***	−0.119***
(Female = 1)		(0.028)	(0.028)	(0.027)		(0.021)	(0.021)	(0.021)
Less Educated Mothers		0.073*	0.074*	0.069		−0.007	−0.007	−0.007
(Less Than High School Completion = 1)		(0.043)	(0.043)	(0.042)		(0.026)	(0.026)	(0.026)
Less Educated Fathers		0.121***	0.116**	0.117***		0.013	0.014	0.013
(Less Than High School Completion = 1)		(0.045)	(0.045)	(0.043)		(0.041)	(0.041)	(0.040)
Less Educated Parents		−0.086	−0.086	−0.086		−0.028	−0.028	−0.028
(Less Than High School Completion = 1)		(0.061)	(0.060)	(0.062)		(0.045)	(0.045)	(0.044)
Other Controls		x	x	x		x	x	x
Fixed Effects	x	x	x	x	x	x	x	x
R−squared	0.038	0.190	0.193	0.157	0.032	0.059	0.059	0.059
N	2647	2647	2647	2647	2647	2647	2647	2647

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the OLS are estimated based on Eq. (1), those of the reduced form are based on Eq. (6) and those of IV are based on Eqs. (1), (4), and (5). Columns (1), (2), (5), and (6) show the result of the OLS, (3) and (7) show the result of the reduced-form analysis, and (4) and (8) show the result of the IV analysis. In Columns (1) to (4), the smoker group includes those who had smoked before although they quit smoking. Therefore, this measures the smoking experience. In "Other Controls," I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. In all the regressions, the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions are controlled. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

it does not decrease that of drinking. This result is similar to Conti et al. (2016). They find that preschool programmes affect smoking behaviour while not for drinking behaviour in the long run.

As discussed in Section 8.2, I investigate the effect of ECEC on parental labour supply. Table 21 shows the result. Although I find that parents were likely to work around one to four percentage points more using ECEC, the magnitude is too small to state the effects of ECEC on children's outcomes comes from the increase in parental income.

Finally, I conducted an analysis of the effect of ECEC on children's subjective wealth at the age of 15, which is the only available data on the income when they were young. As discussed earlier, this variable can be noisy. However, as Table 22 shows, we do not see any effects of ECEC on their households' income, although we need to be a little cautious because of the noise in the measure. This might indirectly imply that the main result is not affected by income effects.

Appendix C. First stage results, robustness check, and marginal treatment effects

This appendix presents the results of first stage and those for the robustness check. Based on Tables 23, 24, and 25, all IV analyses used in this study do not seem to suffer from the weak IV problem. Table 26 shows the result of partially reduced form analysis, which is discussed as a robustness check for the mediation analysis. In the specification, I do not impose the assumption of homogeneity. Instead, I use the propensity score for attending college as a control as follows:

Table 21 Change in women's labour force participation induced by the increase in ECEC

	Percentage Change of Female Employment from (t + j - 1) to (t + j)					
	j = 1	j = 2	j = 3	j = 4	j = 5	j = 6
	(1)	(2)	(3)	(4)	(5)	(6)
Growth in the Number of Enrolments	0.002	0.013***	0.004	0.033***	0.044***	0.001
in ECEC from (t-1) to (t)	(0.001)	(0.004)	(0.004)	(0.009)	(0.012)	(0.001)
R-squared	0.650	0.428	0.316	0.360	0.544	0.744
N	1166	1167	1167	1167	1167	1120

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. Estimated standard errors clustered by each area, cohort, and their interaction are in parentheses. In this analysis, I used the data from 1960 to 1985. There is attrition mainly because Okinawa, one prefecture in the south of Japan, became back a part of Japan in 1972, and we do not have the data before it. Besides, when I take the sixth difference, some variables could not be defined because of the data range. This is why the number of observations for column 6 is smaller than the others. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

Table 22 Effects of ECEC at Age 4 on subjective wealth at the Age 15

	Subjective Wealth at the Age of 15 (1 is Lowest and 5 is Highest)	
	OLS	IV
	(1)	(2)
ECEC Enrolment at Age 4	0.291**	0.191
(Enrol = 1)	(0.099)	(0.478)
Less Educated Mothers	0.059	0.057
(Less Than High School Completion = 1)	(0.124)	(0.118)
Less Educated Fathers	−0.300*	−0.301**
(Less Than High School Completion = 1)	(0.155)	(0.153)
Less Educated Parents	−0.351*	−0.351*
(Less Than High School Completion = 1)	(0.191)	(0.187)
Gender	0.194***	0.194***
(Female = 1)	(0.073)	(0.071)
Other Controls	x	x
Fixed Effects	x	x
R−squared	0.128	0.127
N	2647	2647

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. This first stage is for the regression where the log(income) is the outcome variable. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and I control the dummy for the substitution. In Online Appendix, I show the table where I use hourly wage and calculate the monthly income for those who do not report it. Regression coefficients of the OLS is based on Eq. (1) and that of the IV is based on Eqs. (1), (4), and (6). I exclude sample who lives, at the age 15, outside of Japan or whose living place is unknown. Attrition status is reported in Appendix A. See Table 15 for details. Columns (1) shows the result of the OLS and (2) shows the result of the IV analysis. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

$$y_{ijT} = \boldsymbol{x}'_{it}\boldsymbol{\beta} + d_{ijt}\gamma + \hat{m}_{ijt'}\alpha + f(\mu_j, \nu_t) + \varepsilon_{ijtT}. \tag{11}$$

This partial reduced-form approach does not require assumptions on the IV for college graduation except for its exogeneity, as discussed above. The result in Table 26 shows the main result is robust.

In Tables D5 and D6 of Kawarazaki (2022), I show the result of the same robustness check with different outcome variables. These tables shows the results for the other variables are also robust.

In Table 27, I examine the heterogeneous effects with respect parental education levels. The result shows that if parents' education levels are low, their children

Table 23 First stage result of ECEC at age 4

	Dependent Variable: ECEC Enrolment at Age 4					
	All Samples		Female Samples		Male Samples	
	(1)	(2)	(3)	(4)	(5)	(6)
Propensity Score of ECEC	1.179***	1.174***	1.210***	1.214***	1.131***	1.129***
at Age 4 (Enrol = 1)	(0.144)	(0.141)	(0.221)	(0.226)	(0.153)	(0.151)
Gender		0.006***				
(Female = 1)		(0.016)				
Less Educated Mothers		−0.051*		−0.023		−0.066*
(Below High School Completion = 1)		(0.028)		(0.054)		(0.035)
Less Educated Fathers		−0.029		−0.006		−0.054
(Below High School Completion = 1)		(0.041)		(0.090)		(0.064)
Less Educated Parents		0.031		0.014		0.054
(Below High School Completion = 1)		(0.046)		(0.092)		(0.077)
Other Controls		x		x		x
Fixed Effects	x	x	x	x	x	x
Kleibergen-Paap rk Wald F statistic	66.75***	69.40***	29.87***	28.90***	54.85***	55.90***
R-squared	0.018	0.323	−0.023	0.022	0.275	0.306
N	1915	1915	893	893	1022	1022

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients are based on Eq. (4). These coefficients show the regression where the dependent variable is the logarithm of income. All columns show the result of the first stage of the IV analysis. In "Other Controls," I include the number of younger siblings, the number of older siblings, mothers' age, fathers' age, and missing dummies. In all the regressions, the area (10 areas over Japan) fixed effects, cohort (5-year unit) fixed effects, and their interactions are controlled. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

benefit more from enrolling ECEC. This result is consistent with the result of Yamaguchi et al. (2018b), although they focus on short-term effects.

I further conduct a robustness check with different propensity scores. As discussed above, there is no deterministic way of calculating the propensity score. Therefore, I vary the form and conduct the same analysis. The results are shown in Table 28 and the main result is robust, although we need to be cautious when we interpret the coefficient in the Probit model.

Finally, I investigate the marginal treatment effect, which is studied by Cornelissen et al. (2018), for example. Figure 11 shows that the children who are likely to enrol in ECEC in terms of the unobserved heterogeneity benefit from ECEC more. The effect is significantly positive at 90% significance level for 40% of the children, while there is no negative impact uncovered for other children.

Table 24 First stage result of ECEC at Age 4: Mediation analysis

	First Stage			
	ECEC Enrolment at Age 4 (Enrol = 1)	College Completion (Complete = 1)	ECEC Enrolment at Age 4 (Enrol = 1)	College Completion (Complete = 1)
	(1)	(2)	(3)	(4)
Propensity Score of ECEC	1.076***	0.136	1.069***	0.019
at Age 4	(0.151)	(0.138)	(0.142)	(0.142)
Propensity Score of	0.077	1.085***	0.082	0.914***
College Completion	(0.087)	(0.104)	(0.086)	(0.115)
Poorly Educated Mothers			−0.049*	−0.009
(Less Than High School Completion = 1)			(0.028)	(0.052)
Poorly Educated Fathers			−0.019	−0.122
(Less Than High School Completion = 1)			(0.042)	(0.074)
Poorly Educated Parents			0.027	−0.122
(Less Than High School Completion = 1)			(0.047)	(0.080)
Gender			0.000	0.042
(Female = 1)			(0.017)	(0.032)
Other Controls	x	x	x	x
Fixed Effects	x	x	x	x
Kleibergen-Paap rk Wald F statistic	30.75***		25.40***	
N	1720	1720	1720	1720

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients are estimated based on Eq. (9). All columns show the result of the first stage of the instrumental variables analysis. In all the regressions, I include the number of younger siblings, the number of older siblings, mothers' age and fathers' age as "Other Controls." Furthermore, the cohort fixed effects, area fixed effects, and their interactions are controlled. Kleibergen-Paap rk Wald F statistic is reported for checking a weak IV jointly. Note that $\Pr(F(2,46) > 2.42) = 0.10$, $\Pr(F(2,46) > 3.20) = 0.05$, and $\Pr(F(2,46) > 5.10) = 0.01$. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

Table 25 First stage result of ECEC at Age 4: Mediation analysis with heterogeneous effects

	First Stage			
	ECEC Enrolment at Age 4 (Enrol = 1)	College Completion (Complete = 1)	ECEC Enrolment at Age 4 (Enrol = 1)	College Completion (Complete = 1)
	(1)	(2)	(3)	(4)
Female Samples				
Propensity Score of ECEC	1.095**	0.289	1.100***	0.137
at Age 4	(0.241)	(0.233)	(0.234)	(0.234)
Propensity Score of	0.117	1.019***	0.120	0.830***
College Completion	(0.152)	(0.220)	(0.150)	(0.190)
Kleibergen-Paap rk Wald F statistic	9.744***		7.766***	
N	806	806	806	806
Male Samples				
Propensity Score of ECEC	1.003**	0.027	0.992***	-0.051
at Age 4	(0.166)	(0.248)	(0.246)	(0.246)
Propensity Score of	0.124	1.053***	0.141	0.869***
College Completion	(0.152)	(0.218)	(0.153)	(0.221)
Kleibergen-Paap rk Wald F statistic	10.21***		6.725***	
N	914	914	914	914

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5-year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients are estimated based on Eq. (9). All columns show the result of the first stage of the IV analysis. In all the regressions, I include the number of younger siblings, the number of older siblings, mothers' age and fathers' age. Furthermore, the cohort fixed effects, area fixed effects, and their interactions are controlled. Kleibergen-Paap rk Wald F statistic is reported for checking a weak IV jointly. Note that $\Pr(F(2,46) > 2.42) = 0.10$, $\Pr(F(2,46) > 3.20) = 0.05$, and $\Pr(F(2,46) > 5.10) = 0.01$. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

Table 26 IV result of ECEC at Age 4: Partially reduced-form analysis

	Dependent Variable: log(Income) (in Million Yen)			
	(1)	(2)	(3)	(4)
ECEC Enrolment at Age 4	0.607**	0.471*	0.430*	0.230
(Enrol = 1)	(0.248)	(0.257)	(0.261)	(0.278)
Propensity of College		0.499**		0.772***
Enrolment		(0.229)		(0.222)
Poorly Educated Mothers			0.076	0.060
(Less Than High School Completion = 1)			(0.077)	(0.077)
Poorly Educated Fathers			−0.084	−0.067
(Less Than High School Completion = 1)			(0.101)	(0.094)
Poorly Educated Parents			−0.084	−0.075
(Less Than High School Completion = 1)			(0.117)	(0.115)
Gender			−0.935***	−0.944***
(Female = 1)			(0.087)	(0.085)
Other Controls	x	x	x	x
Fixed Effects	x	x	x	x
R-squared	0.016	0.044	0.313	0.344
N	1720	1720	1720	1720

Estimated standard errors clustered by each area (10 areas over Japan), cohort (5 year unit), and their interaction are in parentheses. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and I control the dummy for the substitution. I exclude sample who lives, at the age 15, outside of Japan or whose living place is unknown. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the IV are based on Eqs. (1), (4), (5), (9), and (10). US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. In all the regressions, I include the number of younger siblings, the number of older siblings, mothers' age and fathers' age in "Other Controls." Furthermore, the cohort fixed effects, area fixed effects, and their interactions are controlled. $*p < 0.1$; $**p < 0.05$; $***p < 0.01$

Acknowledgements I thank Hidehiko Ichimura, Shintaro Yamaguchi, and Gabriella Conti for their instructions and advice; and Francesca Arduini, Christian Dustmann, Taiyo Fukai, Chishio Furukawa, Drew Griffen, Sungwan Hong, Hilary Hoynes, Kei Ikegami, Chihiro Inoue, Takuya Ishihara, Kasahara Hiroyuki, Daiji Kawaguchi, Soyoung Kim, Toru Kitagawa, Bryan Kuek, Shiko Maruyama, Aska Ross, Dan Sasaki, Zejin Shi, Hitoshi Shigeoka, Imran Tahir, and Kensuke Teshima for their useful comments and encouragement. I also thank the participants at the Asian and Australasian Society of Labour Economics 2019, the Empirical Micro Research Seminar, the Center for Research and Education in Program Evaluation, the poster session of the Japanese Economic Association, and Hitotsubashi University as well as the lecture participants at the Microeconomic Policy Seminar for their many comments. Finally, I appreciate three anonymous referees and editors of the *Journal of Population Economics*, especially Shuaizhang Feng and Madeline Zavodny, for their critical comments that have been useful to substantially develop this study. This research uses micro data from the Preference Parameters Study of Osaka University's 21st Century COE Program "Behavioral Macrodynamics Based on Surveys and Experiments" and its Global COE project "Human Behavior and Socioeconomic Dynamics." I acknowledge the program/project's contributors: Yoshiro Tsutsui, Fumio Ohtake, and Shinsuke Ikeda.

Funding This study received financial support from the Japan Society for the Promotion of Science (JSPS Grant Number 20J14435).

Data availability Some appendix tables are available in the working paper version of this paper: Kawarazaki (2022).

Table 27 Heterogeneous effects

Outcomes	With More- Educated mothers and Fathers (At Least High School Completion)				With Less- Educated Mothers (Less Than High School Completion)				With Less- Educated Fathers (Less Than High School Completion)				With Less- Educated mothers and Fathers (Less Than High School Completion)			
	OLS	IV	N	R-squared	OLS	IV	N	R-squared	OLS	IV	N	R-squared	OLS	IV	N	R-squared
log(Income)	−0.010	0.116	268	0.415	−0.007	0.457**	1560	0.360	−0.035	0.500***	1402	0.339	−0.017	0.522***	1315	0.353
	(0.149)	(1.051)				(0.055)	(0.182)			(0.064)	(0.175)		(0.059)	(0.185)		
Working Status	−0.025	−0.470	407	0.218	0.024	−0.023	2121	0.181	0.021	0.013	1912	0.185	0.027	0.001	1793	0.198
(Work = 1)	(0.049)	(0.321)				(0.022)	(0.086)			(0.022)	(0.099)		(0.025)	(0.097)		
Working Hours	−0.632	29.621	219	.	−0.125	−0.063	1207	0.243	−0.078	0.314	1080	0.255	−0.388	0.505	1010	0.249
	(3.049)	(45.784)			(1.011)	(3.682)			(1.106)	(4.316)			(1.154)	(4.351)		
log(Wage)	0.167	0.066	163	0.413	0.049	0.532***	1014	0.146	0.025	0.550***	906	0.123	0.025	0.564***	845	0.134
(in Ten Thousand Yen)	(0.129)	(0.818)			(0.052)	(0.163)			(0.046)	(0.191)			(0.049)	(0.182)		
College Completion	0.130	0.817*	407	0.104	0.095***	0.348***	2121	0.078	0.083**	0.444***	1912	0.016	0.102***	0.464***	1793	0.001
(Complete = 1)	(0.082)	(0.488)			(0.033)	(0.134)			(0.033)	(0.155)			(0.033)	(0.160)		
High School Completion	−0.026	−0.223	407	0.226	0.038**	−0.009	2121	0.052	0.039**	0.009	1912	0.057	0.045**	0.008	1793	0.059
(Complete = 1)	(0.026)	(0.176)			(0.015)	(0.059)			(0.016)	(0.058)			(0.018)	(0.065)		

All the regressions include the controls (the number of younger siblings, the number of older siblings, mothers' age and fathers' age) and the fixed effects (cohort fixed effects, area fixed effects, and their interactions). Estimated standard errors clustered by each area and cohort are in parentheses. Regression coefficients of the OLS are estimated based on the Eq. (1) and those of the IV are based on Eqs. (1), (4), and (6). Sample is selected by omitting observations with missing values in the variables on gender, income, working status, the number of younger siblings, that of older siblings, parent's ages, parents' education levels, high school completion, college completion, enrolment status in ECEC at age 4, propensity scores of enrolment in ECEC at age 4, and living area at age 13. I also exclude sample who lives, at the age 15, outside of Japan or whose living place is unknown. Attrition status is reported in Appendix A. See Table 15 for the detail. Columns (1), (3), (5), (7), (9), (11), (13), and (15) show the result of the OLS and (2), (4), (6), (8), (10), (12), (14), and (16) show the result of the IV analysis.

a. US \$1 ≈ ¥110. I convert the original categorical outcome into the mean of their range. See Appendix A. As to the outcome of "Working Hours," I exclude the observations answering that they do not work. Therefore, I focus on the internal margin here. As to the wage, I mainly use the variable on monthly income. I omit those who did not report it. In Appendix, I show the table where I use hourly wage and calculate the monthly income for those who do not report it. Colleges include both a four-year university/college and a two-year college. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table 28 Propensity score estimation: Enrolment at age 4

	Dependent Variable: ECEC at Age 4 (Enrol = 1)							
	Probit Model						Linear Probability Model	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction at Age 4	1.734***	1.733***	1.733***	1.730***	1.729***	1.729***	0.479***	0.500**
	(0.173)	(0.155)	(0.049)	(0.175)	(0.138)	(0.052)	(0.054)	(0.013)
Gender	0.005	−0.003	−0.003	0.025	0.020	0.020***	0.001	0.005
	(0.048)	(0.048)	(0.002)	(0.056)	(0.052)	(0.006)	(0.013)	(0.002)
Other Controls				x	x	x		x
Fixed Effects								
Area Level	x	x	x	x	x	x	x	x
Prefecture Level	x			x			x	
Cohort Level		x	x		x	x		x
Interaction of		x	x		x	x		x
Area and Cohort								
Different Cluster			x			x		x
Mean of the Fitted Value	0.782	0.783	0.783	0.781	0.782	0.782	0.783	0.782
	(0.147)	(0.119)	(0.119)	(0.149)	(0.123)	(0.123)	(0.146)	(0.122)
R-squared	–	–	–	–	–	–	0.125	0.087
N	2690	2693	2693	2328	2331	2331	2693	2331

Estimated standard errors clustered by prefectures are in parentheses except for Columns with an indicator in Different Cluster. I use the samples who answered the question on the outcome variable. If any control is missing, I substitute the missing with zero and control the dummy for the substitution. I exclude samples who lived outside of Japan or whose living place is unknown at the age of 15. Attrition status is reported in Appendix A. See Table 15 for details. The regression coefficients of the reduced form are estimated based on the Eq. (6) and those of the IV are based on Eqs. (1), (4), and (5). The variable "Fraction at Age 4 " is the fraction of the number of enrolments/capacities in ECEC in each prefecture in each cohort to the population of the the cohort in the prefecture made by Eq. (2). In "Other Controls," I include the number of younger siblings, the number of older siblings, mothers' age and fathers' age. As to the area level cluster, there are ten areas over Japan and divide the 47 prefectures into 10. See Table 13 and Fig. 10 in Appendix A. The cohort level cluster is made as groups of every-five-year cohorts. In Columns (3), (6), and (8), I use the area (10 areas over Japan), cohort (5 year unit), and their interaction level cluster. In the row of "Mean of the Fitted Value," the fitted value calculated based on each specification. The standard deviations are in the parentheses. R-squared is not reported for the Probit model. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

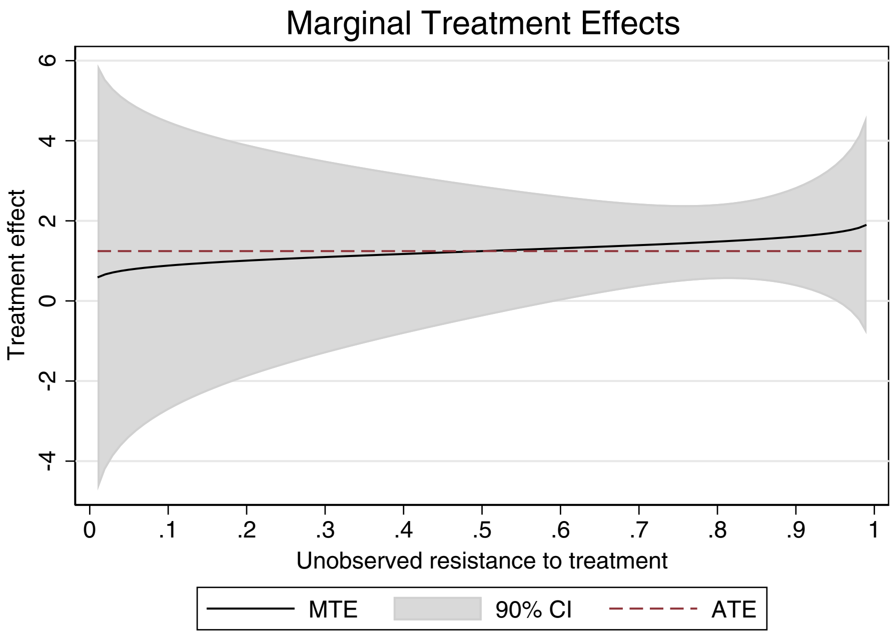

Fig. 11 Marginal treatment effects

References

Akabayashi H, Tanaka R (2013) Long-term effects of preschooling on educational attainments. Keio/Kyoto Joint Global COE Discussion Paper Series

Allport G, Odbert H (1936) Trait-names: A psycho-lexical study. Psychological Monographs 47(1):i

Angrist J (2001) Estimation of limited dependent variable models with dummy endogenous regressors: Simple strategies for empirical practice. Journal of Business & Economic Statistics 19(1):2–28

Angrist J, Pischke J-S (2008) Mostly harmless econometrics: An empiricist's companion. Princeton University Press

Asai Y, Kambayashi R, Yamaguchi S (2015) Childcare availability, household structure, and maternal employment. Journal of the Japanese and International Economies 38:172–192

Asai Y, Kambayashi R, Yamaguchi S (2016) Crowding-out effect of subsidized childcare. Hamilton ON: McMaster University

Baker M (2011) Innis lecture: Universal early childhood interventions: What is the evidence base? Canadian Journal of Economics/Revue canadienne d'économique 44(4):1069–1105

Baker M, Gruber J, Milligan K (2008) Universal child care, maternal labor supply, and family well-being. Journal of Political Economy 116(4):709–745

Baker M, Gruber J, Milligan K (2019) The Long-run impacts of a universal child care program. American Economic Journal: Economic Policy 11(3):1–26

Banerjee A, Banerji R, Berry J, Duflo E, Kannan H, Mukerji S, Shotland M, Walton M (2017) From proof of concept to scalable policies: Challenges and solutions, with an application. Journal of Economic Perspectives 31(4):73–102

Barnett S (2011) Effectiveness of early educational intervention. Science 333(6045):975–978
Baron R, Kenny D (1986) The moderator-mediator variable distinction in social psychological research: Conceptual, Strategic, and statistical considerations. Journal of Personality and Social Psychology 51(6):1173
Becker G (1994) Human capital revisited. In: Human capital: A Theoretical and empirical analysis with special reference to education (3rd Edition). The University of Chicago Press, pp 15–28
Berlinski S, Galiani S (2007) The effect of a large expansion of pre-primary school facilities on preschool attendance and maternal employment. Labour Economics 14(3):665–680
Berlinski S, Galiani S, Gertler P (2009) The effect of pre-primary education on primary school performance. Journal of Public Economics 93(1–2):219–234
Blanden J, Del Bono E, McNally S, Rabe B (2016) Universal Pre-school education: The case of public funding with private provision. The Economic Journal 126(592):682–723
Bold T, Kimenyi M, Mwabu G, Ng'ang'a A, Sandefur J (2018) Experimental evidence on scaling up education reforms in Kenya. Journal of Public Economics 168:1–20
Borghans L, Duckworth AL, Heckman J, Ter Weel B (2008) The economics and psychology of personality traits. Journal of Human Resources 43(4):972–1059
Burgess S, Daniel R, Butterworth A, Thompson S, EPIC-InterAct Consortium (2014) Network mendelian randomization: Using genetic variants as instrumental variables to investigate mediation in causal pathways. Int J Epidemiol 44(2):484–495
Busse A, Gathmann C (2020) Free daycare policies, family choices and child development. Journal of Economic Behavior & Organization 179:240–260
Cameron C, Trivedi P (2005) Microeconometrics: Methods and applications. Cambridge University Press
Campbell F, Conti G, Heckman J, Moon SH, Pinto R, Pungello E, Pan Y (2014) Early childhood investments substantially boost adult health. Science 343(6178):1478–1485
Carneiro P, Ginja R (2014) Long-term impacts of compensatory preschool on health and behavior: Evidence from head start. American Economic Journal: Economic Policy 6(4):135–73
Cascio EU (2009) Maternal labor supply and the introduction of kindergartens into American public schools. Journal of Human Resources 44(1):140–170
Chan M, Liu K (2018) Life-cycle and intergenerational effects of child care reforms. Quantitative Economics 9(2):659–706
Chetty R, Friedman J, Hilger N, Saez E, Schanzenbach DW, Yagan D (2011) How does your kindergarten classroom affect your earnings? Evidence from project STAR. Quarterly Journal of Economics 126(4):1593–1660
Conti G, Heckman JJ, Pinto R (2016) The effects of two influential early childhood interventions on health and healthy behaviour. Economic Journal 126(596):F28–F65
Cornelissen T, Dustmann C, Raute A, Schönberg U (2018) Who benefits from universal child care? estimating marginal returns to early child care attendance. Journal of Political Economy 126(6):2356–2409
Council for Designing 100-Year Life Society (Jinsei 100 Nen Jidai Kousou Kaigi) (2018) A revolution in human resources development: basic concept (Hito Zukuri Kakumei Kihon Kousou). Prime Minister of Japan and His Cabinet (in Japanese)
Currie J, Almond D (2011) Human capital development before age five. In: Handbook of labor economics. vol. 4. Elsevier. pp. 1315–1486
Currie J, Thomas D (1995) Does head start make a difference? American Economic Review 85(3):341–364
Dietrichson J, Kristiansen IL, Viinholt BA (2020) Universal preschool programs and long-term child outcomes: A systematic review. Journal of Economic Surveys 34(5):1007–1043
Drange N, Havnes T (2019) Early childcare and cognitive development: Evidence from an assignment lottery. Journal of Labor Economics 37(2):581–620
Duflo E (2001) Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. American Economic Review 91(4):795–813
Felfe C, Nollenberger N, Rodríguez-Planas N (2015) Can't buy mommy's love? Universal childcare and children's long-term cognitive development. Journal of Population Economics 28(2):393–422
Fitzpatrick MD (2010) Preschoolers enrolled and mothers at work? The effects of universal prekindergarten. Journal of Labor Economics 28(1):51–85
Fitzpatrick MD (2012) Revising our thinking about the relationship between maternal labor supply and preschool. Journal of Human Resources 47(3):583–612

Fort M, Ichino A, Zanella G (2020) Cognitive and noncognitive costs of day care at age 0–2 for children in advantaged families. Journal of Political Economy 128(1):158–205
Garces E, Thomas D, Currie J (2002) Longer-term effects of head start. American Economic Review 92(4):999–1012
Gelbach JB (2002) Public schooling for young children and maternal labor supply. American Economic Review 92(1):307–322
Goldberg L (1990) An alternative “description of personality'': The big-five factor structure. Journal of Personality and Social Psychology 59(6):1216
Goldberg L (1992) The development of markers for the big-five factor structure. Psychological Assessment 4(1):26
Gosling S, Rentfrow P, Swann W Jr (2003) A very brief measure of the big-five personality domains. Journal of Research in Personality 37(6):504–528
Goux D, Maurin E (2010) Public school availability for two-year olds and mothers' labour supply. Labour Economics 17(6):951–962
Griffen A (2018) Evaluating the effects of child care policies on children's cognitive development and maternal labor supply. J Hum Resour
Haeck C, Lebihan L, Merrigan P (2018) Universal child care and long-term effects on child well-being: evidence from Canada. Journal of Human Capital 12(1):38–98
Hanaoka C, Shigeoka H, Watanabe Y (2018) Do Risk preferences change? Evidence from the great east Japan earthquake. American Economic Journal: Applied Economics 10(2):298–330
Hanna R, Olken BA (2018) Universal basic incomes versus targeted transfers: Anti-poverty programs in developing countries. Journal of Economic Perspectives 32(4):201–26
Havnes T, Mogstad M (2011) Money for nothing? universal child care and maternal employment. Journal of Public Economics 95(11–12):1455–1465
Havnes T, Mogstad M (2011) No child left behind: Subsidized child care and children's long-run outcomes. American Economic Journal: Economic Policy 3(2):97–129
Havnes T, Mogstad M (2015) Is universal child care leveling the playing field? Journal of Public Economics 127:100–114
Hayes A (2018) Introduction to mediation, moderation, and conditional process analysis: A regression-based approach, 2nd edn. The Guilford Press, New York
Heckman J (2013) Giving kids a fair chance. MIT Press
Heckman J, Masterov D (2007) The productivity argument for investing in young children. Applied Economic Perspectives and Policy 29(3):446–493
Heckman J, Pinto R, Savelyev P (2013) Understanding the mechanisms through which an influential early childhood program boosted adult outcomes. American Economic Review 103(6):2052–86
Heckman J, Moon SH, Pinto R, Savelyev P, Yavitz A (2010) The rate of return to the highscope perry preschool program. Journal of Public Economics 94(1–2):114–128
Herbst C (2017) Universal child care, maternal employment, and children's long-run outcomes: Evidence from the US Lanham Act of 1940. Journal of Labor Economics 35(2):519–564
House of Councillors, The National Diet of Japan (1967a) Minutes of the committee on education meeting, the Fifty-fifth Meeting of the House of Councillors. No. 20
House of Councillors, The National Diet of Japan (1967b) Minutes of the Fifty-fifth Meeting of the House of Councillors. No. 5
Imai K, Keele L, Tingley D (2010) A general approach to causal mediation analysis. Psychological Methods 15(4):309
Kane T, Rouse CE (1995) Labor-market returns to two-and four-year college. American Economic Review 85(3):600–614
Kawai K (1979) Japan's American interlude. University of Chicago Press
Kawarazaki H (2022) Early childhood education and care: effects after half a century and their mechanisms. SSRN Working Paper No. 3394392
Kikuchi N (2017) Marginal returns to schooling and education policy change in Japan. ISER Discussion Paper No. 996
Knudsen E, Heckman J, Cameron J, Shonkoff J (2006) Economic, neurobiological, and behavioral perspectives on building America's future workforce. Proceedings of the National Academy of Sciences 103(27):10155–10162
Kottelenberg M, Lehrer S (2017) Targeted or universal coverage? assessing heterogeneity in the effects of universal child care. Journal of Labor Economics 35(3):609–653

Kousei Shou Jidou Kyoku (1959) 10 Years of Child Welfare (Jidou Fukushi Junen No Ayumi) (Nihon Jidou Mondai Chosa Kai (in Japanese)

Kuehnle D, Oberfichtner M (2020) Does starting universal childcare earlier influence children's skill development? Demography 57(1):61–98

Lavy V (2018) The long-term consequences of free school choice. Working Paper

Lefebvre P, Merrigan P (2008) Child-care policy and the labor supply of mothers with young children: A natural experiment from Canada. Journal of Labor Economics 26(3):519–548

Matsushima N (2015) Postwar History of "Nursing Care": Popularization of kindergartens/nursery schools and their regional differences ("Hoiku" No Sengo Shi: Youchien/Hoikujo no Hukyu to Sono Chiikisa) (Rikka Shuppan (in Japanese)

Mincer J (1974) Schooling, experience, and earnings. Human Behavior & Social Institutions No. 2 (Natonal Bureau of Economic Research

Minister of Ministry of Education's Secretariat Survey Division (1972) II investigation of social demands on early childhood education (II Yoji Kyoiku Ni Kansuru Syakaiteki Yosei No Chosa Showa 45 Nendo)." Minister of Ministry of Education (in Japanese)

Minister's Secretariat, Ministry of Education, Culture, Sports, Science and Technology (1980) Standard of education in Japan (Waga Kuni No Kyoiku Suijun). Ministry of Education, Culture, Sports, Science and Technology (in Japanese)

Ministry of Education (1979) A hundred year history of kindergarten education (Yochien Kyoiku Hyakunen Shi) (Hikarinokuni Kabushiki Gaisha (in Japanese)

Miyake K (1989) The phychology of infant/child care (Nyuyoji Hoiku Shinrigaku) (Fukuyama Shuppan (in Japanese)

Motoki H, Yamanishi H (2009) Family change and the reformation of day nursery policies. Journal of Kyushu University of Health and Welfare 10:99–110

Myers R (1995) Preschool education in Latin America: Estate of the Practice. PREAL

Niimi K (2002) Economic analysis of market emphasized childcare reform (Shijo Jushi no Hoiku Kaikaku no Keizai Bunseki). Japan Research Review 4:14–111 ((**in Japanese**))

Nollenberger N, Rodríguez-Planas N (2015) Full-time universal childcare in a context of low maternal employment: Quasi-experimental evidence from Spain. Labour Economics 36:124–136

OECD (2016) Who uses childcare? background brief on inequalities in the use of formal early childhood education and care (ECEC) among very young children. OECD Publishing, Paris

Okada M (2014) A Reform of child welfare law and relationship between nursery schools and kinder garten (Jido Hukushi Hou No Hensen To You-Ho No Kankei). In: History of postwar nursery education [4]: Formulating preschool education system (Sengo Hoiku 50 Nen Shi [Dai 4 Kan]: Hoiku Seido Kaikaku Koso). ed. Shoko Ikeda and Teido Tomomatsu, pp. 89–90. Nihon Tosho Center (in Japanese)

Oshio A, Abe S, Cutrone P (2012) Trial of making ten item personality inventory (TIPI-J) in Japanese (Nihongo Ban Ten Item Personality Inventory (TIPI-J) Sakusei No Kokoromi). Pesonality Kenkyu 21(1):40–52

Phillips D, Shonkoff J (2000) From neurons to neighborhoods: The science of early childhood development. National Academies Press

Roberts B, DelVecchio W (2000) The rank-order consistency of personality traits from childhood to old age: A quantitative review of longitudinal studies. Psychological Bulletin 126(1):3

Roberts B, Walton K, Viechtbauer W (2006) Patterns of mean-level change in personality traits across the life course: A meta-analysis of longitudinal studies. Psychological Bulletin 132(1):1

Sanders C, Taber C (2012) Life-cycle wage growth and heterogeneous human capital. Annual Review Economics 4(1):399–425

Shakai Fukushi Jigyo Shinko Kai (1963) Design and commentary of childcare facilities (Hoiku Shisetsu No Sekkei To Kaisetsu) (Shokokusha (in Japanese)

Shimizu T (2008) Changes in policies on foreign people and various proposals (Gaikokujin Sekisaku No Hensen To Kakushu Teigen). In: Foreigners in society under population decline: Comprehensive survey report (Jinkou Genshou Shakai No Gaikokujin Mondai: Sougou Chousa Houkokusho). ed. Kokuritsu Kokkai Toshokan Chousa Oyobi Rippou Kousakyoku, pp. 31–41. Kokuritsu Kokkai Toshokan

Shwalb DW, Shwalb BJ, Sukemune S, Tatsumoto S (1992) Japanese nonmaternal child care: Past, present, and future. In: Lamb M, Sternberg K, Hwang C-P, Broberg A (eds) Child care in context: Cross-cultural perspectives. Chapter 10. Erlbaum Hillsdale, NJ, pp 331–353

Takada M (2014) Relationship between nursery schools and kinder garten (Hoikujo to Yochien no Kankei). In: History of postwar nursery education [4]: Formulating preschool education system (Sengo Hoiku 50 Nen Shi [Dai 4 Kan]: Hoiku Seido Kaikaku Koso). ed. Shoko Ikeda and Teido Tomomatsu, pp 89–90. Nihon Tosho Center (in Japanese)

Takayama N (1982) Pricing day-care services for children-the ability-to-pay principle reconsidered-. Keizai Kenkyu (in Japaneses) 33(3):239–250

The Japan Institute for Labour Policy and Training (2020) Quick look at long-term labor statistics in graphs (Haya Wakari, Gurafu De Miru Choki Roudou Toukei). https://www.jil.go.jp/kokunai/statistics/timeseries/index.html (in Japanese. Accessed 1Apr 2022)

Thomas S (2003) Longer-term economic effects of college selectivity and control. Research in Higher Education 44(3):263–299

Tobin J, Wu D, Davidson D (1991) Preschool in three cultures: Japan, China, and the United States. Yale University Press

Todd P, Wolpin K (2003) On the specification and estimation of the production function for cognitive achievement. Economic Journal 113(485):F3–F33

Todd P, Wolpin K (2007) The production of cognitive achievement in children: Home, School, and Racial Test Score Gaps. Journal of Human Capital 1(1):91–136

Ueyama K (2011) Factors behind the disparities in the university enrollment rate between prefectures and their changes (Daigaku Shingaku-ritsu No Todofuken-kan Kakusa No Youin Kouzou To Sono Henyou). Kyoiku Shakaigaku Kenkyu (in Japaneses) 88:207–227

United Nations (2015) Transforming our world: The 2030 agenda for sustainable development. United Nations, New York

Wooldridge J (2010) Econometric analysis of cross section and panel data. MIT press

Yamaguchi S (2017) Family policies and female employment in Japan. Japanese Economic Review 68(3):305–322

Yamaguchi S, Asai Y, Kambayashi R (2018) Effects of subsidized childcare on mothers' labor supply under a rationing mechanism. Labour Economics 55:1–17

Yamaguchi S, Asai Y, Kambayashi R (2018) How does early childcare enrollment affect children, parents, and their interactions? Labour Economics 55:56–71

Yoshimi M (2001) Historical change of nursery teacher's examination and the problem in the future (Hoikushi Shiken No Rekishiteki Hensen To Kongo No Kadai). Bulletin of Niigata Prefectural Junior College for Women (Kenritsu Niigata Joshi Tanki Daigaku Kenkyu Kiyou) (in Japanese), 38: 15–27

Zen-Nihon Shi You Ren Soumu Iinkai (2017) Handbook (Yoran). Zen-Nihon Shiritsu Yochien Rengo Kai (in Japanese)

Zhang C, Managi S (2021) Childcare availability and maternal employment: New evidence from Japan. Economic Analysis and Policy 69:83–105