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The Effect of a Universal Preschool Programme on Long-Term Health Outcomes: Evidence from Spain

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Abstract

Early childhood education programmes are expected to improve child conditions including educational attainment, labour, and health outcomes. This study evaluates the effect of a Spanish universal preschool programme, which implied a large-scale expansion of full-time high-quality public preschool for three-year-olds in 1991, on long-term health. Using a difference-in-differences approach, I exploit the timing of the policy and the differential initial speed of implementation of public preschool expansion across regions. I compare long-term health of cohorts aged three before to those aged three after the start of the policy residing in regions with varying initial implementation intensity of the programme. The results show that the policy does not affect long-term health outcomes and use of healthcare services, except for two outcomes. A greater initial intensity in public preschool expansion by 10 percentage points decreases the likelihood of being diagnosed with asthma by 2.1 percentage points, but hospitalisation rates increase by 2.7%. The findings indicate that the effect on asthma is larger for men, hospitalisation rates are higher for pregnant women, and disadvantaged children benefit the most in terms of a lower probability of taking medicines and being diagnosed with asthma and mental health disorders.

Keywords: Universal Preschool Programme; Long-Term Effects; Health Outcomes; Difference-in-Differences; Spain.

JEL Codes: I10, I28, J13.

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1. Introduction

Investments in human capital such as education boost the efficiency of the production function of an individual's health capital (Grossman, 1972). These investments are more productive in early life since their rate of return declines as children grow up (Cunha et al., 2006). Early life experiences are considered the cornerstone of the brain architecture accountable for determining long-term cognitive and non-cognitive skills, and physical and mental health (Duncan & Magnuson, 2013; Knudsen et al., 2006; Sapolsky, 2004), and have been found to persistently impact later-life child human capital development (Almond & Currie, 2011). Evidence has established that early childhood education programmes can affect child conditions in many domains ranging from education, income, and employment to health (Almond et al., 2018) throughout the life course (Ruhm & Waldfogel, 2012).

In the last years, discrepancies on whether preschool should be targeted or universal have played the lead in early education policy debates in the United States (Lieberman, 2015). Countries also differ in their approach in Europe where less than half of them provides universal access to preschool at age three and only eight guarantee a place in preschool before age three in 2018/19 (European Commission/EACEA/Eurydice, 2019). Given how decisive early life conditions are for child human capital development, policymakers aim at assessing which type of preschool (whether targeted or universal) benefits children and countries more.

Research on early childhood education interventions has mostly focused on programmes targeted at disadvantaged children (e.g. Perry Preschool Project, Carolina Abecedarian Project, Head Start in the United States), which overall pointed to long-run improvements in a wide set of outcomes including health (e.g. Campbell et al., 2014; Carneiro & Ginja, 2014; Conti et al., 2016; Garces et al., 2002; Heckman et al., 2010; Ludwig & Miller, 2007). These findings however cannot be generalised to *universal* programmes for two reasons (Baker, 2011). First, children at risk might react differently to universal programmes which may be cheaper in terms of cost per child and differentiate less among students than targeted programmes. Second, more advantaged children could show different responses than less advantaged children to common treatments. Few studies analysed instead the impact of universal early education programmes, especially on health, and found mixed results (see Cascio (2015), Dietrichson et al. (2020), and van Huizen & Plantenga (2018) for literature reviews).

In this study, I evaluate a Spanish universal preschool reform (the *Organic Act on the General Organisation of the Education System*, hereafter LOGSE) and its effects on health outcomes (health status, chronic conditions, consumption of medicines, mortality) and healthcare use (doctor, hospital, and emergency service visits, hospitalisations) at ages 11-27. The LOGSE comprised a large-scale expansion of full-time high-quality public preschool for three-year-olds in 1991/92 school year implying an increase in public enrolment rates of almost 20 percentage points (p.p.) over the first four years of implementation, from about 10% in 1990/91 to 30% in 1993/94. Despite being nationally enacted, the implementation of the LOGSE was the responsibility of the Spanish regions. This allows to exploit the fact that the initial intensity in public preschool expansion varied across regions.

To study the effect of the policy on long-term health, I use both survey and administrative data and employ a difference-in-differences (DiD) strategy exploiting the timing and geographical variation of the implementation of the reform. I compare long-term health of cohorts aged three before to those aged three after the start of the programme, across individuals either residing or born in regions with varying initial intensity (measured as the regional increase in public enrolment rates of three-year-olds between 1990/91 and 1993/94) in public preschool implementation.

Overall, the findings show that the LOGSE has no effect on long-term health, except for two indicators. First, an increase of 10p.p. in the initial intensity in public preschool expansion reduces the probability of being diagnosed with asthma by 2.1p.p. for individuals aged three post-policy. This result can be interpreted as children attending preschool might attain higher levels of immunity during childhood (*hygiene hypothesis*), certain illnesses could be detected by preschool teachers and thus treated earlier, or preschool may be a more productive and healthier environment than staying at home. The decrease in the probability of being diagnosed with asthma is larger for men. Second, the LOGSE increases hospitalisation rates by 2.7%. This result is contrary to the main hypothesis that preschool improves children's long-term health. Although the effect on the remaining health outcomes is statistically insignificant, their sign goes in the expected direction indicating that the LOGSE affects positively long-term health and pointing that the rise in hospitalisations is mainly due to a change towards a higher use of healthcare. The increase in hospitalisations due to a higher healthcare use could be explained by the fact that the LOGSE boosts educational attainment and maternal employment (Felfe et al., 2015; Nollenberger & Rodríguez-Planas, 2015) and previous evidence showed that rich and high-educated individuals use specialist healthcare more (van Doorslaer et al., 2004). I also find that the rise in hospitalisations is driven by pregnant women.

I conduct a heterogeneity analysis by parental education to study the potential differing reactions to universal programmes of more and less advantaged children (Baker, 2011). I find that the LOGSE decreases the probability of being diagnosed with asthma for children with low-educated parents and reduces the likelihood of being diagnosed with mental health disorders and taking medicines for children with medium-educated parents. Children with lower socioeconomic status (SES) might have a lower productivity of time spent with parents than the productivity of time spent in formal high-quality childcare. Children with medium-educated parents also have a higher probability of visiting an emergency service.

This study contributes to our understanding of the long-term effects of early childhood education programmes in three ways. First, this investigation contributes to the limited literature on the effect of *universal* early education programmes on long-term health by analysing young adults at ages 11-27, since most studies had a short-term horizon (Baker et al., 2008; Cornelissen et al., 2018; Kottelenberg & Lehrer, 2013, 2014, 2018; van den Berg & Siflinger, 2022). Three studies examined long-term health by analysing teenagers up to age 20 in Canada (Baker et al., 2019; Haeck et al., 2018) and studying adults in their 30s and 40s in Norway (Breivik et al., 2020). Instead of considering only adolescence, I also focus on early adulthood which comprises those years when physical development is at its peak and individuals start taking first lifetime decisions (e.g. emancipating, going to college, entering the labour force, finding a partner, having children).

Second, the effects of early education programmes depend on the counterfactual mode of care that children would enrol in absence of the programme, i.e. parental care, informal out-of-home care, or formal out-of-home care (Blau & Currie, 2006; Havnes, 2012). The limited evidence on the effect of universal early education programmes on long-term health has focused on countries (Norway and Canada) with high female employment rates, policies targeting at work-family balance, and growing economies (Nollenberger & Rodríguez-Planas, 2015). Therefore, these studies interpreted their results as the impacts of universal programmes on health due to a change in the type of out-of-home care from informal to formal (Baker et al., 2019; Breivik et al., 2020; Haeck et al., 2018). Instead, I analyse a setting with low female labour participation, high unemployment rates, low levels of childcare supply, and few family-friendly policies as the case of Spain in the late 1980s and early 1990s, whose universal preschool programme crowded out family care (Felfe et al., 2015; Nollenberger & Rodríguez-Planas, 2015). These differences in characteristics make Spain an interesting case to analyse as previous evidence focused on countries (Norway and Canada) whose general population was likely to have higher SES than that of the Spanish population and, thus, whose universal childcare had potential different effects than the LOGSE.

Third, the only evidence on how the LOGSE affected child outcomes is the study by Felfe et al. (2015) who analysed cognitive development. Instead, I explore for the first time the effect of the LOGSE on (long-term) health by employing different data and slightly deviating from Felfe et al. (2015)'s methodology (using a continuous rather than a binary treatment).

The structure of the remainder of this study is as follows. Section 2 provides a literature review, outlines the mechanisms behind the long-term health effects of universal early education programmes, and explains the institutional setting. Section 3 defines the empirical strategy and Section 4 describes the data. Section 5 presents the main results, Section 6 tests their robustness, and Section 7 studies their heterogeneity. Finally, Section 8 concludes.

2. Background

2.1. Related Literature

Few studies analysed how universal early education programmes affected health with mixed findings[1]. Regarding short-term health outcomes, van den Berg & Siflinger (2022) examined the impact of a day-care reform in Sweden in 2002, which implied a reduction of fees, a supply expansion for children aged one to five, and a crowding out of informal care. Their results showed an improvement in mental health after age three, a rise in infectious and other childhood diseases, but a later decrease due to immunity in the latter outcomes. There was also a rise (decrease) in medical visits at ages two to three (six to seven). Cornelissen et al. (2018) explored a universal childcare programme in 1996 for which a subsidised slot was guaranteed to all children from their third birthday in Weser-Ems, Germany. They found no effect on health measured by body mass index and risk of overweight during childhood.

Several investigations studied the short-term health effects of a universal subsidised childcare programme in Quebec (Canada) in the late 1990s that crowded out informal care. Baker et al. (2008) reported that the childcare programme had a detrimental effect on health status and a

[1] Most studies focused on cognitive skills measured by test scores (e.g. Baker et al., 2008, 2019; Berlinski et al., 2009; Blanden et al., 2016; Carta & Rizzica, 2018; Gormley & Gayer, 2005) and maternal labour supply (e.g. Andresen & Havnes, 2019; Berlinski & Galiani, 2007; Fitzpatrick, 2010; Havnes & Mogstad, 2011b; Herbst, 2017; Lefebvre & Merrigan, 2008). Others also analysed non-cognitive skills (e.g. Berlinski et al., 2009; Datta Gupta & Simonsen, 2010; Felfe & Lalive, 2018), educational attainment (e.g. Berlinski et al., 2008; Havnes & Mogstad, 2011a), and labour outcomes (e.g. Cascio, 2009; Havnes & Mogstad, 2011a, 2015; Herbst, 2017) and to a lesser extent parental well-being (e.g. Baker et al., 2008; Brodeur & Connolly, 2013) and crime behaviour (e.g. Baker et al., 2019; Cascio, 2009). Findings when evaluating these outcomes are also inconclusive.

positive effect on the probability of having nose, throat or ear infection at ages 0-4, mainly driven by being a low-quality programme compared to the counterfactual mode of care. Related studies showed that newer cohorts entering the programme also experienced negative effects (Kottelenberg & Lehrer, 2013), these were greater for those enrolled younger (Kottelenberg & Lehrer, 2014), and results were heterogenous by gender (Kottelenberg & Lehrer, 2018)[2].

Three studies analysed the long-term health effects of universal childcare programmes. Breivik et al. (2020) examined adult health of individuals affected by the 1975 universal childcare reform in Norway, which expanded subsidised childcare places to all children aged three to six. They found that individuals aged 30-47 affected by the reform needed longer sickness absences and more primary healthcare visits related to normal pregnancies by 27% and 7%, respectively. They found that these same individuals used primary and specialist healthcare for mental health by 1.2%-2.0% and 3.3% less, respectively. These effects were driven by children of working mothers since the reform crowded out informal care and had no effect on maternal employment. The remainder of the studies analysed the long-term health effects of the Quebec programme. Baker et al. (2019) found that the negative short-term effects on self-reported health status persisted until ages 12-20 (7.3% increase of a standard deviation), but long-term mental health was not affected. Instead, Haeck et al. (2018) estimated that the negative short-term effects on self-reported health status and asthma attacks vanished as children grew up, and found a lower prevalence of mental health problems at ages 15-19.

Closely related to this study, two articles analysed the effect of the LOGSE in Spain. Felfe et al. (2015) focused on children's cognitive development at age 15 using PISA test data and found that individuals affected by the reform had higher reading test scores by 0.15 standard deviations and a lower prevalence of grade retention in primary school by 2.4p.p. The results are only significant for girls, children from disadvantaged backgrounds, and older cohorts. Second, Nollenberger & Rodríguez-Planas (2015) found an increase in maternal labour force participation using the Spanish Labour Force Survey. They estimated that ten additional three-year-olds enrolled in public preschool implied that two mothers joined the labour force.

[2] Other studies explored universal early childhood programmes that went beyond the expansion of childcare places. For instance, Cattan et al. (2021) analysed the short- and medium-term effects of the Sure Start on hospitalisations in England. The Sure Start is a universal programme that implied the opening of centres in which services to support children and parents were offered. They found that hospitalisations for one-year-olds increased by 10% for an additional centre, while hospitalisations for children aged 11-15 decreased by 7%-11%.

2.2. Mechanisms

The effect of early education programmes on long-term health might be through several channels[3]. The effect largely depends on the type and quality of the counterfactual mode of care. Preschool might imply a more enriching, stimulating and productive learning environment for children than home. Early skill learning is enduring over time, self-strengthening and encouraging in the acquisition of other abilities (*self-productivity*), while making future learning more efficient, productive, and likely to continue (*dynamic complementarity*) (Cunha & Heckman, 2007; Heckman, 2006). Investments in human capital improve individual's stock of health capital (Grossman, 1972) and earlier ones have a higher rate of return than later investments as their benefits are reaped for a lengthier period (Carneiro & Heckman, 2003). All these might imply that competences learnt in preschool may affect the evolution of health capital.

Several diseases might be influenced by genetics, but also shaped by environmental and lifestyle factors (e.g. nutrition, stress, pollution) surrounding children (Gilles et al., 2018; Tsuang et al., 2004). Parallelly, preschool staff can detect health problems at early stages, guide parents and recommend preventive practices (e.g. vaccination, check-ups) to minimise their consequences (Breivik et al., 2020). Early education policies could affect health through an early detection of illnesses as well as changes in child's environmental surroundings. For instance, the *hygiene hypothesis* (Strachan, 1989, 2000) states that children exposed to more pathogens (as may happen in preschool) experience higher infection rates in early life, while developing their immune system and getting protection for future diseases such as infectious and parasitic illnesses, respiratory problems, and allergies. Similarly, preschool might imply a safer environment for children than staying at home. However, childcare attendance is associated with consumption of antibiotics (Thrane et al., 2001), whose overuse can have long-lasting detrimental effects such as metabolic, immune, and neurodevelopmental and behavioural disorders especially if taken during childhood (Neuman et al., 2018). Children could also suffer from anxiety and stress due to being in preschool and separation from the primary caregiver (Howard et al., 2011; Vermeer & Groeneveld, 2017).

[3] The mechanisms presented are based on Breivik et al. (2020).

Indirect effects of preschool programmes may be through improvements in children's well-being and SES due to higher educational attainment, labour force participation, and earnings as well as fostering of parental (mainly maternal) employment and household income.

2.3. Institutional Setting

Education system before the reform

Over the 1970s and 1980s, the Spanish education was regulated by the *Education General Act* (LGE, 1970). Compulsory schooling comprised ages 6-14 (primary education) and non-compulsory education covered the preschool (2-5 years old) and post-obligatory (over 14) periods[4]. Children were grouped in cohorts by year of birth and the school year spanned from September to June. Students enrolled either in public or private schools[5].

Preschool was divided into *Jardín de la Infancia* (ages 2-3) and *Escuela de Párvulos* (ages 4-5), which were offered both in public and private centres. Formal childcare for two- and three-year-olds was limited due to its high price given its private nature, few places being offered, and parents having little interest in enrolment at these ages (Calvo Rueda, 1994). Enrolment rates for two- and three-year-olds in 1990/91 were 7.0% (1.2% in public and 5.8% in private childcare) and 27.9% (10.5% in public and 17.3% in private childcare), respectively[6]. The remaining children under four stayed with their parents or grandparents, while informal care (certified caregivers who provide care in their homes) was almost missing (Felfe et al., 2015).

Full-day preschool enrolment rates at ages four and five were high reaching 94.1% and 100%, respectively. The reasons behind such high rates were that children closer to the compulsory schooling age of six needed several prerequisites to access primary education, more places were available, and the majority was supplied by the Ministry of Education (Calvo Rueda, 1994). Primary schools also supplied education at these ages and priority for five- and six-year-old matriculation existed for those students already enrolled in a specific school. Parents who

[4] The minimum legal working age was 14 until 1980 when it was raised up to 16. Del Rey et al. (2018), Bellés-Obrero, Cabrales, et al. (2021), and Bellés-Obrero, Jiménez-Martín, et al. (2021) analysed the effect of raising the minimum legal working age in Spain on education, labour, and health outcomes.

[5] Public schools were owned by the Ministry of Education or other public institutions. Public centres were free of charge and publicly funded. Private schools were owned by private entities and classified as *escuelas privadas concertadas (semi-private schools)*, whose funds stemmed from public subsidies and parents' payments, and *escuelas privadas no concertadas (private schools)*, completely financed by parents' instalments. The *escuelas privadas concertadas* were first regulated by the *Organic Act on the Right to Education* in 1985 (LODE, 1985).

[6] The source of data in this section comes from the Spanish Ministry of Education and Vocational Training (https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html).

preferred a specific primary school were highly encouraged to access it at the age of four since the probability of being accepted at later ages was much lower (Felfe et al., 2015).

Education system after the reform

The LOGSE was announced in October 1990 (LOGSE, 1990). Preschool continued being non-mandatory and was divided into a first (ages 0-2) and a second (ages 3-5) cycle. After 1990, the government regulated the supply of places for three-year-olds, which started being offered in primary schools, making preschool at three full-time (from 9am to 5pm), free of charge, and universal (enrolment was by lottery conditional on requesting admission) (Calvo Rueda, 1994; Felfe et al., 2015; Nollenberger & Rodríguez-Planas, 2015; van Huizen et al., 2019).

The reform also implied a regulated qualitative improvement in terms of a more pedagogical curriculum, teachers' qualification, and class size in the second cycle. According to the LOGSE, preschool contributed to child's physical, intellectual, affective, social, and moral development through experiences, activities, and games[7]. Teachers were to be graduates in pedagogy with a specialisation in preschool for three-year-olds, which previously was only required to teach four- and five-year-olds (Felfe et al., 2015). Class size was set up to a maximum of 25 in the second cycle (Muñoz-Repiso Izaguirre et al., 1992)[8,9].

The implementation of the LOGSE would extend over ten years, starting with the preschool component in 1991/92 (Real Decreto, 1991) affecting firstly the cohort born in 1988. Figure 1 plots enrolment rates for three-year-olds from 1987/88 to 2002/03 and shows how total enrolment rates for three-year-olds rose from 27.9% in 1990/91 up to 67.3% in 1996/97 and 94.3% in 2002/03, mainly driven by the large increase in public enrolment rates from 10.5% to 43.4% and 64.2% for the same years. The increase in public enrolment might respond to the fact that now parents who wanted their children to be enrolled in a specific primary school should do so at the age of three. Instead, private enrolment rates experienced a smoother growth. Nollenberger & Rodríguez-Planas (2015) and Felfe et al. (2015) estimated that the expansion of public preschool did not crowd out private preschool, but family care.

[7] In particular, it led students to 1) be aware of their body and possibilities for action, 2) interact with others via different ways of expression and communication, 3) observe and explore their natural, family, and social environment, and 4) gradually acquire autonomy in their usual activities (LOGSE, 1990).

[8] Despite being of high-quality, the programme was not exceptionally expensive and its benefits outweighed its costs (van Huizen et al., 2019).

[9] The LOGSE implied an expansion of preschool slots for three-years-olds to incentivise take-up and maternal labour demand, but it did not offer other services such as home visits, parental support, health services, etc.

[Figure 1]

Although the reform was national and funds came from the central government, the law emphasised that regions were fully in charge of the gradual implementation of the reform. Figure 2 illustrates the geographic distribution of the increase in public enrolment rates for three-year-olds in p.p. during the initial expansion period (1990/91-1993/94) across Spanish regions. The initial implementation intensity differed across regions with some of them achieving higher enrolment rates earlier than others. Regions with lower initial implementation intensity had less qualified teachers and tighter classroom space, while other regions implemented preschool faster thanks to the spillovers coming from a prior wider supply of private centres (Felfe et al., 2015).

[Figure 2]

The LOGSE also implied that the minimum school-leaving age increased up to age 16 and the decision about students' career track was postponed from age 14 to 16 (Bellés-Obrero & Duchini, 2021). In addition, compulsory schooling was split into primary (ages 6-12) and secondary (ages 12-16) education. The implementation of the (new) secondary compulsory component began in 1991/92 and had to reach complete enrolment rates by 1998/99 for age 14 and 1999/00 for age 15 (Real Decreto, 1991)[10]. Thus, children born in 1984 onwards were equally affected by the compulsory component (Lacuesta et al., 2020; Robles-Zurita, 2017), but differently by the preschool component as explained in Figure 3.

[Figure 3]

Spanish Health System

Before 1986, a Bismarck model was in place via a Social Security System for which healthcare could only be accessed by Social Security taxpayers (and their relatives) or through private services. Since 1986, the National Health System (NHS) provides healthcare coverage to residents in Spain that is universal, mainly financed through general taxation and free at the point of use, with the exception of co-payments for prescribed medicines (Bernal-Delgado et al., 2018; LGS, 1986). The NHS coexists with civil servants' and private health insurances (Jiménez-Martín & Viola, 2016).

[10] Although both the preschool and compulsory components were implemented in 1991/92, only the compulsory component implied that enrolment rates had to reach 100% after nine years.

Health competences were decentralised and transferred from the central government to the 17 Spanish regions since the introduction of the constitution in 1978 (García-Armesto et al., 2010). The Ministry of Health is accountable for basic health legislation, general coordination of health services, and pharmaceutical policy, while the regional Departments of Health are responsible for the funding, organisation and delivery of health services within their territory (García-Armesto et al., 2010). The transfers took place in 1981 for Catalonia, 1984 for Andalusia, 1988 for the Basque Country and the Valencian Community, 1991 for Navarre and Galicia, 1994 for the Canary Islands, and 2002 for the remaining regions (Costa-Font & Rico, 2006).

3. Methods

To estimate the causal effect of the Spanish universal preschool programme on children's long-term health, I exploit the timing and geographic variation of the expansion of public preschool education for three-year-olds in 1991/92.

Children's exposure to the LOGSE programme is determined both by year of birth and region of residence. The reform was announced in 1990/91, but the LOGSE preschool component started in 1991/92. All children born from 1988 onwards were aged three in 1991 or after and benefited from the programme, while children born in 1987 or earlier were three before 1991 and did not benefit from the policy (Figure 3, Panel A). In this study, the cohorts compared are individuals born in 1988-1991 and thus affected by the reform (post-reform cohorts), and children born in 1984-1987 and thus unaffected (pre-reform cohorts)[11].

The LOGSE affected all regions, however the initial implementation intensity induced by the policy varied across regions. Some regions rapidly expanded public preschool for three-year-olds facing a greater exposure to the policy than other regions that had a less pronounced increase immediately after the reform. Instead of analysing the introduction of a childcare programme as Baker et al. (2019) and Haeck et al. (2018), this study evaluates differences in the initial implementation intensity of a preschool programme (Felfe et al., 2015).

To capture the initial intensity level, I partially follow the strategy of Havnes & Mogstad (2011a) and Felfe et al. (2015) and consider the p.p. difference (increase) in three-year-old public preschool enrolment rates by region in the initial expansion period from 1990/91 to

[11] Previous studies also exploited the variation across cohorts of births instead of time in their DiD analyses (e.g. Bellés-Obrero, Jiménez-Martín, et al., 2021; Duflo, 2001; Hoynes et al., 2016; Pischke, 2007).

1993/94 as the treatment variable. I rely on a continuous treatment variable which measures the varying levels of initial intensity of the programme and exploits a differing "*treatment intensity*" across regions (Angrist & Pischke, 2009)[12,13]. The advantages of employing a continuous treatment over dichotomisation are several including no need to rely on assumptions to define treatment and control groups that might be arbitrary, no information loss, and no categorisation of similar groups at opposite sides of the cut-off point (Altman & Royston, 2006).

Recent literature outlined issues related to DiD with staggered implementation using two-way fixed effects and developed new estimators to overcome the problems associated with this strategy (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). However, the identification strategy followed in this investigation does not rely on a staggered DiD since I do not analyse the introduction of a programme with variation in treatment timing across regions, but the varying levels of the initial implementation intensity of a programme that was introduced at the same time (i.e. 1991/92) in all regions[14]. Recent work also emphasises that DiD with a continuous treatment measure needs an additional assumption to be identified, i.e. the "strong" parallel trends assumption (Callaway et al., 2021). This assumption states that regions with a lower treatment intensity are a good counterfactual for those with a higher treatment intensity if the evolution of health outcomes at the lower treatment intensity would have been the same. Although this assumption is not testable, I test the plausibility of the parallel trends assumption for DiD with continuous treatment below.

Survey data are used to study long-term health outcomes at the individual level. The DiD regression estimated by Ordinary Least Squares (OLS)[15] is defined as:

$$y_{ircw} = \alpha_0 + \alpha_1 \Delta Preschool_r \times Post_c + \boldsymbol{X}_i' \boldsymbol{\alpha_2} + \boldsymbol{Z}_{rc}' \boldsymbol{\alpha_3} + \boldsymbol{\gamma}_r + \boldsymbol{\eta}_c + \omega_w + \varepsilon_{ircw}, \quad (1)$$

[12] Havnes & Mogstad (2011a) and Felfe et al. (2015) ordered regions in a descending way by their increase in preschool enrolment rates in the initial expansion period. To define which regions belong to the treatment and control group, the authors split the list of regions at the median, i.e. treatment regions had an increase above the median and control regions reported an increase below the median. Notice that if the treatment was dichotomised, the DiD would be in its canonical form (two groups, two periods).

[13] Several studies employed a continuous treatment variable to capture the intensity of a policy rather than its introduction in their DiD analyses (e.g. Adhvaryu et al., 2020; Longo et al., 2019; Rosales-Rueda, 2018).

[14] This new literature described that the treatment parameter estimated when applying two-way fixed effects is a weighted sum of the average treatment effects in each group and time. Despite summing to one, weights can be negative if groups switch off and on of being treated across periods (as in a staggered implementation). If treatment effects are heterogeneous across groups and periods, groups treated earlier are weighted more which could imply that the parameter estimated ends up negative despite all average treatment effects being positive.

[15] I apply a linear probability model to estimate equation (1). Probit and logit models are employed as robustness checks for binary outcomes in Table A8 in the Appendix.

where y_{ircw} is a health outcome of individual i residing in region r, born in cohort c, and surveyed in wave w. $\Delta Preschool_r$ is a continuous variable measuring the p.p. regional increase in public enrolment rates for three-year-olds between 1990/91 and 1993/94. $Post_c$ is a dummy equal to one for cohorts affected by the policy (1988-1991) and aged three in 1991 or after, and zero for those unaffected (1984-1987) and aged three before 1991.

$\boldsymbol{X}_i$ is a vector of time-invariant individual characteristics (gender and month of birth). $\boldsymbol{\gamma}_r$ are region fixed effects which control for time-invariant regional factors such as pre-reform characteristics that could have predisposed regions to expand public preschool faster or slower. In addition, I also include a set of pre-reform regional variables[16] ($\boldsymbol{Z}_{rc}$) measured in 1990 interacted with cohort dummies to capture pre-reform regional characteristics that could have predisposed regions towards a more or less intense expansion and a different effect on health outcomes across pre- and post-reform cohorts. Other time-variant individual or regional variables are excluded due to being potentially affected by the policy causing the *bad control* problem (Angrist & Pischke, 2009). $\boldsymbol{\eta}_c$ are cohort fixed effects controlling for time-invariant features of individuals born in the same year and ω_w is a survey-wave fixed effect capturing factors common to all children surveyed in a specific wave (e.g. characteristics of the Spanish economy at the wave in which individuals were surveyed). Finally, ε_{ircw} is the error term.

Other outcomes (hospitalisations and deaths) are measured in each region from administrative sources. The DiD model estimated by OLS is:

$$event_{rct} = \beta_0 + \beta_1 \Delta Preschool_r \times Post_c + \boldsymbol{Z}'_{rc}\boldsymbol{\beta_2} + \boldsymbol{\delta}_r + \boldsymbol{\varphi}_c + \boldsymbol{\lambda}_t + \xi_{rct} \qquad (2)$$

where $event_{rct}$ is hospitalisations/deaths per 100/10,000 individuals in year t (1999-2018) of individuals residing/born in region r and cohort c. $\Delta Preschool_r$, $Post_c$ and $\boldsymbol{Z}_{rc}$ are defined as in equation (1). $\boldsymbol{\delta}_r$ are region fixed effects to control for common factors of all children in a specific region and capture pre-reform regional features. $\boldsymbol{\varphi}_c$ are cohort fixed effects to control for time-invariant characteristics of all individuals born in the same cohort. The time fixed effects, $\boldsymbol{\lambda}_t$, capture any unobserved factor common to all hospital discharges or deaths occurring in a specific year. ξ_{rct} is the error term[17].

[16] Namely GDP per capita, unemployment rate, female labour participation rate, proportion of population with tertiary education, population in thousands, public enrolment rate for three-year-olds, number of centres per 100,000 individuals, and a dummy capturing if the regional president belonged to a left-wing party.

[17] Equations (1) and (2) implicitly assume that pre-reform cohorts were exposed to public enrolment rates for three-years-olds in 1990/91, while post-reform cohorts to those in 1993/94. See Appendix A3 for a derivation.

α_1 and β_1 are the coefficients of interest and measure the effect of increasing the regional initial implementation intensity faced by post-reform cohorts by 1p.p. on long-term health. These also comprise an *intention-to-treat* (ITT) effect, which informs about the full effect of the policy regardless of whether a child was enrolled in public preschool at the age of three.

The impacts of an expansion of the supply of public preschool places depend on the counterfactual mode of care that would have been in place in absence of the programme. Felfe et al. (2015) estimated that the increase in public enrolment rates for three-year-olds stimulated public preschool care, did not crowd out private formal nor informal care, and implied a modest boost of maternal employment[18]. Therefore, the results should be understood as the effect of formal public preschool care that crowds out mainly family care on long-term health. This effect might be explained by supplying more public preschool places, attending high-quality public preschool, improving children's educational attainment, and by an income shock derived from small rises in maternal employment.

I cluster standard errors by region since the treatment varies at the region level, but I compute wild-bootstrapped clustered standard errors with 9,999 repetitions due to few clusters (Cameron et al., 2008). Given the large number of outcomes, I also report adjusted *p*-values (known as *q*-values) for multiple hypotheses testing following Benjamini & Hochberg (1995) and Anderson (2008) to control for the false discovery rate (i.e. the expected proportion of rejections that are type I errors)[19].

There are two underlying assumptions behind the identification strategy of equations (1) and (2)[20]. First, long-term health outcomes across regions should have evolved in parallel in absence of the reform. If regional trends were not parallel, the estimates could be capturing differences in trends rather than the effect of the LOGSE. To check the plausibility of the parallel trends assumption, I substitute $\Delta Preschool_r \times Post_c$ by a set of interactions between the treatment variable and pre-reform cohort dummies in Figure 4 (see Section 4 for information on the data). I choose the cohort of 1984 as the baseline category to test whether there is an

[18] See Panel A of Table 4 and pages 408-409 in Felfe et al. (2015) for more information. I also compute the effect of the LOGSE on public and private enrolment rates in Table A1 in the Appendix and find similar conclusions, i.e. a greater initial intensity in public preschool expansion increases public enrolment rates and has no effect on private enrolment rates.

[19] This method has greater power and reduces the penalty to testing additional hypotheses compared to familywise error rate controlling methods such as the Bonferroni correction (Anderson, 2008; Benjamini & Hochberg, 1995).

[20] Another underlying assumption is that the expansion of public preschool places increased take-up. Although a measure for preschool slots is not available, I show that the programme incentivised take-up by rising public enrolment rates for three-year-olds in Table A1 in the Appendix.

effect on the health of those cohorts still in preschool in 1991/92, born in 1986 and 1987. Moreover, this allows to test whether there is an anticipatory effect since the reform was announced in 1990 but not implemented until 1991/92. Figure 4 reports the coefficients of these interactions with their 95% confidence intervals and shows that almost all estimates of the pre-reform cohort interactions are statistically insignificant (i.e. the parallel trends assumption holds) and no anticipatory effect is found.

[Figure 4]

Although region fixed effects and pre-reform regional variables already control for pre-reform regional differences in levels, I also study whether regional characteristics, which could have affected the public preschool expansion and health outcomes, would have evolved parallelly in absence of the reform. To check this, I re-do the exercise on the plausibility of the parallel trends assumption using regional variables as dependent variables in Figure A1 in the Appendix. Overall, almost all coefficients of the pre-reform cohort interactions are statistically insignificant (except for population in thousands) implying that regions followed similar trends before the policy was implemented[21].

A threat to identification is the presence of contemporaneous reforms/changes that varied across regions and were potentially correlated with the public preschool expansion. Most reforms in the 1980s and early 1990s were implemented at the national level and thus controlled by cohort fixed effects. However, the compulsory component of the LOGSE started in 1991/92 and could also impact individual's health. To address this, I restrict the sample window to those cohorts born between 1984 and 1991 who were equally affected by the compulsory component (see Figure 3)[22]. There are three policies that could invalidate the identification strategy: the LOGSE's qualitative improvement of preschool, the gradual transfer of competences from the central government to the regional governments starting in the 1980s, and the abortion legalisation in 1985. I show that these three policies do not bias the results in Section 6.

[21] Exposure to the LOGSE should not capture changes in characteristics of individuals living/born in regions with higher implementation intensity. I also employ equation (1) to estimate DiD coefficients of observable factors of the three samples analysed (see Section 4) on exposure to the LOGSE in Table A2 in the Appendix. The results show that these estimates are not statistically different from zero at any conventional level, thus concluding that exposure to the LOGSE does not capture changes in characteristics of individuals living/born in regions with higher implementation intensity, at least in terms of observable factors.

[22] Bellés-Obrero & Duchini (2021) analysed the effect of the compulsory component of the LOGSE on education and labour outcomes. They used individuals born in 1977-1985 as the affected cohorts and showed that enrolment rates at age 14 first reached 100% for the cohort born in 1985. Although the bias might be low, the cohort born in 1984 had enrolment rates at age 14 around 95% which could confound the results. The coefficients are fairly robust to the main results when excluding the cohort born in 1984 and are available upon request.

Second, equations (1) and (2) implicitly assume that individual's region of residence and birth are the same as the region when they turned three. However, some families may decide to move across regions, thus biasing the estimates. Several studies showed that migration across and within regions in Spain is low (Bentolila, 2001; Felfe et al., 2015; Jimeno & Bentolila, 1998; Nollenberger & Rodríguez-Planas, 2015). Using the Spanish Labour Force Survey from 1999 to 2018, I estimate a small probability of living in a region that differs from the region of birth (5.9%) at ages 10-29[23]. Using these data, I estimate the association between the treatment variable and the probability of living in a region that differs from the region of birth in Table A3 in the Appendix and find that association coefficients are close to zero[24]. Consequently, selective migration is unlikely to imply a severe bias.

4. Data

This study analyses data from four main sources. The Spanish National Health Survey, the Hospital Morbidity Survey, and the Death Registries provide the dependent variables. The Statistics of Non-tertiary Education report data to measure the treatment variable. Control variables are explained in Appendix A5. Tables A4 and A5 in the Appendix provide an overview of the definitions and sources of all variables.

4.1. Spanish National Health Survey

The Spanish National Health Survey (SNHS) is a cross-sectional survey conducted by the Ministry of Health, Consumer Affairs and Social Welfare and the National Statistics Institute. The survey collects information about the health of the population residing in Spain. The SNHS randomly chooses households in each Spanish region and randomly surveys an adult (aged 16 and over) and a child (aged 0-15) within each household.

I focus on the 2003 and 2006 waves which include information on date of birth. The initial sample consists of 5,281 individuals born between 1984 and 1991 and aged 11-23. I exclude individuals from the Basque Country and Navarre due to their greater fiscal and political autonomy since the mid 1970s and their different educational policy from the remaining regions in Spain, and Ceuta and Melilla owing to their autonomous city status (excluding 520

[23] This probability excludes the Basque Country, Navarre, Ceuta, and Melilla. The probability including them rises to 6.5%. The Spanish Labour Force Survey reports data on age in quinquennial groups. I use the age groups of 10-15, 16-19, 20-24, and 25-29 which are the closest to the individuals of the sample (aged 11-27) and whose probability of living in a region that differs from the region of birth is 3.6%, 4.5%, 5.9%, and 9.0%, respectively.
[24] Table A2 in the Appendix also shows that the probability of living in a region different from the region of birth is not affected by the policy, at least for the sample of deaths (last row).

observations). I only include individuals with a Spanish nationality and exclude immigrants (excluding 285 individuals) since it is unknown whether they were in Spain at the time of the reform[25]. Finally, 15 observations with missing values are also excluded. The final sample consists of 4,461 individuals.

The dependent variables at the individual level derived from the SNHS fall into four categories: health status, chronic conditions, consumption of medicines, and healthcare use. The survey asks individuals to report their health status in the last twelve months with five multiple choice answers where 1 denotes "very good health" and 5 "very bad health". Health status is a dummy variable equal to one if the individual replies "good" or "very good", and zero if "regular", "bad" or "very bad". I also analyse a dummy equal to one if the individual had been diagnosed with a specific chronic condition, and zero otherwise. The chronic conditions studied are chronic allergy, asthma, and mental health disorders[26]. The SNHS asks whether individuals consume medicines and, therefore, I employ a dummy equal to one if the individual was medicated in the last two weeks, and zero otherwise. Several variables related to healthcare use are studied. I focus on a dummy variable equal to one if the individual visited the general practitioner (GP) or specialist doctor in the last month, and zero otherwise. I also consider a binary variable equal to one if the individual stayed at least one night in hospital or visited an emergency service in the last 12 months, and zero otherwise.

4.2. Hospitalisation and Death Registries

The Hospital Morbidity Survey, conducted by the National Statistics Institute, provides annual census data on all overnight hospitalisations in public, private, and military hospitals[27]. The registry collects data on hospital discharges of patients staying overnight occurring within the reference year, regardless of the date of admission. The data include patient's length of stay, date of discharge, main diagnosis, type of hospital admission (ordinary or emergency), reason for discharge, region of hospitalisation, date of birth, gender, and region of residence.

[25] Some individuals with a Spanish nationality could have been born abroad. However, information on country of birth is only available in the SNHS 2006. According to this wave, the percentage of individuals born in a foreign country between 1984 and 1991 with a Spanish nationality is 1.9%.

[26] Chronic allergy, asthma, mental health disorders, and diabetes are common chronic conditions in adult and children surveys for 2003 and 2006 waves. I only analyse chronic allergy, asthma, and mental health disorders since the proportion of individuals with diabetes is very low (0.5%).

[27] The coverage of the registry is extensive; for instance, the proportion of hospitals and patients included sums up to 96% and 99%, respectively (Borra et al., 2021). Although considering private and military hospitals apart from public hospitals, the registry only includes overnight stays and excludes day-cases.

Death Registries contain administrative data for all death certificates of individuals who died in Spain and their sociodemographic characteristics, elaborated by the National Statistics Institute in collaboration with regional authorities[28].

This study focuses on hospitalisations and deaths for individuals born in 1984-1991 occurring between 1999 and 2018 which sum up to 3,988,638 and 24,698 events, respectively. The sample is restricted to hospitalisations (deaths) of individuals residing (born) in Spain and turning 15-27 in the year of hospital discharge (death)[29]. Following Bellés-Obrero, Jiménez-Martín, et al. (2021), I compute hospitalisations (deaths) by collapsing hospital discharges (deaths) by individuals' year of birth, region of residence (birth), and year of discharge (death). The unit of observation is defined as the number of events in each year of birth, region of residence or birth, and year of hospital discharge or death. Again, I exclude the Basque Country, Navarre, Ceuta, and Melilla. The final samples count on 2,323,616 hospital discharges and 13,108 deaths obtaining 1,560 (=8×15×13) observations. I then divide the number of hospitalisations (deaths) in each observation by the number of individuals born in each region and year (1984-1991) from Birth Registries published by the National Statistics Institute and multiply the resulting value by 100 (10,000).

4.3. Statistics of Non-tertiary Education

The Spanish Ministry of Education and Vocational Training and the regional Departments of Education publish information about student enrolment in the Statistics of Non-tertiary Education, which include data on preschool, primary, secondary, special (i.e. visual arts and design, music, dance, dramatic arts, languages, and sports), and adult education. I employ preschool enrolment rates for three-year-olds by region and type of school (public or private) for 1987/88-2002/03 to compute the treatment variable ($\Delta Preschool_r$), defined as the regional difference in p.p. between public enrolment rates for three-year-olds in 1990/91 and 1993/94[30].

[28] Death certificates are completed by the doctor who certifies the death relating it to personal data and cause, the Civil Register which fills data related to the registration, and the declarant who gives data related to deceased's sociodemographic and socioeconomic characteristics. The certificate is completed by the court for deaths occurring in special circumstances and whenever a court intervenes.

[29] Death registries exclude individuals born abroad and consider region of birth. Instead, hospital discharges include individuals living in Spain regardless of their country of birth who could and could not be affected by the reform depending on their date of arrival. Then, the estimated result is a lower bound of the effect of the reform on hospitalisations capturing also any spillover effects on immigrants arriving after the reform.

[30] Appendix A4 includes information on how enrolment rates are created.

5. Results

5.1. Descriptive Statistics

Table 1 shows the summary statistics for dependent (Panel A) and control (Panel B) variables. Regarding health outcomes, 89.2% of the individuals in the sample reply to have had "good" or "very good" health in the last twelve months. In particular, 15.2% of the sample had been diagnosed with chronic allergy, 6.6% asthma, and 2.2% a mental health disorder. Also, 40.5% took a medicine in the last two weeks. Concerning healthcare, 34.3% visited the doctor in the last month, 4.1% stayed overnight in hospital and 32.1% attended an emergency service in the last year. Hospitalisations per 100 individuals were 5.6 and deaths per 10,000 individuals were 3.2.

[Table 1]

The control variables at the individual level are presented in Panel B of Table 1, which shows that 48.5% were women, 45.5% were surveyed in 2006, and individuals were born uniformly across the year in the SNHS sample. Panel B also presents the descriptive statistics for pre-reform regional characteristics, which are snapshots of 1990 and are computed for the 15 Spanish regions. The GDP per capita is 6,007 euros, unemployment rate is 15.5%, and female labour participation rate is 33.8% in 1990 on average. 7.0% of women and 9.0% of men aged 25 or older have tertiary education in the 1991 Census. The average population in thousands is 2,409. Public enrolment rate for three-year-olds is 12.0% and there are 59.5 preschool and primary centres per 100,000 individuals in 1990/91. Finally, 53.3% of the regional presidents belong to a left-wing party in 1990.

Table 2 ranks Spanish regions according to their increase in public enrolment rates (treatment variable) for three-year-olds over the expansion period. The mean is 22.1p.p. and the median is 23.1p.p. corresponding to Castilla-La Mancha. Galicia, Catalonia, and Asturias experienced the largest increase with 48.1, 42.6, and 32.5p.p., respectively. Instead, Andalusia, the Canary Islands, and the Region of Murcia had the lowest rise with 5.6, 4.1, and 3.9p.p., respectively.

[Table 2]

5.2. Main Results

Table 3 presents the main results for equations (1) and (2). The causal effect of interest corresponds to the estimate in the first row together with standard errors clustered at region level in parentheses, *p*-values for wild-bootstrapped clustered standard errors in squared brackets, and adjusted *p*-values for multiple hypotheses testing in curly brackets[31]. For the sake of brevity, I mainly focus on explaining the estimates that are statistically significant at least when employing wild-bootstrapped clustered standard errors.

Overall, Table 3 shows that the LOGSE was not successful in improving long-term health, except for some outcomes. Concerning health at the individual level, a greater initial intensity in public preschool expansion decreases the probability of being diagnosed with asthma and increases the likelihood of staying in hospital overnight for children aged three post-policy. Only the result for asthma survives once allowing for multiple hypotheses testing. Intensifying the initial increase in public enrolment rates by 10p.p decreases the probability of being diagnosed with asthma for children aged three post-policy by 2.1p.p. (or 30.4% relative to pre-reform mean of 6.9%, equivalent to 0.1 (=0.021/0.253) pre-reform standard deviations in asthma outcome). Despite pointing to better health, the effects of the reform on the probability of having "good" or "very good" health status in the last year, being diagnosed with chronic allergy and mental health disorders, and consuming medicines in the last two weeks are not statistically significant. The reform does not affect the likelihood of visiting the doctor in the last month nor attending an emergency service in the last 12 months, but the coefficients are positive.

Table 3 shows that hospitalisations per 100 individuals (unexpectedly) increase by 0.151 or 2.7% (relative to pre-reform mean of 5.658, equivalent to 0.1 (=0.151/2.259) pre-reform standard deviations in hospitalisations) for children aged three post-policy after intensifying the initial increase in public enrolment rates by 10p.p.[32]. The precision of this estimate is robust to multiple hypotheses testing. Finally, the reform does not affect deaths per 10,000 individuals[33].

[Table 3]

Children aged three post-policy residing in regions that faced a greater exposure to the programme have a lower prevalence of being diagnosed with asthma. This result can be explained through two channels. First, environmental factors surrounding children could affect

[31] Table A6 in the Appendix reports the estimates of equations (1) and (2) without including control variables and shows that the results are fairly similar to Table 3, but less precisely estimated.
[32] An increase of 2.7% in hospitalisations rates is equivalent to 33,136 more hospitalisations given the number of 1,227,260 hospitalisations for pre-reform cohorts between 1999 and 2018.
[33] Figure 4 also shows interactions of the treatment variable with post-reform cohort dummies to study heterogeneity effects across years of birth. Again, the results show that the LOGSE does not affect long-term health in general. The plot for asthma shows that the results are mainly driven by children born in 1988 and 1991, while those for hospitalisations by cohorts born in 1989 onwards.

their predisposition to have certain diseases (Gilles et al., 2018). For instance, children in preschool might be exposed to more infectious agents (e.g. bacteria, viruses) affecting negatively their health during childhood, but improving immunisation and protecting them from future diseases (*hygiene hypothesis*). Another example is that there is evidence showing that air pollution is positively correlated with the development of asthma (Royal College of Physicians, 2016). If children residing in areas with poor air quality have the opportunity to attend preschool (probably with cleaner environment), their probability of having asthma might reduce. Second, chronic conditions such as asthma could be detected by teachers more easily if children attended preschool and thus treated early in life (Breivik et al., 2020).

Children affected more intensively by the reform have higher hospitalisation rates. This result together with, despite insignificant, the positive coefficients on the probability of visiting the doctor and attending a hospital or an emergency service (when using survey data) might imply a greater use of healthcare services. This fact could be explained by 1) individuals might have changed their health seeking behaviour towards a higher utilisation of healthcare services, and/or 2) the reform may have worsened their health. Although not all statistically significant, the coefficients on the remaining health outcomes of the SNHS having the expected sign (improving health) point to the first explanation. There is evidence suggesting that patients with higher socioeconomic backgrounds use specialist healthcare services more than patient with lower SES (van Doorslaer et al., 2004). The fact that children affected by the LOGSE have better cognitive skills and their mothers joined the labour force might have made them to be in a higher socioeconomic level explaining then the increase in healthcare use (Felfe et al., 2015; Nollenberger & Rodríguez-Planas, 2015).

The absence of an effect on self-reported health status is in line with the lack of effect estimated by Haeck et al. (2018) for Canada, but in contrast to the persistent negative impact by Baker et al. (2019) for that same country. Haeck et al. (2018) found a decrease in mental health problems at ages 15-19, while I do not find any effect on mental health disorders similarly to Baker et al. (2019). Haeck et al. (2018) reported no effect on having an asthma attack in the past 12 months at ages 12-19 as opposed to the findings of the LOGSE decreasing the likelihood of being diagnosed with asthma at ages 11-23. The finding of a 2.7% rise in hospitalisation rates is in line with the 3% increase in secondary healthcare use for physical-related health due to the Norwegian universal childcare programme, but in contrast to the decrease in primary and secondary healthcare use for mental health (Breivik et al., 2020).

6. Robustness Checks

This section reports alternative specifications testing the robustness of the results. Table 4 shows two falsification tests and Table 5 presents a set of sensitivity analyses in Columns 2-9. Any additional control variable used to test the robustness of the results is explained in Tables A5 and A7 in the Appendix. Overall, the results are fairly robust across these specifications[34].

6.1. Falsification Test

Pre-reform cohorts were not affected by the programme, which implies that the LOGSE should not have impacted their long-term health independently of the region of residence/birth. I perform two falsification tests considering only the pre-reform cohorts of 1984-1987 in Table 4. Table 4 reports the estimates of the causal effect of the LOGSE assuming that the reform took place in 1989/90 and 1990/91 (instead of 1991/92) and using the econometric specification and treatment variable of equations (1) and (2)[35]. Overall, almost all the coefficients show no significant impact of these placebo reforms at 5% significance level, except for the consumption of medicines whose estimate should be taken with caution.

[Table 4]

6.2. Quality of Education and Contemporaneous Reforms

The LOGSE programme implied a national qualitative improvement in preschool for ages 3-5 in terms of the pedagogical curriculum, teacher's qualifications, and class size (see Section 2.3). If the quality of preschool education had varied across regions instead of homogenously, the results in Section 5 could be confounded and explain the effect of the qualitative improvement rather than of the expansion of public preschool places. To show that this is not the case, I add two proxies for the quality of education (i.e. class size and student to teacher ratio) to equations (1) and (2) in Columns 2 and 3 of Table 5. The estimates are similar to the baseline results after including these quality variables.

[Table 5]

The Spanish constitution of 1978 regulated the gradual transfer of competences in the public sector to the Spanish regions over the next decades. The central government transferred

[34] Additional robustness checks can be found in Table A8 and Appendix A6.
[35] The pre-reform region characteristics for the falsification tests are calculated for the year previous to the "fake" policies, except for the proportion of women and men with tertiary education that are for the closest census year, i.e. 1991.

education and/or health competences to Andalusia, the Canary Islands, Catalonia, Galicia, and the Valencian Community before 1991/92 (Bonal et al., 2005; Costa-Font & Rico, 2006). I add an interaction between a dummy for these five regions and $Post_c$ to control for the differential effect that the decentralisation system could have on education and health of pre- and post-reform cohorts in Column 4 of Table 5. Again, the coefficients on the interaction of interest show robustness even after controlling for the pre-reform Spanish decentralisation.

González et al. (2020) argued that the Spanish abortion legalisation in 1985 (LO, 1985) implied a differential effect on women due to different availability of abortion clinics across Spanish provinces. I conduct a test to show that the abortion legalisation does not confound the results and include the treatment variable used by González et al. (2020). That is, I introduce to the model an interaction between a dummy equal to one for cohorts born in 1986 onwards (whose mothers were affected by the abortion legalisation), and a continuous variable capturing the number of clinics that conducted at least one abortion in 1989 per 100,000 individuals at the region level in Column 5 of Table 5. The coefficients are robust to this additional variable concluding that the Spanish abortion legalisation does not bias the results.

6.3. Alternative Treatment Variables

In this section, I show that the results found are not sensitive to the choice of the treatment variable. I tighten (1990/91-1992/93) and widen (1990/91-1994/95) the expansion period in Columns 6 and 7 of Table 5, respectively, and show that the results do not depend on the choice of the initial expansion period.

Following Felfe et al. (2015), I dichotomise the treatment variable into a treatment and a control group. To define which regions belong to the treatment and control group, I split the list of regions in Table 2 at the median, i.e. treatment regions have an increase above the median and control regions report an increase below the median[36]. $\Delta Preschool_r$ is replaced by $Treated_r$, a dummy variable equal to one for treatment regions and zero for control regions, in Column 8 of Table 5. The estimates with a binary treatment generally point to the same direction as with a continuous treatment, although losing some precision[37]. Alternatively, I substitute the

[36] Figure A3 in the Appendix shows that the parallel trends assumption holds when employing a binary treatment.
[37] Considering the two outcomes with statistically significant coefficients in Section 5.2, children aged three post-policy residing in regions with higher implementation intensity (treatment group) have a 4.7p.p. lower probability of being diagnosed with asthma and 0.342 more hospitalisations compared to those in regions with lower implementation intensity (control group). Notice that these results are similar to an increase of 20p.p. in the continuous treatment intensity (4.2p.p. less in asthma, 0.302 more hospitalisations). 20p.p. increase is the difference in average treatment intensity between treatment and control groups.

interaction term by public enrolment rates for three-year-olds by region and cohort of birth in Column 9 of Table 5. Again, the results are closely parallel to the baseline specification.

7. Heterogeneity Analysis

In this section, I analyse the heterogeneity of the results by gender, hospital diagnosis, cause of death, and parental education[38]. I study whether the effect differs between women and men, since no clear gender differences had been found among the studies of universal early childhood education programmes (Dietrichson et al., 2020). I also focus on several hospital diagnoses and causes of death based on the mechanisms explained in Section 2.2 (e.g. mental health disorders, infectious and parasitic diseases, respiratory diseases, metabolic and immunity disorders) and those diagnoses that have a higher prevalence in the samples (e.g. external causes of morbidity and mortality, pregnancy-related diagnoses). Finally, I provide results by parental education given that children from different SES could react differently to universal programmes (Baker, 2011).

7.1. Gender

Table 6 reports the effect of the LOGSE by gender. I split the sample by gender for health outcomes at the individual level, while hospitalisations and deaths are computed by gender. In line with previous studies, there is no gender pattern. The effects on asthma, hospital and emergency service visits are driven by men, and on mental health disorders by women. Intensifying the initial increase in public enrolment rates by 10p.p. decreases the probability of being diagnosed with asthma by 2.5p.p. for boys aged three post-policy. Instead, the likelihood of visiting a hospital and an emergency service increases by 2.6p.p. and 3.6p.p. for men, respectively. The probability of being diagnosed with mental health disorders decreases by 2.1p.p. for women. Female and male hospitalisation rates rise due to the policy, although the effect on women is greater in magnitude. Hospitalisations for women increase by 3.1% (0.236 hospitalisations relative to 7.509 pre-reform mean) and for men by 1.7% (0.067 hospitalisations relative to 3.944 pre-reform mean) for girls and boys, respectively[39].

[Table 6]

[38] Appendix A8 analyses heterogeneity by age and type of hospital admission.
[39] An increase of 3.1% (1.7%) in female (male) hospitalisations rates is equivalent to 24,608 (7,368) more hospitalisations given the number of 793,819 (433,441) hospitalisations for pre-reform cohorts over 1999-2018.

7.2. Hospital Diagnosis and Cause of Death

Figures 5 and 6 plot the effect on hospitalisations disaggregated by diagnosis and deaths by cause, respectively[40]. Figure 5 shows that the effect on hospitalisations is more pronounced for pregnancy-related diagnoses (increase of 4.6% after intensifying the increase in public enrolment rates by 10p.p.; 0.091 hospitalisations relative to 1.977 pre-reform mean)[41], which coincides with the fact that the impact on hospitalisations is greater for women. This result could be explained by 1) pregnant women might have changed their health seeking behaviour, 2) the reform could have worsened women's health and deteriorated their fertile development, and 3) higher fertility rates. The positive effect of the LOGSE on hospitalisations mainly due to women with pregnancy-related diagnoses is in line with the positive effect on primary healthcare use and sickness absences related to normal pregnancies by Breivik et al. (2020) for Norway, although their estimates are larger (7% and 27%, respectively). Finally, Figure 6 shows that the reform does not affect deaths for any cause.

[Figures 5 and 6]

7.3. Parental Education

Table 7 shows the effect of the LOGSE on health outcomes at the individual level by parental education. The sample is split into children with parents having primary or less education (low-educated parents), at least one parent having secondary education (medium-educated parents), and at least one parent having tertiary education (high-educated parents).

Overall, there are few effects of the LOGSE by parental education. Children with low- and medium-educated parents seem to benefit the most from the LOGSE, which closely resembles the results of universal programmes driven by less advantaged children in previous studies (van Huizen & Plantenga, 2018). Children whose parents have primary or less education have a lower probability of being diagnosed with asthma (3.7p.p. less after intensifying the increase in public enrolment rates by 10p.p.). For children aged three post-policy with at least one parent having secondary education, the probability of being diagnosed with mental health disorders reduces by 0.6p.p. and having taken any medicine in the last two weeks decreases by 5.7p.p. after intensifying the increase in public enrolment rates by 10p.p. In contrast, children from

[40] Table A9 in the Appendix provides the point estimates and Appendix A7 includes the diagnoses of hospitalisation and causes of death groups with International Classification of Diseases (ICD) codes. Special access to deaths by cause has been given by the National Statistics Institute.

[41] An increase of 4.6% in hospitalisations rates related to pregnancy diagnoses is equivalent to 9,663 more hospitalisations given the number of 444,505 hospitalisations for pre-reform cohorts between 1999 and 2018.

families with at least one parent having secondary education experience an increase in the likelihood of visiting an emergency service in the last 12 months (4.4p.p. more). Children with at least one parent having tertiary education have a higher probability of staying in hospital, although this result is marginally significant.

[Table 7]

Hospitalisation and death registries do not report information about parental SES. Thus, I split hospitalisations and deaths into regions with low, medium and high education levels, where regions with low/medium/high education are in the first/second/third tercile of the distribution of the proportion of adults aged 25 or older with tertiary education in 1991 Census. Table 8 shows no heterogeneity by regional education across hospitalisations and deaths.

[Table 8]

An explanation for the negative effect on asthma, mental health disorders, and consumption of medicines is that parents (especially, mothers) with low and medium educational levels, respectively, could have taken care of their children at home or could have left the child with grandparents in absence of the reform. Once universal public preschool is offered, low- and medium-educated parents might have been more responsive and prefer enrolling their children in full-time formal care with potentially higher quality than family care without incurring a large income outlay. This might imply that the productivity of time spent with parents (or grandparents) is lower than the productivity of time spent in formal high-quality childcare for children with low- and medium-educated parents. Instead, children with high-skilled parents could have been already enrolled in (private) preschool even if the LOGSE did not take place and thus benefited less. Finally, the increase in the likelihood of visiting an emergency service is in line with the positive effect on hospitalisations found in Section 5. Again, this (unexpected) result might be related to the literature stating that individuals from higher socioeconomic backgrounds use specialist healthcare more than those from lower SES (van Doorslaer et al., 2004).

8. Conclusion

This study has investigated the causal effect of universal preschool programmes on long-term health. It examined a Spanish policy which expanded public preschool places for three-year-olds and substituted care provided by the nuclear family. I tested if early education policies targeting enrolment, educational attainment and maternal employment have also spillover effects on long-term health. In general, the results show that the Spanish universal preschool programme does not affect health and healthcare use in the long run. This finding suggests that expanding the number of places in preschool is not sufficient to affect long-term health in institutional contexts such as Spain.

There are three policy-relevant findings. First, a greater initial intensity in public preschool expansion decreases the probability of being diagnosed with asthma for children aged three post-policy. This result might be explained by several channels. One channel is that children could have been more exposed to pathogens due to attending preschool but acquired higher immunisation levels which protect them from future illnesses (*hygiene hypothesis*). Other channels are an early detection of illnesses by preschool teachers and thus an early treatment of these, or an improved child's environment compared to the counterfactual mode of care (i.e. family care). The reduction in individuals diagnosed with asthma might imply a reduction in health expenditure given that annual costs per patient faced by the society are around €1,726 and by the NHS are €1,533 at 2007 prices in Spain (Martínez-Moragón et al., 2009).

Second, children affected more intensively by the programme have higher hospitalisation rates despite the hypothesis that early childhood education programmes enhance child outcomes. This finding might be explained by a change towards a higher utilisation of healthcare services given that the remaining (statistically insignificant) results point to an improvement in health and more visits to the doctor, hospital, and emergency services. Achieving a higher SES thanks to the LOGSE could also explain this result as it is well-established in the literature that individuals with higher SES use specialist healthcare more (van Doorslaer et al., 2004). However, more hospitalisations could also lead to a rise in health expenditure of around an average of €4,160 per patient (in 2008 prices in Spain) (Ministry of Health Consumer Affairs and Social Welfare, 2008).

Third, the LOGSE mostly improved the health of children from low and medium socioeconomic backgrounds. This might imply that more disadvantaged children enrolled in preschool once the programme started and, thus, benefited the most due to a change from (low-quality) family care to high-quality formal out-of-home care. Universal childhood education programmes have a lower cost per child but larger overall expenditure than targeted ones (Baker, 2011), while they seem to mainly benefit disadvantaged children as targeted programmes do (van Huizen & Plantenga, 2018). These results thus suggest that universal programmes might not be as cost-effective as targeted policies are.

This investigation has some limitations. I analyse individuals born over 1984-1991 who were relatively young (aged 11-27) over 1999-2018. Therefore, I cannot completely assess the effects of the LOGSE on the health of the individuals after their adolescence and early adulthood, when their risk of disease and mortality increases. Similarly, I cannot distinguish whether the reform directly affected long-term health or indirectly through its effects on characteristics during childhood (e.g. health, cognitive and non-cognitive skills). Exploring which mechanisms explain the results is outside of the scope of this study due to data constraints, but future work could focus on the short-term effects to understand what channels drive the results in the long run. Another limitation is that the findings are interpreted as ITT effects since the samples analysed do not report whether individuals attended preschool at the age of three. Moreover, I focus on severe healthcare outcomes (overnight hospitalisations and deaths) and relevant effects might be also found when considering primary/secondary healthcare or hospital day-cases. However, administrative patient-level data may be difficult to gather in a decentralised health system as the Spanish. Overcoming these limitations could be the subject of future research.

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Tables

Table 1. Descriptive Statistics

	Obs.	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Panel A: Health outcomes					
Spanish National Health Survey (2003 & 2006): Health outcomes at the individual level					
Health status (=1 if good or very good)	4,461	0.892	0.310	0	1
Chronic allergy (=1 if diagnosed)	4,461	0.152	0.359	0	1
Asthma (=1 if diagnosed)	4,461	0.066	0.249	0	1
Mental health disorders (=1 if diagnosed)	4,461	0.022	0.146	0	1
Medicines (=1 if taken medicines in last two weeks)	4,461	0.405	0.491	0	1
Doctor visits (=1 if visited in last month)	4,461	0.343	0.475	0	1
Hospital visits (=1 if stayed in hospital in last year)	4,461	0.041	0.197	0	1
Emergency service visits (=1 if visited in last year)	4,461	0.321	0.467	0	1
Hospitalisation and Death Registries (1999-2018): Health outcomes at the region level					
Hospitalisations per 100 individuals	1,560	5.634	1.992	0.345	12.80
Deaths per 10,000 individuals	1,560	3.204	1.855	0	17.26
Panel B: Control variables					
Control variables at the individual level					
Gender (=1 if female)	4,461	0.485	0.500	0	1
Month of birth: January	4,461	0.081	0.273	0	1
Month of birth: February	4,461	0.076	0.265	0	1
Month of birth: March	4,461	0.078	0.269	0	1
Month of birth: April	4,461	0.092	0.289	0	1
Month of birth: May	4,461	0.094	0.291	0	1
Month of birth: June	4,461	0.078	0.269	0	1
Month of birth: July	4,461	0.079	0.270	0	1
Month of birth: August	4,461	0.088	0.283	0	1
Month of birth: September	4,461	0.081	0.273	0	1
Month of birth: October	4,461	0.084	0.278	0	1
Month of birth: November	4,461	0.082	0.274	0	1
Month of birth: December	4,461	0.087	0.282	0	1
Year of survey (=1 if 2006)	4,461	0.455	0.498	0	1
Pre-reform regional characteristics					
GDP per capita (in €)	15	6,007	1,155	4,138	7,885
Unemployment rate (%)	15	15.45	5.332	8.330	25.53
Female labour participation rate (%)	15	33.80	3.997	26.61	40.38
Proportion of women population with tertiary education (%)	15	6.951	1.417	4.855	10.81
Proportion of men population with tertiary education (%)	15	9.032	2.554	5.902	17.22
Population (in thousands)	15	2,409	2,102	263.4	6,937
Public enrolment rate for three-year-olds (%)	15	11.95	8.26	1.980	29.88
Preschool and primary centres per 100,000 individuals	15	59.46	13.66	34.90	87.82
Regional president in left-wing party	15	0.533	0.516	0	1

Note: Data for health outcomes are drawn from the Spanish National Health Survey (2003 & 2006), Hospital Morbidity Survey (1999-2018), and Death Registries (1999-2018). Data for control variables at individual level are drawn from the Spanish National Health Survey (2003 & 2006). Health outcomes and control variables at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Details and sources of pre-reform regional variables are explained in Table A5 in the Appendix. Pre-reform regional characteristics are snapshots and their descriptive statistics are computed for the 15 Spanish regions considered. Obs = observations, Std. Dev. = standard deviation, Min. = minimum, Max = maximum.

Table 2. Increase in Public Enrolment Rates for Three-year-olds in Percentage Points between 1990/91 and 1993/94

Region	**Increase in Public Preschool Enrolment Rates in Percentage Points**
Galicia	48.07
Catalonia	42.63
Asturias	32.53
La Rioja	28.76
Castilla y Leon	28.58
Cantabria	26.29
Community of Madrid	23.98
Castilla-La Mancha	23.07
Extremadura	20.68
Aragon	19.39
Balearic Islands	13.58
Valencian Community	11.01
Andalusia	5.552
Canary Islands	4.098
Region of Murcia	3.866

Regions = 15
Mean = 22.14
Median = 23.07

Note: Data are drawn from the Statistics of Non-tertiary Education (1987/88-2002/03) published by the Spanish Ministry of Education and Vocational Training and the National Statistics Institute. The treatment variable captures the percentage point increase in public preschool enrolment rates for three-year-olds from 1990/91 to 1993/94 for 15 regions.

Table 3. Main Results

	Health Status	Chronic Allergy	Asthma	Mental Health Disorders	Medicines	Doctor Visits	Hospital Visits	Emergency Service Visits	Hospitalisations per 100 Individuals	Deaths per 10,000 Individuals
ITT	0.0002	-0.0012	-0.0021	-0.0006	-0.0019	0.0011	0.0013	0.0023	0.0151	0.0035
	(0.0004)	(0.0010)	(0.0005)***	(0.0004)	(0.0009)*	(0.0011)	(0.0003)***	(0.0009)**	(0.0018)***	(0.0058)
	[0.8074]	[0.5909]	[0.0158]**	[0.4904]	[0.1595]	[0.7196]	[0.0675]*	[0.2959]	[0.0108]**	[0.7431]
	{0.8080}	{0.8080}	{0.0800}*	{0.8080}	{0.3990}	{0.8080}	{0.2260}	{0.5920}	{0.0800}*	{0.8080}
Region FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ind. controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	4,461	4,461	4,461	4,461	4,461	4,461	4,461	4,461	1,560	1,560
Pre-reform mean	0.878	0.152	0.069	0.029	0.451	0.347	0.048	0.329	5.658	3.554

Note: Estimations are based on OLS on equations (1) and (2). Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. The first row presents the *intention-to-treat* (ITT) effects and their corresponding standard errors and *p*-values. Standard errors clustered at region level are in parentheses, *p*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in squared brackets, and *p*-values correcting for multiple hypotheses testing are in curly brackets. Control coefficients are not reported. The last two rows report the number of observations and the mean of health outcomes for pre-reform cohorts. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the standard error or *p*-value.

Table 4. Falsification Tests

	Health Status	Chronic Allergy	Asthma	Mental Health Disorders	Medicines	Doctor Visits	Hospital Visits	Emergency Service Visits	Hospitalisations per 100 Individuals	Deaths per 10,000 Individuals
Policy 1989/90 ITT	0.0004 (0.9054)	0.0012 (0.6808)	0.0012 (0.2625)	0.0007 (0.4619)	0.0070 (0.0326)**	0.0050 (0.0622)*	0.0018 (0.5464)	-0.0022 (0.4145)	0.0051 (0.2147)	-0.0108 (0.1588)
Policy 1990/91 ITT	0.0029 (0.8347)	-0.0110 (0.2406)	0.0008 (0.8148)	0.0009 (0.6024)	-0.0070 (0.3929)	0.0021 (0.6743)	0.0025 (0.4892)	0.0051 (0.4603)	-0.0127 (0.1891)	0.0074 (0.7706)
Observations	1,531	1,531	1,531	1,531	1,531	1,531	1,531	1,531	780	780

Note: Estimations are based on OLS on equations (1) and (2) for pre-reform cohorts (1984-1987). Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. The first row conducts a falsification test assuming the reform took place in 1989/90 that affected the cohorts born in 1986 and 1987, but not those born in 1984 and 1985. The second row conducts a falsification test assuming the reform took place in 1990/91 that affected the cohorts born in 1987, but not those born in 1984, 1985 and 1986. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table 5. Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Quality Measures				**Expansion Period**			
	Baseline	**Class Size**	**Student per Teacher Ratio**	**Decentra-lisation**	**Abortion Legalisation**	**1990/91-1992/93**	**1990/91-1994/95**	**Binary Treatment**	**Enrolment Rates**
Health outcomes at the individual level									
Health status	0.0002	0.0002	0.0003	0.0007	0.0001	0.0002	0.0003	0.0145	0.0009
	(0.8074)	(0.7682)	(0.7270)	(0.4202)	(0.8971)	(0.8423)	(0.7365)	(0.5587)	(0.2615)
Chronic allergy	-0.0012	-0.0010	-0.0012	-0.0013	-0.0013	-0.0014	-0.0011	-0.0575	-0.0013
	(0.5909)	(0.5186)	(0.5713)	(0.5523)	(0.4573)	(0.5976)	(0.5330)	(0.3359)	(0.4938)
Asthma	-0.0021	-0.0021	-0.0021	-0.0024	-0.0020	-0.0028	-0.0020	-0.0471	-0.0024
	(0.0158)**	(0.0217)**	(0.0217)**	(0.0070)***	(0.0237)**	(0.0178)**	(0.0211)**	(0.0640)*	(0.0283)**
Mental health disorders	-0.0006	-0.0006	-0.0006	-0.0006	-0.0007	-0.0008	-0.0006	-0.0301	-0.0004
	(0.4904)	(0.4776)	(0.5555)	(0.4441)	(0.3821)	(0.4736)	(0.4035)	(0.1359)	(0.4301)
Medicines	-0.0019	-0.0018	-0.0021	-0.0015	-0.0021	-0.0025	-0.0018	-0.0690	-0.0026
	(0.1595)	(0.1761)	(0.1792)	(0.1913)	(0.0330)**	(0.1501)	(0.1190)	(0.0521)*	(0.0531)*
Doctor visits	0.0011	0.0009	0.0008	0.0008	0.0006	0.0015	0.0010	0.0056	0.0006
	(0.7196)	(0.7559)	(0.7919)	(0.7908)	(0.7529)	(0.7271)	(0.7778)	(0.8886)	(0.8488)
Hospital visits	0.0013	0.0012	0.0013	0.0012	0.0012	0.0017	0.0012	0.0398	0.0004
	(0.0675)*	(0.0730)*	(0.0544)*	(0.0315)**	(0.0849)*	(0.0750)*	(0.0557)*	(0.0203)**	(0.4873)
Emergency service visits	0.0023	0.0023	0.0023	0.0021	0.0028	0.0034	0.0021	0.0595	0.0027
	(0.2959)	(0.2958)	(0.2924)	(0.3610)	(0.2806)	(0.2752)	(0.3089)	(0.3217)	(0.2017)
Observations	4,461	4,461	4,461	4,461	4,461	4,461	4,461	4,461	4,461
Health outcomes at the region level									
Hospitalisations per 100 individuals	0.0151	0.0152	0.0150	0.0152	0.0149	0.0200	0.0144	0.3423	0.0170
	(0.0108)**	(0.0117)**	(0.0077)***	(0.0393)**	(0.0130)**	(0.0130)**	(0.0289)**	(0.0770)*	(0.0080)***
Deaths per 10,000 individuals	0.0035	0.0032	0.0038	0.0047	0.0041	0.0035	0.0039	0.1671	0.0047
	(0.7431)	(0.7568)	(0.6834)	(0.7128)	(0.7103)	(0.8123)	(0.7289)	(0.5134)	(0.6721)
Observations	1,560	1,560	1,560	1,560	1,560	1,560	1,560	1,560	1,560

Note: Each cell reports the *intention-to-treat* (ITT) effect. Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. Column 1 shows the baseline estimates from Table 3. Columns 2 and 3 introduce two proxies for quality of education (class size and student to teacher ratio). Column 4 controls for the pre-reform decentralisation system in Spain and Column 5 includes a variable controlling for the abortion legalisation in Spain in 1985. Columns 6 and 7 tighten (1990/91-1992/93) and widen (1990/91-1994/95) the expansion period, respectively. Column 8 substitutes the continuous treatment by a binary treatment that splits the list of regions in Table 2 at the median and Column 9 by public enrolment rates for three-year-olds by region and cohort of birth. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table 6. Heterogeneity by Gender

	Health Status	**Chronic Allergy**	**Asthma**	**Mental Health Disorders**	**Medicines**	**Doctor Visits**	**Hospital Visits**	**Emergency Service Visits**	**Hospitalisations per 100 Individuals**	**Deaths per 10,000 Individuals**
ITT for women	-0.0012 (0.3094)	-0.0002 (0.9381)	-0.0012 (0.4756)	-0.0021 (0.0686)*	0.0018 (0.5255)	0.0034 (0.6212)	0.0002 (0.6997)	0.0013 (0.6165)	0.0236 (0.0130)**	0.0118 (0.4119)
Observations	2,175	2,175	2,175	2,175	2,175	2,175	2,175	2,175	1,560	1,560
Pre-reform mean	0.845	0.130	0.068	0.045	0.537	0.416	0.052	0.345	7.509	1.993
ITT for men	0.0015 (0.2683)	-0.0022 (0.3171)	-0.0025 (0.0218)**	0.0008 (0.1692)	-0.0029 (0.1142)	0.0001 (0.9675)	0.0026 (0.0039)***	0.0036 (0.0176)**	0.0067 (0.0599)*	-0.0034 (0.7241)
Observations	2,286	2,286	2,286	2,286	2,286	2,286	2,286	2,286	1,560	1,560
Pre-reform mean	0.908	0.171	0.069	0.014	0.373	0.284	0.044	0.316	3.944	5.000

Note: Each cell reports the *intention-to-treat* (ITT) effects by gender. Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. Control coefficients are not reported. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table 7. Heterogeneity by Parental Education

	Health Status	Chronic Allergy	Asthma	Mental Health Disorders	Medicines	Doctor Visits	Hospital Visits	Emergency Service Visits
ITT for both parents have primary education or less	0.0026 (0.3948)	0.0007 (0.7427)	-0.0037 (0.0688)*	0.0003 (0.7903)	0.0000 (0.9473)	0.0009 (0.3164)	0.0000 (0.9850)	-0.0018 (0.5531)
Observations	1,546	1,546	1,546	1,546	1,546	1,546	1,546	1,546
Pre-reform mean	0.338	0.133	0.068	0.023	0.415	0.333	0.049	0.344
ITT for at least one parent has secondary education	-0.0017 (0.6982)	-0.0029 (0.2227)	-0.0011 (0.6125)	-0.0006 (0.0285)**	-0.0057 (0.0332)**	0.0017 (0.6621)	0.0002 (0.7914)	0.0044 (0.0491)**
Observations	1,865	1,865	1,865	1,865	1,865	1,865	1,865	1,865
Pre-reform mean	0.891	0.166	0.071	0.030	0.488	0.371	0.038	0.313
ITT for at least one parent has tertiary education	0.0009 (0.7962)	-0.0021 (0.3718)	-0.0006 (0.7563)	-0.0008 (0.6327)	-0.0018 (0.9184)	0.0015 (0.8010)	0.0039 (0.0952)*	0.0067 (0.4022)
Observations	765	765	765	765	765	765	765	765
Pre-reform mean	0.908	0.167	0.066	0.034	0.440	0.308	0.047	0.284

Note: Each cell reports the *intention-to-treat* (ITT) effects by parental education. Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. Control coefficients are not reported. The sample size reduces to 4,176 (93.6% of the main sample) since some individuals do not belong to the same household as their parents. The main results are robust to restricting the sample to those individuals with information about parental education and are available upon request. The results are fairly robust when considering solely maternal education and are available upon request. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

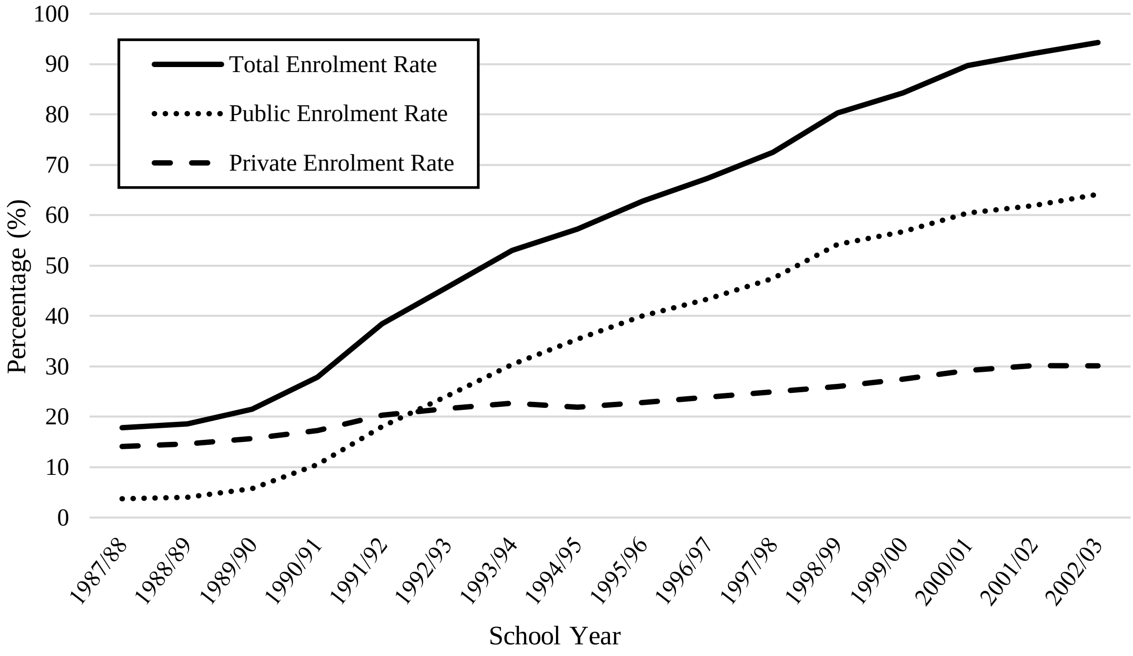
Table 8. Heterogeneity by Regional Education

	Hospitalisations per 100 Individuals	**Deaths per 10,000 Individuals**
ITT for regions with low education	0.0012 (0.6210)	0.0055 (0.6109)
Observations	520	520
Pre-reform mean	5.654	3.874
ITT for regions with medium education	0.0121 (0.2245)	0.0041 (0.3451)
Observations	520	520
Pre-reform mean	5.909	3.521
ITT for regions with high education	0.0109 (0.5257)	0.0149 (0.1909)
Observations	520	520
Pre-reform mean	5.410	3.268

Note: Each cell reports the *intention-to-treat* effect (ITT) by regional education. Regions with low/medium/high education fall in the first/second/third tercile of the distribution of the proportion of adults aged 25 or older with tertiary education in 1991 Census. Regions with low education are Andalusia, Balearic Islands, Galicia, Extremadura, and Castilla-La Mancha. Regions with medium education are Cantabria, Catalonia, La Rioja, Region of Murcia, and Valencian Community. Regions with high education are Community of Madrid, Canary Islands, Aragon, Castilla y Leon, Asturias. Health outcomes, treatment variable, and controls are defined in Section 4. Pre-reform characteristics interacted with cohort fixed effects cannot be included due to problems of collinearity. Control coefficients are not reported. *P*-values for wild-bootstrapped clustered standard errors are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

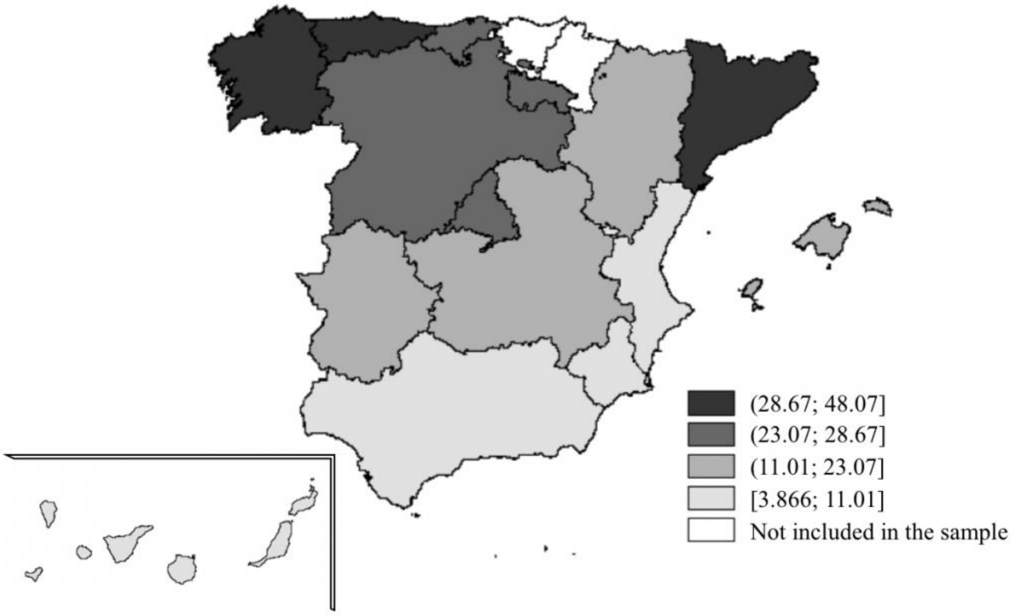
Figures

Figure 1. Preschool Enrolment Rates for Three-year-olds



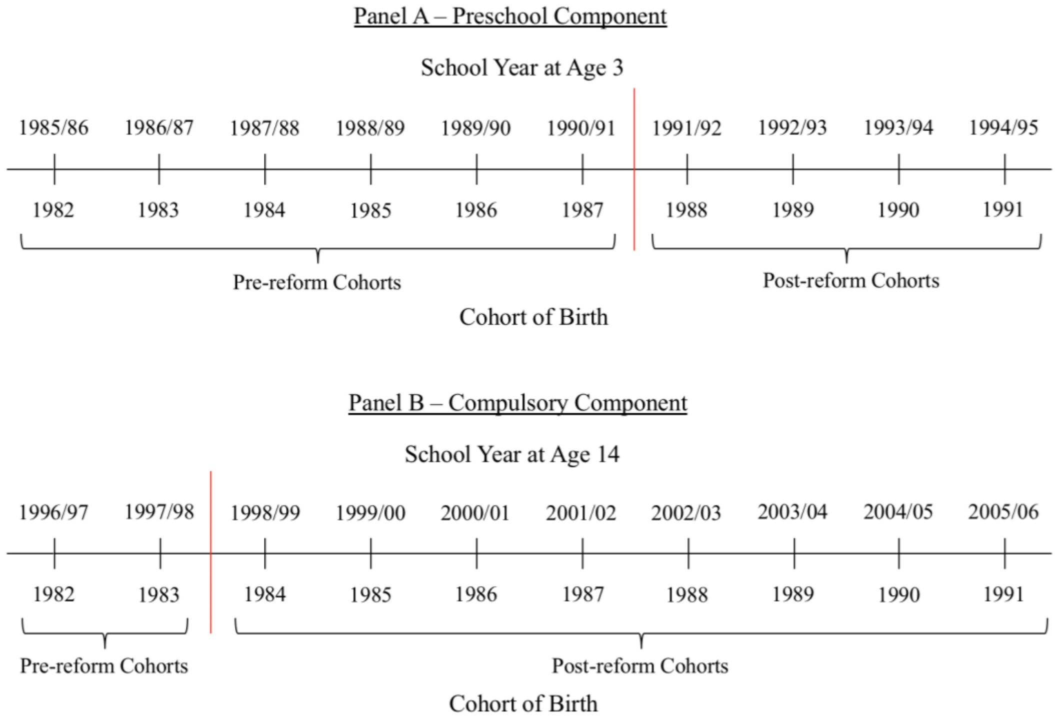
Source: Spanish Ministry of Education and Vocational Training (https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html).

Figure 2. Geographic Distribution of the Increase in Public Enrolment Rates for Three-year-olds in Percentage Points between 1990/91 and 1993/94



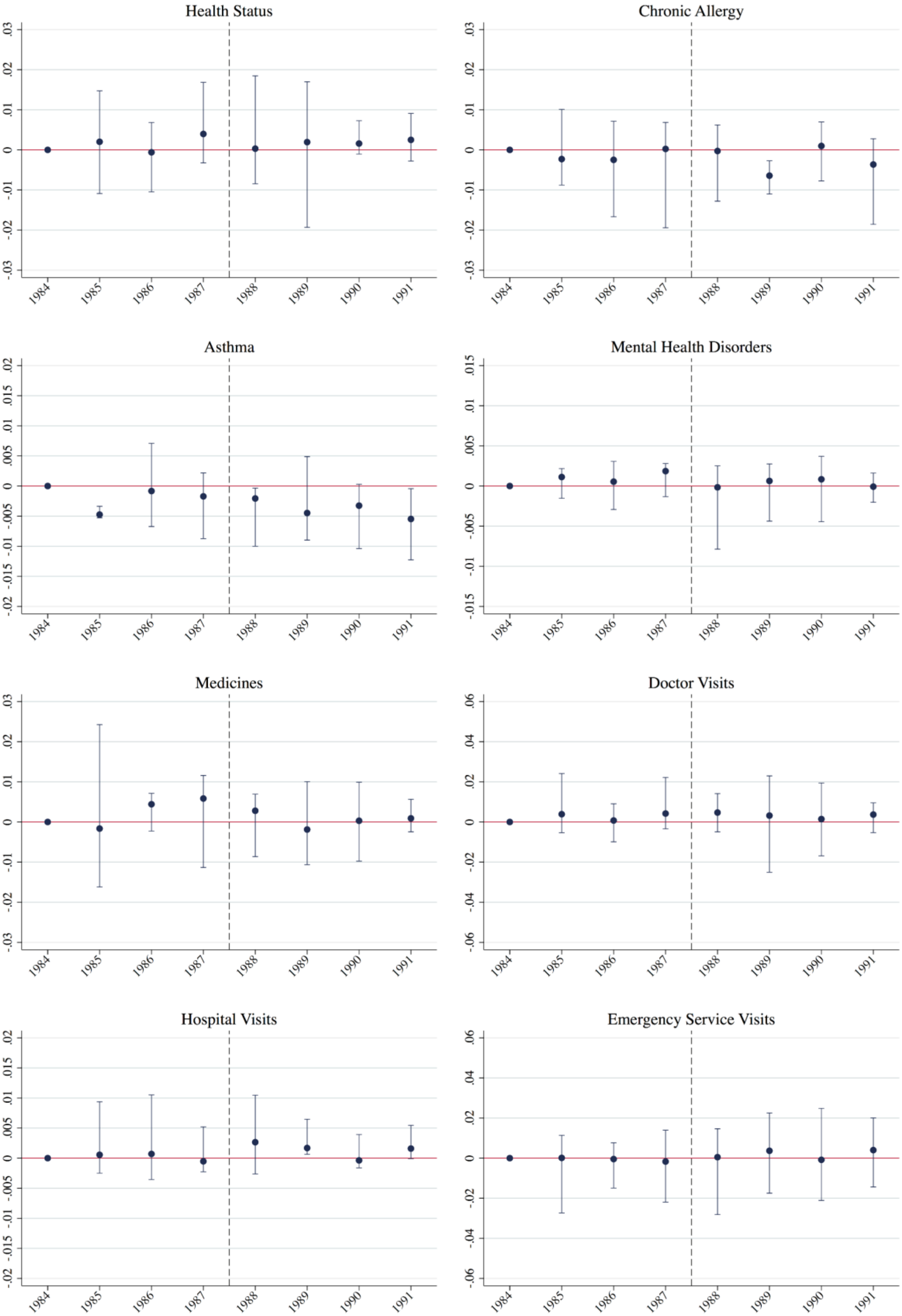
Note: This map illustrates the geographic distribution of the increase in public enrolment rates for three-year-olds in percentage points during the initial expansion period (1990/91-1993/94) across the 15 Spanish regions. The Basque Country, Navarre, Ceuta, and Melilla are excluded from the sample of interest due to different characteristics. The sources of data are the Spanish Ministry of Education and Vocational Training (https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html) and the National Statistics Institute (https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176951&menu=ultiDatos&idp=1254735572981).

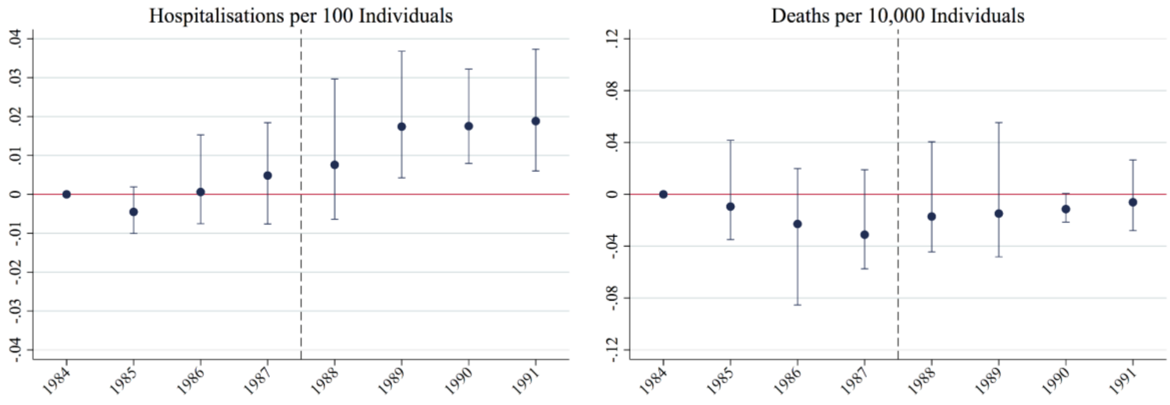
Figure 3. LOGSE and Cohorts of Birth



Source: Author's own creation.

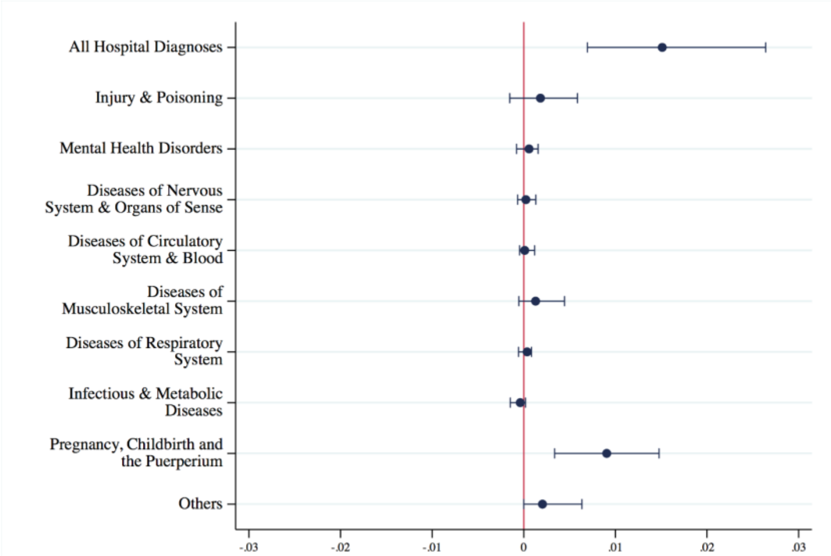
Figure 4. Parallel Trends Assumption for Health Outcomes
and Heterogeneity by Cohort of Birth





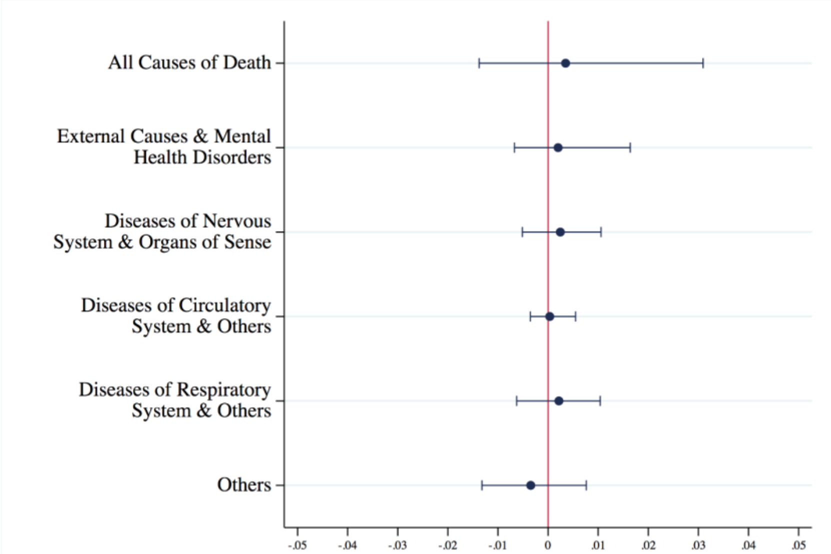
Note: These graphs plot the coefficients of the interactions between $\Delta Preschool_r$ and cohort of birth dummies, and their 95% confidence intervals for all dependent variables. The sample contains cohorts born in 1984-1991. The dashed line splits the cohorts of birth into the pre-reform (left) and post-reform (right) cohorts. The pre-reform cohorts were born in 1984-1987 and the post-reform cohorts in 1988-1991. Cohort born in 1984 is the baseline category. Estimations on health status, all chronic conditions, consumption of medicines, and healthcare use are based on equation (1) and on hospitalisations per 100 individuals and deaths per 10,000 individuals on equation (2). Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations for health outcomes at the individual level = 4,461. Observations for health outcomes at the region level = 1,560. Point estimates are available upon request.

Figure 5. Hospitalisations per 100 Individuals by Diagnosis



Note: Figure 5 plots the coefficients of $\Delta Preschool_r \times Post_c$ and their 95% confidence intervals. Hospitalisations per 100 individuals, treatment variable, and controls are defined in Section 4. Figure 5 focuses on (from top to bottom) hospitalisations 1) for all diagnoses, 2) for injury and poisoning, 3) for mental health disorders, 4) for diseases of the nervous system and organs of sense, 5) for diseases of the circulatory system and diseases of the blood and blood-forming organs, 6) for diseases of the musculoskeletal system and connective tissue, 7) for diseases of the respiratory system, 8) for infectious and parasitic diseases, endocrine, nutritional and metabolic diseases, and immunity disorders, 9) for complications of pregnancy, childbirth, and the puerperium, and 10) for other diagnoses. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations = 1,560.

Figure 6. Deaths per 10,000 Individuals by Cause



Note: Figure 6 plots the coefficients of $\Delta Preschool_r \times Post_c$ and their 95% confidence intervals. Deaths per 10,000 individuals, treatment variable, and controls are defined in Section 4. Figure 6 focuses on (from top to bottom) deaths 1) for all causes, 2) for external causes of morbidity and mortality, and mental and behavioural disorders, 3) for diseases of the nervous system and organs of sense, 4) for diseases of the circulatory system, diseases of the blood and blood-forming organs and certain disorders involving the immune mechanism, and diseases of the musculoskeletal system and connective tissue, 5) for diseases of the respiratory system, certain infectious and parasitic diseases, and endocrine, nutritional and metabolic diseases, and 6) for other causes of death. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations = 1,560.

A1. Appendix Tables

Table A1. Effect of the LOGSE on Public and Private Enrolment Rates

	Public Enrolment Rates	**Private Enrolment Rates**
ITT	0.7898 (0.0006)***	0.1914 (0.1306)
Observations	120	120

Note: *Intention-to-treat* (ITT) effects of the preschool programme on public and private enrolment rates for 1987/88-1994/95 school years (cohorts born in 1984-1991) from estimating equation (2). Enrolment rates, treatment variable and controls are defined in Section 4. Control coefficients are not reported. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table A2. Estimates for Sample Characteristics

	Observations	DiD Estimates
Spanish National Health Survey (2003 & 2006)		
Gender (=1 if female)	4,461	-0.0023 (0.4850)
Semester of birth (=1 if first semester)	4,461	0.0015 (0.3333)
Children aged 10 or younger present in the household	4,461	-0.0015 (0.3147)
Household size	4,461	-0.0010 (0.3408)
Mother present in the household	4,461	0.0005 (0.1669)
Father present in the household	4,461	-0.0007 (0.4993)
Married or cohabiting parents	4,185	-0.0003 (0.7372)
Mother's age at child's birth	4,107	0.0040 (0.8030)
Father's age at child's birth	3,800	0.0249 (0.2217)
At least one parent has secondary education	4,176	0.0015 (0.3688)
At least one parent has university education	4,176	0.0009 (0.4361)
Hospitalisation Registries (1999-2018)		
Gender (=1 if female)	2,323,616	0.0000 (0.7214)
Semester of birth (=1 first)	2,323,616	0.0000 (0.8200)
Death Registries (1999-2018)		
Gender (=1 if female)	13,108	0.0000 (0.9356)
Semester of birth (=1 first)	13,108	-0.0001 (0.8087)
Different region of residence and birth	13,108	0.0004 (0.1408)

Note: Data are drawn from the Spanish National Health Survey (2003 & 2006), Hospital Morbidity Survey (1999-2018), Death Registries (1999-2018), and the Statistics of Non-tertiary Education (1987/88-2002/03). Column 1 reports total observations and Column 2 shows the difference-in-differences (DiD) estimates of characteristics for the three samples together with *p*-values of wild-bootstrapped clustered standard errors with 9,999 repetitions (in parentheses). Each row in Column 2 is a separate regression of sample characteristics on exposure to the LOGSE controlling for region and cohort fixed effects. Parental characteristics have less observations because some individuals born in 1984-1991 do not live with their parents. Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table A3. Association between Regional Public Preschool Increase and Interregional Mobility (Selective Migration)

	Individuals Aged 10-29	**Individuals Aged 10-15**	**Individuals Aged 16-19**	**Individuals Aged 20-24**	**Individuals Aged 25-29**
Regional public preschool increase (based on region of birth)	-0.0002 (0.8174)	-0.0001 (0.8464)	-0.0002 (0.7384)	-0.0003 (0.7122)	-0.0002 (0.9165)
Regional public preschool increase (based on region of residence)	0.0005 (0.7514)	0.0003 (0.7359)	0.0005 (0.6909)	0.0006 (0.7214)	0.0006 (0.8022)
Observations	2,666,759	732,210	537,503	720,129	676,917
Mean	0.059	0.036	0.045	0.059	0.090

Note: Estimations are based on OLS regressions of the probability of residing in a region different from the region of birth at ages 10-29 (interregional mobility) on the regional percentage points increase in public enrolment rates for three-year-olds between 1990/91 and 1993/94. Data on the outcome come from the Spanish Labour Force Survey (1999-2018) and the treatment variable is defined in Section 4. The OLS regression controls for age-band and survey-wave fixed effects. Estimations are weighted using individual weights reported in the Spanish Labour Force Survey. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Control coefficients are not reported. The last two rows report the number of observations and the mean of the outcomes. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table A4. Definitions and Sources of Dependent Variables

Name	Definition
Health status	Dummy variable equal to one if individual replies being "good" or "very good", and zero if "regular", "bad" or "very bad" in the last twelve months.
Chronic allergy, asthma, and mental health disorders	Dummy variable equal to one if individual had been diagnosed with a specific chronic condition (chronic allergy; asthma; mental health disorders), and zero otherwise.
Medicines	Dummy variable equal to one if individual consumed any medicine in the last two weeks, and zero otherwise.
Doctor visits	Dummy variable equal to one if individual visited any doctor (GP or specialist) in the last four weeks, and zero otherwise.
Hospital visits	Dummy variable equal to one if individual stayed in hospital at least one night in the last twelve months, and zero otherwise.
Emergency service visits	Dummy variable equal to one if individual visited any emergency service in the last twelve months, and zero otherwise.
Source: Spanish National Health Survey (2003 & 2006) https://www.mscbs.gob.es/estadEstudios/estadisticas/encuestaNacional	
Hospitalisations per 100 individuals	Hospitalisations by region of residence, cohort of birth (1984-1991), and year of hospital discharge (1999-2018) over births by region of birth and cohort of birth (1984-1991) multiplied per 100.
Source: Hospital Morbidity Survey (1999-2018), Birth Registries (1984-1991) https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176778&menu=ultiDatos&idp=1254735573175 https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736177007&menu=ultiDatos&idp=1254735573002	
Deaths per 10,000 individuals	Deaths by region of birth, cohort of birth (1984-1991), and year of death (1999-2018) over births by region of birth and cohort of birth (1984-1991) multiplied by 10,000.
Source: Death Registries (1999-2018); Birth Registries (1984-1991) https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736177008&menu=ultiDatos&idp=1254735573002 https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736177007&menu=ultiDatos&idp=1254735573002	

Table A5. Definitions and Sources of Treatment and Control Variables

Name	Definition
Treatment variable	
Public preschool increase	Increase in public enrolment rates for three-year-olds in percentage points by region between 1990/91 and 1993/94.
Source: Statistics of Non-tertiary Education (1987/88-2002/03), National Statistics Institute https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176951&menu=ultiDatos&idp=1254735572981	
Control variables at the individual level	
Gender	Dummy variable equal to one if individual is a female, and zero if male.
Month of birth fixed effects	Dummy variable equal to one if individual was born in a specific month, and zero otherwise.
Survey-wave fixed effect	Dummy variable equal to one if individual was surveyed in 2006, and zero if in 2003.
Source: Spanish National Health Survey (2003 & 2006) https://www.mscbs.gob.es/estadEstudios/estadisticas/encuestaNacional	
Pre-reform regional characteristics	
GDP per capita (in €)	Ratio of GDP in current prices, in euros and in 1990 (the base year is 1986) over total population in 1990 by region.
Source: National Statistics Institute https://www.ine.es/dynt3/inebase/index.htm?type=pcaxis&path=/t35/p010/a1996&file=pcaxis https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176951&menu=ultiDatos&idp=1254735572981	
Unemployment rate (%)	Average of quarterly regional unemployment rates derived from the Spanish Labour Force Survey in 1990.
Female labour participation rate (%)	Average of quarterly regional female labour participation rates derived from the Spanish Labour Force Survey in 1990.
Source: National Statistics Institute https://www.ine.es/dynt3/inebase/index.htm?type=pcaxis&path=/t22/e308/pae/px/&file=pcaxis	

Continued

Table A5. Definitions and Sources of Treatment and Control Variables (Cont.)

Name	**Definition**
Proportion of women and men population with tertiary education (%)	Proportion of women and men population older than 25 with tertiary education from the 1991 Census by region.
Source: National Statistics Institute (1991 Census) https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_&cid=1254736176992&menu=ultiDatos&idp =1254735572981	
Population (in thousands)	Total population in 1990 in thousands by region.
Source: National Statistics Institute https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176951&menu=ultiDatos&idp=1254735572981	
Public enrolment rate for three-year-olds (%)	Public enrolment rate for three-year-olds in 1990/91 by region.
Preschool and primary centres per 100,000 individuals	Preschool and primary centres in 1990/91 over total population in 1990 per 100,000 individuals by region.
Source: Statistics of Non-tertiary Education (1990/91), National Statistics Institute https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176951&menu=ultiDatos&idp=1254735572981	
Regional president in left-wing party	Dummy variable equal to one if the regional president in 1990 belonged to a left-wing party, and zero if belonged to a right- or centre-wing party.
Source: https://www.senado.es/web/wcm/idc/groups/public/@cta_rrdc/documents/document/mdaw/mdmy/~edisp/ccaa1_ptes_gobiernos.pdf	
Contemporaneous reforms (used in Section 6)	
Decentralisation of competences	Dummy variable equal to one if individual resides/is born in a region which received health and/or education competences before 1991/92, and zero otherwise.
Source: Bonal et al. (2005) and Costa-Font & Rico (2006)	
Abortion legalisation	Number of clinics that conducted at least one abortion in 1989 per 100,000 individuals at the region level
Source: Ministry of Health, Consumer Affairs and Social Welfare (1989), National Statistics Institute https://www.mscbs.gob.es/profesionales/saludPublica/prevPromocion/embarazo/docs/IVEs_anteriores/IVE_1989.pdf https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736176951&menu=ultiDatos&idp=1254735572981	

Continued

Table A5. Definitions and Sources of Treatment and Control Variables (Cont.)

Name	Definition
Quality measures (used in Section 6)	
Class size	Preschool students over preschool units (classrooms) in 1987/88-1994/95 by region.
Preschool students to teachers ratio	Preschool students over preschool teachers in 1987/88-1994/95 by region.
Source: Statistics of Non-tertiary Education (1987/88-1994/95) https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html	

Table A6. Main Results without Controls

	Health Status	Chronic Allergy	Asthma	Mental Health Disorders	Medicines	Doctor Visits	Hospital Visits	Emergency Service Visits	Hospitalisations per 100 Individuals	Deaths per 10,000 Individuals
ITT	0.0005 (0.0006) [0.7910] {0.7910}	-0.0002 (0.0007) [0.7652] {0.7910}	-0.0012 (0.0006)* [0.2938] {0.5880}	-0.0003 (0.0004) [0.6323] {0.7910}	-0.0018 (0.0010)* [0.2464] {0.5880}	-0.0015 (0.0008)* [0.2063] {0.5880}	0.0002 (0.0004) [0.6192] {0.7910}	0.0004 (0.0011) [0.7674] {0.7910}	0.0073 (0.0036)* [0.1112] {0.5880}	0.0066 (0.0052) [0.1726] {0.5880}
Region FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Ind. controls	×	×	×	×	×	×	×	×	×	×
Regional controls	×	×	×	×	×	×	×	×	×	×
Observations	4,461	4,461	4,461	4,461	4,461	4,461	4,461	4,461	1,560	1,560
Pre-reform mean	0.878	0.152	0.069	0.029	0.451	0.347	0.048	0.329	5.658	3.554

Note: Estimations are based on OLS on equations (1) and (2) only controlling for region, cohort, and survey/year of hospital discharge/year of death fixed effects. Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes and treatment variable are defined in Section 4. The first row presents the *intention-to-treat* (ITT) effects and their corresponding standard errors and *p*-values. Standard errors clustered at region level are in parentheses, *p*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in squared brackets, and *p*-values correcting for multiple hypotheses testing are in curly brackets. The last two rows report the number of observations and the mean of health outcomes for pre-reform cohorts. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the standard error or *p*-value.

Table A7. Descriptive Statistics of Controls in Robustness Checks

	Obs.	**Mean**	**Std. Dev.**	**Min.**	**Max.**
Panel A: Contemporaneous reforms					
Decentralisation of competences	15	0.333	0.488	0	1
Abortion legalisation	15	0.162	0.176	0	0.631
Panel B: Quality measures					
Class size	120	24.43	2.413	19.15	30.95
Preschool student to teacher ratio	120	24.16	3.423	17.40	44.69

Note: Details of all variables are explained in Table A5. Variables in Panel A are snapshots and their descriptive statistics are computed for the 15 Spanish regions considered. Variables in Panel B vary by region and cohort of birth and their descriptive statistics are computed for the 15 Spanish regions and eight cohorts considered. Obs = observations, Std. Dev. = standard deviation, Min. = minimum, Max = maximum.

Table A8. Additional Robustness Checks

	(1) Baseline	(2) Exclusion of Richest and Poorest Regions	(3) Probit Model	(4) Logit Model	(5) Interaction of Fixed Effects	(6) Linear Cohort Trend	(7) Quadratic Cohort Trend
Health outcomes at the individual level							
Health status	0.0002	0.0000	-0.0005	-0.0007	0.0002	0.0002	0.0002
	(0.8074)	(0.8484)	(0.5036)	(0.4054)	(0.7040)	(0.7828)	(0.7828)
Chronic allergy	-0.0012	-0.0032	-0.0011	-0.0011	-0.0011	-0.0012	-0.0012
	(0.5909)	(0.1499)	(0.2294)	(0.2219)	(0.6036)	(0.5894)	(0.5895)
Asthma	-0.0021	-0.0011	-0.0018	-0.0018	-0.0020	-0.0021	-0.0021
	(0.0158)**	(0.3209)	(0.0001)***	(0.0002)***	(0.0170)**	(0.0170)**	(0.0170)**
Mental health disorders	-0.0006	-0.0015	-0.0006	-0.0008	-0.0007	-0.0006	-0.0006
	(0.4904)	(0.1053)	(0.3311)	(0.3855)	(0.3956)	(0.4886)	(0.4887)
Medicines	-0.0019	-0.0037	-0.0017	-0.0016	-0.0017	-0.0019	-0.0019
	(0.1595)	(0.1618)	(0.0523)*	(0.0645)*	(0.2743)	(0.1444)	(0.1445)
Doctor visits	0.0011	-0.0010	0.0010	0.0010	0.0010	0.0010	0.0010
	(0.7196)	(0.2632)	(0.4818)	(0.4383)	(0.7235)	(0.7367)	(0.7367)
Hospital visits	0.0013	0.0020	0.0024	0.0027	0.0011	0.0012	0.0012
	(0.0675)*	(0.2340)	(0.0046)***	(0.0027)***	(0.0832)*	(0.0614)*	(0.0613)*
Emergency service visits	0.0023	0.0023	0.0025	0.0024	0.0020	0.0024	0.0024
	(0.2959)	(0.5834)	(0.0234)*	(0.0234)*	(0.3052)	(0.2929)	(0.2929)
Observations	4,461	4,016	4,461	4,461	4,461	4,461	4,461
Health outcomes at the region level							
Hospitalisations per 100 individuals	0.0151	0.0167	-	-	0.0278	0.0151	0.0151
	(0.0108)**	(0.1187)			(0.0435)**	(0.0108)**	(0.0108)**
Deaths per 10,000 individuals	0.0035	0.0074	-	-	0.0000	0.0035	0.0035
	(0.7431)	(0.4735)			(0.9973)	(0.7428)	(0.7430)
Observations	1,560	1,352	-	-	1,560	1,560	1,560

Note: Each cell reports the *intention-to-treat* (ITT) effect. Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. Column 1 shows the baseline estimates from Table 3. Column 2 excludes the richest (the Balearic Islands) and poorest (Extremadura) regions based on the GDP per capita in 1990. For health outcomes at the individual level, Columns 3 and 4 apply a probit and a logit model, respectively, and provide marginal effects. Columns 3 and 4 include $\Delta Preschool_r$ and $Post_c$ variables instead of region and cohort fixed effects, respectively, for mental health disorders and hospital visits due to problems to calculate the maximum likelihood estimator. Column 5 adds an interaction of region fixed effects with survey, year of hospital discharge or year of death fixed effects. Columns 6 and 7 include the $Post_c$ variable and a linear and quadratic cohort trend, respectively, instead of cohort fixed effects. *P*-values for wild-bootstrapped clustered standard errors with 9,999 repetitions are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

Table A9. Heterogeneity by Hospital Diagnosis and Cause of Death

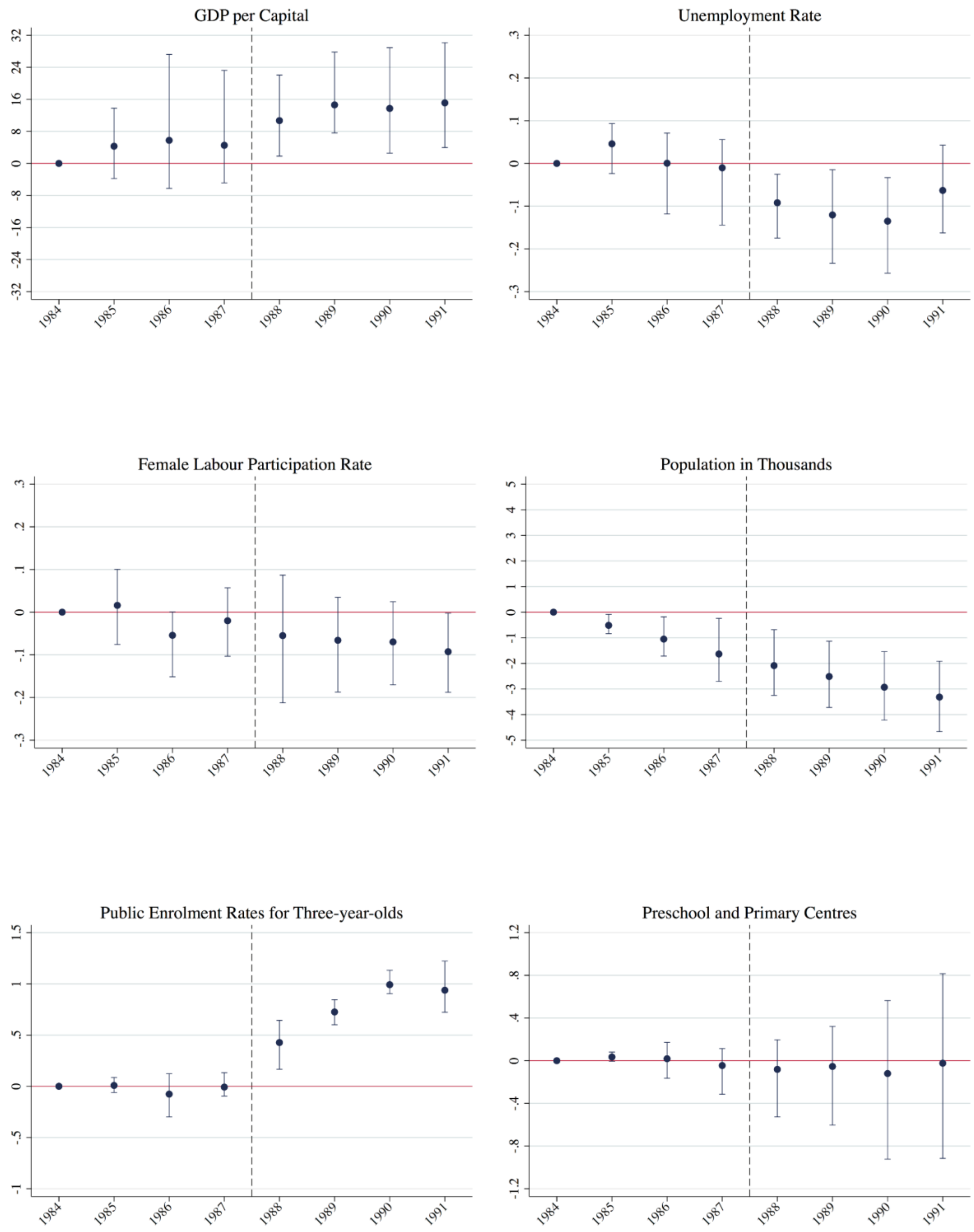
Hospitalisations per 100 Individuals	**All Hospital Diagnoses**	**Injury & Poisoning**	**Mental Health Disorders**	**Diseases of the Nervous System & Organs of Sense**	**Diseases of the Circulatory System & Blood**	**Diseases of the Musculoskeletal System**	**Diseases of the Respiratory System**	**Infectious & Metabolic Diseases**	**Pregnancy, Childbirth, and the Puerperium**	**Others**
ITT	0.0151 (0.0108)**	0.0018 (0.2900)	0.0006 (0.2295)	0.0002 (0.3863)	0.0001 (0.6679)	0.0013 (0.1457)	0.0004 (0.2213)	-0.0004 (0.1486)	0.0091 (0.0218)**	0.0020 (0.0501)*
Observations	1,560	1,560	1,560	1,560	1,560	1,560	1,560	1,560	1,560	1,560
Pre-reform mean	5.658	0.680	0.220	0.148	0.129	0.358	0.381	0.178	1.977	1.585

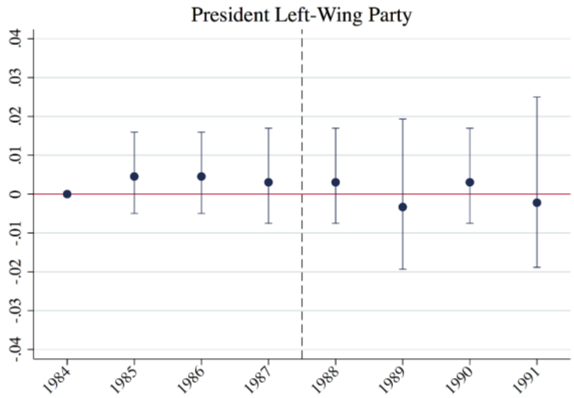
Deaths per 10,000 Individuals	**All Causes of Death**	**External Causes & Mental Health Disorders**	**Diseases of the Nervous System & Organs of Sense**	**Diseases of the Circulatory System & Others**	**Diseases of the Respiratory System & Others**	**Others**
ITT	0.0035 (0.7431)	0.0020 (0.6825)	0.0024 (0.3363)	0.0003 (0.8072)	0.0022 (0.5709)	-0.0035 (0.4992)
Observations	1,560	1,560	1,560	1,560	1,560	1,560
Pre-reform mean	3.554	2.084	0.219	0.242	0.226	0.785

Note: Each cell reports the *intention-to-treat* effect (ITT) by hospital diagnosis and cause of death. Health outcomes, treatment variable, and controls are defined in Section 4. Information related to hospital diagnoses and causes of death is explained in Appendix A7. Control coefficients are not reported. *P*-values for wild-bootstrapped clustered standard errors are in parentheses. Parameters statistically significant at 1% (***), 5% (**), and 10% (*) levels are reported next to the *p*-value.

A2. Appendix Figures

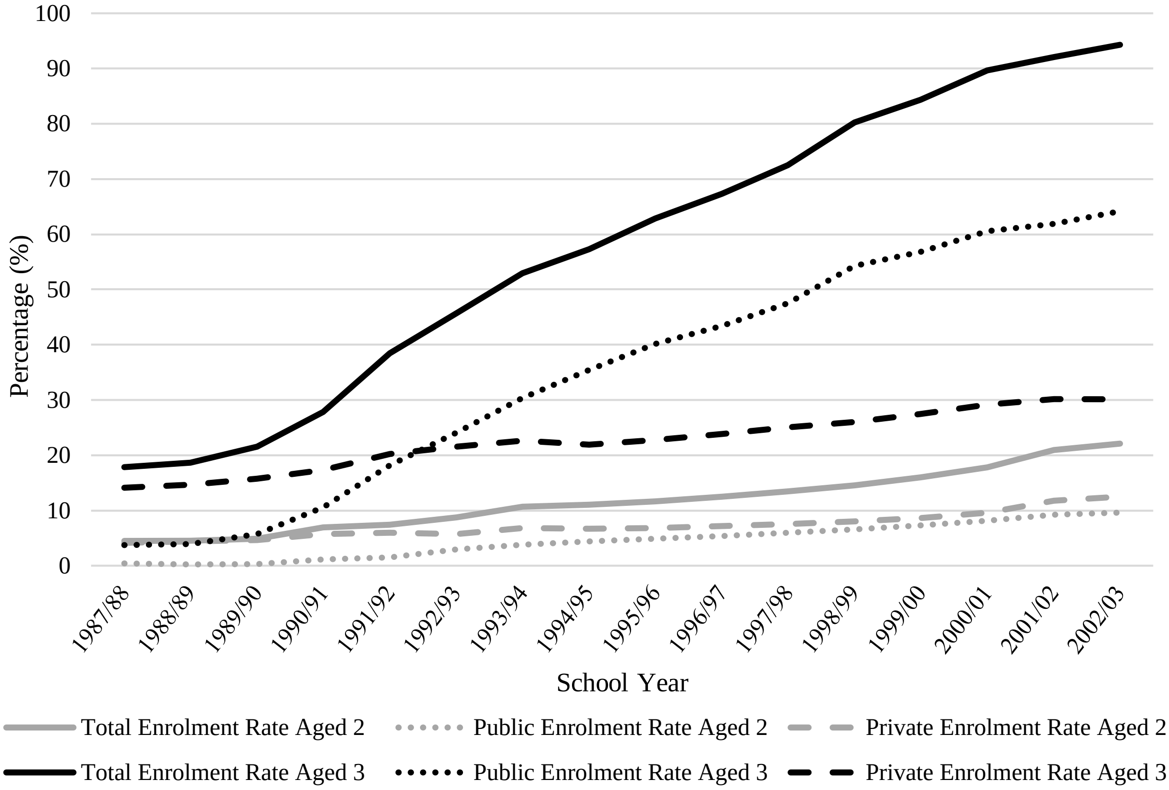
Figure A1. Parallel Trends Assumption for Regional Variables by Cohort of Birth





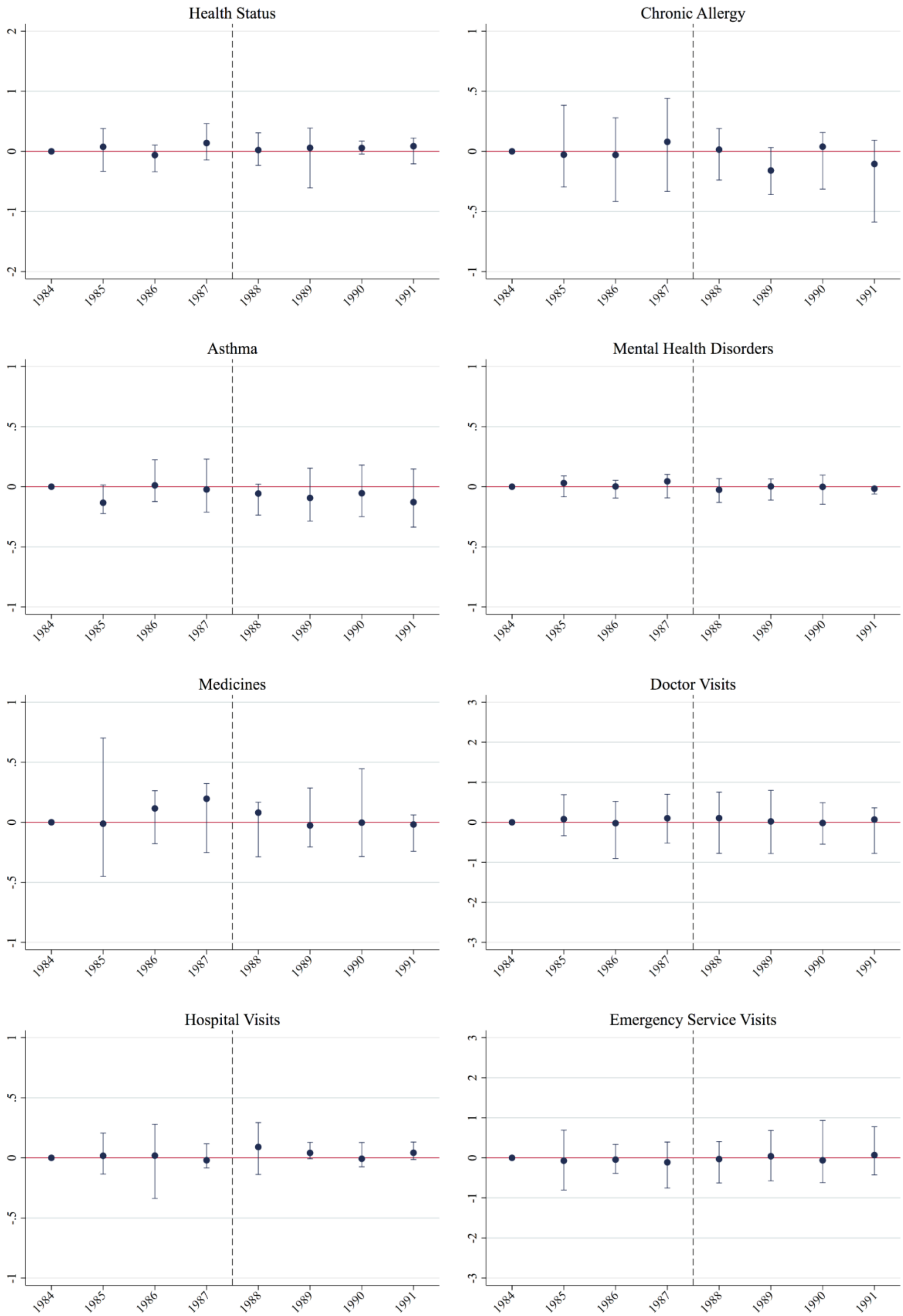
Note: These graphs plot the coefficients of the interactions between $\Delta Preschool_r$ and cohort of birth dummies, and their 95% confidence intervals for seven regional characteristics from estimating equation (2). The sample contains cohorts born in 1984-1991. The dashed line splits the cohorts of birth into the pre-reform (left) and post-reform (right) cohorts. The pre-reform cohorts were born in 1984-1987 and the post-reform cohorts in 1988-1991. Cohort born in 1984 is the baseline category. Regional characteristics are measured in the year when the cohorts were aged three (1987-1994). Regional characteristics, treatment variable and controls are defined in Section 4. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations =120. Point estimates are available upon request.

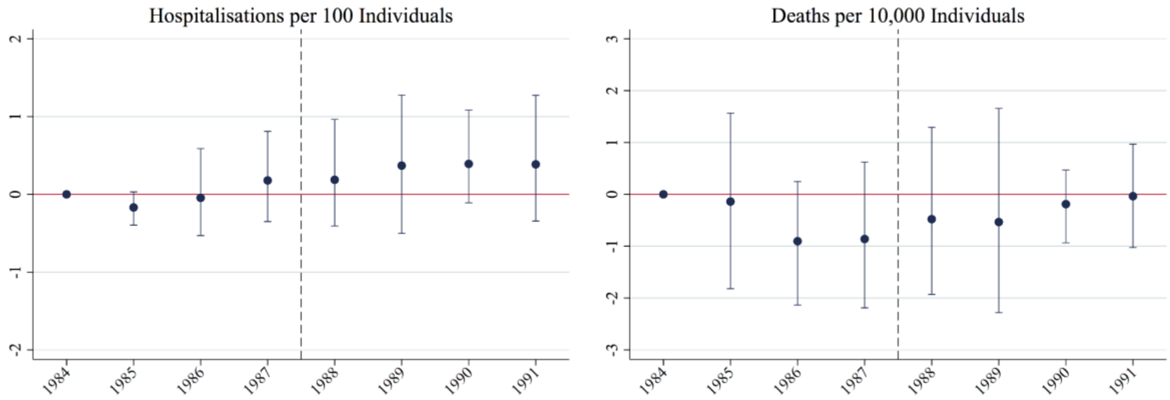
Figure A2. Preschool Enrolment Rates for Two- and Three-year-olds



Source: Spanish Ministry of Education and Vocational Training (https://www.educacionyfp.gob.es/servicios-al-ciudadano/estadisticas/no-universitaria/alumnado/matriculado.html).

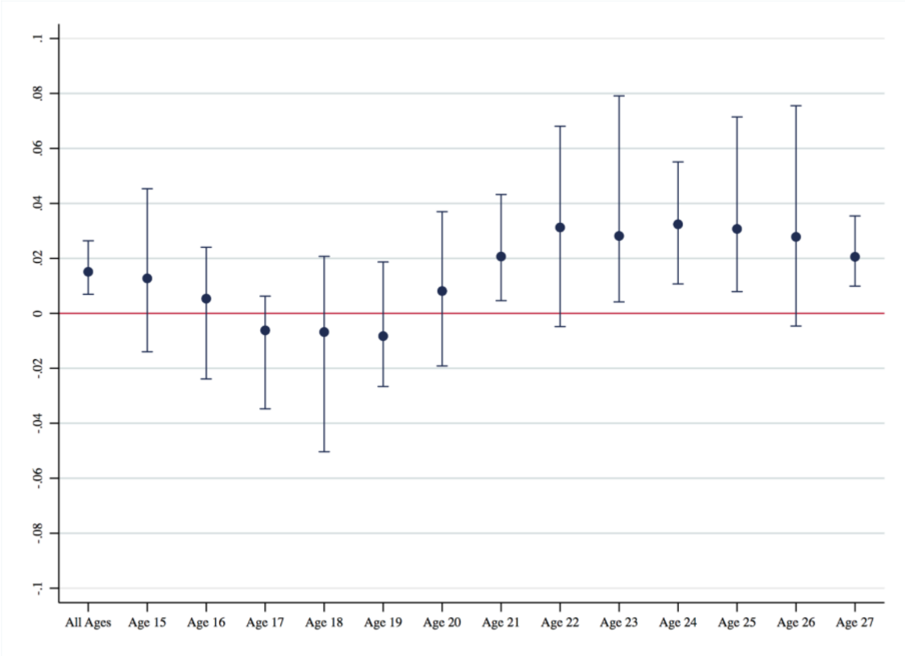
Figure A3. Parallel Trends Assumption for Health Outcomes with Binary Treatment





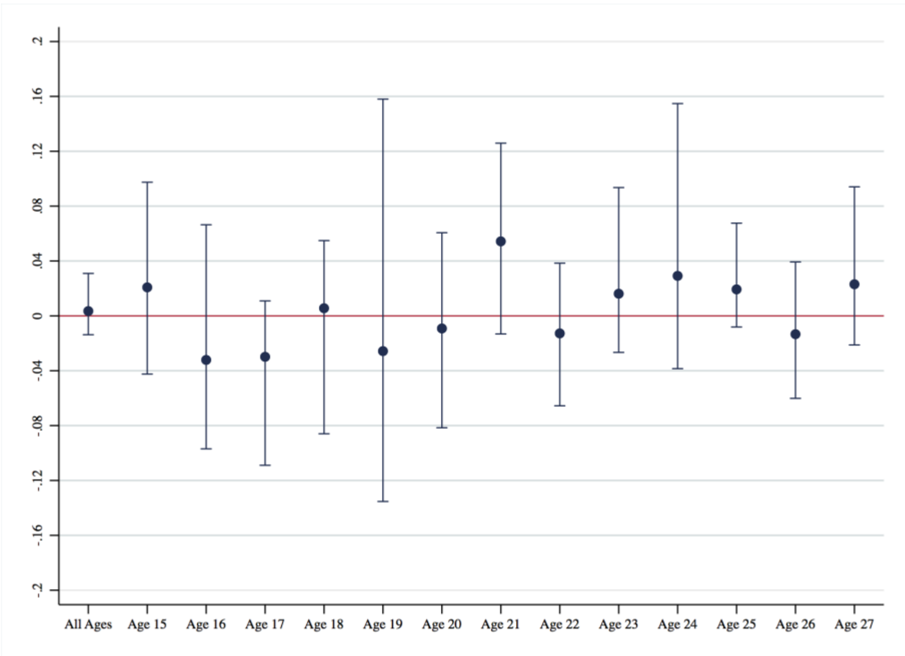
Note: These graphs plot the coefficients of the interactions between $Treated_r$ and cohort of birth dummies, and their 95% confidence intervals for all dependent variables. $Treated_r$ is a binary treatment that splits the list of regions in Table 2 at the median. The sample contains cohorts born in 1984-1991. The dashed line splits the cohorts of birth into the pre-reform (left) and post-reform (right) cohorts. The pre-reform cohorts were born in 1984-1987 and the post-reform cohorts in 1988-1991. Cohort born in 1984 is the baseline category. Estimations on health status, all chronic conditions, consumption of medicines, and healthcare use are based on equation (1) and on hospitalisations per 100 individuals and deaths per 10,000 individuals on equation (2). Estimations using health outcomes at the individual level are weighted using individual weights reported in the Spanish National Health Survey in 2003 and 2006. Health outcomes, treatment variable, and controls are defined in Section 4. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations for health outcomes at the individual level = 4,461. Observations for health outcomes at the region level = 1,560. Point estimates are available upon request.

Figure A4. Hospitalisations per 100 Individuals by Age



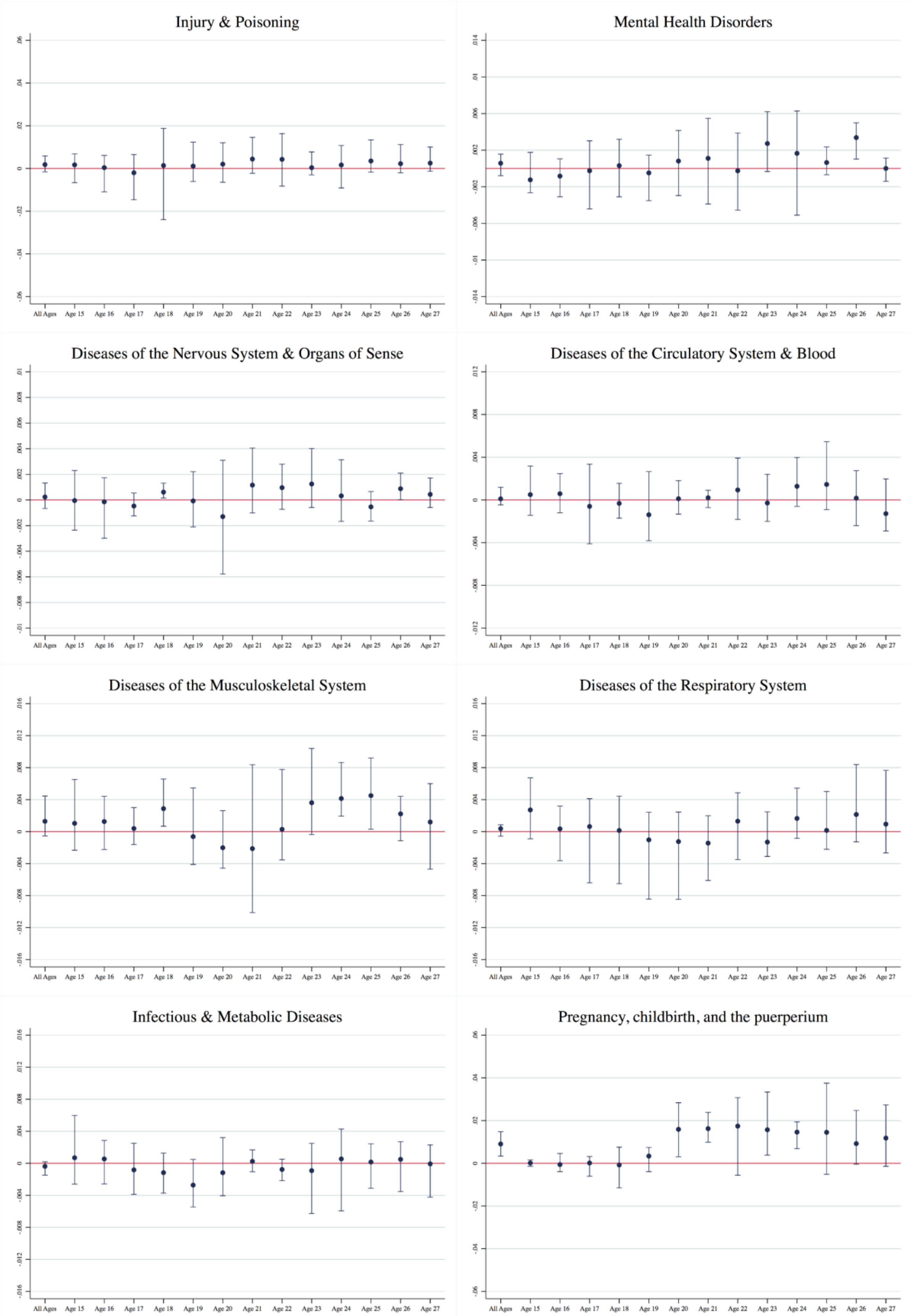
Note: Figure A4 plots the coefficients of $\Delta Preschool_r \times Post_c$ by age and their 95% confidence intervals. Hospitalisations per 100 individuals, treatment variable, and controls are defined in Section 4. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations for all ages are 1,560 and for each age are 120.

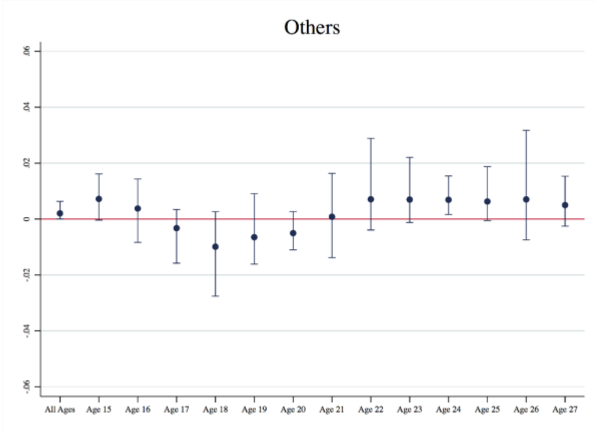
Figure A5. Deaths per 10,000 Individuals by Age



Note: Figure A5 plots the coefficients of $\Delta Preschool_r \times Post_c$ by age and their 95% confidence intervals. Deaths per 10,000 individuals, treatment variable, and controls are defined in Section 4. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations for all ages are 1,560 and for each age are 120.

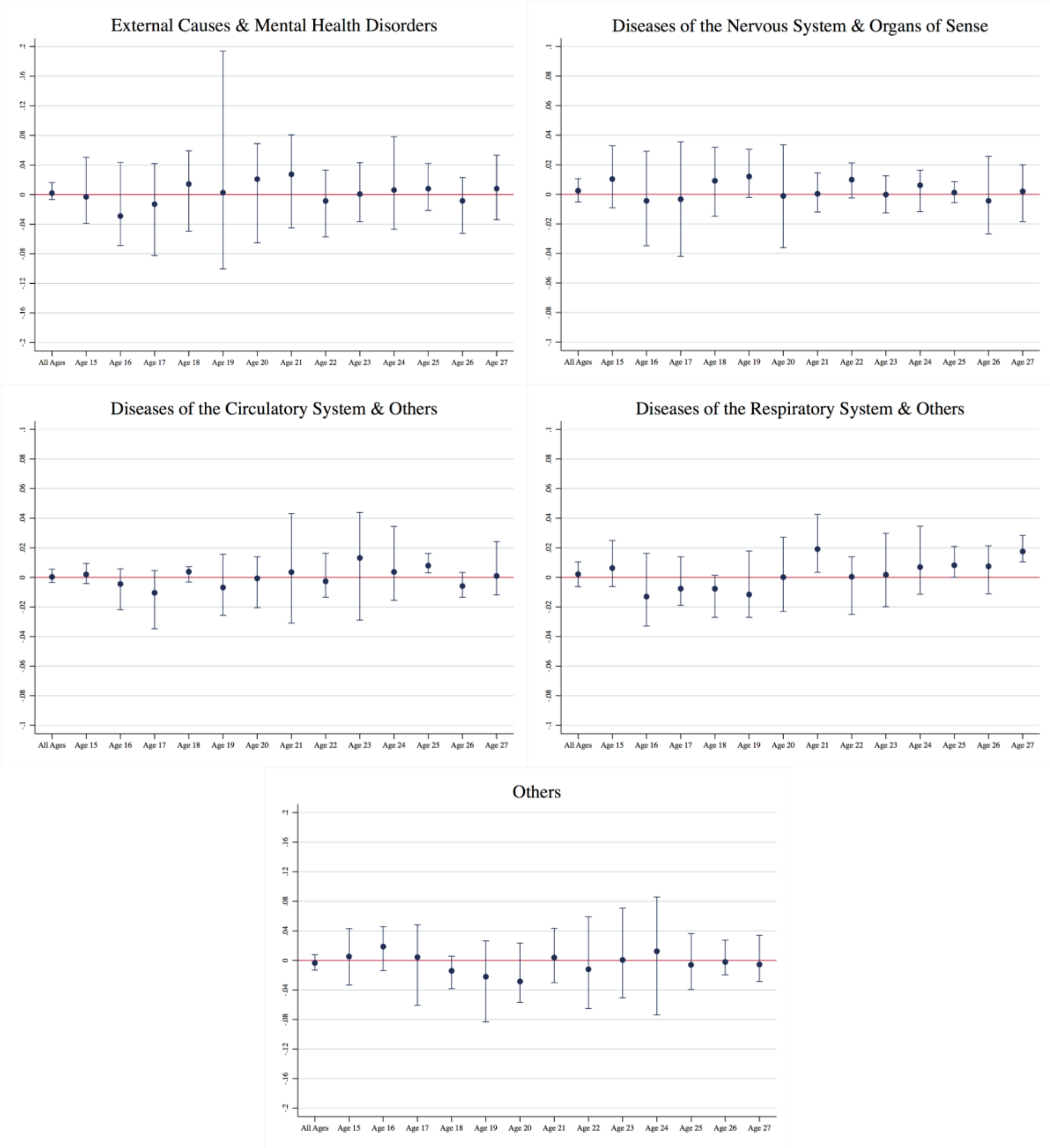
Figure A6. Hospitalisations per 100 Individuals by Hospital Diagnosis and Age





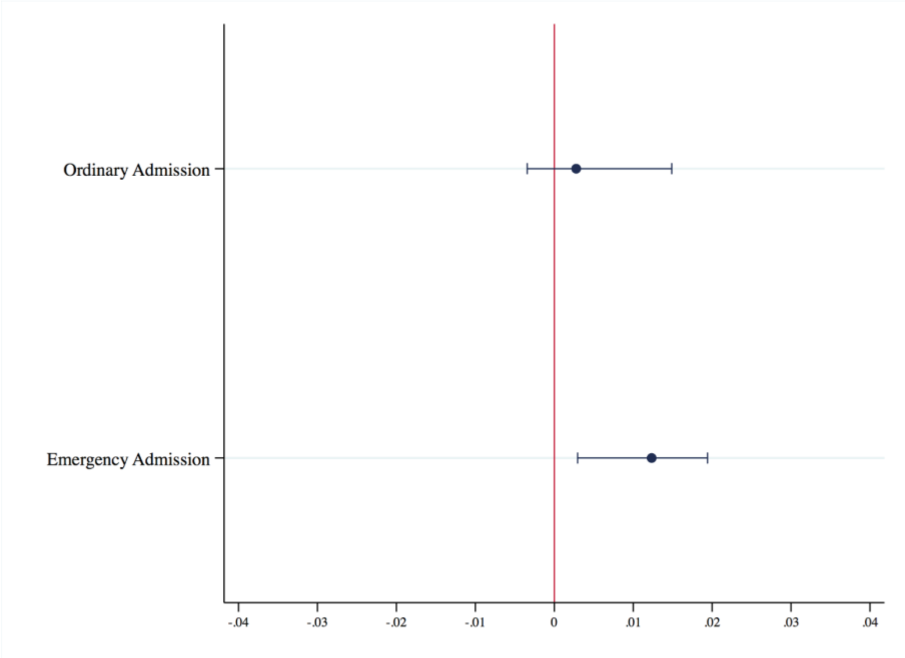
Note: Figure A6 plots the coefficients of $\Delta Preschool_r \times Post_c$ by hospital diagnosis and age and their 95% confidence intervals. Hospitalisations per 100 individuals, treatment variable, and controls are defined in Section 4. Figure A6 focuses on (from left to right, top to bottom) hospitalisations 1) for injury and poisoning, 2) for mental health disorders, 3) for diseases of the nervous system and organs of sense, 4) for diseases of the circulatory system and diseases of the blood and blood-forming organs, 5) for diseases of the musculoskeletal system and connective tissue, 6) for diseases of the respiratory system, 7) for infectious and parasitic diseases, endocrine, nutritional and metabolic diseases, and immunity disorders, 8) for complications of pregnancy, childbirth, and the puerperium, and 9) for other diagnoses. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations for all ages are 1,560 and for each age are 120.

Figure A7. Deaths per 10,000 Individuals by Cause of Death and Age



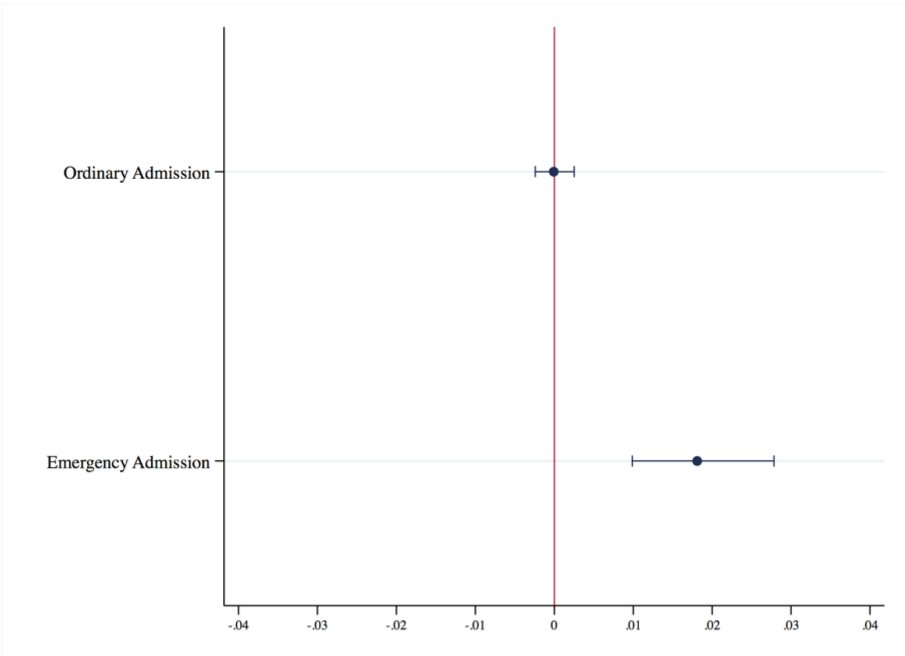
Note: Figure A7 plots the coefficients of $\Delta Preschool_r \times Post_c$ by cause of death and age and their 95% confidence intervals. Deaths per 10,000 individuals, treatment variable, and controls are defined in Section 4. Figure A7 focuses on (from left to right, top to bottom) deaths 1) for external causes of morbidity and mortality, and mental and behavioural disorders, 2) for diseases of the nervous system and organs of sense, 3) for diseases of the circulatory system, diseases of the blood and blood-forming organs and certain disorders involving the immune mechanism, and diseases of the musculoskeletal system and connective tissue, 4) for diseases of the respiratory system, certain infectious and parasitic diseases, and endocrine, nutritional and metabolic diseases, and 5) for other causes of death. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations for all ages are 1,560 and for each age are 120.

Figure A8. Hospitalisations per 100 Individuals by Type of Hospital Admission



Note: Figure A8 plots the coefficients of $\Delta Preschool_r \times Post_c$ by type of hospital admission and their 95% confidence intervals for all hospitalisations. Hospitalisations per 100 individuals, treatment variable, and controls are defined in Section 4. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations = 1,560.

Figure A9. Hospitalisations per 100 Individuals for Pregnancy-Related Diagnoses by Type of Hospital Admission



Note: Figure A9 plots the coefficients of $\Delta Preschool_r \times Post_c$ by type of hospital admission and their 95% confidence intervals for hospitalisations for complications of pregnancy, childbirth, and the puerperium. Hospitalisations per 100 individuals, treatment variable, and controls are defined in Section 4. All specifications are estimated by OLS. Confidence intervals are estimated by wild-bootstrap cluster method with 9,999 repetitions. Observations = 1,560.

A3. Derivation of Equations (1) and (2)

Equations (1) and (2) in Section 3 implicitly assume that pre-reform cohorts (born in 1984-1987) were exposed to public enrolment rates for three-year-olds in 1990/91 and post-reform cohorts (born in 1988-1991) to these in 1993/94. This appendix shows how this assumption evolves to equations (1) and (2).

First, I assume that health *y* for cohort *c* in region *r* is a function of public preschool enrolment rates for three-year-olds in *c* and *r* (i.e. a kind of probability of attending public preschool education, $Preschool_r^c$) and a set of characteristics ($\boldsymbol{W}_{rc}$) including time-invariant and time-variant regional and cohort factors. Moreover, *y* depends on an idiosyncratic shock, ν_{rc}. For simplicity, subscripts *i*, *w*, and *t* are not included. Thus,

$$y_{rc} = f(Preschool_r^c, \boldsymbol{W}_{rc}, \nu_{rc}).$$

Second, I also assume that *f*(.) is linear and that pre-reform cohorts ($Post_c = 0$) were exposed to pre-policy public enrolment rates in 1990/91 since the policy was implemented in 1991/92. Instead, post-reform cohorts ($Post_c = 0$) are exposed to post-policy public enrolment rates in 1993/94. Alternatively, one could use public enrolment rates in 1992/93 and 1994/95 or public enrolment rates by *c* and *r*, although the latter does not capture the initial implementation intensity. I show that results are robust to these alternative specifications in Section 6. Then:

$$y_{rc} = \theta_0 + \theta_1 Preschool_r^{1990/91} \times (1 - Post_c) + \theta_2 Preschool_r^{1993/94} \times Post_c + \boldsymbol{W}'_{rc}\boldsymbol{\theta}_3 + \nu_{rc} \qquad \text{(A1)}$$

where θ_0 is the basic health of the Spanish population and θ_1 is the effect of public enrolment rates in 1990/91 on the health of pre-reform cohorts. θ_2 is the effect of public enrolment rates in 1993/94 on the health of post-reform cohorts, i.e. the *additional* health benefit that post-reform cohorts experience due to having a higher public preschool enrolment rate.

If $Preschool_r^{1993/94} = Preschool_r^{1990/91} + \Delta Preschool_r$, equation (A1) is rewritten as:

$$\begin{aligned} y_{rc} = \theta_0 + \theta_1 Preschool_r^{1990/91} + (\theta_2 - \theta_1) Preschool_r^{1990/91} \times Post_c \\ + \theta_2 \Delta Preschool_r \times Post_c + \boldsymbol{W}'_{rc}\boldsymbol{\theta}_3 + \nu_{rc}. \end{aligned} \qquad \text{(A2)}$$

Notice that $Preschool_r^{1990/91}$ and $Preschool_r^{1990/91} \times Post_c$ can be included in $\boldsymbol{W}_{rc}$. To be more precise, $Preschool_r^{1990/91}$ is captured by region fixed effects $\boldsymbol{\gamma}_r$ and $\boldsymbol{\delta}_r$ and $Preschool_r^{1990/91} \times Post_c$ by $\boldsymbol{Z}_{rc}$ in equations (1) and (2), thus:

$$y_{rc} = \theta_0 + \theta_2 \Delta Preschool_r \times Post_c + \boldsymbol{W}'_{rc}\boldsymbol{\theta}_3 + \nu_{rc} \qquad \text{(A3)}$$

where $\theta_0 = \alpha_0$ and $\theta_0 = \beta_0$, and $\theta_2 = \alpha_1$ and $\theta_2 = \beta_1$ in equations (1) and (2), respectively. Then, equation (A3) is analogous to equations (1) and (2).

A4. Creation of Preschool Enrolment Rates

The Spanish Ministry of Education and Vocational Training together with the regional Departments of Education publish information related to student enrolment in the Statistics of Non-tertiary Education, which include data on preschool, primary, secondary, special (i.e., visual arts and design, music, dance, dramatic arts, languages, and sports), and adult education. I employ enrolment rates for three-year-olds by region and type of school (public or private) for 1987/88-2002/03 to compute the treatment variable (i.e. $\Delta Preschool_r$), which is defined as the difference in p.p. between public enrolment rates for three-year-olds by region in 1990/91 and 1993/94.

For the period before 1991/92, enrolment rates are unavailable. Instead, the Ministry of Education and Vocational Training reports enrolment by group of age (2-3 and 4-5 years), region, and type of school. National enrolment rates for two-year-olds were much lower than that for children aged three (see Figure A2). For instance, public enrolment rates for children aged two were 1.2%, while for children aged three were 10.5% in 1990/91. In fact, national public enrolment rates for children aged two did not exceed 1.5% in 1987/88-1991/92. Therefore, I divide enrolment for individuals aged 2-3 years by region and type of school over regional population for three-year-olds (from the National Statistics Institute) to approximate regional enrolment rates for children aged three for the period from 1987/88 to 1990/91.

Data on enrolment rates for three-year-olds by region are publicly available from 1991/92 onwards, however they are not disaggregated by type of school. The absolute number of students enrolled by age, region, and type of school from 1992/93 to 2002/03 was received by the Ministry of Education and Vocational Training. Enrolment by group of age (0-3 and 4-5 years), region, and type of school is published for 1991/92. National enrolment rates were 0.4% and 1.9%, public enrolment rates were 0.1% and 0.5%, and private enrolment rates were 0.3% and 1.3% for zero- and one-year-olds in 1991/92, respectively. Then, I use enrolment for 0-3-year-olds by region and type of school to proxy enrolment for children aged three in 1991/92. Then, I multiply total enrolment rates by the proportion of students enrolled in public and private centres to split them into public and private rates, respectively. Finally, I also compute a linear interpolation for the Valencian Community due to missing enrolment data from 1989/90 to 1991/92.

A5. Control Variables

I employ several time-invariant control variables measured at the individual level in equation (1) using the Spanish National Health Survey for 2003 and 2006. Gender is a dummy equal to one for women, and zero for men. I add month of birth fixed effects and a dummy variable equal to one if the individual was surveyed in 2006, and zero if in 2003 (survey-wave fixed effect).

Several pre-reform control variables measured at the region level are included in equations (1) and (2), which control for the varying levels of implementation intensity across regions. First, I add macro and demographic variables reported by the National Statistics Institute. GDP per capita is defined as the ratio of GDP in current prices, in euros and in 1990 (the base year is 1986) over total population in 1990. I include the average of quarterly total unemployment and female labour participation rates derived from the Spanish Labour Force Survey in 1990. I also use the proportion of men and women older than 25 with tertiary education from the 1991 Census and the population in thousands in 1990.

Second, I control for pre-reform regional preschool coverage and endowments in 1990/91. Regions with higher coverage rates and more preschool endowments right before the implementation of the reform could have expanded more intensively. Preschool coverage is proxied by public enrolment rates for three-year-olds as defined in Section 4.3. Public enrolment rates in 1990/91 interacted with cohort dummies have to be included in equations (1) and (2) according to the derivation in Appendix A3. I consider preschool centres as endowments and include the number of preschool and primary centres per 100,000 individuals. Data on preschool and primary centres are added as both types of centres supplied places for three-year-olds from 1991/92. These variables are published by the Spanish Ministry of Education and Vocational Training.

Finally, left-wing regional governments could have accepted more easily policies introduced by the left-wing national government (*Partido Socialista Obrero Español*, PSOE) in 1990. Therefore, I use a dummy variable equal to one if the regional president in 1990 belonged to a left-wing party, and zero if belonged to a right-wing or centrist party.

A6. Additional Robustness Checks

Changes in individual and regional characteristics should not depend on the exposure to the LOGSE programme. I exclude the richest and poorest regions of Spain to homogenise the sample and further address any potential bias from differential regional characteristics in Column 2 of Table A8. The coefficients for most of health outcomes become larger in magnitude (in absolute values), but they are less precisely estimated potentially due to the small sample sizes.

I also check the sensitivity of the results by running probit and logit models for the health outcomes at the individual level and report their marginal effects in Columns 3 and 4 of Table A8, respectively. The findings are robust to using non-linear models instead of employing a linear probability model. Moreover, the results are robust to adding interaction terms between region fixed effects and wave, year of hospital discharge or year of death fixed effects, and substituting cohort fixed effects by linear and quadratic cohort trends. These robustness checks can be found in Columns 5, 6, and 7 of Table A8.

A7. Hospitalisations by Diagnosis and Deaths by Cause

Registries on hospitalisations by diagnosis and deaths by cause are conducted by the National Statistics Institute. Access to data on deaths by cause has been given by the National Statistics Institute.

Hospitalisations occurring until 2015 are coded according to the International Classification of Diseases 9th Edition Clinical Modification (ICD-9-CM), while those after 2016 according to the International Classification of Diseases 10th Edition Clinical Modification (ICD-10-CM). To homogenise the sample, I map ICD-10-CM codes to ICD-9-CM codes using the General Equivalence Mapping processed by the Centers for Medicare and Medicaid Services[42].

The eight groups of hospitalisations by diagnosis include (ICD-9-CM codes in parentheses):

1. Injury and poisoning (800-999).
2. Mental health disorders (290-319).
3. Diseases of the nervous system and organs of sense (320-389). Diseases of the nervous system (320-358), diseases of the eye and adnexa (360-379), diseases of the ear and mastoid process (380-389).
4. Diseases of the circulatory system (390-459), and diseases of the blood and blood-forming organs (280-289).
5. Diseases of the musculoskeletal system and connective tissue (710-739).
6. Diseases of the respiratory system (460-519).
7. Infectious and parasitic diseases (001-139), and endocrine, nutritional and metabolic diseases, and immunity disorders (240-279).
8. Complications of pregnancy, childbirth and the puerperium (630-679).

Other diagnoses: neoplasms (140-239), diseases of the digestive system (520-579), diseases of the genitourinary system (580-629), diseases of the skin and subcutaneous tissue (680-709), congenital anomalies (740-759), certain conditions originating in the perinatal period (760-779), symptoms, signs and ill-defined conditions (780-799), factors influencing health status and contact with health services (V01-V91), discharges without diagnosis (855-857), discharges with ICD-10-CM codes that cannot be mapped to a unique ICD-9-CM code (0.04% of the sample).

[42] Source: https://www.cms.gov/Medicare/Coding/ICD10.

Deaths are coded according to International Classification of Diseases 10th Edition (ICD-10) throughout all the years in the sample. The four groups of deaths by cause include (ICD-10 codes in parentheses):

1. External causes of morbidity and mortality (V01-Y98), and mental health and behavioural disorders (F00-F99).
2. Diseases of the nervous system and organs of sense. Diseases of the nervous system (G00-G99), diseases of the eye and adnexa (H00-H59), diseases of the ear and mastoid process (H60-H95).
3. Diseases of the circulatory system (I00-I99), diseases of the blood and blood-forming organs and certain disorders involving the immune mechanism (D50-D89), and diseases of the musculoskeletal system and connective tissue (M00-M99).
4. Diseases of the respiratory system (J00-J99), certain infectious and parasitic diseases (A00-B99), and endocrine, nutritional and metabolic diseases (E00-E90).

Others diagnoses: neoplasms (C00-D48), diseases of the digestive system (K00-K93), diseases of the skin and subcutaneous tissue (L00-L99), diseases of the genitourinary system (N00-N99), pregnancy, childbirth and the puerperium (O00-O99), certain conditions originating in the perinatal period (P00-P96), congenital malformations, deformations and chromosomal abnormalities (Q00-Q99), symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified (R00-R99).

A8. Further Heterogeneity Analysis

Age. A heterogeneity analysis by age can be pursued for hospitalisations and deaths as the samples are restricted so that all individuals are aged 15-27 from 1999 to 2018. Examining these results gives the opportunity to learn whether the effects of the Spanish universal preschool programme are different across ages given the fact that behaviour during adolescence could be different from that in young adulthood. For instance, individuals aged 15 might behave differently to those aged 18 or older who can legally drink, drive, go to nightclubs, among others. Figures A4 and A5 plot the estimates of hospitalisations and deaths by age, respectively. Figure A4 shows that hospitalisations rise for older individuals aged 21 or older, while Figure A5 confirms that the LOGSE did not impact deaths at any age.

Figure A4 emphasises that the rise in hospitalisations rates is driven by individuals in early adulthood, who might behave riskier than teenagers but who may also start making their first lifetime decisions (e.g. having children). To explore this, Figure A6 graphs the estimates for hospitalisations by hospital diagnosis and age, and Figure A7 for deaths by cause and age. Overall, Figures A6 and A7 show that the LOGSE did not affect hospitalisations for any diagnosis and age, and deaths for any cause and age. There are two exceptions. Figure A6 shows that there is a small rise in hospitalisations for diseases of the musculoskeletal system at ages 18, 23, and 24. Interestingly, the estimates of hospitalisations for pregnancy-related diagnoses by age show that the effect is significant for women aged 20-23 and thus drive by young adult pregnancy instead of teenage pregnancy.

Type of Hospital Admission. I also estimate effects of the LOGSE on hospitalisations by type of hospital admission (i.e. ordinary and emergency admission) in Figure A8. The estimates show that the positive coefficient on all hospitalisations is driven by individuals with an emergency admission. The same result is found when focusing on hospitalisations for pregnancy-related diagnoses in Figure A9, which it is expected since most childbirths are admitted to hospital as an emergency case.