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# Preschool Duration, Family Background and Long-Term Outcomes: Evidence From the Expansion of *École Maternelle* in France

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## ABSTRACT

This paper analyses the long-term effects of preschool education in France. We focus on the effect of preschool duration on educational attainment and labour market earnings in adulthood. Building on an old tradition of free and universal pre-primary education, France implemented in the late 1960s a large-scale expansion of preschooling facilities, leading to a massive increase in the supply of preschool. We rely on the independent variation across counties and cohorts in preschool supply, induced by this policy, to assess the effect of preschool using a Two-Sample Two-Stage Least Squares estimator. While the preschool expansion increased preschool duration (+0.69 years per additional school created), we find *on average* small and non-significant long-term effects on employment and earnings, with some positive impacts on educational attainment. We also investigate heterogeneity by parental socio-economic status and find that children from an advantaged family background benefit significantly more in terms of both education and earnings than their peers from less advantaged backgrounds. One additional year in preschool thus increases the achievement gap for most outcomes including the likelihood of graduating from high school (+8.3 percentage points) or college (+13.7 pp), employment (+13.3 pp), unemployment (−6.4 pp) and earnings (+5.4%).

**JEL Classification:** I26, J24, D63

## 1 | Introduction

The last decades have witnessed an increasing commitment worldwide to give children a better start through high-quality pre-primary education programmes. These early-childhood interventions are expected to yield high returns in adulthood by fostering human capital accumulation in the first years of life, when skills are highly malleable (e.g., [1, 2]). Preschool programmes are also expected to equalise life chances by compensating for low parental investment in children from disadvantaged family backgrounds. Empirical evidence on the long-term effect of preschool enrolment remains, however, relatively sparse. This paper contributes to the assessment of universal preschool education programmes and evaluates their long-term effects on educational attainment, labour market outcomes in adulthood and inequality therein.

Our analysis focuses on France and examines a large preschool expansion programme. Nowadays, preschool education in France takes the form of a full-fledged public schooling programme, with mandatory enrolment from age 3 and possible access from age 2. The French *École maternelle* model of preschool education was

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introduced in 1882 but was significantly expanded during the 1960s through a massive programme of investment in school facilities. We study the effect of this preschool expansion programme on preschool enrolment and duration and its consequences on individual long-term outcomes.

The empirical literature has so far produced mixed results on the effectiveness of preschool education.[1] On the one hand, evidence on targeted intervention programmes indicates positive short-run effects on child development[2] as well as positive long-term impacts.[3] Targeted programmes tend to yield higher benefits for children from disadvantaged family backgrounds, thereby reducing inequality of opportunity.

On the other hand, available evidence derived from universal pre-primary education programmes appears more uncertain. van Huizen and Plantenga [16] provide an overview of the evidence on universal programmes, together with a meta-analysis. The literature is inconclusive on the short-run effects on educational attainment and achievement, with positive effects of preschool attendance on children's cognitive development found in Spain, Uruguay, Argentina, Germany negative effects in Canada and mixed or non-significant effects in Denmark and France.[4] The limited evidence on long-run labour market effects is equally mixed. Baker, Gruber and Milligan [26] find negative average effects of child care expansion in Canada on earnings. Havnes and Mogstad [31] find positive effects of universal kindergarten expansion in Norway but only at the bottom of the parental income distribution. In the case of Norway, Andreoli, Havnes and Lefranc [28] find heterogeneous effects of preschool attendance on long-term earnings by family background and report larger effects for individuals from low family background.

Much of the literature has focused on the impact of increasing preschool *attendance*, modelled as a binary treatment, through the implementation of new preschool programmes. For instance, Havnes and Mogstad [31] examine the introduction of universal public kindergarten in a context of near-zero child care coverage. Evidence on the effect of preschool *duration*, defined as the number of years of preschool attendance, is much sparser and mostly focused on short-term schooling outcomes.[5] This stands at odds with policy concerns when several countries currently consider *extending* existing preschool programmes along the intensive margin by lowering enrolment age, thereby raising preschool duration. In the French context, we show that the expansion programme mostly increased duration along this intensive margin, in a context where the majority of children already attended at least 1 year of preschool. We contribute to the literature by providing novel estimates of the long-term labour market effects of preschool duration, which have received limited attention.

Existing pre-primary education programmes also vary substantially across countries and over time in terms of institutional arrangements. While this often challenges the comparability of country-specific estimates, it also questions the optimal structuring of pre-elementary education. van Huizen and Plantenga [16] provide a theoretical framework for understanding the consequences of preschool institutional arrangements. They distinguish among three main dimensions, each of which is akin to an input in an education production function *à la* Hanushek [32]: starting age and number of years of attendance, preschool intensity (full-time vs. part-time) and preschool quality (in terms of student supervision and teacher training). Other relevant dimensions are also worth stressing, including the universal or targeted nature of preschool education, the degree of standardisation and the share of private schooling. This paper focuses on one specific dimension, preschool duration, within the context of a universal, public, free, high-intensity and high-quality programme, as discussed below.

In this paper, we resort to an instrumental variables approach to assess the long-term effects of preschool duration on education, earnings and labour market participation and to assess heterogeneity in these effects by family background. Our main instrument is the preschool supply measured at the county-cohort level. Our identification of the effect of preschool duration requires that individual long-term potential outcomes and preschool duration are jointly mean independent from county-cohort changes in preschool supply, conditional on controls. This is achieved by exploiting quasi-random geographic variation in the local implementation path of the national preschool infrastructure expansion plan, implemented in France in the 1960s. Identification also requires that the preschool facilities expansion only indirectly affected later individual outcomes through their effect on preschool duration (*exclusion restriction*). In our estimation, the confounding influence of local contexts and subsequent schooling policy reforms is captured through a detailed control strategy. In particular, to address possible unobserved heterogeneity in outcomes across counties and cohorts, our estimates also control for county and cohort fixed effects and region-specific trends.

In the absence of longitudinal data providing information on both preschool attendance and labour market outcomes, we use a Two-Sample Two-Stage Least Squares procedure [33, 34]. Our estimation combines three main data sources. The French Labour Force Survey provides information on educational attainment, employment status and wages at age 35–45 for a large representative sample of individuals born 1964–1973, who were of preschool age during the phase-in period of the preschool expansion programme. The 1993 Education and Training survey provides additional information on preschool duration. These two surveys can be matched with national register data to assess the number of preschools in the geographical area where individuals were living at preschool age.

We first examine the reduced-form effect of the expansion programme on preschool enrolment, individual earnings and educational attainment. We find that an increase in preschool supply (by one additional preschool facility per 100 children) has small and non-significant effects on labour market outcomes, but contributes to reducing the chances of failing bare minimum educational requirements by 4.6 percentage points (pp) (95% Confidence Interval: [−8.9, −0.3]) while rising the chances of attaining an high school degree or above by 5.9 pp [0.8,11.0]. When analysing reduced-form effects by parental SES, we find that a change in preschool supply has a positive yet barely significant effect on educational outcomes for the low-SES group (it reduces the chances of achieving a primary education degree or less by −4.3 pp [−8.6,0] and rises the chances of attaining a high school diploma or more by 3.8 pp [−1.3,8.9]), whereas effects are even smaller and largely insignificant for labour market outcomes for this group. Conversely, an increase in preschool supply increases the outcome gap between high- and low-SES children in terms of probability of achieving a high education degree (by 4.1 pp [0.031,0.051]), employment (by 4.2 pp [2.8,5.6]), unemployment (by −1.9 pp [−2.7, −1.1]) and monthly earnings (by 1.7% [0.13%,3.27%]). The reduced form effects can be attributed to the impact of slackening preschool supply constraints on preschool duration, which increased by 0.69 years [0.069,1.327] for every unit increase in preschool supply. The Two-Sample Two-Stage estimator yields second stage effects that are comparable in sign and significance to the reduced form effects. Preschool duration increased by about 10% the SES gradient in human capital.

In the rest of the paper, we describe the institutional background and the preschool expansion policy in Section 2 and discuss our identification strategy. Section 3 presents the econometric model and the data. Results are reported in Section 4.

## 2 | Institutional Background and Identification Strategy

### 2.1 | The French Preschool Expansion

France has a long tradition of public, universal-access, preschool education. The French preschool programme, *École Maternelle*, was introduced in 1882 and conceived from the onset with the stated objective to prepare children for primary education by helping them develop basic cognitive and non-cognitive skills and learning attitudes. Preschool in France is highly inclusive, universal, publicly funded and free of tuition fees. It is, for the most part, publicly supplied and all schools must comply with strict national standards defined by the ministry of education, regulating the curriculum, school organisation and quality of services offered.[6] Historically, attendance to pre-primary education has been voluntary and access is now granted by law from the age of 3 and, in some cases, possible from age 2. Nowadays, almost all children aged 3 to 5 attend pre-primary education and follow a curriculum defined over 3 years.[7]

Universal attendance to the full 3-year preschool programme is the result of a steady expansion that took place during the 1960s and early 1970s. Around 1960, about 20% of a cohort did not attend preschool at all. Another 20% attended only 1 year of preschool education, which was usually delivered in a supplemental class organised within primary schools and offering a limited number of positions, typically to children aged 5. At the same date, only 25% of a cohort would have attended preschool for 3 years before entering primary education, at the mandatory age of 6. Despite widespread public agreement on the social role of preschool and its importance for children's development, enrolment was largely constrained by the lack of school infrastructures, in a context of high demographic pressure [36]. The need to expand pre-primary education by addressing the lack of school infrastructure was recognised by the policymakers as one of the key priorities of the *National Five-Year Plans for Economic and Social Development*.[8] This priority was also motivated by the objective to support the rise in female labour force participation. This political commitment was made concrete by the Fifth National Plan 1966–1970 that introduced an important budget dedicated to the construction of preschool facilities. This led to a large increase in the number of preschools from 5395 in 1960 to 13,639 in 1977, with an increase in the number of classes from 18,641 to 51,830. Supply growth flattened in the 1980s in most regions. Panel (a) of Figure 1 illustrates the steady dynamics of this expansion in preschool supply, measured by the total number of institutions offering preschool classes in France for a selection of French counties (on the left-hand-side axis), as well as the distribution of preschool supply across counties (on the right-hand-side axis).

The explicit objective of the National Plan was to reach an enrolment rate of 95% of 4-year-old children and 80% of children aged 3. Panel (b) of Figure 1 shows the increase in preschool enrolment occurring during the National Plan expansion period. Since children can enter preschool from the age of 3, the relevant school supply for a given cohort is the number of preschools 3 years after birth. For example, the relevant number of preschools for cohort 1964 is observed in year 1967. In the remainder, we focus on cohorts 1964 to 1973. Between these two cohorts, France experienced an impressive rise in preschool infrastructure of +60%. The boxplot in Panel (a) of Figure 1 further indicates that this increase occurred in all countries irrespective of their initial rank in the distribution of facilities.

This expansion was accompanied by an increase in both preschool participation and preschool duration. Between cohorts 1961 and 1973, the share of children not attending any preschool recessed from 12.6% to a low of 5.9%. At the other end of the spectrum, the share of children completing the full three-year curriculum increased from 27.5% to 44%. As a result, the main change occurring during the expansion period was not predominantly the enrolment of new children into preschool but the increase in the schooling duration of the 80% of children who, as soon as the early 1960s, already experienced some degree of preschool participation.

Whether this massive increase in enrolment triggered a fall in the quality of educational services provided is, of course, a legitimate concern. In particular, the preschool expansion required massive teacher hirings and might have encountered staff constraints. Aggregate historical evidence indicates that the French Ministry of Education was able to implement sizable hirings to accompany the development of preschool. Between 1950 and 1960, the number of preschool teachers doubled from 11,762 to 20,414 and grew by 80% between 1960 and 1970 ([36], Chapter 5).[9] Figure 1, Panel (c), presents national evidence on the evolution of the number of pupils and teachers in preschool and indicates rather parallel trends leading to a fairly stable pupil-teacher ratio over the period we investigate. The small decline in the number of teachers per school shown in Panel (d) might reflect the change in school size that accompanied the development of preschool across the French territory. Had a fall in preschool quality occurred, our estimates of the effect of the preschool expansion would likely be biased downward. However, available evidence suggests that the quality of preschool education was stable over this period.

Despite these evolutions, the ambitious objective of the National Plan was, however, not reached until the 1980s, due to the persisting under-supply of preschool infrastructures [36]. Public investments in preschool infrastructure indeed continued after the initial push triggered by the Fifth National Plan and remained on the agenda of subsequent plans.

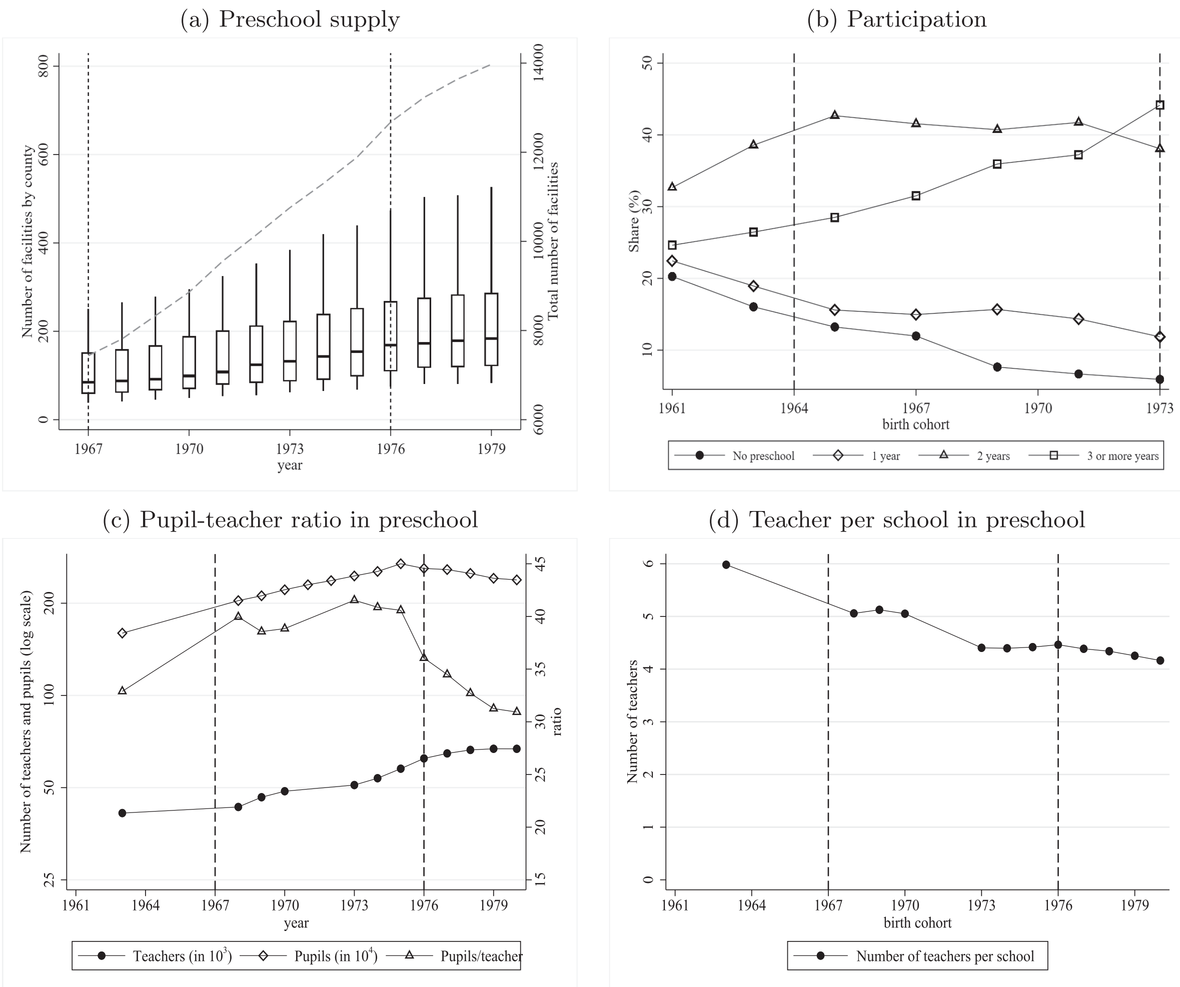


**FIGURE 1** | Preschool supply and enrolment in France. Authors' computation based on the National School Register (Panel a) and microdata from the Education and Training survey (FQP) for counties where preschool supply was observed in 1967 (Panel b) and INSEE [35] (Panels c and d). See Section 3 for details.

## 2.2 | Preschool Institutional Arrangements and Alternative Child Care Modes

Since the late 19th century, French preschool services have been nationally standardised, centrally administered and overseen by the Ministry of Education. The foundational decree of 1882 outlined the missions, organisational principles and curriculum of French preschools,[10] and established three core objectives: to provide day care, to offer developmentally appropriate education and to enhance primary school readiness [39]. Major revisions of this decree occurred in 1908, 1921 and 1977 without significant changes in these objectives or organisational principles.

Preschool in France is offered to children from 2 years of age, before they enter compulsory elementary school at the age of six, conditional on existing school capacity. Although admission from age 2 was possible, very few children were enrolled in preschool before the age of 3 during or before the preschool expansion analysed here.[11] Thus, while the increase in preschool duration, resulting from the preschool expansion, is predominantly a consequence of a younger enrolment age, this enrolment age is in the vast majority of cases at least three. This should be kept in mind when gauging the effect of increased duration reported below. Some studies have raised the concern that child care enrolment at age two or below might lead to negative psycho-emotional development effects [40]. In our case, we anticipate that such negative effects will be very limited.

Preschool in France is almost exclusively public and state-run: over the period studied here, the share of private preschools equals 2.8%. From the outset, the Ministry of Education established comprehensive regulations governing preschool organisation, in terms of class sizes, school capacity, the layout of school premises, and so forth. Except for the rare private preschools, the Ministry also manages the hiring and training of preschool teachers, who are national civil servants with qualifications comparable to those of primary school teachers, which could be considered high-quality training following van Huizen and Plantenga [16]. Although the pupil-teacher ratio reported in Figure 1 may seem high, by contemporary standards, teachers were typically seconded by child care assistants, mandated by law and hired by the municipalities.

Preschool education is organised through a standardised and integrated curriculum, featuring explicit objectives and detailed activities. The 1977 curriculum, of particular relevance to this research, places significant emphasis on child development, explicitly referencing advancements in psychology, pedagogy and neurophysiology. It defines six core domains of child development: affective development, motor skill development, visual-representational abilities, plastic expression, oral and written language acquisition and cognitive development.[12] Weekly schooling duration is set at 27 h, over 4 and a half days a week.[13] The school day is organised with 3 h class sessions in the morning and afternoon, with a midday break for lunch and the possibility of a nap in between. Municipalities often implement a school catering service and, over the period, increasingly offer after-school child care services.

To summarise, on the basis of these elements, the French *École maternelle* appears to offer standardised full-time universal high-quality preschool education emphasising child development and school readiness.

The ability of the French preschool to cater to the heterogeneous needs of children coming from diverse socio-economic backgrounds is an open issue. Considering the greater emphasis placed by the French preschool model on developmental and learning objectives, research in the sociology of education and in educational sciences has highlighted the greater "*social affinity*" [41] between the preschool learning environment and the intellectual and social dispositions of high-SES families. This "*affinity*" suggests that high-SES family inputs might be complementary with the preschool intake. Education science indeed reveals, in the current context, that preschool pedagogical practices are predicated on implicit and unformalised competencies, the acquisition of which varies across family environments, thereby triggering differential responses to preschool exposure according to SES.[14]

When evaluating the effect of preschool education, it is important to characterise the alternative child care arrangement that is being replaced by preschool, as estimates of the returns to preschool will be evaluated as a difference to these alternative environments. The evolution of child care arrangements after 1945 is discussed in Norvez [44]. Detailed information on individual child care modes is only available from 1982.[15] Despite data limitations, we can identify four significant patterns in child care modes over the period of interest.

First, the dominant form of care for young children, over the period, is in-home care by the mother. Table A8 in the appendix presents the distribution of child care modes for children below 3 and between 3 and 6 years old, based on the INSEE 1982 Family survey. It shows that, in 1982, 61.2% of all children below 3 are looked after by their mother, and this rises to 90.9% when the mother is not working. In 1982, the vast majority of children between 3 and 6 years old are enrolled in preschool but those not enrolled in school are, again, predominantly cared for by their mother (73.7%).

Second, child care modes are rather similar across family socio-economic backgrounds. The main difference pertains to children aged 3 to 6 years old not attending preschool and whose mother is working. Among those, children from high-SES backgrounds are about 10 pp more likely to be cared for by a non-family member and 2 pp more likely to attend a child care centre. In other cases, differences across socio-economic backgrounds appear very small in 1982.

Third, over the late 1960s and early 1970s, female labour force participation increases, but remained limited, especially among mothers. As discussed in the appendix, the average participation rate for women with children younger than 6 was below 45% in 1975, and 10 pp lower in 1965. High-SES households show a 10 pp higher female labour participation rate.

Fourth, the post-war decades are characterised by a large deficit of child care supply, which results from the lack of adequate policy response to the large rise in female labour force participation. As a result, in 1982, while one million children under age three needed child care when their mothers were at work, only half of them were looked after in child centres or by registered nannies, the remaining being cared for by relatives or unlicensed childminders [45].

### 2.3 | Capturing Geographic Variation in Preschool Expansion

While the school construction was funded nationally and largely independent of the local budget, the *decision* to develop new preschool facilities was decentralised at the municipality level. This entailed geographic variation in the path of preschool expansion.

The geographic expansion of preschool facilities triggered by the National Plan can be assessed using the administrative data from the National School Register maintained by the French ministry of education.[16] For each educational institution, both public and private, the register records the type of school, the service offered (from which we can isolate those institutes offering preschool classes), the opening and (possibly) closing dates and the school location. We aggregate information at the *département* level. *Départements* (denoted *counties* in the remainder) correspond to an intermediate geographical and administrative division of the French territory. Mainland France has 96 counties. A county defines the finer level of spatial aggregation at which representative samples of the reference population are collected in national surveys and correspond to the most detailed geographical information available in the surveys used in this article.

One significant data limitation is that the National School Register appears to have been gradually consolidated throughout the 1960s and is only sparsely filled at the beginning of this period. As a result, information on school facilities operating before 1965, at both preschool and elementary school levels, is missing for most counties. Information is gradually introduced until 1969 and, by 1970, as shown in the appendix, information on preschool and primary school facilities is available in almost all counties. In the present paper, the gradual availability of data at the county level raises an important trade-off between the geographical and the temporal coverage when defining the scope of our analysis. Starting our analysis after 1970 would guarantee full geographical coverage at the cost of foregoing valuable information about school openings occurring in the first years of implementation of the National Plan, for counties that provide information before this period. To balance the spatial and temporal dimensions of

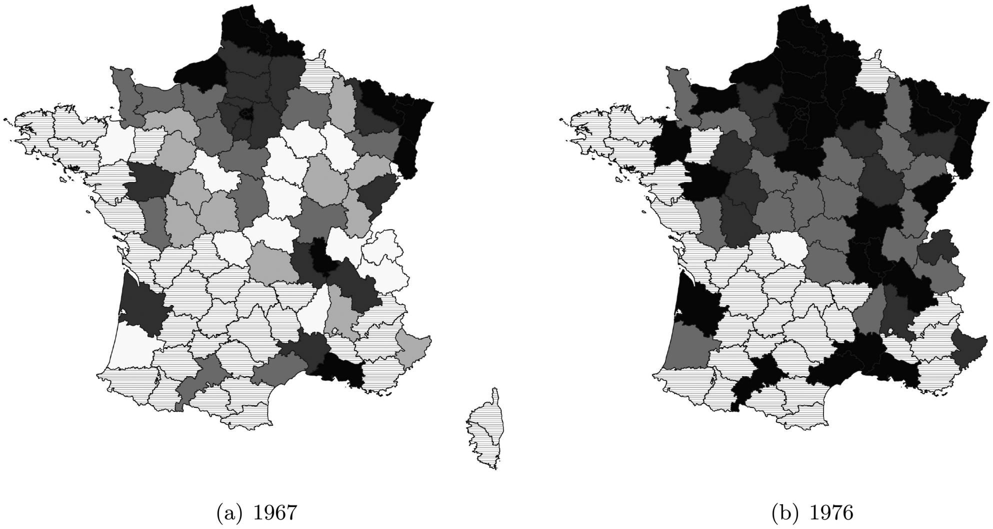


**FIGURE 2** | Supply of preschool by county. Authors' computation based on the National School Register. Categories correspond to quintiles of preschool supply in 1967.

variation in preschool openings, we use 1967 as the starting year for our analysis and we focus on counties where valid information on school supply is available at that date, as discussed in the appendix.

Figure 2 reports available information on the number of facilities in all 96 counties. The 32 counties where information in the School Registry was not reliable in 1967 are indicated in dashed grey and will be excluded from this study. In the remaining 64 counties, supply of preschool facilities has increased substantially over 10 years as a consequence of implementation of the National Plan, with heterogeneous patterns across counties. The median supply of preschool was about 100 facilities in 1967, the figure almost doubling in 1976. After the phase-in period of the National Plan, the growth of preschool supply was substantially reduced and supply plateaued in the 1980s for a majority of counties. Figure A4 in the appendix displays the spatial variability in preschool supply changes over this period.

In addition, out of the 64 counties for which reliable preschool supply data are available, we further restrict the using sample to 58 counties where preschool demand drivers are also observed. A number of checks confirm the robustness of our estimates to sample selection issues (see Tables A4 and A5 in the appendix).

We now explore the consequences of variability across locations and time in the preschool supply that is induced by the phasing-in of the National Plan to identify quasi-random shocks in preschool local supply.

## 2.4 | Identification Strategy

Our objective is to assess the long-run benefits of preschool attendance and the effect of the French preschool expansion programme. The main challenge in estimating these effects lies in the endogeneity of preschool enrolment with respect to factors that independently determine later life success, which can arise from a variety of channels. Demand for preschool services may vary with characteristics of the parents (e.g., human capital, income, labour supply decisions), of the family (e.g., family composition) and the rearing environment (e.g., urban density, quality of alternative child-care arrangements) that also affect children's educational attainment and labour market results. Along similar lines, the quality of local preschool supply is likely to be correlated with the quality of primary and secondary education available and ultimately to the long-run outcomes of children. As this paper is concerned with the long-run returns to preschool, our main empirical challenge is to separate the causal effect of preschool enrolment from endogenous selection into participation.

In this paper, we resort to an instrumental variable strategy and rely on the expansion of preschool facilities implemented by the National Plans to isolate quasi-random variation in preschool attendance and duration. The opening of new preschool facilities represents a slackening of the preschool supply constraints and produces a large and discontinuous growth in supply of preschool capacity, at the local level, which impacts preschool attendance along two margins. First, it increases opportunities to attend preschool for children otherwise excluded from the programme due to shortages in the availability of preschool. Beyond attendance, it also allows for earlier enrolment and longer duration in preschool. In any case, our identification rests on the assumption that the relaxing of capacity constraints induced by the larger supply of preschool infrastructure discontinuously increased the number of preschool positions and raised preschool attendance and duration. At the aggregate level, Figure 1 shows that the supply expansion induced by the National Plan is positively associated with the growing share of children attending preschool for 3 years or more and with a reduction of the share of individuals never attending. In fact, the National Plan mandated new openings to satisfy primarily the demand for preschool services for children aged 5; however, each new school should also invest in opening additional facilities benefiting younger cohorts, thereby increasing duration for affected cohorts.

We rely on the staggered implementation of the preschool expansion programme at the local level and exploit variation in preschool supply across counties and cohorts. Our continuous treatment variable is the extent of the preschool supply, measured by the number of preschool facilities for each county-cohort cell at the time of entering preschool, divided by the number of children of preschool age (i.e., aged 3–5). This ratio is scaled by 100 to represent the number of preschools per 100 eligible children. For data availability reasons discussed above, we start observing county preschool supply in 1967 which affects children born in 1964 (the theoretical entry age in preschool at the time of the expansion was 3 years). Our period of analysis includes all years until 1976, when the cohort born in 1973 was impacted. This covers the facility expansion triggered by the Fifth and subsequent National Plans.

Identification is achieved under the *conditional mean independence assumption*, which posits that individual heterogeneity in long-term potential outcomes and the preschool duration are jointly mean independent from county-cohort variation in preschool supply, conditional on controls. Under this assumption, any effect of the preschool expansion programme on long-term observed outcomes will be attributable to preschool participation exclusively. This requires that the preschool expansion does not directly affect individual potential outcomes, under different participation scenarios, but only influences individual chances to participate in the preschool programme.

Conditional mean independence may be violated under different circumstances. First, to the extent that our estimation exploits variation across cohorts and counties in preschool enrolment, any heterogeneity in long-term outcomes associated with both dimensions would confound the effect of preschool duration. For instance, preschool quality and child rearing environment may vary across counties and cohorts in a way that affects the returns to preschool attendance, thereby confounding the effects of duration. We address these points by controlling for county and cohort fixed effects, and geographic trends.

Second, the increase in preschool supply across county-cohort cells may partly reflect changes in the demand for preschool services. If the drivers of heterogeneity in preschool demand also impact long-run outcomes and the returns to preschool, our identification might be compromised. This will in particular be the case if variation across county-cohort cells cannot be captured by a combination of cohort fixed effects and geographic trends and fixed effects. To address this concern, we can control for a rich set of drivers of preschool demand measured at the county-year level. Our controls include measures of female labour force participation in child-rearing age, potential demand for preschool services as measured by the number of children of preschool age, local socio-demographic composition, health care quality, local economic resources and controls for the quantity and quality of primary education supply. In order to avoid simultaneity bias, all drivers are pre-determined (3 years lag) with respect to the date at which preschool supply is observed.

As preliminary evidence on the quasi-random variation in preschool supply expansion with respect to county-cohort heterogeneity, we report in Table 1, Column 1, the results of a balancing test where preschool supply is regressed on the various measures of county socio-demographic composition and economic development, observed for 580 county-cohort cells. The detailed definition of the variables and data sources is deferred to Section 3. These estimates largely support the conditional mean independence assumption, at least on the basis of measurable county-cohort heterogeneity. None of the potential drivers of preschool demand that we consider is significantly associated with changes in preschool supply across county-cohort cells, with the exception of female activity rate. That female labour supply is associated with the expansion of preschool supply should indeed come as no surprise, considering that it was one of the objectives of the National Plan's preschool expansion to support female labour participation. In the rest of the analysis, we explicitly control for female labour supply as well as other potential drivers of preschool demand. It is also important to note that the county population aged 3–5, an obvious measure of preschool potential demand, is uncorrelated with the supply of preschool facilities, thus lending support to our identification assumption.

Identification also requires the maintained *exclusion restriction* that preschool supply expansion has an effect on long-term outcomes only through its impact on preschool attendance. This requirement may be violated if preschool supply is correlated with heterogeneity in *other* dimensions of educational services offered to individuals, in quantity or quality. This could take the form of increases in the supply of primary, secondary and tertiary education, correlated to the rise in preschool supply and that would directly affect, later in life, the same county-cohort cells.[17] Alternatively, the quality of preschool or later schooling could be correlated with the preschool supply expansion at the county-cohort level. These confounding factors would, in this case, bias our estimates. Columns 2 to 4 in Table 1 assess the association between preschool supply and the supply of educational services in later educational stages. We find that increased preschool supply is not associated with a larger supply of educational services in later stages, after controlling for preschool supply drivers, fixed effects and trends. Column 1 also indicates that county-cohort preschool supply differences are not related to the supply of primary schools or the expansion of higher education, as measured by the lagged value of tertiary education graduation rate. Similarly, if we interpret the share of private schools as a proxy for differences in schooling quality, we do not find evidence of a correlation between school quality and the preschool expansion.[18] Put together, the results in Table 1 are thus supportive of the view that preschool expansion across county-cohorts cells was as good as randomly assigned and offers a plausible experiment for assessing the effect of preschool education.

Last, identification also requires the absence of spill-over effects between treated and non-treated individuals, which is often referred to as the *Stable Unit Treatment Value Assumption* (SUTVA). This assumption will typically be violated in the presence of peer effects or general equilibrium effects, which are usually expected in large-scale educational reforms. We discuss in Section 4 their relevance in our case and show that these concerns are largely rejected.

**TABLE 1** | Determinants of school expansion.

| Dependent variable | Preschool supply | Primary school supply | Junior high school supply | High school supply |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Preschool supply ($S_{cd}$) | | −0.028 | −0.038 | 0.029 |
| | | (0.042) | (0.084) | (0.047) |
| *Schooling related controls* | | | | |
| Primary school supply (log) | −0.056 | | | |
| | (0.081) | | | |
| Share of private primary schools | −0.048 | 0.110 | 0.197 | −0.092** |
| | (0.144) | (0.068) | (0.127) | (0.041) |
| Share with tertiary degree | −0.038 | 0.008 | −0.026 | −0.015 |
| | (0.082) | (0.058) | (0.043) | (0.039) |
| Population age 3–5 (log) | 0.488 | 0.306*** | 0.182 | 0.147 |
| | (0.383) | (0.093) | (0.188) | (0.128) |
| *Labour market-related controls* | | | | |
| Female participation rate (age 25–34, in %) | 0.023*** | 0.006* | 0.003 | −0.000 |
| | (0.007) | (0.004) | (0.005) | (0.003) |
| Share of top-tier occupations among employed women | −0.017 | 0.009 | −0.010 | −0.011 |
| | (0.018) | (0.009) | (0.012) | (0.007) |
| *Regional development-related controls* | | | | |
| Share living in rural area | 0.013 | −0.001 | −0.002 | −0.008* |
| | (0.010) | (0.004) | (0.006) | (0.004) |
| Share of $SES^H$ | 0.346 | 0.006 | 0.113 | 0.016 |
| | (0.229) | (0.090) | (0.113) | (0.099) |
| Mortality rate at age 40 | −9.574 | 5.074 | −2.964 | 3.670 |
| | (5.735) | (3.841) | (4.950) | (4.114) |
| Share of national taxable income | −0.002 | 0.001** | 0.000 | −0.000 |
| | (0.001) | (0.000) | (0.001) | (0.000) |
| Housing quality index | 0.000 | −0.000 | −0.002 | −0.004 |
| | (0.006) | (0.004) | (0.004) | (0.005) |
| N | 580 | 580 | 580 | 580 |
| Counties | 58 | 58 | 58 | 58 |
| Cohorts | 10 | 10 | 10 | 10 |
| R-squared | 0.986 | 0.994 | 0.997 | 0.999 |
| *$\chi^2$ joint significance tests (p-value in parentheses)* | | | | |
| Schooling controls | 0.84 | | | |
| | (0.5030) | | | |
| Regional development controls | 1.78 | | | |
| | (0.1315) | | | |
| All controls | 3.75 | | | |
| | (0.0005) | | | |

*Note:* The school supply is measured in the county of birth, at the theoretical age of entry in the relevant educational level, that is, age 3 for preschool, age 6 for primary, age 11 for junior high school and age 15 for high school. All models feature county and cohort FE and cohort trends by region of birth. All drivers are measured at birth. Robust standard errors clustered at the county of birth. Significance levels: * = 10, ** = 5% and *** = 1%.

*Source:* National School Registry, Censuses, the French Regional Dataset and the Labour Force survey 1962–1975.

We now discuss the statistical and empirical implementation of this identification strategy.

## 3 | Estimation and Data

### 3.1 | Estimation

#### 3.1.1 | Reduced Form

Our first objective is to estimate the effect of the preschool expansion on various individual long-term outcomes (e.g., educational attainment, wage, etc.). Let $y_{icdt}$ denote the outcome of individual $i$, born in cohort $c \in \{1964, \ldots, 1973\}$ and county $d$, observed in a particular year $t$. Moreover, let $S_{cd}$ denote the level of preschool supply for cohort $c$ in county $d$. Our main specification for recovering the reduced form effect of the preschool expansion of the National Plan is:

$$y_{icdt} = \gamma_0 + \gamma_1 S_{cd} + \delta_1 X_i^1 + \delta_2 X_{cd}^2 + \theta_c + \theta_d + \theta_t + \theta_{tc} + \varepsilon_{icdt} \quad (1)$$

In this model, the main parameter of interest, $\gamma_1$, captures the average effect on individual outcomes of a unit increase in the supply of preschool, $S_{cd}$, which measures the number of preschool facilities in a given county-cohort per 100 eligible children.

Model (1) controls for individual-specific characteristics, $X_i^1$, including age, age squared, gender and parental background. We allow for fixed-effect unobserved heterogeneity across cohorts and county of birth by including dummy variables for individual cohorts, $\theta_c$ and county of birth, $\theta_d$. We also include region-specific cohort trends as well as controls for preschool demand that vary across counties and cohorts in the vector $X_{cd}^2$. Since model (1) is estimated on a dataset pooling multiple cross-sectional waves of a survey, collected in years $t$, we augment the model with survey-year fixed effects ($\theta_t$) and interactions with the cohort of birth $\theta_{ct}$ (age effects), which are capable of capturing the life-cycle dynamics which may vary across cohorts and features of the regional labour markets.

Throughout the paper, we explicitly allow for heterogeneity of the effects of the preschool expansion policy across populations with different parental backgrounds. We thus consider an extended version of the above model:

$$\begin{aligned} y_{icdt} = {} & \gamma_0' + \gamma_1' S_{cd} + \gamma_2' S_{cd} \times \mathrm{SES}_i^H + \delta_1' X_i^1 + \delta_2' X_{cd}^2 + \theta_c' + \theta_d' \\ & + \theta_t' + \theta_{tc}' + \varepsilon_{icdt}' \end{aligned} \quad (1')$$

where $\mathrm{SES}_i^H$ is a dummy variable equal to 1 if individual $i$ is of high socio-economic family background and 0 otherwise.[19]

The reduced form model (1′) yields two effects of interest. The first parameter $\gamma_1'$ captures the effect of an increase in the supply of preschool facilities at the county level on the long-term outcomes for individuals with low socio-economic status (SES) fathers. This effect is interesting per se since the National Plan preschool expansion was also intended to expand educational opportunities of worse-off students. The second parameter is $\gamma_2'$, which is a measure of the gain or penalty from preschool expansion experienced by children from better-off parental background. This effect is positive in the presence of complementarities between preschool and parental quality, as proxied by parental SES. $\gamma_2'$ is therefore a measure of the impact of preschool expansion on unfair inequality between advantaged and disadvantaged children: if $\gamma_2' > 0$ then increasing preschool supply will, on average, raise education and labour market inequality attributable to parental background. Lastly, one may also examine the total effect of the preschool expansion on the long-term outcomes of children with better parental circumstances, measured by $\gamma_1' + \gamma_2'$.

#### 3.1.2 | Instrumental Variables

While the reduced-form estimates capture the average benefit of the increased participation in preschool triggered by the facility expansion, they cannot distinguish between different sources of participation increase. As a matter of fact, increased participation can arise from two distinct channels: along the extensive margin, the facilities expansion allowed the enrolment of individuals who would have otherwise not attended preschool; along the intensive margin, it also potentially increased the duration of preschool attendance among individuals who would have enrolled anyway. It is important to disentangle these different channels and to separately assess the benefit associated with increasing preschool duration.

Regressing observed outcomes on individual preschool duration could lead to biased estimates due to the endogeneity of duration decisions with respect to unobserved family and individual characteristics. We resort to an instrumental variables (IV) approach, within a two-sample setting, and exploit the effect of the National Plan on preschool duration. In the presence of capacity constraints, the preschool facilities expansion increases opportunities for preschool duration. The quasi-random changes in preschool supply across counties and cohorts guarantee that preschool supply rises independently from the unobserved heterogeneity driving parents' demand for preschool.

Let $Dur_{icd} \in \{0,1,2,3\}$ denote preschool duration, measured in years, for individual $i$ born in cohort $c \in \{1964, \ldots, 1973\}$ in county $d$. Let $Dur_{icd}^H = Dur_{icd} \times \mathrm{SES}_i^H$ denote the preschool duration, interacted with an indicator for high-SES family background. Using the above notations for control variables, our equation of interest writes as:

$$y_{icdt} = \beta_0 + \beta_1 Dur_{icd} + \beta_2 Dur_{icd}^H + \zeta_1 X_i^1 + \zeta_2 X_{cd}^2 \phi_c + \phi_d + \phi_t + \phi_{tc} + \nu_{icdt} \quad (2)$$

The coefficients of interest in the above model are $\beta_1$ and $\beta_2$. They represent the effect of a 1 year change in the duration of preschool on the outcome $y$. More precisely, $\beta_1$, measures the effect of preschool duration on the low-SES group, while $\beta_2$ measures the differential effect on the high-SES group, that is, the parental background gradient on returns to preschool duration. If the gradient is positive and significant, then children with high-SES parental background are expected to gain more out of one additional year of preschool compared to low-SES children. Note that the precise interpretation of each coefficient varies with the measure of outcome we consider. For binary outcomes, the effects are interpreted as changes in probability. When outcomes are log earnings, the coefficients have the usual interpretation of earnings returns to schooling.

Model (2) is estimated by Two-Sample Two-Stage Least Squares, using the preschool expansion programme as instrument. In the first stage, duration variables, $Dur_{icd}$ and $Dur^{H}_{icd}$ are regressed on two instruments, the level of preschool supply and its interaction with parental background, together with other control variables and fixed effects:

$$Dur_{icd} = \alpha_0 + \alpha_1 S_{cd} + \alpha_2 S_{cd} \times \mathrm{SES}^{H}_{i} + \kappa_1 X^{1}_{i} + \kappa_2 X^{2}_{cd} + \psi_c + \psi_d + \eta_{icd} \tag{3a}$$

$$Dur^{H}_{icd} = \alpha'_0 + \alpha'_1 S_{cd} + \alpha'_2 S_{cd} \times \mathrm{SES}^{H}_{i} + \kappa'_1 X^{1}_{i} + \kappa'_2 X^{2}_{cd} + \psi'_c + \psi'_d + \eta'_{icd} \tag{3b}$$

Estimates of Model (3) allow to define predicted duration variables, $\widehat{Dur}_{icd}$ and $\widehat{Dur}^{H}_{icd}$, conditional on the two instruments $S_{cd}$ and $S_{cd} \times \mathrm{SES}^{H}$. These instrumented values can in turn be used in the second step model:

$$y_{icdt} = \beta_0 + \beta_1 \widehat{Dur}_{icd} + \beta_2 \widehat{Dur}^{H}_{icd} + \zeta_1 X^{1}_{i} + \zeta_2 X^{2}_{cd} \phi_c + \phi_d + \phi_t + \phi_{tc} + e_{icdt} \tag{4}$$

Equations (3a, 3b) are thus instrumental for the two endogenous variables of Model (2), $Dur_{icd}$ and $Dur^{H}_{icd}$. Furthermore, Equation (3a) also allows to assess and compare the effect of a change in preschool supply on preschool duration in each of the two SES groups, since it regresses, for all individuals, the preschool duration on the treatment variable and its interaction with parental SES. Hence $\alpha_1$ measures the impact of preschool supply on duration for the reference group of low-SES children, whereas $\alpha_2$ measures the differential effect of the treatment for high-SES parental background. Note that the same interpretation does not apply to $\alpha'_1$ and $\alpha'_2$ as the dependent variable in Equation (3b) is the product of the preschool duration by a dummy variable for high-SES and is thus zero for low-SES children.

Several conditions are required for IV estimates of Equation (4) to identify the causal effect of preschool duration on individual outcomes. Beyond the conditional mean independence assumption and the exclusion restrictions already discussed, two additional conditions need to be satisfied. First, that the instrument (preschool supply expansions) is relevant for explaining preschool duration of those that attend the programme (*first stage relevance*). Second, that the change in supply affects all individuals in the same direction. This *monotonicity* assumption is satisfied if children with heterogeneous parental background characteristics are equally exposed to the same within-county-cohort preschool supply shock, or if these characteristics do not determine different patterns of sorting into preschool duration when exposed to a slackening of preschool supply.[20]

#### 3.1.3 | Two-Sample Two-Stage Least Squares

The instrumental variable procedure outlined above does not require estimating the first and second stages on the same sample. As a matter of fact, in many situations, it is difficult to gather information on outcomes, explanatory variables and instruments in a single data set. In this context, it is possible to estimate the first and second steps of the above procedure on different data sources. Angrist and Krueger [33] have shown that this procedure, known as Two-Sample Two-Stage Least Squares (TSTSLS), leads to unbiased estimates in the presence of endogeneity arising from unobserved heterogeneity. TSLSLS have been used in a variety of contexts where information on treatment and outcomes are available on separate representative samples of the same reference population (e.g., [33, 34, 47, 48]). Inoue and Solon [49] also argue for the efficiency of the two-step procedure *vis-à-vis* alternative estimators when data are split in different samples.

In our case, we cannot rely on a single dataset reporting information on both preschool duration and on long-term outcomes, but can exploit two distinct surveys, the details of which are presented in the next section. The first survey is the French Education and Training survey. The 1993 wave of this survey provides information on preschool duration for a representative sample of individuals born in cohorts affected by the preschool expansion (1964–1973). This survey also reports information on county and year of birth and socio-demographic characteristics and allows to estimate the first stage regressions of Model (3). For the estimation of the second stage Equation (4), we resort to the French Labour Force Survey, that yields measures of our long-term outcomes of interest $y$. We use a set of linking variables that are common to both databases (cohort, county of birth, demographics) to predict preschool duration in the Labour Force Survey using estimates of (3). The procedure corresponds to a 2SLS estimator performed on separate samples.

The validity of the estimator rests on the fact that both samples and data sets are representative of the same population. Differently from matching estimators, TSTSLS is not concerned with the distribution of unobserved heterogeneity across samples, since first-stage coefficients are identified from quasi-random variation in preschool supply that is orthogonal to it.

Lastly, a note of caution is also needed on inference issues. Second-stage standard errors for the TSTSLS estimator are biased downward, insofar as additional variability in the main regressors introduced from the estimation of the auxiliary regressions is ignored. Murphy and Topel [50] provide an analytical adjustment of the second-stage standard error for the case in which auxiliary regressions are estimated on a different sample than the second-stage sample. The adjusted standard errors estimator accommodates for the possibility that residuals are correlated across auxiliary regressions.[21]

#### 3.1.4 | Addressing Selection in Earnings Equations

There are no a priori reasons to expect the returns to preschool to differ across gender, unless there is strong evidence of a gender gradient in participation. The baseline specification of the reduced form Model (1) as well as the instrumental variable regression should thus be estimated on the whole sample and not include interactions between the main treatment and a female indicator.[22] While this applies for most outcomes, which are observed for all individuals, earnings raise specific issues as they are only observed for the selected group of women who participate in the labour market. In our baseline estimates pooling the samples of males and females, we ignore the issue of selection. When we use earnings as an outcome and we focus on the sample of women, we amend the above estimation procedure following Heckman [51] by including, as additional control, the inverse

Mills ratio estimated from an auxiliary labour force participation model. This correction is applied to both the reduced form models (1 and 1′) and the instrumental variables Model (4).[23] The auxiliary regression is estimated in the second-stage sample of women for which we observe educational outcomes (excluding unemployed and self-employed for whom earnings are undetermined). The selection equation predicts the probability that a woman in the main sample works (an indicator for whether the respondent wage is observed or not), as a function of covariates and drivers of the employment decision that are conditionally independent of unobservable heterogeneity determining potential earnings (always not observed) and that can thus be excluded from second-stage regressions.[24]

## 3.2 | Data

### 3.2.1 | Datasets and Main Variables

Our estimation strategy requires combining four main datasets, as summarised in Table 2.

Data on individual long-term outcomes are taken from the *Labour Force Survey* (LFS, *Enquête Emploi*), collected annually by the French Statistics Institute (INSEE), which provides a 1/400 representative sample of the French population. We pool yearly survey waves over the period 2003 to 2018.[25] We consider both educational and labour market outcomes. Educational attainment is assessed from the highest degree obtained and we consider three binary outcomes: primary school degree or less, high-school degree or more, and higher-education degree. For characterising labour market outcomes, we consider three nested binary variables for individual labour market status: the first variable indicates, for all individuals, whether they are currently employed (value equal to 1) or either unemployed or out of the labour force (0); the second variable focuses on individuals in the labour force and indicates whether they are currently unemployed (1) or employed (0); the third variable focuses on individuals in employment and distinguishes between individuals working part-time (1) or full-time (0). We also consider two measures of earnings for individuals in salaried employment, namely the monthly labour earnings and the hourly labour earnings, obtained as the ratio between monthly salary and usual hours of work.[26] All earnings variables are expressed in 2015 euros.

In addition to these outcome variables, the LFS also provides information on individual cohort and county of birth, and family background. We assess parental socio-economic status (SES) based on the one-digit occupation of the father. In both the LFS and the FQP (see below) data, father's SES is based on the occupation of the father at the time when the respondent left the schooling system. Individuals are classified as high-SES if their father was a higher- or lower-grade professional or artisan and low-SES for children of farmers, manual or non-manual workers.[27]

Information on preschool supply comes from the French Ministry of Education *National School Register* (NSR), which collects information on all educational institutions operating in France. As previously discussed, the NSR provides for each municipality and each year the number or schools offering pre-primary classes. We aggregate this information at the county level for each year. Our measure of the preschool supply $S_{cd}$ is the number of schools per 100 children observed in county $d$ when children from cohort $c$ could first enter preschool (i.e., in year $c + 3$). This preschool supply variable can be matched by county and cohort to the LFS sample (and to the FQP sample as well—see below). The NSR data also allows to measure the schooling supply at other educational levels, as used for instance in Table 1. For each cohort, we measure the schooling supply by the number of schools of a given educational level in a county, in the theoretical year of entry into this educational level. For instance, cohort $c$ in county $d$ is assigned primary school supply in year $c + 6$, and so forth.

We also rely on *Census data* and older waves of the LFS to construct measures of aggregate drivers of preschool demand at the county-cohort level and county-cohort heterogeneity. First, we use municipality-level summary tables from the 1962, 1968 and 1975 French censuses to measure, at the county level, the share of the population living in a rural municipality (less than 2000 residents), a proxy for the extent of urbanisation of the birth place of the respondent upon entry in preschool. We expect demand for preschool to be larger in more urbanised counties. Second, we use the same tables to assess the female employment by occupational group. These groups may capture compositional differences in female employment which may be correlated with preschool attendance. Third, we consider the share of employed women in the group of women aged 25–34, a proxy for maternal activity rate, where the age group broadly represents the population of women in childbearing age in the 1960s. We aggregate municipality information at the county level. Due to missing observations

**TABLE 2** | Datasets and variables.

| Variables | LFS | FQP | Census, FRD and LFS 1962–1975 | NSR |
|---|---|---|---|---|
| Preschool duration (0–3) years | | ✓ | | |
| County of birth | ✓ | ✓ | ✓ | ✓ |
| Cohort of birth | ✓ | ✓ | ✓ | ✓ |
| Father SES and demographics | ✓ | ✓ | | |
| Labour market outcomes | ✓ | | | |
| Educational outcomes | ✓ | | | |
| Preschool supply (facilities by county) | | | | ✓ |
| Preschool demand drivers at birth | | | ✓ | |

Abbreviations: FQP, Education and Training survey; FRD, French Regional Database; LFS, Labour Force Survey; NRS, National School Register.

in the 1962 and 1968 censuses, aggregate measures of preschool demand drivers cannot be computed for six counties. We check for the sensitivity of our results to these additional controls and to the omission of these counties and find no effect.[28] Lastly, we include information on housing quality (captured by the share of housing units constructed before the 1940s) as a proxy for the availability of quality space for care at home.

We rely on the *French Regional Database* (FRD), assembled by Bonnet and d'Albis, to gather information on health and income at the county-year level.[29] Health disparities are captured by life expectancy at age 40 in each county and year. Income disparities are captured by the share of national taxable income observed in a certain county and year. All measures are pre-determined with respect to the timing of preschool supply expansion.

To assess preschool duration and the effect of the preschool expansion on enrolment, we use the 1993 wave of the Education and Training survey (*Formation et Qualification*, henceforth FQP). This survey is collected by INSEE and is representative of the working-age population (19 or above in 1992) in mainland France. The data contain retrospective information on preschool duration, as well as information on cohort and county of birth and parental socio-economic background.[30] As in the LFS, parental background refers to parental occupation when the respondent left the schooling system. Preschool duration is measured as an integer number of years between 0 and 3, with 0 indicating less than one completed preschool year, 1 indicating at least one and less than two completed school years, and so forth.[31]

All data sets can be matched by county and cohort of birth. In addition to county and survey year fixed effects, we also allow for region-specific time trends in all specifications. We use the 1982 administrative region partitioning where regions gather about five counties. We introduce region rather than county trends in the interest of data parsimony.

#### 3.2.2 | Sample Selection Rules and Descriptive Statistics

Since the FQP data only surveys individuals aged 19 and above in 1992, our analysis is restricted to individuals born in cohort 1973 or earlier. Furthermore, as discussed in the previous section and in the appendix, information on preschool supply is missing in numerous counties before 1967. We thus further restrict our analysis to individuals born in 1964 and later and who were born in a county with valid information on preschool supply, as presented in Figure 2. The resulting FQP sample consists of 2432 individuals (1245 females), aged 24.47 years on average when interviewed in 1992.

For similar reasons, the LFS sample is restricted to French respondents born in years 1964–1973, in counties where information on preschool participation is available. For each birth cohort, in order to obtain information on mid-career labour market outcomes, we further restrict our sample to LFS waves when individuals were aged between 35 and 45 years old. For instance, for cohort 1964, only LFS waves 2003–2009 are retained (average age 42.1 years), whereas for cohort 1973 only waves 2008–2018 are retained (average age 40 years). This selection is illustrated in Table A6 in the appendix. The resulting sample consists of 65,125 individuals (33,329 female) aged 40.6 years on average.

Descriptive statistics for the samples used in our analysis are given in Table 3. Panel A describes the distribution of outcomes in the LFS data, for the whole sample and by gender.

Despite the fact that all the cohorts used in our sample were impacted by the Berthoin educational reform that largely expanded mandatory education, a sizable fraction (14.1%) of the sample exhibits a low level of education, with a primary education degree or lower.[32] This suggests impediments to the accumulation of human capital in early life. At the other end of the distribution of human capital, 34.9% of the sample holds a university qualification and about half of the sample holds a high school degree (*Baccalauréat*). Overall, the female distribution of education shows a positive education gap. Turning to labour market outcomes, the participation rate of prime-age males is high (95.8%) and unemployment is moderate (5.9%). In comparison, the female sample displays a lower participation rate (86.5%), a higher unemployment rate (7.6%) and a sizeable share of part-time work among employed workers (31.5%). The gross gender wage gap is also sizeable: 0.326 log points in monthly wage and 0.180 log points in hourly wage rate.

Panel B of Table 3 provides summary statistics for other variables. Preschool duration, available only in the FQP sample, equals 2.02 years, on average, whereas 35.4% attended preschool for the entire three-year cycle and 90.4% report having attended preschool for at least 1 year. Within our sample, the average preschool supply at the county level is 0.826; that is, on average, almost one school per 100 children of preschool age living in the county. Our partition of the population according to father's occupation leads to a fairly equal share of individuals in the high (about 45% of the sample) and low SES groups.

Preschool demand drivers include the share of the population living in rural municipalities (about 28%), maternal activity rate (about 48%), and the proportion of mothers with high-SES (about 27%). Drivers are specific to the cohort and county of birth.

Last, we examine whether there exist significant differences in the distribution of main variables between the LFS and FQP data, which is a condition for the validity of our TSTSLS procedure. Test results are presented in Tables 3 and A9. We find a significant difference in maternal employment estimates across samples (Table 3, Panel B, balancing tests). Violations may be ascribed to chance or to systematic differences in the composition of county-cohort cells across samples. A battery of tests for independence, reported in Table A9, largely fail to reject that differences in composition between LFS and FQP samples across regions of birth or region-cohort of birth cells are due to randomness, thereby ruling out compositional effects.[33]

## 4 | Results

### 4.1 | Reduced-Form Estimates

We first examine the reduced-form effects of the preschool expansion programme on three long-term outcomes: education, labour

**TABLE 3** | Descriptive statistics.

**Panel A: Main outcomes, LFS sample**

| | **All** | | | **Male** | | | **Female** | | |
|---|---|---|---|---|---|---|---|---|---|
| | **Mean** | **SD** | **Count** | **Mean** | **SD** | **Count** | **Mean** | **SD** | **Count** |
| Primary education or less (1 = yes) | 0.141 | 0.348 | 65,118 | 0.151 | 0.358 | 31,789 | 0.131 | 0.337 | 33,329 |
| High school degree (Bac) or more (1 = yes) | 0.515 | 0.500 | 65,118 | 0.475 | 0.499 | 31,789 | 0.554 | 0.497 | 33,329 |
| University degree (Bac + 2) or more | 0.349 | 0.477 | 65,118 | 0.326 | 0.469 | 31,789 | 0.371 | 0.483 | 33,329 |
| Labour force participation (1 = yes) | 0.911 | 0.285 | 65,125 | 0.958 | 0.200 | 31,793 | 0.865 | 0.342 | 33,332 |
| Unemployed (1 = yes) | 0.067 | 0.251 | 59,307 | 0.059 | 0.236 | 30,468 | 0.076 | 0.265 | 28,839 |
| Part-time job (1 = yes) | 0.171 | 0.376 | 54,904 | 0.037 | 0.189 | 28,468 | 0.315 | 0.464 | 26,436 |
| Self-employed (1 = yes) | 0.113 | 0.317 | 55,310 | 0.144 | 0.351 | 28,660 | 0.080 | 0.271 | 26,650 |
| Monthly wage (log) | 7.448 | 0.487 | 46,421 | 7.612 | 0.410 | 23,236 | 7.284 | 0.502 | 23,185 |
| Hourly wage (log) | 2.431 | 0.403 | 46,107 | 2.522 | 0.390 | 23,096 | 2.341 | 0.396 | 23,011 |

**Panel B: Other variables, LFS and FQP samples**

| | **LFS sample** | | | **FQP sample** | | | **Balancing test** | |
|---|---|---|---|---|---|---|---|---|
| | **Mean** | **SD** | **Count** | **Mean** | **SD** | **Count** | **Coef.** | **SE** |
| Female (1 = yes) | 0.512 | 0.500 | 65,125 | 0.498 | 0.500 | 2432 | 0.014 | 0.010 |
| Preschool duration (in integer years) | | | 0 | 2.025 | 0.935 | 2405 | | |
| Preschool attendance (1 if preschool duration > 0) | | | 0 | 0.904 | 0.294 | 2405 | | |
| Preschool completion (1 if preschool duration ≥ 3) | | | 0 | 0.354 | 0.478 | 2405 | | |
| $\text{SES}^H$ (1 if household is high-SES) | 0.433 | 0.495 | 65,125 | 0.446 | 0.497 | 2432 | −0.013 | 0.010 |
| Preschool supply ($S_{cd}$) | 0.826 | 0.539 | 65,125 | 0.818 | 0.544 | 2432 | 0.008 | 0.011 |
| Share living in rural area (county level, in %) | 28.094 | 18.487 | 65,125 | 28.083 | 18.586 | 2432 | 0.012 | 0.384 |
| Female participation rate (age 25–34, in %) | 48.949 | 9.683 | 65,125 | 48.381 | 9.857 | 2432 | 0.568*** | 0.203 |
| Share of top-tier occupations among employed women (%) | 27.467 | 4.233 | 65,125 | 27.421 | 4.194 | 2432 | 0.046 | 0.087 |
| Mortality rate at age 40* | 0.004 | 0.001 | 65,125 | 0.004 | 0.001 | 2432 | −0.000* | 0.000 |
| Share of national taxable income (county level, in %)* | 5.797 | 10.916 | 65,125 | 5.645 | 10.520 | 2432 | 0.152 | 0.218 |
| Housing quality index | 47.026 | 11.082 | 65,125 | 47.588 | 11.368 | 2432 | −0.562** | 0.235 |
| Share of private primary school (county level, in %) | 0.109 | 0.118 | 65,125 | 0.103 | 0.122 | 2432 | 0.006** | 0.003 |
| Share with tertiary degree (%) | 25.906 | 7.400 | 65,125 | 25.445 | 7.320 | 2432 | 0.460*** | 0.151 |

*Note:* Top-tier occupations refer to higher- and lower-grade professionals and artisans; the balancing test in panel B reports the difference in mean value across the LFS and FQP samples and standard error thereof. Variables marked with are from Bonnet [55].
*Source:* LFS and FQP data sets; Sample: French respondents born in mainland France between 1964 and 1973.

market status and wages. Table 4, panel A, provides estimates of Equation (1) for the average effect of county preschool supply on our baseline sample, including both males and females and controlling for gender. The expansion of preschool facilities yields sizeable and significant effects on human capital accumulation (Columns 6–8): one additional preschool facility per 100 children decreases by 4.6 pp the probability of holding at most a primary education degree and increases by 5.0 pp (respectively 5.9 pp) the probability of holding a high school degree or more (resp. a university degree). However, on average, none of these educational benefits translates into significant gains in labour market outcomes. More precisely, while coefficients indicate improvements in both labour market participation (Columns 1–3) and earnings (Columns 4–5), in response to the increase in preschool supply, none of the coefficients appears statistically significant.[34]

Panel B of Table 4 estimates heterogeneous effects of the preschool expansion by parental background (Equation 1′) on our baseline sample. The reference group corresponds to low-SES individuals. For this group, we find small and non-significant effects, reported in the first line of Panel B, for all outcomes. However, the coefficient on the interaction between preschool supply and high-SES background reveals, for all outcomes, a large and significant socioeconomic gradient in the effect of the preschool expansion. In terms of education, high-SES children enjoy a 2.4 pp increase in the probability of achieving at least a high-school qualification and a 4.1 pp increase in the probability of achieving a university diploma, compared to low-SES children. For the high-SES group, the full effect of the reform on educational attainment is positive and statistically significant. The last line of Panel B provides the value of the net effect of the reform on the high-SES group, which is the sum of the main and interacted effects. The probability of completing a secondary

**TABLE 4** | Reduced form estimates.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Panel A — homogeneous effect | | | | |
| Preschool supply ($S_{cd}$) | 0.012 | −0.022 | 0.026 | 0.015 | 0.010 | −0.046** | 0.050* | 0.059** |
| | (0.041) | (0.025) | (0.020) | (0.023) | (0.027) | (0.022) | (0.028) | (0.026) |
| $SES^H$ | 0.038*** | −0.016*** | −0.008* | 0.186*** | 0.196*** | −0.121*** | 0.298*** | 0.281*** |
| | (0.007) | (0.004) | (0.004) | (0.005) | (0.006) | (0.005) | (0.007) | (0.007) |
| N | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 |
| | | | | Panel B — heterogeneous effect by family background | | | | |
| Preschool supply ($S_{cd}$) | −0.010 | −0.012 | 0.031 | 0.011 | 0.001 | −0.043* | 0.037 | 0.038 |
| | (0.041) | (0.025) | (0.020) | (0.023) | (0.028) | (0.022) | (0.028) | (0.026) |
| $S_{cd} \times SES^H$ | 0.042*** | −0.019*** | −0.009 | 0.008 | 0.017** | −0.005 | 0.024*** | 0.041*** |
| | (0.007) | (0.004) | (0.009) | (0.005) | (0.008) | (0.008) | (0.008) | (0.005) |
| $SES^H$ | 0.002 | 0.000 | −0.000 | 0.179*** | 0.182*** | −0.117*** | 0.278*** | 0.247*** |
| | (0.006) | (0.004) | (0.007) | (0.008) | (0.009) | (0.008) | (0.008) | (0.008) |
| N | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 |
| *Coeff. linear combination* | | | | | | | | |
| $S_{cd} + S_{cd} \times SES^H$ | 0.032 | −0.031 | 0.022 | 0.019 | 0.018 | −0.048** | 0.061** | 0.078*** |
| | (0.040) | (0.024) | (0.020) | (0.023) | (0.026) | (0.023) | (0.029) | (0.026) |

*Note:* All models controls for individual age, father SES, cohort and county fixed effects, regional trends, and preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parenthesis; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS data.

educational level increases by 6.1 pp and by 7.8 pp for tertiary education. The average effect of preschool expansion on education found in Panel A is thus unevenly distributed across parental backgrounds, implying that the gains from preschool expansion are also associated with an increase in educational inequality.

We also find a positive and significant socioeconomic gradient in the effect of preschool expansion on labour market outcomes. Compared to the low-SES group, an increase in preschool supply leads to a significant increase in the probability of employment (4.2 pp), a fall in the risk of unemployment (−1.9 pp) and an increase of 1.7% in the monthly wage gap between high-SES and low-SES groups.[35] While the total effect on labour market outcomes for the high-SES group is not significant, as indicated on the last line of Panel B, it is worth stressing that the socio-economic gradient is, itself, statistically significant and contributes to widening the labour market achievement gap between low- and high-SES children.

Table 5 provides separate estimates of Equation (1′) by gender. Results are, for the most part, similar for males (Panel A) and females (Panel B) and confirm the findings of Table 4. For both sub-samples, we fail to find significant effects of the preschool expansion on long-run labour market outcomes, whereas we do find evidence of a positive socioeconomic gradient, consistently across outcomes, indicating that high-SES individuals derive higher benefits from the reform, irrespectively of gender. As shown by the test of coefficients reported on the last two rows of the table, there are no significant differences in the magnitude of the gradient across males and females, except for the extensive margins of labour supply, where females display a larger gradient. For other outcomes, coefficients appear very similar across genders and we also find indications of a significant net effect on the educational outcomes for both males (higher probability of reaching higher education degrees) and females (lower chances of low educational achievement).

The literature usually measures the outcome response to a change in preschool coverage rate, that is, the proportion of children attending preschool in the relevant age group. In our case, obtaining the effect of a 10% increase in coverage rate requires rescaling our estimates by a factor 0.098.[36] Havnes and Mogstad [29] provide estimates of the effects of changes in coverage rate on a number of long-term outcomes, in Norway, that can be compared to ours. In particular, they find that a 10 pp increase in preschool coverage increased the probability of attending college by 0.203 pp and reduced the probability of being an high-school drop-out by −0.342 pp.[37] For France, the corresponding reduced form estimates from Table 4, Panel A, are 0.59 pp and −0.45 pp which have the same direction but are larger in magnitude than those in Havnes and Mogstad. We find similar effects when the focus is on high-SES children (Panel B of Table 4).

Overall, the reduced-form estimates indicate that the preschool expansion implemented under the National Plan did not, on average, yield any statistically significant improvements in individual long-term outcomes. The preschool expansion had, however, a significant differential impact on high-SES children thereby increasing social inequality in long-term achievement. This evidence allows us to rule out potential concerns about violations of

**TABLE 5** | Reduced form estimates by gender.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Panel A — Male | | | | | | | | |
| Preschool supply ($S_{cd}$) | 0.008 | −0.009 | 0.027 | −0.027 | −0.053 | −0.024 | 0.052 | 0.067* |
| | (0.039) | (0.032) | (0.025) | (0.041) | (0.042) | (0.035) | (0.040) | (0.038) |
| $S_{cd} \times \text{SES}^H$ | 0.024*** | −0.017*** | 0.000 | 0.014* | 0.021** | −0.003 | 0.022** | 0.042*** |
| | (0.006) | (0.005) | (0.003) | (0.008) | (0.010) | (0.007) | (0.009) | (0.007) |
| $\text{SES}^H$ | 0.007 | 0.001 | 0.006** | 0.184*** | 0.185*** | −0.122*** | 0.282*** | 0.239*** |
| | (0.006) | (0.005) | (0.003) | (0.012) | (0.013) | (0.009) | (0.011) | (0.011) |
| N | 31,686 | 30,369 | 28,381 | 23,096 | 23,236 | 31,682 | 31,682 | 31,682 |
| *Coeff. linear combination* | | | | | | | | |
| $S_{cd} + S_{cd} \times \text{SES}^H$ | 0.032 | −0.026 | 0.028 | −0.013 | −0.032 | −0.027 | 0.075* | 0.108*** |
| | (0.038) | (0.031) | (0.026) | (0.042) | (0.043) | (0.034) | (0.040) | (0.039) |
| Panel B — Female | | | | | | | | |
| Preschool supply ($S_{cd}$) | −0.024 | −0.016 | 0.035 | 0.036 | 0.051 | −0.062** | 0.024 | 0.011 |
| | (0.057) | (0.032) | (0.036) | (0.055) | (0.067) | (0.030) | (0.041) | (0.045) |
| $S_{cd} \times \text{SES}^H$ | 0.059*** | −0.021*** | −0.020 | 0.018* | 0.016 | −0.008 | 0.025*** | 0.040*** |
| | (0.010) | (0.005) | (0.017) | (0.010) | (0.012) | (0.009) | (0.009) | (0.007) |
| $\text{SES}^H$ | −0.002 | 0.000 | −0.006 | 0.175*** | 0.182*** | −0.112*** | 0.274*** | 0.255*** |
| | (0.009) | (0.006) | (0.014) | (0.010) | (0.012) | (0.009) | (0.009) | (0.010) |
| *Inv. Mills ratio* | | | | 0.208*** | 0.044** | | | |
| | | | | (0.011) | (0.018) | | | |
| *N* | 33,214 | 28,735 | 26,347 | 23,011 | 23,185 | 33,211 | 33,211 | 33,211 |
| *Coeff. linear combination* | | | | | | | | |
| $S_{cd} + S_{cd} \times \text{SES}^H$ | 0.035 | −0.038 | 0.015 | 0.054 | 0.068 | −0.070** | 0.049 | 0.051 |
| | (0.055) | (0.032) | (0.034) | (0.055) | (0.067) | (0.034) | (0.043) | (0.045) |
| *Test: coeff. difference between Panel A and Panel B* | | | | | | | | |
| $\Delta\ S_{cd}$ | 0.032 | 0.008 | −0.007 | −0.062 | −0.105 | 0.038 | 0.028 | 0.056 |
| | (0.052) | (0.040) | (0.047) | (0.060) | (0.089) | (0.048) | (0.060) | (0.065) |
| $\Delta\ (S_{cd} + S_{cd} \times \text{SES}^H)$ | −0.035*** | 0.004 | 0.020 | −0.004 | 0.005 | 0.005 | −0.003 | 0.002 |
| | (0.009) | (0.006) | (0.015) | (0.014) | (0.016) | (0.006) | (0.008) | (0.009) |

*Note:* All models controls for individual age, father SES, cohort and county fixed effects, regional trends, and preschool demand drivers; Columns 4–5 exclude self-employed but second stage regressions in Panel B (female sample) have been augmented with a selection equation for labour market participation; Robust standard errors clustered at county of birth in parenthesis; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS data.

the SUTVA arising from the interference of general equilibrium effects and peer effects. Whether our main finding of a stronger benefit of the reform for high-SES children might partly result from peer effects on the non-treated group is open for discussion. In particular, our findings could be the spurious result of peer effects if there are specific negative peer effects of the reform on non-treated high-SES children, something that we do not regard as highly plausible.

In the rest of the paper, we focus on this achievement gap and assess how the preschool expansion affected individual outcomes.

## 4.2 | TSTSLS Estimates

In the reduced-form estimates of the previous section, our preschool variable is akin to a measure of the intention to treat. Two mechanisms may account for the larger effect of the preschool expansion on high-SES versus low-SES children. On the one hand, the *actual* treatment of children may have differed according to their family background, as a given increase in preschool facilities may have led to different average increases in preschool duration and enrolment, conditional on parental SES, as a result of selection into treatment. In other words, preschool exposure may have differed by SES, as a result of the expansion.

**TABLE 6** | First stage regression estimates.

| Specification | Baseline | | | Interaction $S_{cd}$ × female | | |
|---|---|---|---|---|---|---|
| Dependent variable | *Dur* | *Dur* | $Dur^H$ | *Dur* | *Dur* | $Dur^H$ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $S_{cd}$ | 0.698** | 0.722** | 0.085 | 0.717** | 0.742** | 0.102 |
| | (0.321) | (0.325) | (0.192) | (0.319) | (0.324) | (0.193) |
| $SES^H$ | 0.145*** | 0.180** | 1.860*** | 0.144*** | 0.179** | 1.860*** |
| | (0.042) | (0.069) | (0.056) | (0.042) | (0.069) | (0.056) |
| $S_{cd} \times SES^H$ | | −0.042 | 0.307*** | | −0.043 | 0.306*** |
| | | (0.053) | (0.037) | | (0.053) | (0.037) |
| $S_{cd}$ × female | | | | −0.047 | −0.048 | −0.042 |
| | | | | (0.053) | (0.053) | (0.029) |
| N | 2405 | 2405 | 2405 | 2405 | 2405 | 2405 |
| F-test | 4.732 | 2.542 | 36.934 | 5.042 | 2.668 | 37.196 |
| F-p | 0.034 | 0.088 | 0.000 | 0.029 | 0.078 | 0.000 |

*Note:* All models control for father SES, cohort and county fixed effects, regional trends; Robust standard errors clustered at county of birth in parentheses; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* FQP data.

On the other hand, a similar increase in preschool participation may have translated into higher long-term benefits due to a higher marginal return on preschool duration for high-SES children. In other words, conditional on preschool exposure, individual responses may have differed by SES, as a result of the expansion. These two mechanisms obviously have different policy implications which we address in the context of a two-step procedure.

We first assess the effect of the preschool expansion on preschool exposure, using FQP data. Table 6 reports estimates of the first-stage regressions of Equation (3). In Column 1, individual preschool duration is regressed on preschool supply at age 3, without introducing any interaction term. Results show that one additional school per 100 students raises preschool duration by 0.7 years for the baseline sample, including male and female respondents. Column 2 reports the results of the estimation of Equation (3a), where individual preschool duration is further regressed on the interaction between preschool supply and a binary indicator of high-SES. As previously discussed, this allows us to assess the differential effect of the reform on the two SES groups. The estimated main effect is very close to the one reported in the first column (0.722 years) and the interaction term is small and not significant, indicating that the preschool duration response to an increase in the preschool supply was very similar across both parental SES groups.[38] Overall, results in Table 6 indicate that high-SES children stay slightly longer in preschool compared to low-SES children on average (+0.180 years against an average duration of 2.025 years), but there is no evidence that they benefit differently from the preschool expansion, in terms of preschool duration, compared to low-SES children.

Columns 4 and 5 extend the estimation of the effect of the preschool expansion on preschool duration to account for the possibility of differentiated effects by gender. The coefficient on the interaction between preschool supply and a gender indicator variable is always small and never statistically significant, providing clear indication, again, that the school expansion affected children's preschool treatment independently of gender.

The estimates of Equations (3a, 3b) presented in Columns 2 and 3 of Table 6 are based on the FQP sample. F-tests of the significance of the preschool supply instruments cannot reject joint significance. Besides, while we cannot rule out the possibility of weak instruments based on the F-test in Column 2, it should be noted that the usual consequences in terms of second-stage inconsistency do not apply in our case, for TSTSLS estimation relies on two independent samples for each stage.[39]

Second stage results are reported in Table 7.[40] Unsurprisingly, the results in Table 7 are qualitatively similar to reduced form results. However, they allow quantifying the returns to years of preschool attended.

For the low-SES group, the effect of preschool duration on long-term labour market outcomes appears small in magnitude, and is never statistically different from zero. Table 7 shows, however, that increasing preschool duration has strong and significant distributional consequences for most outcome variables. One additional year in preschool increases the gap between low and high SES groups in terms of employment, rising the probability of employment by 13.3 pp and reducing the risk of unemployment by −6.4 pp for high-SES children relative to low-SES ones. While preschool duration has no effect on the probability of part-time work, it also increases the educational attainment gap: one additional year of preschool raises the gap in human capital accumulation between high- and low-SES groups, and increases by 8.3 pp (resp. 13.7 pp) for the gap in the probability of achieving at least a high-school (resp. university) diploma.

Preschool duration is also found to increase the earnings gap across family background. The effect of one additional year of preschool is also small and insignificant on earnings for the low-SES group. However, high-SES children experience earnings gains both in relative and absolute terms. One additional year in

**TABLE 7** | TSTSLS regression estimates.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\widehat{Dur}$ | −0.029 | −0.009 | 0.045 | 0.012 | −0.004 | −0.057 | 0.042 | 0.037 |
| | (0.066) | (0.040) | (0.034) | (0.034) | (0.042) | (0.041) | (0.052) | (0.059) |
| $\widehat{Dur}^{H}$ | 0.133*** | −0.064*** | −0.022 | 0.029* | 0.054** | −0.024 | 0.083*** | 0.137*** |
| | (0.024) | (0.015) | (0.029) | (0.016) | (0.025) | (0.027) | (0.031) | (0.027) |
| $\text{SES}^{H}$ | −0.240*** | 0.120*** | 0.033 | 0.123*** | 0.083 | −0.062 | 0.116* | −0.015 |
| | (0.053) | (0.033) | (0.059) | (0.037) | (0.051) | (0.059) | (0.066) | (0.061) |
| N | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 |
| *Coeff. linear combination* | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur} \times \text{SES}^{H}$ | 0.104 | −0.073* | 0.023 | 0.041 | 0.049 | −0.081 | 0.125** | 0.174*** |
| | (0.071) | (0.043) | (0.041) | (0.037) | (0.043) | (0.051) | (0.062) | (0.067) |

*Note:* All models control for individual age, father SES, cohort and county fixed effects, regional trends, and preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parentheses and corrected for two-step estimation; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS and FQP data.

preschool is found to rise the earning gap between the two groups by 5.4% in terms of monthly earnings and 2.9% in terms of hourly earnings.[41] The magnitude of the wage return to preschool education can be compared to estimates of the returns to schooling found in the literature (e.g., [60]). Brunello, Fort and Weber [61] estimate returns to one additional year of education around 9% in Europe. Maurin and McNally [62] find a value of 14% in France. Our estimates are smaller than these. Yet, one should stress that the literature typically estimates marginal returns to education while we are here estimating the effect of inframarginal years of education, occurring at the onset of the educational process. For these reasons, one might have expected to find higher returns. This might be ascribed to the mixed objective of preschool in France, which, as noted, follows the dual objective of fostering human capital accumulation while at the same time providing day care services. This might also explain why additional preschool duration does not mechanically trigger higher educational attainment.

There is no consensus on the finding of higher earnings returns for the advantaged group of high-SES children. Exploiting changes in mandatory schooling laws [61] find higher returns in the bottom of the earnings distribution. Yet, it is important to recall that the socioeconomic gradient cannot be explained by differences in the effect of preschool expansion on preschool attendance and duration according to background of origin or gender. It might reflect the greater "*social affinity*" between high-SES children and the preschool environment, as noted in the sociological literature surveyed in Section 2.2.

Last, we investigate differences by gender in the long-term impact of preschool duration. Separate estimates by gender are provided in Table A10 in the appendix.[42] Similar patterns are found across genders. Duration appears to have no impact on the low-SES groups across all outcomes, for both males and females. Preschool duration increases the gap in human capital attainment and in most labour market outcomes between low- and high-SES individuals, with no significant difference between males and females, except for the probability of being employed, for which the effect of preschool duration is double in magnitude for females.

## 4.3 | Robustness Analysis

We now discuss the robustness of our main findings with respect to changes in our sample and specification. The specific pathways through which preschool expansion operates warrant further discussion. As already mentioned, the preschool expansion helped increase the attendance of children who would not have otherwise attended but also allowed for an earlier and longer duration enrolment of children who would have attended at least some preschool even in the absence of the reform. When evaluating the effect of preschool programmes, distinguishing between these extensive and intensive margins will matter only if we expect the effect of preschool duration to be non-linear.[43] Unfortunately, the information in our data is too limited to relevantly test for such non-linearities. However, beyond the mean duration increase (Table 6), we can assess more precisely how the increase in preschool supply affected the distribution of individual preschool duration. Table 8 presents the marginal effect of the preschool expansion for each preschool duration between 0 (no attendance) and 3 (full completion). It builds upon the specification in Table 6 column 2 but uses an ordered logit to model the discrete probability distribution of duration. The preschool expansion resulted in an increase in the probability of preschool completion, at the expense of all other categories. For the latter, the biggest fall is for the probability of completing only one year (−17.1 pp). The effect on the probability of non-attendance is smaller (−13.5 pp) but, as previously noted, the share of the population in that category was already small to start with. To summarise, the dominant effect of the preschool expansion was to shift individuals with already positive preschool exposure to higher durations of preschool rather than moving individuals along the extensive margin, from non-attendance to some preschool exposure. As a result, and to the extent that non-linearities in the effect of preschool prevail, estimates presented above will mostly reflect the effect of preschool extension rather than preschool implementation.

We also gauge the sensitivity of our results to our sample selection rule. Our baseline estimates control for a large array of confounding factors, which are not available for all counties in the

**TABLE 8** | Effect of preschool supply on the distribution of preschool duration—ordered logit estimates, marginal effects.

| Predicted outcome | ***Dur* = 0** | ***Dur* = 1** | ***Dur* = 2** | ***Dur* = 3** |
|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** |
| $S_{cd}$ | −0.135** | −0.171*** | −0.100*** | 0.407*** |
| | (0.055) | (0.064) | (0.039) | (0.156) |
| $SES^H$ | −0.023** | −0.030** | −0.017** | 0.070** |
| | (0.011) | (0.014) | (0.008) | (0.032) |
| $S_{cd} \times SES^H$ | 0.002 | 0.003 | 0.002 | −0.007 |
| | (0.009) | (0.012) | (0.007) | (0.028) |

*Note:* The table reports marginal effects for the ordered logit estimation of model Equation (3a), using discrete values in {0,1,2,3} as possible discrete duration outcomes; All models controls for father SES, cohort and county fixed effects, regional trends; Robust standard errors clustered at county of birth in parenthesis; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* FQP data.

period that we consider. A more parsimonious approach in terms of controls for preschool demand (i.e., limiting controls to mortality rates and income distribution statistics) allows us to include additional counties in the analysis. Our results appear robust to this extension. Table A13 presents reduced form, first stage, and second stage estimates based on this broader sample. Results are very similar to those discussed in the previous section. The expansion of preschool expansion increases the outcome gap between low- and high-SES groups, thereby fostering inequalities due to parental background, though we also find, in this extended sample, that the preschool duration response to the reform is similar across social backgrounds. Last, we find that increasing preschool duration raises education and earnings but only for the group of high-SES children, with earnings returns around 7%.

We next assess the possibility of spatial heterogeneity in the effect of the preschool expansion. Historically, the introduction of independent preschool classes in the late 19th century occurred primarily in urban areas, starting with France's major cities. In comparison, the expansion of preschool facilities triggered by the National Plan was a global phenomenon, affecting rural, urban and suburban areas. One may suspect that the preschool expansion produced different effects in hitherto less endowed areas and especially in rural areas. Furthermore, to the extent that the preschool endowment of major urban areas displays less variation than the rest of the counties, one may wonder if their inclusion affects our estimates.

To address these issues, we perform two main robustness checks. First, we investigate heterogeneous responses in the effect of the preschool supply expansion on individual preschool enrolment by estimating differentiated effects for the 25% least urbanised counties. Second, we examine the sensitivity of estimated long-run effects of preschool expansion and duration to the exclusion of the counties hosting the three largest French cities (Paris, Lyon and Marseille). Results are given in Table A14 and suggest that the effect of the preschool expansion on enrolment was not statistically different in rural areas compared to the rest of the counties, and that the results are very robust to the exclusion of the most urban counties.

We also investigate the sensitivity of the estimates of heterogeneous effects to our partition of family background. The above results admittedly rest on a coarse binary partition that only distinguishes between low- and high-SES background. The advantage of this partitioning is to split our sample into two groups of roughly equal size. One should however keep in mind that the literature indicates the possibility of non-monotonic interactions between the effect of preschool and family background (e.g., [28]). We thus replicate our analysis using a three-tiered partition of family background that distinguishes between low-SES (if father was a farmer, a manual or a non-manual worker), middle-SES (if father was an artisan or lower-grade professional) and high-SES (if father was a higher-grade professional). Results are given in Table A15. Three conclusions emerge from this complementary analysis. First, in terms of preschool attendance, as captured by our first stage regression, the preschool expansion benefitted all children irrespectively of family background. Second, the effect of the reform and of preschool attendance on individual long-term outcomes is of very similar magnitude in the middle- and high-SES groups and larger than the effect estimated for the low-SES group. Last, although we cannot rule out that the estimated effects are equal between the middle- and high-SES groups, only the middle group appears to be significantly different from the low-SES group. However, using this three-tiered partition, only few observations are classified in the high-SES group (about 1/6th of our sample) and this may account for the lack of statistical significance. As a conclusion, using the extended partition of family background does not support the existence of differentiated effects between middle- and high-SES groups but confirms that children from low-SES family background benefitted less from preschool enrolment than more socially advantaged children.

One last concern pertains to the robustness of our specification to biases stemming from dynamic treatment heterogeneity. Our estimation relies on the staggered implementation of the preschool infrastructure development plan and exploits heterogeneity across county-cohort cells in the supply of preschool, controlling for two-way fixed effects (TWFE, in our case at the county and cohort levels). A recent series of articles has underscored the lack of consistency of TWFE estimators in the case of treatment effect heterogeneity and dynamic treatment response. In particular, TWFE estimators are potentially plagued by "forbidden comparisons" whereby treated units are erroneously compared with other units treated at different times and experiencing different response intensities (e.g., [63, 64]).

Several estimators address this potential bias in the case of binary treatment (e.g., [65]) and also continuous treatment, which comes closer to our setting [66]. Usual solutions restrict the comparison of treated units to not (yet) treated ones. In our case, we cannot resort to such strategies as preschool supply was positive in all counties even before the implementation of the National Plan. We thus propose, as a robustness check, a simple DiD estimator that compares outcomes of individuals between early and late cohorts in high- vs. low-expansion counties. Under the usual common trends assumption, this DiD estimator is unaffected by dynamic heterogeneity and provides a valid estimator of the effect of the National Plan. Details and results are given in the appendix. Estimates, although imprecise, are supportive of our main results. We find that the preschool expansion had no significant impact on the employment and unemployment of low-SES children, but benefited high-SES children. DiD estimates of the effects on earnings are qualitatively similar to those in Table 4, with significant and sizable effects for treated high-SES treated individuals.

## 5 | Conclusion

Our analysis portraits a mixed picture of the effect of preschool attendance on long-term individual outcomes. Three main results emerge from our study. First, the massive preschool facility expansion programme undertaken in France throughout the 1960s and 1970s indeed raised preschool attendance. At the county level, cohorts exposed to larger preschool supply attended preschool for a significantly longer duration, 0.69 years on average (95% Confidence Interval: [0.069,1.327]). Furthermore, this effect was similar across both gender and social background. In this respect, the French National Plan was successful at reaching the objective of promoting preschool attendance, although the target enrolment rate of 80% of 3-year-old children was only reached later.

Second, based on our estimates, we cannot conclude that the increase in preschool enrolment significantly affected long-term individual outcomes *on average*. We considered three main types of outcomes: education, labour force participation and earnings. Average reduced-form effects for labour market participation and earnings appear positive but are never statistically significant. This lack of significance may partly reflect a lack of precision of our policy variable. In this paper, we can only match individual outcomes with preschool supply at the county level, which represents a relatively broad level of geographic aggregation. While the school registry data allow us to track school openings at the municipal level, geographic aggregation at the county level is constrained by the use of labour force survey data and might hamper the statistical precision of our analysis. Similar limitations are also likely to affect our two-sample two-stage estimates, since our auxiliary microdata sample from the Education and Training survey (FQP) is also relatively small in size, which might impact the second-stage standard errors.

Third, results also show that preschool enrolment produces differentiated effects on long-term outcomes according to family background: in our case, high-SES children benefitted significantly more from an increase in preschool duration than their low-SES counterparts, in terms of education (for the probability of achieving a high education degree, the gap rises by 13.7 pp [8.4,19.0]), employment (by 13.3 pp [8.6,18.0]), unemployment (by −6.4 pp [−9.3, −3.5]) and monthly earnings (by 5.4% [0.5%,10.3%]). TSTSLS estimates show that a one year increase in preschool duration raised earnings by 4.9% (albeit the estimation is imprecise, with a CI: [−3.5%,13.3%]) for children from high-SES families—a number comparable to standard estimates of the earnings returns to education—whereas the effect for low-SES children was close to zero and statistically insignificant (−0.4% [−8.6%,7.8%]).

It is often argued and hoped that early schooling interventions in general, and preschool education in particular, equalise life chances by levelling individual advantage or disadvantage arising from family background. Our empirical analysis suggests that the opposite may be true in the case of the French universal preschool programme. While, for one thing, we find similar effects by gender, our estimates also clearly indicate a disequalising effect of preschool enrolment along the social gradient which leads to a widening of the outcome gap between high- and low-SES children. This finding indicates that preschool investment might be complementary rather than a substitute to family resources and circumstances. Several mechanisms might underlie this complementarity. It may be partly driven by the specific design of the French preschool programme, as reflected for instance in the curriculum, the learning objectives, the interactions between preschool teachers and children, etc., which might put children from the middle and upper class at an advantage compared to their lower class counterparts. Disentangling these different aspects would probably require a detailed, qualitative and comparative analysis of the French educational experience, in particular from a didactic perspective. This would allow us to assess whether the complementarity found here in the French case applies more generally to other contexts and would also help amend the preschool programme in order to favour inclusiveness with respect to least advantaged children.

Last, one should also mention the possibility that part of the effects measured in our estimates stem from changes occurring in the family environment, in causal response to the change in preschool enrolment. For instance, better preschool opportunities might indirectly increase parental employment outcomes (as well as family resources) or improve the quality of parental time input in ways that ultimately result in enhanced human capital accumulation and long-term benefits. Furthermore, these indirect effects might be heterogeneous according to family background, thus possibly reinforcing the above mentioned complementarity. Disentangling these different effects—which are usually lumped together with direct effects in estimates of the effect of preschool—is a difficult task but would deserve further investigation in future research.

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### Data Availability Statement

The code and data for all figures and tables are available on the Zenodo repository, DOI: https://doi.org/10.5281/zenodo.17393291.

### Endnotes

[1]See Ruhm and Waldfogel [3] and Nores and Barnett [4] for a general review of the literature.

[2]Examples are the Carolina Abecedarian Program [5], the Head Start project [6], the Perry Preschool program [7], WIC and SNAPS projects [8], EITC program [9] and the Moving to Opportunity program [10].

[3]On earnings, see Bartik, Gormley and Adelstein [11] Chetty et al. [12] for the US and Gertler et al. [13] for Jamaica. See also Arteaga et al. [14] and Reynolds et al. [15] on education and criminal behaviour.

[4]The main empirical studies are gathered in Table A1. See in particular Felfe and Lalive [21], Cornelissen, Dustmann, Raute and Schönberg [23], Busse and Gathmann [22] for Germany, Felfe, Nollenberger and Rodriguez-Planas [24] for Spain, Berlinski, Galiani and Manacorda [25] and Berlinski, Galiani and Gertler [17] for Uruguay and Argentina respectively, Baker, Gruber and Milligan [18] for Canada, Datta Gupta and Simonsen [19] for Denmark, and Caille [20] and Goux and Maurin [30] for France.

[5]See in particular Nores and Barnett [4], Arteaga et al. [14], Busse and Gathmann [22] and references therein.

[6]Over the period investigated, the incidence of public facilities was 97.7% in 1967 and 96.3% in 1976, and the majority of private schools were run under a formal contract with national schooling authorities.

[7]Enrollment in primary school is compulsory and starts from September in the year children turn 6.

[8]*Plans de Dévelopement économique et social*. See in particular Commissariat général du plan [37] and Prost [36].

[9]These large hirings were in fact only a small share of the massive hirings implemented to meet the needs created by the demographic expansion of the baby boom.

[10]Decree regulating the pedagogical organisation and teaching programs of public maternal schools, July 28, 1882. Main official legal and ministerial documents can be found in Luc [38].

[11]Table A8, Panel A.

[12]Circular no. 77–266, Ministry of Education, 1977.

[13]Decree organising the school week, August 7, 1969.

[14]Following the seminal work of Lahire [42], Joigneaux [43] relies on the analysis of official preschool programs and ethnographic observation to show that the early emergence of school inequalities in preschool is partly explained by differences in early (graphical) literacy skills and cognitive autonomy.

[15]Family surveys conducted by INSEE in 1954 and 1975 providing a detailed account of women's demographic events (marriage, births, divorce, widowhood, etc.) but information on child care mode is only available from the 1982 wave.

[16]See the file fr-en-adresse-et-geolocalisation-etablissements-premier-et-second-degre. Data can be navigated online through the ministry's portal https://data.education.gouv.fr/explore/.

[17]For instance, Cottini, Ghinetti and Moriconi [46] note the important expansion of French universities from the late 1950s to the 1990s.

[18]To our knowledge, data on school quality are not available at the county level over the expansion period we study. However, we do not expect important variation in preschool quality given the high degree of centralization and regulation of schooling supply in France, as discussed in Section 2.2.

[19]When implementing this approach, socio-economic family background is proxied by the occupation of the father when the individual left the schooling system.

[20]IV identification assumptions are presented in a DAG representation in Figure A1.

[21]Coefficient variance–covariance estimators implemented in this paper also account for clustering at the county level.

[22]A gender dummy is, however, included in the model.

[23]Wooldridge [52] advises to separate the estimation of the selection equation from that of the first stage in a two step procedure: instruments (in our case, preschool supply and preschool supply interacted with high-SES parental background indicator) should enter into the selection equation as additional controls, whereas first stage predictions should not.

[24]In line with the literature, the following variables provide independent sources of variation in the selection equation: marital status, the total number of children, the number of children below age 6, and house ownership status, as a proxy for household resources. See, for example, Mulligan and Rubinstein [53] and Blau, Kahn, Boboshko and Comey [54] for a recent discussion.

[25]The LFS sample is a rotating panel with a rate of rotation of six trimesters. The outcomes of interest, however, are reported only once by respondents. For this reason, the LFS sample is a repeated cross-section roughly corresponding to one-sixth of the available survey size. The sample construction has changed significantly in 2002, making it difficult to combine waves of LFS collected in or before 2002 to the pooled sample.

[26]Earnings are not available for self-employed workers.

[27]While other family circumstances could in principle be used to assess family background, parents' education is not available in the LFS and mother's occupation is by definition not observed for mothers out of the labour force.

[28]These counties are 18 (Cher), 28 (Eure-et-Loir), 36 (Indre), 37 (Indre-et-Loire), 41 (Loir-et-Cher), 45 (Loiret). Table A13 in the appendix reproduces our main estimates without controls for preschool demand, and including these six counties. Results are unaffected.

[29]Data can be accessed at https://frdata.org/en/. For details, see Bonnet [55] and Bonnet, d'Albis and Sotura [56].

[30]After 1993, later waves of the FQP survey provide binary information on whether individuals ever attended preschool; however, none of the other waves reports information on preschool duration. The fact that information is reported retrospectively in adulthood legitimately raises concerns about the reliability of the preschool duration data. Assessing the quality of the information reported by individual respondents would require validation data which are, unfortunately, not available for this particular survey. In general, the presence of measurement error introduces biases in regression estimates. However, instrumental-variable estimation can be proved to be immune to such biases, as demonstrated by Griliches [57] in the classical measurement case, and Biener, Meyerhoefer and Cawley [58] in the non-classical case, as long as instruments are independent of individual measurement errors.

[31]We set the value of preschool duration to 0 for individuals who report having attended some preschool for a duration lower than 1 year and pool them with individuals who report not having attended any preschool. The former individuals represent about 18% of individuals coded with a duration of 0. This is arbitrary and reflects the fact that preschool duration in months is not available in our data. Arbitrarily setting the duration to 0.5 year or 1 year for these individuals has no effect on the results of our first step model in Table 6.

[32]The Berthoin reform, voted in 1959, added 2 years of compulsory education by raising the minimum school leaving age from 14 to 16 for all cohorts born 1953 onwards.

[33]In Table A9, samples' independence is also tested when cells are further partitioned by parental background and for the subsample of individuals with low educational background, which we can identify in both LFS and FQP. In all cases we cannot reject independence, implying that any compositional difference between FQP and LFS samples can be interpreted as random.

[34]As a robustness check, we also consider augmenting the reduced form regressions and the second stage regressions for the pooled males and females dataset with the selection equation for earnings. In this case, we interact drivers of selection with a female indicator, so that the level of the instruments is always set to zero for males. Results presented in Tables A11 and A12 in the appendix (which include the inverse Mills ratio $\lambda$) replicate the main results.

[35]We find smaller (0.8%) and insignificant effects of preschool supply expansion on the high-SES gap in hourly earnings, reflecting the potential role of self-selection of women into employment. When accounting for selection, the estimated SES gradient rises to at least 1.8% for both hourly and monthly earnings (see Table A11 in the appendix).

[36]Let $c_0$ and $c_1$ respectively denote the pre- and post-reform coverage rates and $S_0$ and $S_1$ the preschool supply. Define $\Delta x \equiv x_1 - x_0$. Under the assumptions that the coverage rate grows proportionally to the number of preschool facilities, we have $\Delta S/S_0 = \Delta c/c_0$, implying $\Delta S = \Delta c \times (S_0/c_0)$. Hence, a 10 pp ($\Delta c = 0.1$) change in $c$ requires a change in $S$ equal to $0.1 \times S_0/c_0$. The coverage rate in the pre-reform periods was equal to 82% and the average value of $S_0$ is 0.8, so the adjustment factor equals $0.1 \times 0.8/0.82 = 0.098$.

[37]Table 5, Panel B, Column 5. The table reports the effect of a 1 pp increase in coverage. We multiplied these effects by 10.

[38]As discussed in Section 3, estimates of Equation (3b) given in column 3 cannot be interpreted along similar lines since the dependent variable is the interaction between preschool duration and SES and is zero for all children from the low-SES group. This equation is only instrumental in the TSTSLS procedure.

[39]See, for example, Angrist and Krueger [47] and Angrist, Imbens and Krueger [59].

[40]While all models feature gender-specific intercepts, we do not consider further interactions of gender indicators with preschool duration, given little evidence of reduced form effects of the reform along the gender lines and no first stage impact of gender on duration. Robustness checks support this strategy.

[41]As a robustness check, we consider augmenting the baseline second stage model with the employment selection equation. Effects of duration on the father SES gradient are slightly larger and closer to 6% for both monthly and hourly earnings (see Table A12 in the appendix), reflecting the fact that hourly wages, more than monthly wages, are affected by extensive margins of labour supply of female workers.

[42]Both models share the same first stage regressions, which are based on the entire sample of males and females.

[43]The shape of non-linearities is a priori undetermined. The effect of preschool duration could be concave in the presence of decreasing returns to preschool human capital, but the relation could also be convex if there are complementarities in the various skills acquired through preschool. In the former case educational policy should seek to implement some minimal degree of preschool attendance for all children, while in the latter, it should emphasise preschool completion.

[44]The definition slightly differs from our main sample since it is based on the contemporaneous occupation of the head of household rather than on the occupation of the father when the child finished school.

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## Appendix A

### Measuring Preschool Supply

#### The National School Register

The National School Register, NSR, supplied by the Ministry of Education allows to track all educational institutions operating in France at the level of preschool, primary school and secondary school. The NSR collects information about the opening date and period of operation of every school and allows to identify schools offering preschool education. For each school, the NSR also provides the detailed address. We aggregate this information at the county level in order to follow the dynamic of preschool supply over the period of implementation of the National Plan and beyond. The county level (*départements*) corresponds to an intermediate geographical and administrative division. Mainland France has 96 counties.

#### Selection of Counties

One limitation of the data is the lack of accuracy in the reporting of school operation and school opening dates for the early years of our study. Before 1965, the data were sparsely filled and in many counties, information was missing on both preschool and primary school facilities. For schools already in operation at the time when the NSR was consolidated, it seems that the school opening date appearing in the data does not always reflect the true opening year but eventually the year when the school was entered in the NSR. For the purpose of this paper, cases where information on preschool facilities is missing is of course ambiguous as it might signal either the lack of preschool infrastructure in a given county or a misreport of existing infrastructures. However, cases where, for a given county, information on primary school is also missing in the early years are a clear indication of data unreliability of the NSR as primary school is mandatory over this period and most municipalities host at least one school. To discard county-year information on school infrastructures as unreliable, we use information on both preschool and primary school. We classify county-year cells as unreliable whenever the reported number of preschool or primary school is missing. Additionally, when, for a given county, the number of primary schools increases by more than 50% between year $t-1$ and $t$, we discard year $t-1$ as unreliable, as it signals ongoing consolidation of the NRS for that particular county. We apply this rule iteratively.

Figure A2 and Table A2 report information on counties with reliable information over the period of observation. As of 1967, a large majority of counties have reported reliable information on the stock of schools available, whereas complete coverage by available data was almost reached as of 1969. The aim of the National Plan was to trigger the construction of new preschool facilities over a decade-long period. Since our objective is to exploit the time variation in preschool supply at the county level, we focus our analysis on geographical divisions where preschool supply was already observed as of 1967 in order to reduce to the minimum the risk of measurement error in school supply stock and its variations.

Table A3 provides the distribution of reliable counties by region. For most regions, our sample gathers all possible counties. Two regions are absent from our baseline sample, *Centre* and *Corse*, for which information on preschool demand drivers is missing.

Overall, we gather a sample of 64 counties for which preschool supply can be reliably measured in the period of implementation of the National Plan. Out of the 64 counties, 58 report information about local demand drivers. Our baseline estimates (which control for demand drivers) are hence based on this 58 counties sample and compared to a less demanding model (where demand drivers are not considered) spanning the 64 counties for which reliable preschool information is available. The robustness of estimates to the inclusion of these counties is discussed in Section 4.3. Below, we investigate the similarity of the main sample (58 counties) with respect to the largest sample available (based on 96 counties), disregarding information on preschool supply (not available for 32 counties).

Table A4 offers a comparison of the distribution of outcomes and covariates available in LFS and FQP for cohorts 1964–1973, separately for the baseline sample (consisting of individuals born in one of the 58 counties for which reliable information is available) and for the largest available samples from LFS and FQP spanning across 96 counties. The data show that, for most of the outcomes, differences are small and statistically insignificant. The using sample is younger (−0.029 years), less educated (+0.6 pp chances of holding only a primary degree or less) but richer (+1% in wages) compared to the largest sample. Such differences may reflect the possibility that counties characterised by lower educational attainment were, to some extent, prioritised by the rollout of the National Plan, implying a comparatively faster increase in preschool supply development in these places. The algorithm that we employ to determine the extent of reliability of a county crucially picks up this effect and increases the likelihood that those counties appear in the using sample. This effect is of little concern for our identification strategy, which already features county fixed effects and trends, since the baseline sample does not differ from the main sample in terms of pre-determined characteristics (differences in averages are all statistically insignificant) and, as shown in the next table, we do not detect evidence of pre-trends.

We investigate pre-trends in outcomes and covariates by looking at the sample of individuals born in the pre-implementation period (the analysis is limited to cohorts 1962–1963, which include individuals that are at least 40 years old when interviewed in LFS). We further distinguish between those born in the 58 counties where reliable information on preschool supply is available and the largest available sample (96 counties). Table A5 reports descriptive statistics about the outcome distribution in pre-implementation cohorts and compares the two samples. The data show that none of the differences in outcomes or pre-determined characteristics of the respondents in pre-expansion cohorts (i.e., cohorts that are too old to benefit in full from the growth in preschool supply introduced by the plan) are statistically significant. Additionally, we do not detect differences in patterns of attendance to preschool across the two FQP samples, as highlighted by the estimates reported in Panel B of both Tables A4 and A5.

As a robustness check (see Table A13), we also show that our baseline estimates are robust to enlarging the sample to all 64 counties for which reliable information on preschool supply is available (but additional controls are not).

**TABLE A1** | Early interventions and their effects.

| Study | Country | Universal programme | Long-term effects | Whether preschool | Programme name | Main outcomes and effects | Heterogeneity |
|---|---|---|---|---|---|---|---|
| Arteaga et al. [14] | USA | | ✓ | ✓ | Chicago public schools | Year K standardised readiness test: +3.314 (0.532) | Ambiguous effects by the mother's education |
| Bastian and Michelmore [9] | USA | | ✓ | | EITC | Completing high school: +1.3%, completing college: +4.2%, employment: +1.0%, earnings: +2.2% | Only children in low-income families |
| Chetty et al. [12] | USA | | ✓ | ✓ | Project STAR | No effects on earnings except for quality indicators | Not available |
| Chetty et al. [10] | USA | | ✓ | < 13 year old | MTO | Earnings: +1,339.8 (671.3), college: +2.5% (1.14) | Only low-income families receiving voucher. |
| Garcia et al. [5] | USA | | ✓ | | ABC/CARE | Education: +2.14 to +0.66 years, Income: +10.8% to 64% | Not available. Programme targeted at low-income families. |
| Heckman et al. [7] | USA | | ✓ | ✓ | Perry preschool | IRR: 7%–10% | Not available. Programme targeted at low-income families. |
| Reynolds et al. [15] | USA | | ✓ | | Child–Parent Centre | Difference between treated and control: School completion rates (71.4% vs. 63.7%, $p = 0.01$), full-time employment (42.7% vs. 36.4%, $p = 0.04$) | Not available |
| Gertler et al. [13] | Jamaica | | ✓ | ≤ 4 year old | Jamaica study | Log earnings: +0.3 (0.01) | Effects available for growth stunted children. |
| Berlinski et al. [17] | Argentina | ✓ | | ✓ | | Math test: +6.527 (2.024), Spanish test: +5.983 (2.24) | Mixed evidence by household poverty status |
| Baker et al. [18] | Canada | ✓ | | ≤ 4 year old | Quebec subsidised care | Motor skills: −1.65 (0.46), Excellent health: −0.55 (0.016) | Not available |
| Datta Gupta and Simonsen [19] | Denmark | ✓ | | ✓ | | No significant effects at age 7 | Outcome deteriorates for low educated mothers |
| Caille [20] | France | ✓ | | ✓ | École maternelle | Limited impact on skills upon school entrance | Not available |
| Felfe and Lalive [21] | Germany | ✓ | | | ECC | Motor skills: +0.258 (0.060) | Stronger effect in motor and language skills for migrants. |
| Busse and Gathmann [22] | Germany | ✓ | | ✓ | | Skills (Vineland scale): −0.073 (0.034), Language: −0.062 (0.032) | Positive effects for low-skilled, poor families and single parents. |
| Cornelissen et al. [23] | Germany | ✓ | | ✓ | | School readiness: +0.205 (0.105) | Stronger effects for children with migration background. |
| Felfe et al. [24] | Spain | ✓ | | ✓ | | Reading at age 15: +0.15 SD, grade retention: +0.25 SD | Stronger for low SES families. |
| Berlinski et al. [25] | Uruguay | ✓ | | ✓ | | Attendance at age 15: +0.274 (0.056), Completed years of schooling: +0.788 (0.157) | Stronger effects for low educated mothers |
| Bartik et al. [11] | USA | ✓ | | √ | TPS pre-K | Average test scores percentile: +17.93 (1.729) | Mixed evidence by family income |
| Baker et al. [26] | Canada | ✓ | ✓ | ≤ 4 year old | Quebec subsidised care | Negative effects on multiple measures of non-cognitive skills, Poor health: +0.073 (0.019), Low life satisfaction: +0.043 (0.017) | Not available |
| Dumas and Lefranc [27] | France | ✓ | ✓ | | École maternelle | High school diploma: +0.027 (0.015), grade retention at 11: −0.068 (0.016), grade retention at 16: −0.098 (0.027) | Smaller effects for high-SES fathers. |
| Andreoli et al. [28] | Norway | ✓ | ✓ | ✓ | | Distributional effects on adult income: positive and significant for low parental income centiles, negative for top centiles | Positive but small effects for low-income households, negative effects for high-income households |
| Havnes and Mogstad [29] | Norway | ✓ | ✓ | ✓ | | Attend college: +0.012 (0.003), Having low earnings: −0.0064 (0.0025), Having at least average earnings: +0.009 (0.003) | Positive effects are stronger effects for children with low-educated mothers |

*Note:* Column description: 'Universal programme' indicates whether the programme is universal; 'Long-term effects' indicates whether the study reports long-term effects of the programme; 'Whether preschool' indicates whether the programme is a preschool programme or some other form of intervention (including day care or early child care) and ≤ $x$ year old indicates that the programme is targeting children less than $x$ years old. Standard errors are reported in parenthesis.

Abbreviations: ECC, early child care; EITC, earned income tax credit; IRR, internal rate of return; MTO, moving to opportunity; TPS, Tulsa public schools.

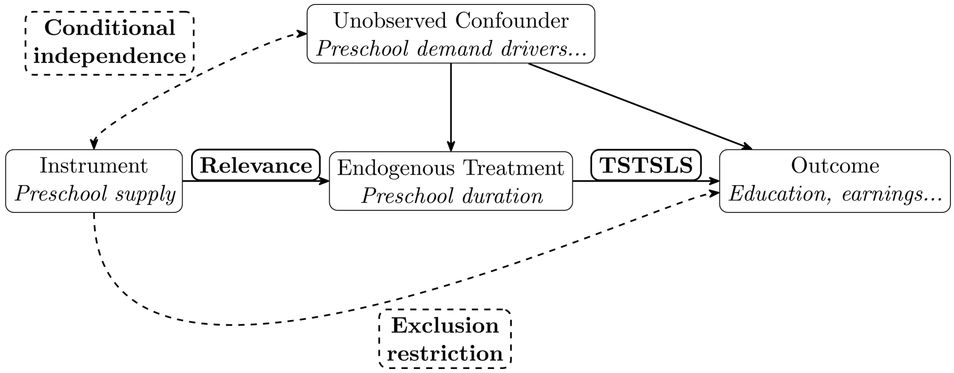
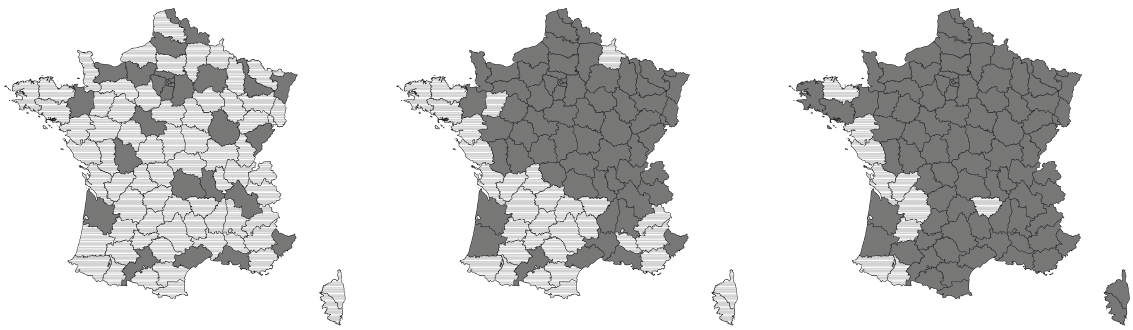



**FIGURE A1** | DAG for the instrumental variable representation displaying all key assumptions.



(a) 1965 (b) 1967 (c) 1969

**FIGURE A2** | Information on preschool supply by county. Authors' computation based on the National School Register. Dashed areas indicate counties where information on preschool facilities is unreliable.

**TABLE A2** | Counties with reliable information on preschool supply.

| Year | Cohort | Number of reliable counties | Total number of preschool facilities reported in NSR | Number of preschool facilities in 1967-reliable counties | Average number of preschool facilities in 1967-reliable counties |
|---|---|---|---|---|---|
| 1964 | 1961 | 0 | 100 | . | . |
| 1965 | 1962 | 28 | 3859 | 3854 | 60.219 |
| 1966 | 1963 | 45 | 6034 | 6029 | 94.203 |
| 1967 | 1964 | 64 | 7486 | 7437 | 116.203 |
| 1968 | 1965 | 77 | 8566 | 7810 | 122.031 |
| 1969 | 1966 | 86 | 9579 | 8333 | 130.203 |
| 1970 | 1967 | 94 | 10,870 | 8860 | 138.438 |
| 1971 | 1968 | 96 | 11,791 | 9557 | 149.328 |

**Sample Selection**

**LFS and FQP data**

The LFS sample is obtained by pooling repeated yearly cross-sections over the period 2003–2018. The sample gathers independent observations of males and females born 1964–1973 who are aged 35 to 45 in the period considered. Haider and Solon [67] estimate that life cycle bias in earnings is minimised around the age of 40. Unfortunately, the labour market, earnings and education for those born between 1964 and 1967 are only available for a narrower age range. This is due to the fact that the LFS survey has undergone a major change in the panel sample composition in 2002, making earlier waves incomparable to post-2002 waves in terms of sample composition. For this reason, we focus on repeated cross sections drawn from LFS using waves 2003–2018. The sample is limited to individuals born in mainland France with French fathers in one of the counties for which reliable information on preschool supply is made available.

Table A6 shows the sample composition by cohort of FQP and for the combined LFS waves. The total sample size is 106,608 observations. The table displays the cohort composition within each survey year (Columns 2–17) and in the aggregate (Column 20). The proportion of cohorts in the sample is balanced, ranging on average from 9.7% to 10.3% of the total number of individuals. The table reports in bold character the cohorts

**TABLE A3** | Regional distribution of reliable counties and sample size.

| Region | Counties number (list): Population composition | Counties number (list): Sample composition | Sample size by region | Average sample size by county |
|---|---|---|---|---|
| Île-de-France | 8 (75, 77, 78, 91, 92, 93, 94, 95) | 8 | 12,899 | 1612.3 |
| Champagne-Ardenne | 4 (08, 10, 51, 52) | 3 (10, 51, 52) | 2498 | 832.7 |
| Picardie | 3 (02, 60, 80) | 3 | 3066 | 1022 |
| Haute-Normandie | 2 (27, 76) | 2 | 3182 | 1591 |
| Centre | 6 (18, 28, 36, 37, 41, 45) | 0 | — | — |
| Basse-Normandie | 3 (14, 50, 61) | 3 | 2882 | 960.7 |
| Bourgogne | 4 (21, 58, 71, 89) | 4 | 2686 | 671.5 |
| Nord-Pas-de-Calais | 2 (59, 62) | 2 | 8289 | 4144.5 |
| Lorraine | 4 (54, 55, 57, 88) | 4 | 3814 | 953.5 |
| Alsace | 2 (67, 68) | 2 | 2514 | 1257 |
| Franche-Comté | 4 (25, 39, 70, 90) | 4 | 2348 | 587 |
| Pays de la Loire | 5 (44, 49, 53, 72, 85) | 2 (49, 72) | 2457 | 1228.5 |
| Bretagne | 4 (22, 29, 35, 56) | 1 (35) | 1362 | 1362 |
| Poitou-Charentes | 4 (16, 17, 79, 86) | 2 (79, 86) | 1503 | 751.5 |
| Aquitaine | 5 (24, 33, 40, 47, 64) | 2 (33, 40) | 1814 | 907 |
| Midi-Pyrénées | 8 (09, 12, 31, 32, 46, 65, 81, 82) | 1 (31) | 1065 | 1065 |
| Limousin | 3 (19, 23, 87) | 1 (23) | 251 | 251 |
| Rhône-Alpes | 8 (01, 07, 26, 38, 42, 69, 73, 74) | 8 | 6729 | 841.1 |
| Auvergne | 4 (03, 15, 43, 63) | 2 (03, 63) | 1490 | 745 |
| Languedoc-Roussillon | 5 (11, 30, 34, 48, 66) | 2 (30, 34) | 1501.0 | 750.5 |
| Provence-Alpes-Côte d'Azur | 6 (04, 05, 06, 13, 83, 84) | 2 (06, 13) | 2543 | 1271.5 |
| Corse | 2 (2A, 2B) | 0 | — | — |

*Note:* Numbers in parentheses in Columns 2 and 3 refer to official French county codes. See https://static.data.gouv.fr/resources/departements-de-france.

**TABLE A4** | Selected counties versus all counties: Differences in outcomes and covariates.

**Panel A—Main outcomes, LFS sample**

| | Main sample of counties (58) | | | All counties (96) | | | |
|---|---|---|---|---|---|---|---|
| | **Mean** | **SD** | ***N*** | **Mean** | **SD** | ***N*** | **Diff (SE)** |
| Age (in years) | 40.616 | 3.007 | 65,125 | 40.645 | 3.001 | 84,504 | −0.029** (0.013) |
| Female (1 = yes) | 0.512 | 0.500 | 65,125 | 0.511 | 0.500 | 84,504 | 0.001 (0.003) |
| SES$^{H}$ (1 if father high-SES) | 0.433 | 0.495 | 65,125 | 0.421 | 0.494 | 84,504 | 0.012** (0.003) |
| Primary education or less (1 = yes) | 0.141 | 0.348 | 65,118 | 0.135 | 0.342 | 84,496 | 0.006** (0.002) |
| High school degree (Bac) or more (1 = yes) | 0.515 | 0.500 | 65,118 | 0.515 | 0.500 | 84,496 | 0.000 (0.003) |
| University degree (Bac + 2) or more | 0.349 | 0.477 | 65,118 | 0.347 | 0.476 | 84,496 | 0.002 (0.003) |
| Labour force participation (1 = yes) | 0.911 | 0.285 | 65,125 | 0.914 | 0.281 | 84,504 | −0.003 (0.002) |
| Unemployed (1 = yes) | 0.067 | 0.251 | 59,307 | 0.064 | 0.244 | 77,223 | 0.003 (0.002) |
| Part-time job (1 = yes) | 0.171 | 0.376 | 54,904 | 0.172 | 0.378 | 71,746 | −0.001 (0.002) |
| Self-employed (1 = yes) | 0.113 | 0.317 | 55,310 | 0.117 | 0.322 | 72,299 | −0.004 (0.002) |
| Monthly wage (log) | 7.448 | 0.487 | 46,421 | 7.437 | 0.485 | 57,934 | 0.011** (0.002) |
| Hourly wage (log) | 2.431 | 0.403 | 46,107 | 2.422 | 0.401 | 57,526 | 0.009** (0.002) |

**Panel B—Other variables, FQP sample**

| | Main sample of counties (58) | | | All counties (96) | | | |
|---|---|---|---|---|---|---|---|
| | **Mean** | **SD** | ***N*** | **Mean** | **SD** | ***N*** | **Diff (SE)** |
| Female (1 = yes) | 0.498 | 0.500 | 2432 | 0.502 | 0.500 | 3176 | −0.004 (0.014) |
| SES$^{H}$ | 0.446 | 0.497 | 2432 | 0.432 | 0.495 | 3176 | 0.014 (0.014) |
| Preschool duration (in years) | 2.025 | 0.935 | 2405 | 1.988 | 0.948 | 3143 | 0.037 (0.024) |
| Preschool attendance (1 duration > 0) | 0.904 | 0.294 | 2405 | 0.900 | 0.300 | 3143 | 0.004 (0.009) |
| Preschool completion (1 if duration ≥ 3) | 0.354 | 0.478 | 2405 | 0.343 | 0.475 | 3143 | 0.011 (0.014) |

*Note:* Samples include all observations for which information on outcomes is available. The last column features differences in averages across samples (SE in parentheses). Drivers (not reported) are not available for larger sample of counties. Significance levels: ** = 5%.
*Source:* LFS and FQP data.

**TABLE A5** | Selected counties versus all counties: Differences in pre-trends.

| | Panel A—Main outcomes, LFS sample | | | | | | |
|---|---|---|---|---|---|---|---|
| | **Main sample of counties (58)** | | | **All counties (96)** | | | |
| | **Mean** | **SD** | ***N*** | **Mean** | **SD** | ***N*** | **Diff. (SE)** |
| Age, in years (above 15) | 42.637 | 1.631 | 6225 | 42.656 | 1.637 | 8044 | −0.019 (0.028) |
| Female (1 = yes) | 0.511 | 0.500 | 6225 | 0.515 | 0.500 | 8044 | −0.004 (0.009) |
| SES$^H$ (1 if household is high-SES) | 0.378 | 0.485 | 6225 | 0.367 | 0.482 | 8044 | 0.011 (0.009) |
| Primary education or less (1 = yes) | 0.195 | 0.396 | 6222 | 0.187 | 0.390 | 8041 | 0.008 (0.008) |
| High school degree (Bac) or more (1 = yes) | 0.384 | 0.486 | 6222 | 0.379 | 0.485 | 8041 | 0.005 (0.009) |
| University degree (Bac + 2) or more (1 = yes) | 0.239 | 0.427 | 6222 | 0.233 | 0.422 | 8041 | 0.006 (0.008) |
| Labour force participation (1 = yes) | 0.901 | 0.298 | 6225 | 0.906 | 0.292 | 8044 | −0.005 (0.006) |
| Unemployed (1 = yes) | 0.071 | 0.258 | 5611 | 0.066 | 0.248 | 7285 | 0.005 (0.005) |
| Part-time job (1 = yes) | 0.167 | 0.373 | 5165 | 0.172 | 0.377 | 6741 | −0.005 (0.008) |
| Self-employed (1 = yes) | 0.110 | 0.313 | 5210 | 0.114 | 0.318 | 6804 | −0.004 (0.007) |
| Monthly wage (log) | 7.415 | 0.511 | 4495 | 7.405 | 0.511 | 5587 | 0.010 (0.011) |
| Hourly wage (log) | 2.407 | 0.425 | 4464 | 2.398 | 0.422 | 5548 | 0.009 (0.009) |
| | **Panel B—Other variables, FQP sample** | | | | | | |
| | **Main sample of counties (58)** | | | **All counties (96)** | | | |
| | **Mean** | **SD** | ***N*** | **Mean** | **SD** | ***N*** | **Diff (SE)** |
| Female (1 = yes) | 0.486 | 0.500 | 500 | 0.495 | 0.500 | 675 | −0.009 (0.029) |
| SES$^H$ (1 if household is high-SES) | 0.372 | 0.484 | 500 | 0.344 | 0.475 | 675 | 0.028 (0.028) |
| Preschool duration (in integer years) | 1.765 | 1.023 | 486 | 1.685 | 1.029 | 654 | 0.080 (0.068) |
| Preschool attendance (1 if duration > 0) | 0.840 | 0.367 | 486 | 0.823 | 0.382 | 654 | 0.017 (0.024) |
| Preschool completion (1 if duration $\geq$ 3) | 0.272 | 0.445 | 486 | 0.243 | 0.429 | 654 | 0.029 (0.028) |

*Note:* LFS and FQP data; Samples include all observations for which information on outcomes is available. The last column features differences in averages across samples (SE in parentheses). Drivers (not reported) not available for larger sample of counties. Significance levels: ** = 5%.
*Source:* LFS and FQP data.

**TABLE A6** | Cohort composition of samples.

| | **Survey** | | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **FQP** | **LFS** | | | | | | | | | | | | | | | | | | | |
| | **1993** | **2003** | **2004** | **2005** | **2006** | **2007** | **2008** | **2009** | **2010** | **2011** | **2012** | **2013** | **2014** | **2015** | **2016** | **2017** | **2018** | **Age mean** | **Age SD** | **2003–18** | **2003–18 aged 40** |
| **Cohort of birth** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** | **(9)** | **(10)** | **(11)** | **(12)** | **(13)** | **(14)** | **(15)** | **(16)** | **(17)** | **(18)** | **(19)** | **(20)** | **(21)** |
| 1964 | 0.112 | **0.100** | **0.100** | **0.108** | **0.095** | **0.101** | **0.098** | **0.107** | 0.101 | 0.112 | 0.104 | 0.097 | 0.104 | 0.102 | 0.102 | 0.100 | 0.106 | 42.124 | 2.092 | 0.103 | 0.065 |
| 1965 | 0.102 | **0.100** | **0.097** | **0.102** | **0.109** | **0.099** | **0.100** | **0.102** | **0.102** | 0.101 | 0.107 | 0.100 | 0.099 | 0.095 | 0.107 | 0.103 | 0.104 | 41.727 | 2.368 | 0.102 | 0.076 |
| 1966 | 0.106 | **0.103** | **0.093** | **0.098** | **0.100** | **0.097** | **0.095** | **0.105** | **0.104** | **0.098** | 0.095 | 0.105 | 0.101 | 0.097 | 0.098 | 0.103 | 0.108 | 41.341 | 2.678 | 0.100 | 0.087 |
| 1967 | 0.088 | **0.090** | **0.096** | **0.099** | **0.091** | **0.100** | **0.097** | **0.099** | **0.095** | **0.099** | **0.095** | 0.095 | 0.095 | 0.097 | 0.111 | 0.094 | 0.093 | 40.919 | 2.895 | 0.097 | 0.095 |
| 1968 | 0.084 | **0.098** | **0.093** | **0.092** | **0.096** | **0.096** | **0.092** | **0.096** | **0.096** | **0.101** | **0.098** | **0.101** | 0.098 | 0.103 | 0.101 | 0.093 | 0.100 | 40.453 | 3.178 | 0.097 | 0.105 |
| 1969 | 0.093 | 0.095 | **0.097** | **0.101** | **0.096** | **0.095** | **0.104** | **0.101** | **0.097** | **0.094** | **0.098** | **0.103** | **0.097** | 0.101 | 0.092 | 0.105 | 0.094 | 40.407 | 3.111 | 0.098 | 0.108 |
| 1970 | 0.098 | 0.107 | 0.100 | **0.100** | **0.102** | **0.096** | **0.100** | **0.100** | **0.100** | **0.099** | **0.099** | **0.104** | **0.106** | **0.102** | 0.096 | 0.100 | 0.097 | 40.426 | 3.058 | 0.100 | 0.112 |
| 1971 | 0.097 | 0.104 | 0.111 | 0.099 | **0.104** | **0.102** | **0.105** | **0.102** | **0.104** | **0.098** | **0.106** | **0.112** | **0.102** | **0.099** | **0.099** | 0.101 | 0.099 | 40.301 | 3.023 | 0.103 | 0.117 |
| 1972 | 0.100 | 0.101 | 0.102 | 0.099 | 0.108 | **0.103** | **0.106** | **0.098** | **0.097** | **0.102** | **0.099** | **0.096** | **0.102** | **0.104** | **0.095** | **0.101** | 0.100 | 40.161 | 3.062 | 0.101 | 0.117 |
| 1973 | 0.120 | 0.101 | 0.111 | 0.102 | 0.100 | 0.112 | **0.103** | **0.091** | **0.103** | **0.095** | **0.100** | **0.088** | **0.096** | **0.100** | **0.100** | **0.099** | **0.100** | 40.058 | 3.105 | 0.100 | 0.116 |
| N | 2432 | 6448 | 6420 | 5389 | 5141 | 5490 | 5180 | 7513 | 7468 | 7913 | 7275 | 6819 | 7199 | 7248 | 7198 | 6818 | 7089 | | | 106,608 | 65,125 |

*Note:* Columns (18), (19) report respectively the mean and the standard deviation of the age for each cohort of individuals aged 35–45 in the LFS sample. Column (20) reports the composition by cohort of birth of the LFS sample (all survey years 2003–2018). Column (21) restricts the LFS sample to individuals aged 35–45.

that contribute to the estimation sample in each survey year. The sample belonging to these cohorts consists of 65,125 individuals, who are aged on average ˜40 years old in the survey year. As expected, cohorts 1964–1967 are observed only when aged 40 years or later. Specific controls for age structure in the regression models help address potential confounding cohort composition effects.

**TABLE A7** | Earnings and education advantage by father occupational status.

| Paternal socio-occupational status | Monthly wage (log) | | | Years of education | | |
|---|---|---|---|---|---|---|
| | Mean | Standard deviation | *N* | Mean | Standard deviation | *N* |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Father socio-occupational status* | | | | | | |
| Manual worker | −0.112 | 0.454 | 18,935 | −1.166 | 2.426 | 25,631 |
| Farmer | −0.060 | 0.448 | 2338 | −0.056 | 2.459 | 3555 |
| Non-manual worker | −0.027 | 0.468 | 5846 | −0.142 | 2.648 | 7641 |
| $SES^L$ | *−0.089* | *0.458* | *27,119* | *−0.846* | *2.524* | *36,827* |
| Artisan | 0.037 | 0.488 | 5471 | 0.274 | 2.734 | 8287 |
| Low-grade professional | 0.086 | 0.475 | 8599 | 0.914 | 2.669 | 11,186 |
| High-grade professional | 0.244 | 0.504 | 6096 | 2.173 | 2.565 | 8593 |
| $SES^H$ | *0.120* | *0.495* | *20,166* | *1.110* | *2.762* | *28,066* |

*Note:* LFS sample; Means and standard deviation of log monthly wage and years of education by father occupational status are residuals from a regression controlling for age, cohort and year of survey fixed effects and their interactions.

**TABLE A8** | Primary child care mode, children below 6 years old, 1982.

| Family SES | All | | | $SES^H = 0$ | | | $SES^H = 1$ | | |
|---|---|---|---|---|---|---|---|---|---|
| Mother currently working | All | No | Yes | All | No | Yes | All | No | Yes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | Panel A—Children aged 0 to 35 months old | | | | | | | | |
| Children cared for at home by: | 67.7% | 91.5% | 36.3% | 70.3% | 91.9% | 37.5% | 62.4% | 90.6% | 34.5% |
| Their mother | 61.2% | 90.9% | 22.1% | 64.3% | 91.5% | 22.8% | 55.1% | 89.6% | 21.0% |
| A family member | 4.8% | 0.4% | 10.6% | 5.1% | 0.4% | 12.2% | 4.3% | 0.6% | 8.0% |
| Another person | 1.7% | 0.2% | 3.7% | 1.0% | 0.1% | 2.5% | 3.0% | 0.4% | 5.6% |
| Children cared outside the home by: | 24.5% | 1.1% | 55.3% | 22.3% | 0.9% | 54.9% | 28.9% | 1.6% | 55.8% |
| A day care centre | 3.8% | 0.3% | 8.3% | 2.9% | 0.2% | 7.0% | 5.5% | 0.5% | 10.4% |
| A registered childminder | 10.4% | 0.4% | 23.6% | 8.9% | 0.2% | 22.2% | 13.3% | 0.7% | 25.7% |
| A family member | 6.7% | 0.3% | 15.1% | 7.1% | 0.3% | 17.5% | 5.9% | 0.3% | 11.3% |
| Another person | 3.6% | 0.1% | 8.2% | 3.3% | 0.1% | 8.1% | 4.3% | 0.2% | 8.4% |
| Children in preschool | 7.8% | 7.4% | 8.4% | 7.4% | 7.2% | 7.6% | 8.7% | 7.8% | 9.6% |
| *N* | 36,518 | 20,743 | 15,775 | 24,269 | 14,656 | 9613 | 12,249 | 6087 | 6162 |
| | Panel B—Children aged 36 to 71 months old | | | | | | | | |
| Children in preschool | 85.9% | 84.2% | 87.9% | 84.4% | 83.0% | 86.6% | 88.4% | 86.8% | 89.7% |
| Mode of care if not in preschool: | | | | | | | | | |
| At home by their mother | 73.7% | 98.1% | 35.0% | 77.1% | 98.2% | 38.0% | 65.7% | 97.7% | 29.7% |
| At home by a family member | 6.4% | 0.5% | 15.7% | 6.0% | 0.6% | 16.0% | 7.3% | 0.3% | 15.2% |
| At home by another person | 1.8% | 0.3% | 4.3% | 1.1% | 0.2% | 2.8% | 3.4% | 0.4% | 6.8% |
| Outside the home by a day care centre | 1.6% | 0.5% | 3.2% | 1.2% | 0.5% | 2.4% | 2.5% | 0.5% | 4.7% |
| Outside the home by a registered childminder | 6.3% | 0.3% | 15.9% | 5.3% | 0.2% | 14.8% | 8.7% | 0.5% | 17.8% |
| Outside the home by a family member | 7.3% | 0.4% | 18.3% | 7.0% | 0.4% | 19.5% | 8.0% | 0.5% | 16.4% |
| Outside the home by another person | 3.0% | 0.0% | 7.6% | 2.3% | 0.0% | 6.6% | 4.5% | 0.1% | 9.4% |
| *N* | 35,126 | 19,226 | 15,900 | 22,292 | 13,250 | 9042 | 12,834 | 5976 | 6858 |

*Source:* Authors' computation based on INSEE Family Survey (1982).

Earnings for self-employed workers are not observable (only imperfect measures of income are) or do not correspond to a standard working contract (implying that unobservable hours of work should be included among controls). We address these limitations by narrowing the empirical analysis to employed respondents. There are 47,428 employees in the sample, for which we observe log monthly wages (converted in PPP 2015 Euros), as well as the conditions of their contract (employed part-time and/or in the public sector). Monthly and hourly estimated wages for males are respectively 17% and 9% larger than the corresponding wages for female workers.

The FQP sample gathers individuals born in cohorts 1964–1973 in the counties for which information on preschool drivers is available. FQP data report information on preschool duration, measured in years. Labour market outcomes, earnings and education are reported as well in the survey. The reference sample is, however, too young in 1992 (when aged 19 to 28 years old, that is, 24.47 years old on average in our data) to provide reliable measures of life-time earnings of labour market opportunities. For this reason, FQP does not allow for the construction of reliable inference on reduced form and second stage estimates. Both LFS and FQP data report occupational information for both parents when the respondent

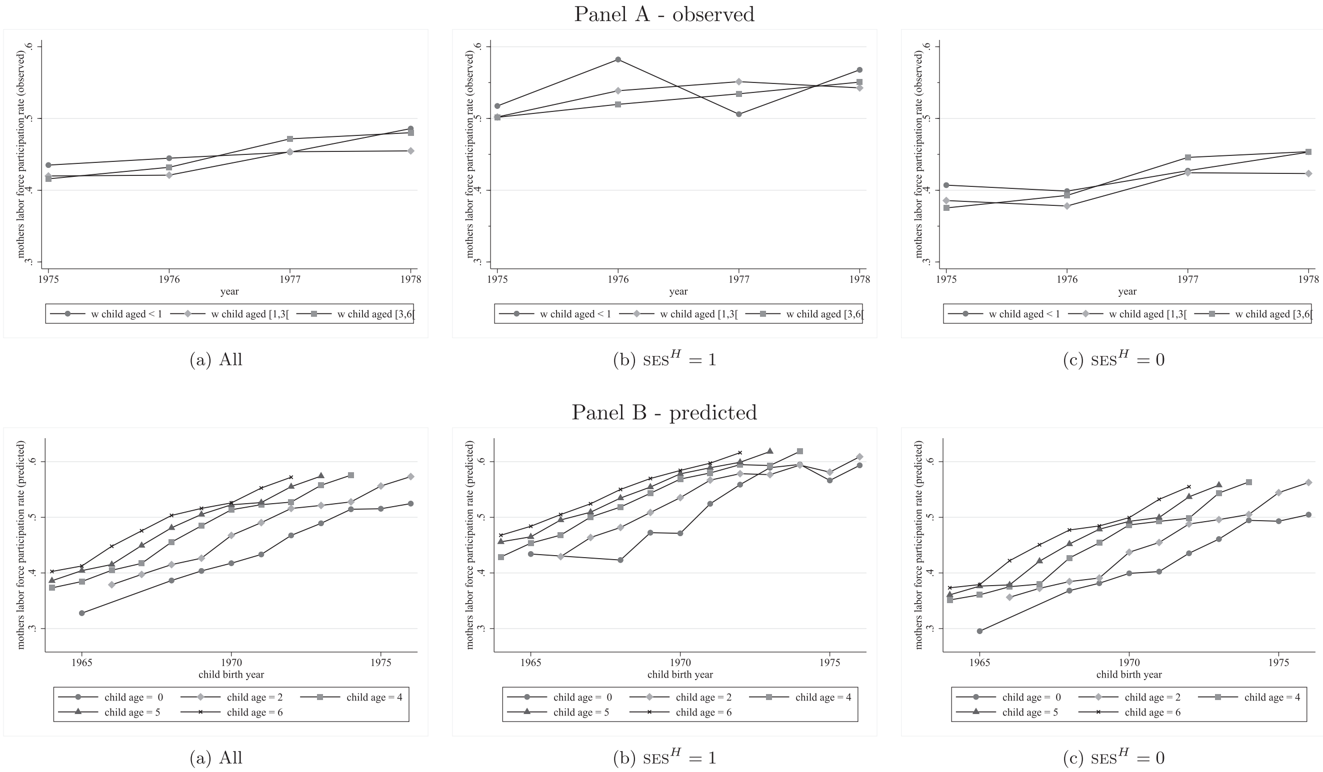
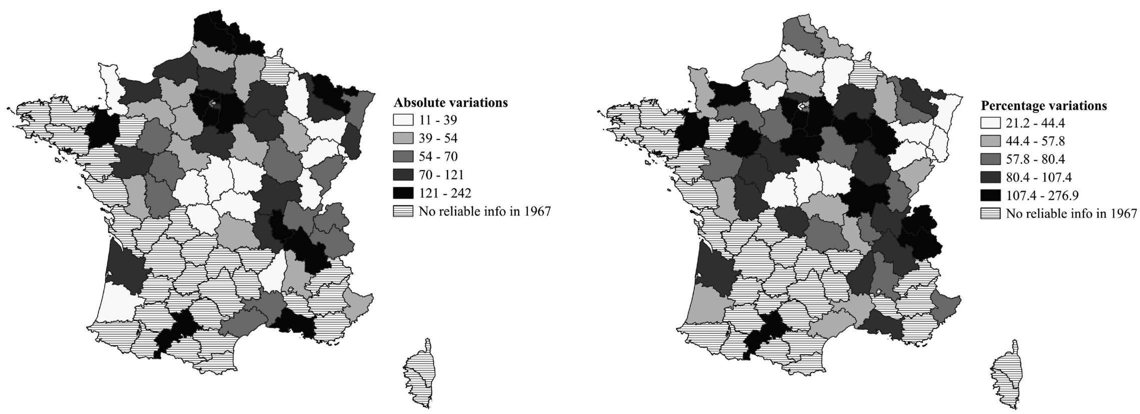



**FIGURE A3** | Mothers' labour force participation. Authors' computation based on the LFS data for the years 1965 and 1968–1978.




(a) Absolute Value (b) Percentage

**FIGURE A4** | Variation in supply of preschool by county. Authors' computation based on the National School Register. Maps have separate legends, colours based on quintiles of growth rates.

was in education (as late as 1993). Occupational status for mothers is often missing in the data, making it difficult to distinguish cases in which mothers were unemployed from genuine missing observations. Our analysis is made conditional on father SES.

### Father SES Groups

Besides cohort and county of birth, both FQP and LFS report the socio-economic status of the family of origin of the respondent. Our focus is on the occupation of the respondent's father. An alternative, widely used measure of parental background is education of the father and the mother. These are available in FQP but not in LFS and are hence discarded. We further select the father occupation as the main circumstance of origin for at least three reasons. The first reason is that the mother occupation is missing for all mothers out of the labour force. The second reason is data reliability of the reported information. Male workers (fathers) display a relatively stable occupation pattern over the life cycle, implying that the employment status measured in LFS and FQP when the respondent (children) is about 20 years old is a good proxy for the employment status when the children were in preschool age (i.e., when we would like to measure availability of intra-household resources). The third reason is that father SES is a good predictor of father's earning potential and hence income affluence of the family of origin.

LFS and FQP report five categories for father SES. We group them into two groups. The first group, denoted $\text{SES}^L$ is the low socio-economic background and gathers fathers occupied as farmers, manual and non-manual workers. This group corresponds to 56%–58% of the LFS sample. The second group is denoted $\text{SES}^H$ is the high SES status and gathers fathers

**TABLE A9** | Test of independence on controls.

| Test | Birth region | SES | Birth region if low-edu | Birth region and SES | Birth region and SES if low-edu |
|---|---|---|---|---|---|
| Cohort | (1) | (2) | (3) | (4) | (5) |
| 1964 | 0.806 | 0.002 | 0.896 | 0.895 | 0.859 |
| | (0.619) | (0.967) | (0.524) | (0.573) | (0.642) |
| 1965 | 0.663 | 0.362 | 0.777 | 1.069 | 1.070 |
| | (0.798) | (0.548) | (0.692) | (0.375) | (0.374) |
| 1966 | 0.610 | 1.043 | 0.330 | 0.590 | 0.541 |
| | (0.804) | (0.307) | (0.986) | (0.884) | (0.966) |
| 1967 | 0.840 | 2.410 | 0.603 | 0.856 | 0.728 |
| | (0.601) | (0.121) | (0.810) | (0.632) | (0.789) |
| 1968 | 0.780 | 0.497 | 1.291 | 0.866 | 1.221 |
| | (0.690) | (0.481) | (0.203) | (0.638) | (0.207) |
| 1969 | 0.968 | 2.354 | 0.402 | 0.943 | 0.931 |
| | (0.483) | (0.125) | (0.972) | (0.537) | (0.559) |
| 1970 | 1.287 | 0.017 | 0.716 | 1.067 | 0.685 |
| | (0.220) | (0.896) | (0.759) | (0.376) | (0.849) |
| 1971 | 1.288 | 0.245 | 1.535 | 1.212 | 1.056 |
| | (0.199) | (0.621) | (0.073) | (0.220) | (0.384) |
| 1972 | 0.958 | 0.774 | 0.775 | 0.811 | 0.882 |
| | (0.489) | (0.379) | (0.701) | (0.711) | (0.632) |
| 1973 | 0.410 | 2.380 | 0.696 | 0.738 | 0.992 |
| | (0.964) | (0.123) | (0.777) | (0.798) | (0.472) |
| All | 1.200 | 0.519 | 0.646 | 1.222 | 0.696 |
| | (0.297) | (0.471) | (0.829) | (0.244) | (0.863) |

*Note:* For each cohort, the first row shows the corrected Pearson F statistics, and the associated *p*-value is reported on the second row. The null hypothesis is that the distribution of the variables given in columns is similar across the FQP and LFS samples for years indicated in each row. Low education level corresponds to primary school degree or less. Father SES is a dummy for high SES, as defined above.

occupied as artisans, lower-grade and higher-grade professionals (including white collars). The same groups are observed in FQP. Balancing tests reveal that differences in group proportions in FQP and LFS samples are marginal in size and never significant. In some robustness checks, we consider a finer partition where the $SES^H$ group is further divided to distinguish high-grade professional from the rest.

The labelling of the group follows the ranking of the underlying father occupations in terms of child earnings and education conditional on group belonging (see, for instance, Dumas and Lefranc [27]). Table A7 highlights patterns of economic advantage attributable to each group as measured by child outcomes. A negative (positive) sign on the coefficients reported in the table stands for average group income that is below (above) the sample average, after correcting for age, cohort, year and place of birth fixed effects. Children of manual workers are by far the lower performers in terms of average earnings and education. Also, children with fathers occupied as non-manual workers and farmers underperform on average. Children with fathers in these occupational categories belong to the lower class. Conversely, we find strong evidence that children with high SES parents display higher outcomes than the average and notably with respect to the low-SES group. Table A7 reveals a form of inequality between individuals that is attributable to differences in the background of origin rather than individual choices. Arguably, such inequalities may be seen as intolerable from a social perspective and as such deserve compensation [28].

### Mothers Labour Force Participation

This subsection examines levels and trends in mothers' labour force participation during the preschool expansion period. Assessing the extent of labour force participation is important for understanding the alternative child care arrangement that children would have experienced if and when they were not enrolled in preschool. If mothers' participation is low, one may presume that children are likely to be cared for by their mother when not enrolled in preschool. This information is all the more relevant given that, to the best of our knowledge, nationally representative information is missing regarding the distribution of child care modes in France during the 1960s and 1970s, and only available from the 1980s.

Unfortunately, the labour force participation rate of mothers cannot be consistently assessed by cohort and age across our full sample but only towards the end of the period. For years 1975–1978, labour force surveys (LFS) provide both the labour force participation status and the number of children of women for detailed child age brackets. The brackets are the following: children below age 1, between 1 and 3 years old, and between 3 and 6 years old. Hence this allows us to assess the labour force participation rate of the mothers of children born between 1969 and 1978, at different ages ranging from 0 to 6. Based on the occupation of the household head, we can also identify families in the high vs. low SES groups.[44] Results are shown in Figure A3, Panel A. Between 1975 and 1978, the labour force participation of women with children aged less than 6 is always below 50%, slightly increasing from about 42% to around 46%. It shows minor and non-systematic variation with child age.

These data suggest that the most likely alternative to child care during the expansion period would have been maternal care at home. In the expansion period, other modes of care would have included child care provided by relatives or professional nannies (*nourrices* or *assistantes maternelles*) and, for a smaller fraction of children, attendance at a day care centre. However, as previously discussed, France experienced a shortage of non-maternal alternative care modes, as revealed by data on child care mode for the early 1980s presented in Table A8. Participation rates are higher for high SES families (between 50% and 55%) than for low SES families (between 38% and 48%). This suggests that child care by the mother

**TABLE A10** | TSTSLS estimates by gender.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Panel A — Male | | | | |
| $\widehat{Dur}$ | 0.002 | −0.005 | 0.037 | −0.042 | −0.081 | −0.032 | 0.063 | 0.076 |
| | (0.058) | (0.047) | (0.039) | (0.059) | (0.067) | (0.051) | (0.070) | (0.081) |
| $\widehat{Dur}^{H}$ | 0.078*** | −0.057*** | 0.006 | 0.040 | 0.058* | −0.013 | 0.081** | 0.146*** |
| | (0.021) | (0.017) | (0.013) | (0.029) | (0.034) | (0.024) | (0.035) | (0.036) |
| $\text{SES}^{H}$ | −0.138*** | 0.108*** | −0.012 | 0.117* | 0.092 | −0.092* | 0.120 | −0.046 |
| | (0.045) | (0.037) | (0.031) | (0.065) | (0.076) | (0.052) | (0.078) | (0.083) |
| *N* | 31,686 | 30,369 | 28,381 | 23,096 | 23,236 | 31,682 | 31,682 | 31,682 |
| *Coeff. linear combination* | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur}^{H}$ | 0.080 | −0.062 | 0.043 | −0.001 | −0.023 | −0.045 | 0.145* | 0.222** |
| | (0.062) | (0.051) | (0.046) | (0.071) | (0.079) | (0.057) | (0.081) | (0.094) |
| | | | | Panel B — Female | | | | |
| $\widehat{Dur}$ | −0.055 | −0.015 | 0.055 | 0.029 | 0.057 | −0.083 | 0.024 | 0.001 |
| | (0.091) | (0.050) | (0.057) | (0.085) | (0.092) | (0.058) | (0.063) | (0.070) |
| $\widehat{Dur}^{H}$ | 0.185*** | −0.071*** | −0.057 | 0.109*** | 0.080** | −0.036 | 0.085*** | 0.129*** |
| | (0.035) | (0.020) | (0.052) | (0.035) | (0.039) | (0.035) | (0.032) | (0.028) |
| $\text{SES}^{H}$ | −0.335*** | 0.135*** | 0.090 | −0.034 | 0.022 | −0.031 | 0.113 | 0.016 |
| | (0.075) | (0.045) | (0.106) | (0.077) | (0.082) | (0.077) | (0.071) | (0.066) |
| *Inv. Mills ratio* | | | | 0.374*** | 0.116*** | | | |
| | | | | (0.031) | (0.034) | | | |
| N | 33,214 | 28,735 | 26,347 | 23,011 | 23,185 | 33,211 | 33,211 | 33,211 |
| *Coeff. linear combination* | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur}^{H}$ | 0.130 | −0.086 | −0.002 | 0.138 | 0.137 | −0.119 | 0.109 | 0.129 |
| | (0.099) | (0.057) | (0.064) | (0.092) | (0.099) | (0.075) | (0.078) | (0.082) |
| *Test: coeff. difference between Panels A and B* | | | | | | | | |
| $\Delta\ \widehat{Dur}$ | 0.056 | 0.009 | −0.018 | −0.071 | −0.138 | 0.050 | 0.039 | 0.075 |
| | (0.108) | (0.069) | (0.069) | (0.103) | (0.114) | (0.078) | (0.094) | (0.107) |
| $\Delta\ \widehat{Dur}^{H}$ | −0.107*** | 0.014 | 0.063 | −0.069 | −0.022 | 0.023 | −0.003 | 0.018 |
| | (0.040) | (0.026) | (0.053) | (0.045) | (0.052) | (0.043) | (0.048) | (0.046) |

*Note:* All models control for individual age, father SES, cohort and county fixed effects, regional trends, and preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parentheses; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS data.

would have been a slightly more frequent alternative to preschool for low SES children.

There is no straightforward way to extend this analysis to the full period since, for earlier LFS waves, the data only indicate, for each woman, the total number of children below age 16, without further information on child age. In this case, we rely on an indirect approach. First, for each child cohort of birth, we compute the distribution of birth cohorts of their mothers using LFS data. Second, for each observation year and for each woman's birth cohort, we compute the participation rate among women with children aged less than 16. Third, for each observation year and each child birth cohort, we predict the mothers' participation rate using a weighted average of the cohort-specific participation rate of women with children, using the weights obtained in the first step.

Results are given in Figure A3, Panel B. Comparing the true participation rates obtained for the 1975–1978 observation years with the prediction for child birth cohorts 1973–1976 when aged 0 and 2 indicates that our prediction tends to overestimate the participation rate by 5 to 10 percentage points. This is most likely due to the inclusion of women with older children. The predicted data also indicate a clear upward trend in the participation of mothers between 1965 and 1975 and a positive SES gradient. Assuming that the over-estimation bias is constant, the likely participation rate for the mothers of birth cohort 1965, at age 2–4, would have been around or below 40% for high SES families and around 30% for low SES ones. This confirms that home care by the mother would have been the most likely alternative to preschool.

## Additional Results

### A Difference-In-Differences Estimator Robust to Dynamic Heterogeneity

To address possible treatment heterogeneity biases in our TWFE estimations, we implement a simple Difference-in-Differences (DiD) estimator of the effect of the preschool expansion. It compares the change in outcomes between two groups of cohorts — the cohorts entering preschool before the implementation of the National Plan (i.e., born in 1964–1966) versus those born after the Plan completion (born in 1971–1973) — and across two groups of counties — the counties that experienced a large growth in preschool supply (the *treatment* counties) versus those counties

**TABLE A11** | Robustness check—Reduced form estimates controlling for females' selection into the labour market.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Panel A—Homogeneous effect | | | | | | | | |
| Preschool supply ($S_{cd}$) | 0.012 | −0.022 | 0.026 | 0.017 | 0.010 | −0.046** | 0.050* | 0.059** |
| | (0.041) | (0.025) | (0.020) | (0.038) | (0.043) | (0.022) | (0.028) | (0.026) |
| $SES^{H}$ | 0.038*** | −0.016*** | −0.008* | 0.195*** | 0.198*** | −0.121*** | 0.298*** | 0.281*** |
| | (0.007) | (0.004) | (0.004) | (0.004) | (0.004) | (0.005) | (0.007) | (0.007) |
| *Inv. Mills ratio* | | | | 0.256*** | 0.039** | | | |
| | | | | (0.015) | (0.018) | | | |
| N | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 |
| Panel B—Heterogeneous effect by family background | | | | | | | | |
| Preschool supply ($S_{cd}$) | −0.010 | −0.012 | 0.031 | 0.007 | 0.000 | −0.043* | 0.037 | 0.038 |
| | (0.041) | (0.025) | (0.020) | (0.038) | (0.043) | (0.022) | (0.028) | (0.026) |
| $S_{cd} \times SES^{H}$ | 0.042*** | −0.019*** | −0.009 | 0.019*** | 0.018** | −0.005 | 0.024*** | 0.041*** |
| | (0.007) | (0.004) | (0.009) | (0.007) | (0.008) | (0.008) | (0.008) | (0.005) |
| $SES^{H}$ | 0.002 | 0.000 | −0.000 | 0.179*** | 0.182*** | −0.117*** | 0.278*** | 0.247*** |
| | (0.006) | (0.004) | (0.007) | (0.007) | (0.008) | (0.008) | (0.008) | (0.008) |
| *Inv. Mills ratio* | | | | 0.257*** | 0.039** | | | |
| | | | | (0.015) | (0.018) | | | |
| N | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 |
| *Coeff. linear combination* | | | | | | | | |
| $S_{cd} + S_{cd} \times SES^{H}$ | 0.032 | −0.031 | 0.022 | 0.026 | 0.019 | −0.048** | 0.061** | 0.078*** |
| | (0.040) | (0.024) | (0.020) | (0.038) | (0.043) | (0.023) | (0.029) | (0.026) |

*Note:* All models control for individual age, father SES, cohort and county fixed effects, regional trends, and preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parentheses; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS data.

**TABLE A12** | Robustness check—TSTSLS estimates controlling for females' selection into the labour market.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\widehat{Dur}$ | −0.029 | −0.009 | 0.045 | 0.002 | −0.007 | −0.057 | 0.042 | 0.037 |
| | (0.066) | (0.040) | (0.034) | (0.041) | (0.043) | (0.041) | (0.052) | (0.059) |
| $\widehat{Dur}^{H}$ | 0.133*** | −0.064*** | −0.022 | 0.063*** | 0.059** | −0.024 | 0.083*** | 0.137*** |
| | (0.024) | (0.015) | (0.029) | (0.019) | (0.025) | (0.027) | (0.031) | (0.027) |
| $SES^{H}$ | −0.240*** | 0.120*** | 0.033 | 0.061 | 0.074 | −0.062 | 0.116* | −0.015 |
| | (0.053) | (0.033) | (0.059) | (0.042) | (0.051) | (0.059) | (0.066) | (0.061) |
| *Inv. Mills ratio* | | | | 0.258*** | 0.041** | | | |
| | | | | (0.017) | (0.019) | | | |
| *N* | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 |
| *Coeff. linear combination* | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur} \times SES^{H}$ | 0.104 | −0.073* | 0.023 | 0.066 | 0.053 | −0.081 | 0.125** | 0.174*** |
| | (0.071) | (0.043) | (0.041) | (0.045) | (0.043) | (0.051) | (0.062) | (0.067) |

*Note:* All models controls for individual age, father SES, cohort and county fixed effects, regional trends, and preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parentheses and corrected for two-step estimation; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS and FQP data.

experiencing a relatively weak growth in preschool supply (the *control* counties). Our DiD estimates are consistent with the results obtained using the TWFE estimator, although effects appear less precisely estimated, as discussed below.

To define control and treatment counties, we follow Havnes and Mogstad [29] and Havnes and Mogstad [31]. We clean county-cohort preschool supply from the effects of common cohort trends and county-level time-varying preschool demand drivers. We then compute for each

**TABLE A13** | Robustness check — Using reliable counties with missing census information.

| Dependent variable | Labour market participation: Employed | Unemployed | Part-time | Wage (log): Hourly | Monthly | Education level: ≤ Primary | ≥ Bac | ≥ Bac + 2 | Preschool: *Dur* |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Panel A — Reduced form and first stage estimates | | | | | | | | | |
| Preschool supply ($S_{cd}$) | −0.032 | 0.002 | 0.012 | 0.015 | 0.011 | −0.038** | 0.032 | 0.026 | 0.824** |
| | (0.036) | (0.023) | (0.022) | (0.035) | (0.039) | (0.019) | (0.025) | (0.025) | (0.325) |
| $S_{cd} \times \text{SES}^H$ | 0.041*** | −0.019*** | −0.011 | 0.020*** | 0.020** | −0.005 | 0.025*** | 0.040*** | −0.065 |
| | (0.007) | (0.004) | (0.008) | (0.007) | (0.008) | (0.007) | (0.008) | (0.005) | (0.057) |
| $\text{SES}^H$ | 0.004 | −0.001 | 0.003 | 0.179*** | 0.180*** | −0.117*** | 0.276*** | 0.249*** | 0.209*** |
| | (0.006) | (0.004) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.008) | (0.068) |
| N | 68,330 | 62,278 | 57,696 | 48,621 | 48,950 | 68,323 | 68,323 | 68,323 | 2525 |
| F-test | | | | | | | | | 3.733 |
| F-*p* | | | | | | | | | 0.029 |
| Panel B — Second stage estimates | | | | | | | | | |
| $\widehat{Dur}$ | −0.044 | 0.012 | 0.016 | 0.019 | 0.024 | −0.052 | 0.042 | 0.030 | |
| | (0.053) | (0.027) | (0.025) | (0.038) | (0.043) | (0.032) | (0.040) | (0.043) | |
| $\widehat{Dur}^H$ | 0.129*** | −0.060*** | −0.034 | 0.070*** | 0.072*** | −0.029 | 0.093*** | 0.140*** | |
| | (0.024) | (0.014) | (0.025) | (0.020) | (0.024) | (0.027) | (0.031) | (0.026) | |
| $\text{SES}^H$ | −0.228*** | 0.109*** | 0.062 | 0.043 | 0.041 | −0.053 | 0.094 | −0.019 | |
| | (0.051) | (0.030) | (0.051) | (0.044) | (0.051) | (0.057) | (0.065) | (0.058) | |
| *N* | 68,330 | 62,278 | 57,696 | 48,621 | 48,950 | 68,323 | 68,323 | 68,323 | |
| *Coeff. linear combination* | | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur}^H$ | 0.085 | −0.047 | −0.018 | 0.089** | 0.095** | −0.080* | 0.135** | 0.169*** | |
| | (0.060) | (0.031) | (0.029) | (0.044) | (0.046) | (0.042) | (0.052) | (0.053) | |

*Note:* All models control for individual age, father SES, cohort and county fixed effects, regional trends; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parenthesis and corrected for two-step estimation; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS and FQP data for all 64 counties for which reliable information on preschool supply is available.

county the growth rate in this residualised measure of preschool supply between the years 1967–1969 and the years 1974–1976 (i.e., 3 years after the birth of the cohorts of interest). A county belongs to the treatment group if its residualised preschool supply growth rate is above the 75th percentile of the distribution across counties. It belongs to the control group if its growth rate is below the median of the distribution. Counties with growth rates between $p_{50}$ and $p_{75}$ are omitted. The choice of a relatively high threshold to consider a county as treated responds to the need of identifying only large variations in preschool supply as actually relevant for identification. The fact that some counties (with a preschool supply growth above the median level but below the 75th percentile) are dropped, reflects the need to differentiate treated from control units. In operating these cuts, we loose a significant share of our sample both in terms of counties and *interim* years.

Using previous notations, our homogeneous-effect DiD model comes as follows:

$$y_{icdt} = \gamma_0 + \gamma_1 Treat_d + \gamma_2 Post_c + \gamma_3 Treat_d \times Post_c + \delta_1 \text{SES}_i + \theta_c + \theta_d + \theta_t + \theta_{tc} + \varepsilon_i,$$

where $Treat_d \in \{0, 1\}$ indicates whether county $d$ is treated and $Post_c \in \{0, 1\}$ indicates whether cohort $c$ was of preschool age after the implementation of the national plan. As in all DiD settings, the coefficient $\gamma_3$ can, under the common trends assumption, be interpreted as the causal effect of experiencing a high rate of preschool expansion (treated) compared to low-expansion counties. The additional FE control for county, cohort and age-specific differences. $\theta_{tc}$ denote survey-year-specific linear cohort trends. These effects reinforce identification validity by allowing for some heterogeneity within the treatment/control and pre/post groups. The model is estimated on the sample of male respondents only, in order to avoid estimating the selection model for female labour market participation on data with limited size and variability.

Additionally, the model is extended by allowing for heterogeneous effects of the main treatment indicator by considering a *fully interacted* version of the model estimated above with the father high-SES indicator.

Table A16 reports the estimated coefficients. Homogenous effects ($Treat \times Post$ regressor) are small and only marginally significant, similar to effects in Table 4 Panel A. One exception is for education variables. It is found that preschool expansion leads to a significant increase in the probability of achieving a low level of education (the probability rises by 4.8 pp), whereas no effect on the chances of achieving a diploma or post-diploma qualifications. Heterogenous effects are captured by the effects of coefficients $Treat \times Post$ and $Treat \times Post \times \text{SES}^H$. For employment and unemployment margins, preschool expansion has no significant impact on low-SES children, whereas for high-SES it leads to a rise in employment by 3.4 pp and a reduction in unemployment of 1.5 pp, the figures being similar to the estimates of Table 4 but not statistically significant. DiD estimates of the effect on earnings are qualitatively similar to those in Table 4, with significant and sizable effects (about 8 pp) attributed to high-SES treated individuals.

To conclude, the DiD approach implemented here provides estimates that are robust to dynamic heterogeneity, but at the cost of excluding observations and aggregating detailed information on preschool supply into a binary treatment variable identifying fast preschool expansion. Results obtained here are overall consistent with those reported in the main text, although at the cost of lower estimation precision.

**TABLE A14** | Robustness check — urban versus rural counties.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | | Preschool | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Employed** | **Unemployed** | **Part-time** | **Hourly** | **Monthly** | **≤ Primary** | **≥ Bac** | **≥ Bac + 2** | ***Dur*** | ***Dur*** |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** | **(9)** | **(10)** |
| | Panel A — Reduced form and first stage estimates | | | | | | | | | |
| Preschool supply ($S_{cd}$) | −0.080** | 0.024 | 0.033 | −0.028 | −0.005 | −0.002 | 0.038 | 0.064** | 0.858** | 0.734** |
| | (0.037) | (0.028) | (0.027) | (0.047) | (0.053) | (0.024) | (0.033) | (0.027) | (0.405) | (0.324) |
| $S_{cd} \times SES^H$ | 0.045*** | −0.021*** | −0.012 | 0.022*** | 0.023*** | −0.007 | 0.028*** | 0.042*** | −0.056 | −0.043 |
| | (0.007) | (0.004) | (0.007) | (0.007) | (0.008) | (0.006) | (0.007) | (0.005) | (0.068) | (0.053) |
| $SES^H$ | 0.003 | 0.001 | 0.000 | 0.175*** | 0.178*** | −0.117*** | 0.275*** | 0.244*** | 0.156** | 0.180** |
| | (0.007) | (0.005) | (0.007) | (0.007) | (0.008) | (0.007) | (0.008) | (0.008) | (0.074) | (0.069) |
| $S_{cd} \times$ Rural | | | | | | | | | | 0.487 |
| | | | | | | | | | | (1.454) |
| *N* | 57,349 | 52,252 | 48,430 | 41,006 | 41,280 | 57,343 | 57,343 | 57,343 | 2133 | 2405 |
| F-test | | | | | | | | | 2.400 | 2.641 |
| F-*p* | | | | | | | | | 0.100 | 0.080 |
| | Panel B — Second stage estimates | | | | | | | | | |
| $\widehat{Dur}$ | −0.074 | 0.019 | 0.034 | −0.023 | 0.005 | −0.006 | 0.058 | 0.096 | | |
| | (0.055) | (0.036) | (0.035) | (0.039) | (0.048) | (0.031) | (0.062) | (0.083) | | |
| $\widehat{Dur}^H$ | 0.136*** | −0.065*** | −0.032 | 0.070*** | 0.076*** | −0.025 | 0.103*** | 0.159*** | | |
| | (0.028) | (0.016) | (0.025) | (0.019) | (0.024) | (0.024) | (0.036) | (0.047) | | |
| $SES^H$ | −0.237*** | 0.118*** | 0.054 | 0.050 | 0.036 | −0.069 | 0.076 | −0.064 | | |
| | (0.061) | (0.036) | (0.051) | (0.043) | (0.053) | (0.052) | (0.079) | (0.105) | | |
| *N* | 57,349 | 52,252 | 48,430 | 41,006 | 41,280 | 57,343 | 57,343 | 57,343 | | |
| *Coeff. linear combination* | | | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur}^H$ | 0.062 | −0.047 | 0.002 | 0.046 | 0.081 | −0.032 | 0.161* | 0.255** | | |
| | (0.069) | (0.045) | (0.046) | (0.047) | (0.057) | (0.047) | (0.082) | (0.109) | | |

*Note:* All models control for individual age, father SES, cohort and county fixed effects, regional trends, and county-cohort measures of preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parentheses and corrected for two-step estimation; Significance levels: * = 10, ** = 5% and *** = 1%.

*Source:* LFS and FQP data.

**TABLE A15** | Robustness check — alternative family background partition.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | | Preschool |
|---|---|---|---|---|---|---|---|---|---|
| | **Employed** | **Unemployed** | **Part-time** | **Hourly** | **Monthly** | **≤ Primary** | **≥ Bac** | **≥ Bac + 2** | ***Dur*** |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** | **(9)** |
| Panel A — Reduced form and first stage estimates | | | | | | | | | |
| Preschool supply ($S_{cd}$) | −0.025 | −0.004 | 0.028 | 0.007 | 0.008 | −0.037* | 0.037 | 0.037 | 0.799** |
| | (0.036) | (0.023) | (0.021) | (0.039) | (0.044) | (0.021) | (0.028) | (0.027) | (0.304) |
| $S_{cd} \times SES^M$ | 0.036*** | −0.018*** | −0.008 | 0.015** | 0.015* | −0.003 | 0.020* | 0.032*** | −0.010 |
| | (0.007) | (0.004) | (0.011) | (0.008) | (0.009) | (0.008) | (0.011) | (0.006) | (0.047) |
| $S_{cd} \times SES^H$ | 0.057*** | −0.023*** | −0.009 | 0.009 | 0.005 | −0.004 | 0.012 | 0.035*** | −0.138 |
| | (0.007) | (0.006) | (0.007) | (0.011) | (0.013) | (0.010) | (0.015) | (0.007) | (0.129) |
| $SES^M$ | 0.005 | −0.000 | −0.001 | 0.138*** | 0.139*** | −0.106*** | 0.226*** | 0.186*** | 0.123* |
| | (0.006) | (0.005) | (0.008) | (0.007) | (0.008) | (0.007) | (0.009) | (0.007) | (0.069) |
| $SES^H$ | −0.005 | 0.002 | 0.000 | 0.298*** | 0.309*** | −0.148*** | 0.423*** | 0.420*** | 0.333** |
| | (0.008) | (0.006) | (0.008) | (0.011) | (0.013) | (0.010) | (0.012) | (0.011) | (0.126) |
| N | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 | 2405 |
| F-test | | | | | | | | | 2.411 |
| F-*p* | | | | | | | | | 0.076 |
| Panel B — Second stage estimates | | | | | | | | | |
| $\widehat{Dur}$ | −0.070 | 0.012 | 0.039 | 0.002 | 0.007 | −0.040 | 0.034 | 0.017 | |
| | (0.053) | (0.032) | (0.028) | (0.033) | (0.039) | (0.032) | (0.043) | (0.047) | |
| $\widehat{Dur}^M$ | 0.103*** | −0.051*** | −0.024 | 0.045** | 0.045 | −0.011 | 0.058* | 0.093*** | |
| | (0.024) | (0.013) | (0.031) | (0.019) | (0.030) | (0.024) | (0.032) | (0.022) | |
| $\widehat{Dur}^H$ | 0.235*** | −0.106** | −0.021 | 0.048 | 0.029 | −0.048 | 0.083 | 0.187* | |
| | (0.076) | (0.046) | (0.034) | (0.053) | (0.062) | (0.064) | (0.089) | (0.097) | |
| $SES^M$ | −0.170*** | 0.089*** | 0.037 | 0.057 | 0.057 | −0.082* | 0.118* | 0.017 | |
| | (0.047) | (0.027) | (0.063) | (0.041) | (0.059) | (0.049) | (0.065) | (0.048) | |
| $SES^H$ | −0.465*** | 0.216** | 0.030 | 0.199* | 0.249* | −0.036 | 0.243 | 0.030 | |
| | (0.173) | (0.104) | (0.077) | (0.120) | (0.135) | (0.143) | (0.197) | (0.217) | |
| *N* | 64,900 | 59,104 | 54,728 | 46,107 | 46,421 | 64,893 | 64,893 | 64,893 | |
| *Coeff. linear combination* | | | | | | | | | |
| $\widehat{Dur} + \widehat{Dur}^M$ | 0.033 | −0.039 | 0.015 | 0.047 | 0.052 | −0.050 | 0.092* | 0.111** | |
| | (0.052) | (0.033) | (0.038) | (0.034) | (0.035) | (0.037) | (0.048) | (0.052) | |
| $\widehat{Dur} + \widehat{Dur}^H$ | 0.165* | −0.094* | 0.018 | 0.050 | 0.036 | −0.088 | 0.117 | 0.204** | |
| | (0.095) | (0.054) | (0.048) | (0.065) | (0.065) | (0.070) | (0.094) | (0.098) | |
| $\widehat{Dur}^H - \widehat{Dur}^M$ | 0.132* | −0.056 | 0.003 | 0.003 | −0.016 | −0.037 | 0.025 | 0.093 | |
| | (0.079) | (0.047) | (0.032) | (0.052) | (0.050) | (0.052) | (0.078) | (0.101) | |

*Note:* All models control for individual age, father SES, cohort and county fixed effects, regional trends, and county-cohort measures of preschool demand drivers; Columns 4–5 exclude self-employed; Robust standard errors clustered at county of birth in parentheses and corrected for two-step estimation; Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS and FQP data.

**TABLE A16** | Robustness check—difference-in-differences estimates.

| Dependent variable | Labour market participation | | | Wage (log) | | Education level | | |
|---|---|---|---|---|---|---|---|---|
| | Employed | Unemployed | Part-time | Hourly | Monthly | ≤ Primary | ≥ Bac | ≥ Bac + 2 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Panel A—Homogeneous effect | | | | |
| Treat × Post | 0.003 | −0.004 | −0.003 | −0.012 | −0.010 | 0.048** | −0.036* | −0.016 |
| | (0.017) | (0.012) | (0.011) | (0.020) | (0.021) | (0.018) | (0.020) | (0.022) |
| Treat | −0.038*** | 0.042*** | 0.018** | 0.081*** | 0.027* | −0.005 | 0.012 | 0.019 |
| | (0.012) | (0.008) | (0.008) | (0.013) | (0.015) | (0.013) | (0.014) | (0.015) |
| Post | −0.000 | 0.013 | −0.005 | −0.054*** | −0.049** | −0.071*** | 0.201*** | 0.117*** |
| | (0.010) | (0.009) | (0.007) | (0.019) | (0.020) | (0.013) | (0.021) | (0.020) |
| $SES^H$ | 0.032*** | −0.016*** | 0.009** | 0.199*** | 0.204*** | −0.127*** | 0.310*** | 0.284*** |
| | (0.005) | (0.003) | (0.003) | (0.009) | (0.011) | (0.006) | (0.010) | (0.011) |
| *N* | 13,384 | 12,819 | 11,981 | 9791 | 9845 | 13,383 | 13,383 | 13,383 |
| | | | | Panel B—Heterogeneous effect by family background | | | | |
| Treat × Post | −0.012 | 0.002 | −0.015 | −0.045** | −0.042* | 0.061* | −0.039 | −0.020 |
| | (0.027) | (0.020) | (0.012) | (0.022) | (0.021) | (0.031) | (0.031) | (0.025) |
| Treat × Post × $SES^H$ | 0.034 | −0.015 | 0.024* | 0.086** | 0.087** | −0.045 | 0.014 | 0.006 |
| | (0.030) | (0.026) | (0.014) | (0.036) | (0.041) | (0.035) | (0.049) | (0.054) |
| Treat | −0.024 | 0.023* | 0.037*** | 0.059*** | 0.004 | −0.014 | 0.037* | −0.029 |
| | (0.019) | (0.013) | (0.008) | (0.015) | (0.015) | (0.020) | (0.021) | (0.018) |
| Treat × $SES^H$ | −0.025 | 0.038** | −0.040*** | −0.073*** | −0.070** | 0.017 | −0.016 | 0.018 |
| | (0.020) | (0.018) | (0.010) | (0.026) | (0.030) | (0.023) | (0.033) | (0.038) |
| Post | 0.003 | 0.020 | −0.007 | −0.047** | −0.037* | −0.101*** | 0.222*** | 0.131*** |
| | (0.014) | (0.013) | (0.009) | (0.020) | (0.021) | (0.020) | (0.025) | (0.019) |
| Post × $SES^H$ | −0.009 | −0.001 | −0.002 | −0.051 | −0.040 | 0.080*** | −0.035 | −0.032 |
| | (0.021) | (0.018) | (0.010) | (0.035) | (0.037) | (0.018) | (0.032) | (0.043) |
| $SES^H$ | 0.031 | 0.007 | 0.027 | 0.307*** | 0.300*** | −0.098*** | 0.256*** | 0.216*** |
| | (0.038) | (0.029) | (0.034) | (0.068) | (0.070) | (0.036) | (0.068) | (0.074) |
| N | 13,384 | 12,819 | 11,981 | 9791 | 9845 | 13,383 | 13,383 | 13,383 |

*Note:* All models control in addition for county, cohort and age FE, a regional trend. Post is an indicator for whether the respondent is born after the implementation of the National Plan (i.e., in years 1971–1973) rather than before (i.e., in years 1964–1966); Treated is an indicator of whether the individual lives in a high-expansion county, as defined in the main text. Robust standard errors in parentheses. Significance levels: * = 10, ** = 5% and *** = 1%.
*Source:* LFS data.