Keep Working and Spend Less? Collective Childcare and Parental Earnings in France*

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Abstract

I leverage the staggered expansion of subsidized childcare facilities across municipalities in response to a succession of national plans to investigate the effect of collective childcare on parents' labor outcomes and childcare choices in France between 2007 and 2015. These plans did not lead to any substantial change in parents' labor outcomes or in paid parental leave take-up. Instead, these collective childcare expansions crowded out more costly formal childcare solutions, such as childminders or at-home childcare. These crowding-out effects highlight a downside of family policy strategies that foster the coexistence of multiple childcare arrangements.

Keywords: Labor supply, childcare, event-study, parental leave.

JEL Classification: J13, J16, J18, J22.

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1 Introduction

In international comparisons, France is widely seen as a success in terms of family policies that promote the work-family balance and gender equality. Among both OECD and EU countries, it ranks high in terms of fertility rate, female employment rate and formal childcare coverage (see e.g. OECD, 2011). In contrast to other countries, childcare arrangements in France are extremely diverse: its long-lasting institutional history has led to the coexistence of paid parental leave and highly subsidized formal childcare services, the latter including a continuum from individual at-home childcare to collective services provided by daycare centers. This unique diversity is supported by policy-makers and the general public, and is assumed to provide families with freedom to choose the childcare arrangements most suited to their heterogeneous preferences and constraints.

In this paper, I highlight a downside of this institutional setting. Namely, this diversity leaves room for potentially large substitution effects across childcare solutions. Consequently, massive investment plans aimed at increasing the overall provision of formal childcare may simply crowd out other subsidized solutions, instead of further enhancing the work-life balance and gender equality. Specifically, I investigate the consequences in terms of both parental labor earnings and labor supply, and childcare choices, of a series of national plans launched in the 2000s and designed to increase the supply of daycare centers to provide particularly affordable collective childcare for very young children. My main results show that: (i) these plans did not trigger any substantial change in the parental labor supply, and especially that of mothers; (ii) instead, they resulted in families shifting away from more expensive individualized childcare solutions.

While focused on the unique French setting, this paper is relevant to more general questions regarding the impact of affordable childcare on maternal labor supply. Indeed, null effects have commonly been reported in the literature (e.g. Fitzpatrick, 2010; Havnes and Mogstad, 2011a), and have been widely attributed to substitution effects across childcare solutions. However, with few exceptions, this mechanism remains somewhat speculative because it usually involves the crowding-out of informal childcare solutions (e.g. childcare provided by a relative or a neighbor). The main problem is that these informal arrangements are not observed in the data with sufficient precision and frequency to correctly identify the causal effect of affordable childcare supply on childcare choices when these policies fail to enhance mothers' labor outcomes. Informal primary childcare providers are quite uncommon in France, however, which implies that gathering data on the multiple formal childcare solutions is generally sufficient to cover almost all relevant childcare choices. I am therefore able to provide clear evidence that the null effects of affordable childcare provision on maternal labor supply do indeed arise from crowding-out effects.

My empirical approach focuses on the staggered expansion of affordable collective childcare across narrow geographical areas in response to a succession of national plans that aimed at increasing the overall collective formal childcare provision. Specifically, I leverage differences in the timing of major expansion events *across* municipalities, *within* groups of municipalities that experienced increases of similar magnitude, to identify the causal effect of affordable childcare on parents' labor earnings, labor supply and childcare choices. I apply this framework to a combination of detailed administrative datasets: childcare and parental leave records kept by by the Family branch of the French Social security, as well as both cross-sectional and longitudinal birth records and payroll data.

I find that these sharp increases in affordable collective childcare provision at the municipal level did not trigger any substantial change in parental labor outcomes. Specifically, my estimates are incompatible with causal effects of childcare expansions on maternal employment larger than 0.05 percentage points per percentage point increase in the childcare coverage rate

I then shed light on the underlying mechanisms that generate these null effects. Firstly, I consider the possible substitution with paid parental leave to which most parents of very young children are entitled. While most empirical studies consider childcare provision and parental leave as two separate policies, this parameter bears substantial policy relevance as it predicts whether a change in the childcare provision is likely to affect the demand for parental leave or not. Consistent with my labor supply estimates, I find that the expansion of affordable collective childcare does not trigger any change in the take-up of parental leave benefits, which suggests that these substitution effects are limited at best.

Secondly, I focus on the supply of other formal and more costly childcare solutions, i.e. childminders and nannies providing at-home childcare. Applying the same approach, I provide evidence of a very substantial crowding-out of these childcare solutions after collective childcare expansions. Specifically, in municipalities with the sharpest increases in affordable collective childcare provision, the medium-run drop in individualized childcare supply is equivalent in magnitude to that of the increase in collective childcare provision. This implies that the increased childcare capacity of daycare centers likely benefits parents who would have otherwise turned to individualized and more expensive formal childcare solutions.

This suggests that these families have a high propensity to rely on formal childcare solution, regardless of the availability of affordable childcare, which may stem from either a strong taste for working mothers, or strong incentives for mothers to remain in the labor force (for instance if the hourly price of individualized childcare is only a fraction of mothers' hourly wages). It does *not* follow that, due to either preferences or incentives, the numerous families who did not benefit from a collective childcare place would not change their labor supply decision in response to childcare places being made available to them. Indeed, my estimates are only informative about the subpopulation of families who are offered a childcare place thanks to the local childcare expansion, but would not have been so before the expansion took place. Extrapolating these effects to never-treated families is not straightforward, and would likely require additional data on the allocation of childcare places, at both the application and the selection level of the process.

Literature When seeking to identify the labor supply effects of childcare provision the main empirical challenge to overcome is the fact that childcare and labor supply decisions are made jointly: the causal impact of childcare on labor supply cannot be identified from the correlation between actual childcare and labor supply choices. As a result, researchers have resorted to either a careful specification of the joint decision process (e.g. Heckman, 1974; Michalopoulos, Robins, and Garfinkel, 1992; Domeij, 2013; Bick, 2016) or quasi-experimental evidence arising from plausibly exogenous policy changes (e.g. Gelbach, 2002; Baker, Gruber, and Milligan, 2008; Fitzpatrick, 2010; Bauernschuster and Schlotter, 2015; Gathmann and Sass, 2018; Carta and Rizzica, 2018).

Especially relevant to this paper are studies that infer the causal impact of affordable childcare on maternal labor outcomes by exploiting heterogeneity between geographical areas in the timing of publicly subsidized childcare expansions in response to national-level policy reforms (Berlinski and Galiani, 2007; Havnes and Mogstad, 2011a; Nollenberger and Rodríguez-Planas, 2015; Yamaguchi, Asai, and Kambayashi, 2018; Andresen and Havnes, 2019; Müller and Wrohlich, 2020). Broadly speaking, such papers manage to get around the endogeneity with respect to labor supply of both individual childcare choices and the local childcare availability by relying on a fuzzy difference-in-difference framework like that of Duflo (2001). Specifically, they leverage the fact that some areas experience large and sudden increases in affordable childcare provision, while other do not, or may experience them later on. The former are thus considered as a treated group, while the latter are used like a control group, under the assumption that, absent the treatment, labor outcomes in the treated group would have evolved in the same way as those in the control group, so as to capture any change that occurs at a national level. My identification strategy relies on a variation of this approach.

In terms of results, this literature is somewhat contrasted between papers that find substantial positive effects of affordable childcare provision on maternal labor supply, and others that emphasize null effects. In the US and Canada, Blau and Currie (2006) report estimates of maternal labor supply elasticity with respect to the price of childcare. Across the 20 studies analyzed, these estimates vary from -3.60 to +0.06. For a more recent perspective on the literature, Morrissey (2017) reports elasticities that range from -1.1 to -0.025 in the US. Variation may stem from the age of the targeted children, the educational attainment or labor force attachment of the targeted mothers, or broader variation in national or historical context; even so, the results are not always easy to reconcile. When combined with quasi-experimental approaches to child penalties such as that of Kleven, Landais, and Søgaard (2019), the difference-in-difference approach of Havnes and Mogstad (2011a) yields contrasting results: Nix and Andresen (2019) suggest that early childcare has the potential to alleviate the child penalty in Norway, whereas Kleven et al. (2019) emphasize that increased childcare provision has no effect on the child penalty in Austria.

Null effects are thus quite common in this literature, and have been attributed to substitution across childcare solutions. To date, Cascio (2009) provides the most compelling evidence as to these crowding-out effects, but empirical facts regarding such effects remain otherwise scarce. While Baker, Gruber, and Milligan (2008) provide direct evidence of crowding-out effects, although in a context where maternal labor supply effects are actually positive, Asai, Kambayashi, and Yamaguchi (2015) suggest that these effects may explain the observed heterogeneity in the maternal labor supply effect between two-generation and three-generation families, in a context in where childcare is frequently provided by grandparents. Bassok, Fitzpatrick, and Loeb (2014) document substitution effects between public and private childcare, with the magnitude of crowding-out depending on the type of intervention (e.g. a voucher program as opposed to direct public-sector childcare provision), but do not provide evidence as to the labor supply consequences of such crowding-out effects. These substitution effects are also relevant to more general questions about the impact of regulation on the childcare market (Hotz and Xiao, 2011).

Few researchers have examined the French setting. Among them, both Choné, Le Blanc, and Robert-Bobée (2004) and Allègre, Simonnet, and Sofer (2015) use a joint model of childcare choices and labor supply decisions, but reach different conclusions as to the effect of childcare prices on childcare choices and maternal labor supply. This may arise from differences in the level of detail of the childcare data they use. Closer to a quasi-experimental approach, Maurin and Roy (2008) examine the difference between families that obtained a childcare place and those who did not among all families who applied in a particular city, and find a positive effect on maternal labor supply. Goux and Maurin (2010) focus on the availability of pre-school places for 2-years olds, and find a positive impact for single mothers, but not for mothers with a cohabiting partner. Lastly, Givord and Marbot (2015) examine the effects of a policy reform implemented in 2004 that led to a sharp decrease in childcare costs for some families; they find a positive but small impact on maternal labor supply.

The remainder of the paper is organized as follows. The next section presents the institutional setting. Section 3 describes the data and section 4 details the identification strategy. Section 5 presents the results on parental earnings and labor supply. Section 6 investigates the underlying mechanisms, i.e. substitution across childcare solutions, and lastly, section 7 concludes.

2 Institutional setting

2.1 Early childcare coverage

France is among OECD countries with the broadest access to early childcare outside the home: in 2016, over 56% of children aged 2 or less were enrolled in early childcare, a share that only Denmark, Belgium and Iceland exceed (OECD, 2016). I focus exclusively on childcare for children under age 3 given that children in France can enter pre-school from age 3 and the enrollment rate is over 99%.

France has achieved this broad childcare coverage by fostering very diverse childcare arrangements, with daycare centers representing only a fraction of the total. Formal individualized childcare solutions, such as childminders and, to a lesser extent, individual at-home childcare are also quite common. Few parents rely heavily on informal solutions in France: less than 3% of families with young children relied on a relative as their primary childcare provider in 2013 (Villaume and Legendre, 2014).

In this paper, I focus on one type of formal childcare provided outside the home, that of daycare centers, i.e. formal collective solutions, in contrast with formal individualized solutions (e.g. childminders or at-home childcare provided by nannies) or informal solutions (e.g. childcare provided by relatives). These collective solutions, coined as *Établissements d'Accueil du Jeune Enfant* (EAJE) accounted for 31% of total theoretical formal early childcare capacity in 2014 (IGAS/IGF, 2017).

2.2 EAJE-PSU facilities

Broadly speaking, EAJE facilities provide childcare to children up to age 6. However, because almost 100% of children attend school from age 3, they are more generally targeted towards children aged 0 to 2.[1] These facilities are often run by local authorities, sometimes through an association.

Specifically, I investigate the provision of childcare by EAJE facilities funded under the *Prestation de Service Unique* (PSU) scheme. Local offices (*Caisse d'Allocations Familiales*, CAF) of the Family branch (*Caisse Nationale d'Allocations Familiales*, CNAF) of the French Social Security system fund a large share of EAJE facilities through this scheme. To obtain this funding, it is required that an EAJE facility bases its pricing on a national fee schedule that makes it the cheapest formal childcare solution for families.[2] Figure 1 emphasizes this fact by displaying estimates of the prices paid by families across formal childcare solutions, and the corresponding burden for public finances.

Allocation of EAJE-PSU childcare places is decided at local level. Criteria may vary from one place to another, but they generally take into account the parents' place of residence, their employment status and the socio-economic background of the family. The only universal criterion is the municipality of residence (Onape, 2012).

[1]Less than 1% of children aged 3 to 6 attend EAJE facilities in the evening (Villaume and Legendre, 2014).

[2]Appendix A.1 details other criteria that EAJE facilities have to meet, and the pricing of childcare places.

2.3 National expansion plans

Until the early 2000s, the development of EAJE facilities was mostly decided by local authorities. In June 2000, the first national *plan crèche* (daycare center plan) was launched. Its main aim was to increase the availability of formal collective childcare, either by expanding pre-existing facilities, or by creating new ones. Since then, several other national plans have followed: the 9th *plan crèche* was launched in 2018. These plans are coordinated at national level by the CNAF, and implemented by local authorities with the help of local CAF offices. Local CAF offices usually allocate subsidies based on the number of formal childcare places relative to the number of children aged 3 or less, as observed at the municipality level.[3]

Between 2000 and 2016, 150 000 new subsidized childcare places were created, 2/3 of which were so through the opening of new facilities. Whether directly subsidized by these plans or not, the number of collective childcare places increased by 70 000 between 2007 and 2015, my period of interest. This is a relatively modest increase, at the national level, given that the number of children aged 2 or less over the same period was between 2.3 and 2.4 million.

2.4 Parental leave policies

Benefits may be granted when a parent interrupts his or her career or opts to work part-time (previously *Complément Libre Choix d'Activité* (CLCA) and now *Prestation Partagée d'Éducation de l'enfant* (PreParE)). Additionally, parents are entitled by law to extend the duration of their parental leave if they are not offered a formal childcare place.[4] This policy is effective with the first birth and provides a fixed non-means-tested monthly amount for the maximum duration of 6 months; the duration increases up to 2 years from the second child on.[5] Contrary to Sweden, for instance, the benefits do not depend on parents' past income: they amount to approximately €400 per month in the case of career interruption and to nearly €200 in the case of 80% part-time work.

Lastly, in Appendix A.4, I analyze survey data to determine parents' childcare preferences, and the constraints affecting their childcare choices. The main lessons are that (i) there is strong parental demand for collective childcare; (ii) this demand

[3] See Appendix A.2 for additional details

[4] Article L531-4 of the French Social Security Code.

[5] Appendix A.3 further details this policy.

is likely not met by current supply thereof and; (iii) the lack of such collective childcare solutions may have negative consequences for maternal labor supply.

3 Data

My analysis combines several administrative records to recover (i) a measure of the supply of formal and collective childcare at a narrow geographical level and; (ii) labor market trajectories and fertility decisions of a large sample of individuals of whom the municipality of residence is observed. Table 1 sums up the main characteristics of these datasets.

3.1 Family insurance data

First, I use data provided by the CNAF, the Family branch of the French Social Security system, to get information on the supply of affordable collective childcare at the municipal level. Specifically, these data cover all EAJE facilities funded under the PSU scheme. For each municipality between 2007 and 2015, the data give the number of such facilities within each municipality and the number of childcare places they offer, as defined by their accreditation certificate, granted by the local authorities that specifies a maximum capacity for each facility.[6]

The Family branch of the French Social Security system also has data on the take-up of paid parental leave. Specifically, for each municipality from 2009 to 2018, this dataset gives the number of families that were entitled to either the CLCA or the PreParE in December of each year.[7] In order to obtain to these allowances, families must submit an application and meet several criteria. This dataset therefore provides a relevant measure of the number of families that receive these parental leave allowances, as it only covers families who applied and are eligible.

[6] I exclude data on one département (Tarn), whose data would suggest that no EAJE-PSU facility existed in 2007, even though many municipalities had such facilities in 2008. In 2007, the Tarn département accounted for 0.6% of the total French population.

[7] Due to data issues related to a policy reform that took place in 2015, I restrict my analysis of this data to the 2009-2014 time-period: see Appendix B.1 for additional details.

3.2 Labor market data

My labor market data is drawn from the *Déclarations Annuelles de Données Sociales* (DADS). By law,[8] French employers have to fill in a DADS form for every employee subject to payroll taxes. The form contains detailed information about gross and net wages, days paid, hours paid, employer location (at municipality level), other job characteristics (beginning, duration and end of a period of employment and part-time employment), employer characteristics (industry, size, and region) and individual characteristics (age, gender and municipality of residence). In Appendix B.2.1, I provide further details on how earnings and time worked are measured, and especially on how paid maternity leave is included in my measure of labor supply.

Specifically, I take advantage of two declination of these data. Firstly, I rely on the DADS panel, a longitudinal sample at rate 4.4% to track parents' labor supply and labor earnings from 2007 to 2015, thanks to an anonymized personal identifier based on their social security number that allows me to link this information to birth records. Secondly, I aggregate comprehensive cross-sectional DADS registers at the municipality level to recover earnings and hours paid to childminders and nannies from 2009 to 2015. Appendix B.2 further details how I proceed and the limitations of these datasets.

3.3 Fertility data

My analysis also relies on birth records. Births are registered by an individual who was present at the time of birth, usually the father, but in some cases a doctor or a midwife. I again rely on two different versions of these records. Firstly, I take advantage of cross-sectional comprehensive birth records to compute the number of children born to women living in a given municipality in any year between 2005 and 2015, which gives an approximate measure of the trends in potential demand for childcare at a narrow geographical level. Secondly, I use on a longitudinal version of these records at the individual level extracted from the *Échantillon Démographique Permanent* (permanent demographic sample, EDP) to obtain information on the timing of births. Thanks to the NIR, this dataset can be merged with the longitudinal version of the DADS, and allows me to separate parents with young children from the rest of the population. Appendix B.3 provides additional information as to how I proceed and a few shortcomings of these data.

[8] The absence of DADS as well as incorrect or missing answers are punished with fines.

3.4 Data preparation

I first estimate the supply and potential demand for childcare at a narrow geographical level. For each municipality and each year the data provide information as to (i) the number of childcare places available in each municipality, and; (ii) the number of children born to mothers who live in the relevant geographical area. This allows me to compute a measure of the relative supply, i.e. the share of children with potential access to daycare center. Specifically, I define the relative childcare supply $S_{c,t}$, where c denotes municipality and t stands for a particular year, as the ratio:

$$S_{c,t} = \frac{N_{c,t}^{\text{places}}}{N_{c,t}^{\text{birth}} + N_{c,t-1}^{\text{birth}} + N_{c,t-2}^{\text{birth}}} \tag{1}$$

where $N_{c,t}^{\text{places}}$ is the number of EAJE-PSU childcare places in municipality c during year t, and $N_{c,t}^{\text{birth}}$ the number of children born to women who lived in c at time t. In other words, this measure assumes that children's place of residence does not change during the first three years of their life.

Figure 2 displays the trend in relative supply at the national level between 2007 and 2015. It increased by roughly 3.5 percentage points, and almost linearly over the period. An interesting feature of this continuous expansion of affordable childcare is its heterogeneity across geographical units. The map in Figure 3 displays the change in relative childcare supply level for each municipality from 2007 to 2015. It shows clearly that this moderate increase was concentrated in relatively few areas, where affordable childcare provision increased strongly, in contrast with most municipalities where the supply barely changed.

In a second step, I recover data at the individual level. I restrict the sample to individuals who experienced childbirth between 2005 and 2015, who therefore actually have children of the targeted age group at some point between 2007 and 2015, and to individuals between ages 20 and 60. As the municipality of residence is only observed in the labor market data, I further require that these individuals have been salaried employees at least once between 2002 and 2015.[9]

My analysis pays attention to the extensive margin of labor supply, which is crucial when considering mother's time allocation decisions. For individuals who are not found in my labor market data for a particular year,[10] I impute zero labor

[9]Empirically, this is the case of 94% of parents throughout my time-period of interest. In Appendix F.1, I show that under reasonable assumptions, this sample selection does not impede the identification of average treatment effects.

[10]I also drop observations with very low earnings or working time, see Appendix B.4.

earnings, and consider them to be outside the labor force.[11] As a result, I am able to decompose labor earnings responses between the extensive and intensive margins of labor supply on the one hand, and hourly wages on the other.

I finally merge this individual-level data with the geographical data on affordable childcare. This leaves me with 1.5 million observations of parents with children aged 0 to 2, covering 430 000 individuals. Table 2 gives summary statistics on the sample. The gender gap in labor outcomes is extremely salient: on average, while mothers of young children tend to be more educated, they earn only just over half the average earnings of their male counterparts. This gap is largely driven by labor supply decisions: among those in wage employment, the gender gap in hourly wages is much smaller yet still sizable, at about 15%.

4 Empirical analysis

4.1 Granular childcare expansions

My empirical approach leverages the granularity of national-level childcare expansion, i.e. the fact that (i) the smooth increase in childcare provision at the national level (Figure 2) is actually concentrated on a few municipalities where provision has increased sharply, in contrast with most municipalities where it has remained flat (Figure 3); and that (ii) among these municipalities in where childcare provision has increased massively, this rise is generally attributable to a single event, i.e. a sharp increase in affordable childcare provision between two consecutive years, for instance due to the opening of a new daycare center, rather than to a continuous increase over the years.

To take advantage this granularity more salient, I first compute the maximum growth in relative childcare supply $S_{c,t}$ between two consecutive years in each municipality. Figure 4 displays the distribution of this maximal growth at the municipality level (weighted by the number of children aged 2 or less in each municipality as measured in 2007). In 2007, a quarter of children aged 2 or less lived in municipalities that experienced no growth in childcare supply of any kind between 2007 and 2015. In fact, these are mainly municipalities where the supply is actually nonexistent throughout the relevant time period, plus a handful of municipalities where the supply decreased due to the closure of a daycare facility. In municipalities that did experience growth, there is considerable heterogeneity

[11] By contrast, all other observations correspond to individuals who are in employment.

in its maximum yearly magnitude: the 80th percentile of the distribution is 4 percentage points, the 90th percentile is 7.6 percentage points, but the 99th percentile is over 33 percentage points.

There is no obvious cut-off in the distribution. Nevertheless, I choose to partition municipalities into four treatment groups: those where the supply never increases (bottom 25%), those between the 25th and the 80th percentile, then those that rank between the 80th and the 90th percentile, and finally the top 10%. Dividing municipalities into separate groups according to the position in the distribution of a continuous variable is by no means straightforward; however this approach is somewhat similar to that of Havnes and Mogstad (2011a) who group municipalities according to their position below or above the median. Furthermore, and in contrast to theirs, my approach does not rely on heterogeneity across these groups. In Appendix C, I describe these groups in terms of pre-treatment observables, i.e. using data from the 2006 Census at the municipality level. The main lesson is that P90-P100 municipalities which are key to my identification strategy, are relatively small municipalities, with slightly less than 8,000 inhabitants in average.

I then define the timing of the childcare shock that corresponds to this maximum yearly growth. In municipalities that did experience positive growth, the definition is straightforward: the event takes place at the time when the relative childcare supply increases the most. For the bottom 25% of municipalities where the supply never increases, the counterfactual treatment time is drawn randomly in the distribution of actual treatment timings in the other groups.

Figure 5 displays the average relative supply of affordable childcare over time within each treatment group, depending on the timing of the municipal childcare shock. In the never-treated group, this supply remains at around 0 from 2007 to 2015. For the three other groups, the figure clearly shows that within each group, the pre-shock level, the post-shock level and the size of the shock are very similar across municipalities with different timings of the shock itself. Basically, in the P25-P80 group of municipalities supply was 16-18% and increased by 1 percentage point; in the P80-P90 group supply was about 20%, and increased by 5 percentage points; and in the P90-P100 group, pre-shock coverage was 15-20% and increased sharply 15 by percentage points. In this last group, this event corresponds typically to the opening of the first or the second facility in the municipality, which represents about 15 to 20 new childcare places.

4.2 Event-study analysis

I rely on differences in the timing of the childcare shock across municipalities that experience shocks of similar magnitudes to identify the causal impact of childcare expansions. Let y_{it} denote the annual earnings (resp. salaried employment dummy, working hours, hourly wages) of parent i at time t, living in municipality $c = c(i,t)$ that belongs to the treatment group $g = g(c)$.[12] In this within-group event-study setting, I estimate:

$$y_{it} = \alpha_{c(i,t)} + \sum_{g,\tau} \beta_{g\tau} \mathbb{1}\{t - E_{c(i,t)} = \tau,\, g(c(i,t)) = g\} + \sum_{g,T} \gamma_{gT} \mathbb{1}\{t = T,\, g(c(i,t)) = g\} + \epsilon_{it} \tag{2}$$

where α_c is a municipality-level fixed effect, E_c denotes the year of the childcare shock for municipality c, and ϵ_{it} is an idiosyncratic shock of mean 0. The $\beta_{g\tau}$ coefficients capture the dynamic effects of the childcare expansions and represent my parameter of interest.

As noted by Borusyak and Jaravel (2017), Model 2 is underidentified. This is because (i) the inclusion of municipality fixed effects means that the time effects are only identified up to a constant; and more importantly, (ii) within each cohort defined by the timing of the treatment E_c, calendar time t and time-to-treatment $t - E_c$ are colinear.[13] This is actually a special case of the well-known underidentification problem of Age-Period-Cohort models, with age corresponding to time-to-treatment, period to calendar time, and cohort to the timing of the treatment. Due to this collinearity, the $\beta_{g\tau}$ coefficients are only identified up to a constant plus a linear trend.

To resolve this underidentification problem, Borusyak and Jaravel (2017) note that in settings where it is plausible to assume that (i) the treatment is exogenous conditional on unit (here: municipality) fixed-effects, and that (ii) there are no anticipation effects, coefficients belonging to the subset $(\beta_{g\tau})_{\tau<0}$ should all be equal to 0. As a result, they suggest that Model 2 be estimated first, while setting two coefficients of the subset to 0, which is akin to APC modeling approach proposed by Mason et al. (1973). This makes it possible to test the hypothesis that other coefficients are also equal to 0. After ensuring that this no-pretrend assumption holds, they recommend estimating a semi-dynamic version of Model 2 in which all

[12] Because the relevant municipality is the one in which parent i lives at time t, my approach takes into account families who may move from one municipality to another due to the opening of new childcare places.

[13] Municipality fixed effects can be replaced with cohort (time-of-the-treatment) fixed effects without changing the identification properties of the model.

coefficients $(\beta_{g\tau})_{\tau<0}$ are constrained to 0.

Lastly, they point out that when treatment effects are dynamic, i.e. when there is variation in the coefficients of the subset $(\beta_{g\tau})_{\tau\geq 0}$, the overall treatment effect is not identified by the canonical regression in which time-to-treatment dummies are replaced by a post-treatment dummy. This is because this regression weights long-run effects negatively: as a result, the estimator does not have the no-sign reversal property, so even the sign of the effect can be wrong. Instead, they recommend first fitting the semi-dynamic model, and then manually summing the coefficients of the subset $(\beta_{g\tau})_{\tau\leq 0}$, for instance with weights proportional to the sample size.

I follow their recommendation closely. My only departure is that as a first step, I do not normalize the pre-trend by setting two coefficients to 0. Instead, I apply a solution to the underidentification of APC models proposed by Deaton and Paxson (1994). Specifically, my approach basically involves imposing two normalizations on the pre-trend: (i) that on average, $\beta_{g\tau}$ coefficients before the event are equal to 0, i.e. $\sum_{\tau<0}\beta_{g\tau} = 0$; and (ii) that the vector $(\beta_{g\tau})_{\tau<0}$ is orthogonal to any linear time trend, i.e. $\sum_{\tau<0}\tau\beta_{g\tau} = 0$.

Recent investigations of this approach show that these regressions can generate spurious results when treatment effects are heterogeneous across cohorts, as defined by the timing of the treatment (Sun and Abraham, 2020). In Appendix F.3.5, I show that moving to a correction based on a fully interacted model does not affect my results.

4.3 Instrumental variable approach

This event-study approach captures the consequences of childcare expansions without any reference to their magnitude. As a second step, I frame it into the fuzzy difference-in-difference approach developed by Duflo (2001) to rescale my estimates. In this setting, Model 2 is regarded as the reduced-form version of an instrumental variable regression, and is simply divided by the average magnitude of childcare expansions within the treatment group of the relevant municipality. Specifically, keeping the same notations, I estimate:

$$y_{it} = \kappa_{c(i,t)} + \lambda S_{c(i,t),t} + \sum_{g,T} \mu_{gT} \mathbb{1}\{t = T,\, g(c(i,t)) = g\} + \nu_{it} \tag{3}$$

while instrumenting the relative childcare supply S_{ct} by time-to-treatment interacted with treatment group dummies:

$$S_{ct} = \phi_c + \sum_{g,\tau \geq 0} \psi_{g\tau} \mathbb{1}\{t - E_c = \tau,\, g(c) = g\} + \sum_{g,T} \chi_{gT} \mathbb{1}\{t = T,\, g(c) = g\} + \omega_{ct} \tag{4}$$

The λ parameter can be interpreted at the individual level in an intention-to-treat sense: it corresponds to the effect on parents' labor outcomes of being offered a childcare place,[14] for the restricted subset of parents who would not have been offered such a place before the local childcare expansion, but actually are due to the local childcare expansion. This interpretation rests on a Stable Unit Treatment Value Assumption which states that, within municipalities and conditional on whether they are assigned a childcare place or not, parents' labor supply decisions are independent of the assignment of childcare places to other families. In other words, there should be no peer effects in terms of labor supply, an assumption that is somewhat unrealistic (Maurin and Moschion, 2009). If this assumption fails, then my estimates should be interpreted as a more macro effect, incorporating social multipliers due to peer effects. In this case, when divided by 100, the λ parameter represents the causal effect of a one percentage-point increase in childcare provision at the municipality level, expressed as the fraction of children aged 2 or less covered by local EAJE-PSU facilities, on parents' labor outcomes.

This fuzzy difference-in-difference framework has recently been investigated by econometricians who raise questions issues as to its ability to identify causal parameters of interest in realistic settings (de Chaisemartin and D'Haultfœuille, 2018). In Appendix F.3.6, I discuss these concerns and provide solutions to address them in the specific setting of this paper.

4.4 Identifying assumption

My empirical framework is based on an event-study design. As such, it does not rely on differences between municipalities exposed to increases of different magnitudes in the supply of collective childcare. In other words, differences between the P90-P100 group and other treatment groups are not directly relevant for my approach: I do not assume that the assignment to any of these groups is exogenous.

Instead, key to my framework are differences in the timing of the shock across municipalities of the same group, and especially of the P90-P100 group. Specifi-

[14] regardless of whether they actually use it or not.

cally, my identifying assumption is that *within* the P90-P100 group, the counterfactual trend in parental labor earnings *absent* the local childcare shock is mean-independent of the year when this shock takes place.

The allocation of subsidies directed towards the opening of new childcare places may depart from this assumption if either (i) the decision of municipalities to apply to these subsidies, or (ii) the decision of local CAF offices to grant these subsidies are based on factors that also determine this counterfactual trend. As noted in Subsection 2.3, the attribution of these subsidies by local CAF offices is to a large extent only based on the level of the childcare coverage rate in the municipality (and not, for instance, its evolution). However, the municipalities decision to first apply and its determinants remain unknown.

To assess the plausibility of my identifying assumption in this context, I resort to Census data at the municipality level. This allows me to test whether, within treatment groups, the timing of the childcare shock is correlated with observable characteristics that could plausibly affect the counterfactual trend in parental labor supply. A substantial correlation would seriously question the validity of the mean-independence assumption upon which my framework rests.

I show that, within the P90-P100 treatment group, municipalities that are treated in the beginning of the 2007-2015 time-period are, in 2006, virtually indistinguishable from municipalities that are treated later on. Specifically, these municipalities differ very little in terms of labor market composition, couple and marriage formation and dissolution, and arrival of new residents. The only significant differences are that (i) municipalities with lower coverage rate in 2007 are treated earlier, which is consistent with Subsection 2.3; (ii) larger municipalities tend to be treated earlier; and (iii) municipalities with higher birth rates are treated later. Even so, these differences explain very little (2% at best) of the variance the timing of the local childcare shock. Appendix F.2 details these findings.

5 Parental earnings and labor supply effects

5.1 Graphical analysis

Figure 6 displays my estimates of the event-study approach to the labor earnings of mothers with children aged 0 to 2 respectively. First, it displays my estimates of the full dynamic model, in which the pretrend is normalized in line with the approach proposed by Deaton and Paxson (1994). Such estimates allow me to verify that all coefficients corresponding to time periods that predate the childcare expansions are not significantly different from 0, which is indeed the case. In other words, within each treatment group, and before they are treated, mothers' labor earnings evolve in parallel across municipalities with different timings of the childcare shock. This sustains the credibility of the no-pretrend assumption upon which my event-study approach is based.

This allows me to consider the estimates of the semi-dynamic model, i.e. the event-study model in which the pretrend is set to 0. I find that my estimates are never significantly different from 0 at the usual 95% level. My point estimates do not suggest that the effect becomes significantly positive over time, so these results are not driven by short-run frictions.

An additional feature of my setting is that I can display estimates of the effect of non-existent or extremely small shocks to affordable collective childcare provision by considering the first two treatment groups. Consistent with the rationale, I find that such shocks have no effect on mothers' labor outcomes, which bears out the credibility of my identifying assumptions.[15]

Finally, I map these dynamic estimates into a single effect for each treatment group by summing the coefficients with weights proportional to the sample size. Table 3 displays my estimates, not only for labor earnings, but also for the potential margins of adjustment: labor force participation, working days, working hours per day and hourly wages. Consistent with my previous findings, I cannot detect any significant effect of the childcare shocks on mothers and fathers' labor earnings and labor supply. Moreover, these estimates are much more precise than my semi-dynamic estimates, so that economically significant effects can be largely ruled out: in the P90-P100 group, the aggregate effect of collective childcare expansions on mothers' salaried employment rate cannot exceed 2.6 percentage points.

5.2 Instrumental variable estimation

I then turn to the results of the related instrumental variable regression. These are merely the same results, but rescaled using the magnitude of the childcare shock as a first stage.

Table 4 displays my estimates. Consistent with my previous findings, I cannot

[15] The negative effects in the never-treated group are not significant once the pre-trend is set to 0 (additional identification constraint in the event-study setting), and are not significant when aggregated in a single estimate. It is driven by a strong negative trend in the number of days worked.

detect any significant effect of affordable collective childcare provision on parents', and especially mothers' labor outcomes. While my standard errors may be quite large for overall labor earnings, they are sufficiently small for labor supply decisions at the extensive margin. Indeed, the upper bound of my 95% confidence intervals allows me to rule out effects larger than 5.3 percentage points, my point estimate being -1.7 percentage points.

To make sure that these results are driven by municipalities where collective childcare provision increased substantially, as opposed to others where childcare shocks are almost nonexistent, I restrict my sample to the P90-P100 group, and run the same regression. My results are in line with those from the whole sample: when only the P90-P100 treatment group is considered, the upper bound of the 95% confidence interval is 4.3 percentage points. This confirms that these results do indeed arise from the top of the distribution of childcare shocks.

Because a very large share of the overall growth in childcare coverage at the national level is driven by these childcare shocks, my estimates are to a large extent informative about the aggregate effect of the national plans. Appendix D.1 shows that this implies that these plans had virtually no effect on the aggregate salaried employment rate of mothers.

Lastly, in Appendix F.3, I discuss a variety of concerns about the validity of my identification strategy, e.g. the possible correlation with other policy changes, sampling issues or the validity of the fuzzy difference-in-difference setting. I show that these concerns do not affect my finding, i.e. that obtaining a childcare place does not lead to massive changes in mothers' labor outcomes.

6 Substitution across childcare solutions

I now investigate the crowding-out of other childcare solutions by the expansion of collective daycare provision, which might explain my null effects for maternal labor outcomes. To this end, I first consider the take-up of paid parental leave, and then investigate the demand for individualized childcare provided by childminders and nannies.

6.1 Paid parental leave

I use the CNAF dataset that provides information on the number of families receiving parental leave allowances at the municipality level as of 2009. Specifically, I divide this number by the number of children aged 2 or less to determine the share of parents receiving parental leave allowances for either a full-time or part-time parental leave.

I then apply my event-study analysis to these municipality-level data, on a restricted subset of municipalities that experienced a childcare shock between 2010 and 2014.[16][17] Figure 7 displays my estimates. Consistent with this rationale, I find that the expansion of affordable collective childcare facilities does not trigger any substantial change in the share of families with young children who receive parental leave allowances.

6.2 Individualized childcare

I rely on a cross-sectional and comprehensive version of the DADS dataset that provides information on earnings and hours paid to childminders and nannies, paid directly by households, as of 2009. Specifically, I aggregate hours at the municipality level for the entire 2009-2015 time period.

Childminders are subject to a strict regulation in terms of child-to-adult ratios, as are collective childcare facilities. Specifically, the law was changed in 2009, raising a chilminders' maximum childcare capacity from 3 to 4 children.[18] As a result, I propose a measure of the relative supply of formal individualized childcare at the municipality level as the total number of hours paid to childminders and nannies, multiplied by 4 and, divided by (i) the annual number of full-time employment spell hours (1820 hours), and (ii) the total number of children aged 0 to 2:

$$S_{c,t}^{\text{indiv}} = \frac{4H_{c,t}^{\text{indiv}}}{1820\left(N_{c,t}^{\text{birth}} + N_{c,t-1}^{\text{birth}} + N_{c,t-2}^{\text{birth}}\right)} \tag{5}$$

where $H_{c,t}^{\text{indiv}}$ is the number of hours paid to childminders and nannies in municipality c during year t, and $N_{c,t}^{\text{birth}}$ is the number of children born to women who lived in c at time t. This measure approximates the concept of how many hours childminders and nannies work relative to how much they would be working if all children were under their care. It is not a perfect measure of this relative supply concept, however, because: (i) the legal four-children threshold includes the child-

[16] I weight the data by the number of children aged two or less as observed in 2007.

[17] Specifically, I implement the Sun and Abraham (2020) specification of the event-study design that allows for heterogeneous treatment effects across cohorts.

[18] Loi n° 2008-1330 du 17 décembre 2008 de financement de la sécurité sociale pour 2009

minder's own children, who I cannot observe; and (ii) a childminders' maximum childcare capacity is fixed by an agreement quite similar to that of an EAJE facility, depends on their education, experience, and equipment (e.g. the number of rooms in their home). Four is the upper bound for this capacity. However, in 2014, the average number of children per childminder was 3.3 (Vroylandt, 2016) so that, while imperfect, this measure is not meaningless.

I then replicate my event-study analysis, with $S_{c,t}^{\text{indiv}}$ as the outcome, on a restricted subset of municipalities that experienced a childcare shock between 2010 and 2014.[19][20] Figure 8 displays my estimates. I find that in the medium run, in municipalities that experienced the largest shocks on collective childcare supply, substitution effects dominate: demand for childminders and nannies drops substantially. The magnitude of my estimates, about 13 percentage points, is almost equal to the magnitude of the corresponding collective childcare expansions (14 p.p.). This suggests sizeable crowding-out effects are at play: in other words, childcare expansions tend to shift families away from costly individualized childcare solutions.

On top of explaining the null effect of collective childcare on maternal labor supply, these substitution effects are crucial for the evaluation of the policy at stake. Indeed, in the French context in which all formal childcare solutions are subsidized through different channels, taking them into account changes the cost of a collective childcare place for public finances quite dramatically. Appendix D.2 develops this point and shows that, when the crowding-out of individualized childcare solutions is taken into account, the estimated cost of the policy is divided by 2.7.

Lastly, in Appendix F.4, I assess the robustness of these results to various concerns regarding the validity of my identification strategy, e.g. correlation with other policy changes or division bias. I find my results on parental leave take-up and demand for individualized childcare to be unaffected by these issues.

7 Conclusion

In this paper, I leverage differences across French municipalities in the timing of collective childcare expansions to identify the causal impact of affordable collective

[19] I weight the data by the number of children aged 2 or less as observed in 2007.

[20] Here again, I implement the Sun and Abraham (2020) specification of the event-study design that allows for heterogeneous treatment effects across cohorts.

childcare on parents' labor outcomes. Applying an event-study framework to a combination of administrative records, I show that such expansions did not trigger any substantial change in the labor earnings and labor supply of parents with children in the targeted age groups. Interpreted as a local average treatment effect (LATE), my instrumental variable estimates suggest that, among mothers who obtained a collective childcare place thanks to these expansions, this treatment did not strengthen labor market attachment. This is because the expansion of affordable collective childcare did not make mothers any less likely to benefit from paid parental leave. I provide evidence that instead, these expansions resulted first and foremost in a substantial crowding-out of individualized and more costly childcare solutions.

As these estimates are only informative about the choices of parents who were offered a childcare place under the national plans that I investigate, these results do not contradict the intuition that the lack of affordable childcare solutions may prevent some mothers from entering the workforce when they have young children (see Appendix A.4). Instead, they draw attention to the selection of recipients of these newly created childcare places, who, my results suggest, would have otherwise relied on other formal childcare solutions.

Two mechanisms may explain these results. The first one deals is into application: it might be that families who would benefit most from a place are less likely to apply, due possibly to heterogeneity in preferences, exposure to social norms or heterogeneous returns on time spent in the labor market. For instance, strong cultural norms regarding childcare provided by mothers may prevent some families from applying for a collective childcare place, even though obtaining a place would actually change their work-family balance. The second is selection into treatment: in this setting, among actual applicants, childcare place may be offered preferentially to families who will benefit less from them. Survey data suggests, for instance, that one of their roles is to foster a better work-family balance, about two thirds of EAJE-PSU facilities give higher priority to families in which both parents hold a full-time job (Onape, 2012). Conditioning treatment on actual observed outcomes, instead of unobserved treatment effects would then result in inefficiencies (Yamaguchi, Asai, and Kambayashi, 2018). Disentangling the two mechanisms therefore has relevant policy implications, but requires additional data on childcare preferences, application and selection into collective facilities.

Lastly, this empirical policy evaluation exercise does not consider how childcare choices affect children themselves, whose long-term outcomes may be substantially affected. Indeed, early childcare choices may affect children's health and early learning, thereby affecting their future socialization, education and labor market prospects (see e.g. Havnes and Mogstad, 2011b; García et al., 2020). These potential lifecycle benefits must be taken into account to achieve a meaningful normative analysis of these policies.

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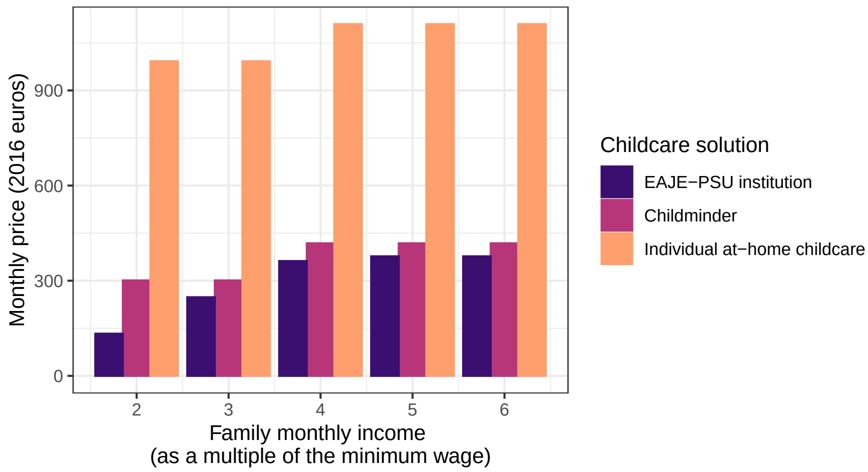
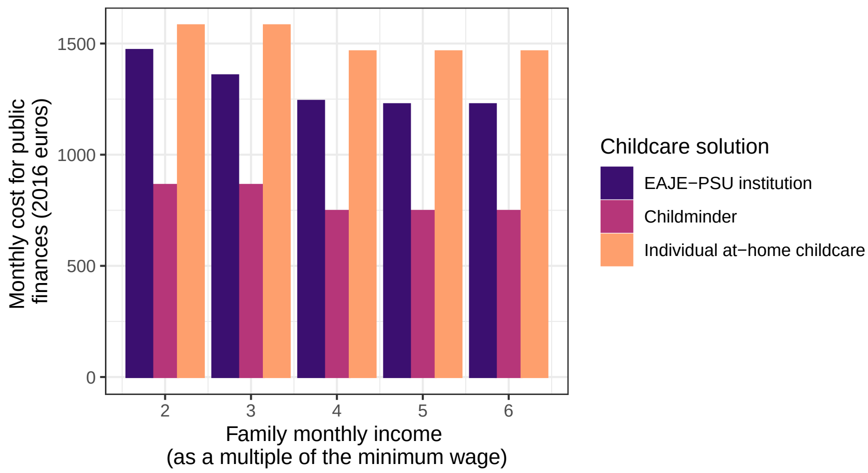
Figures

Figure 1 – Childcare prices along the income distribution

(a) Prices paid by families

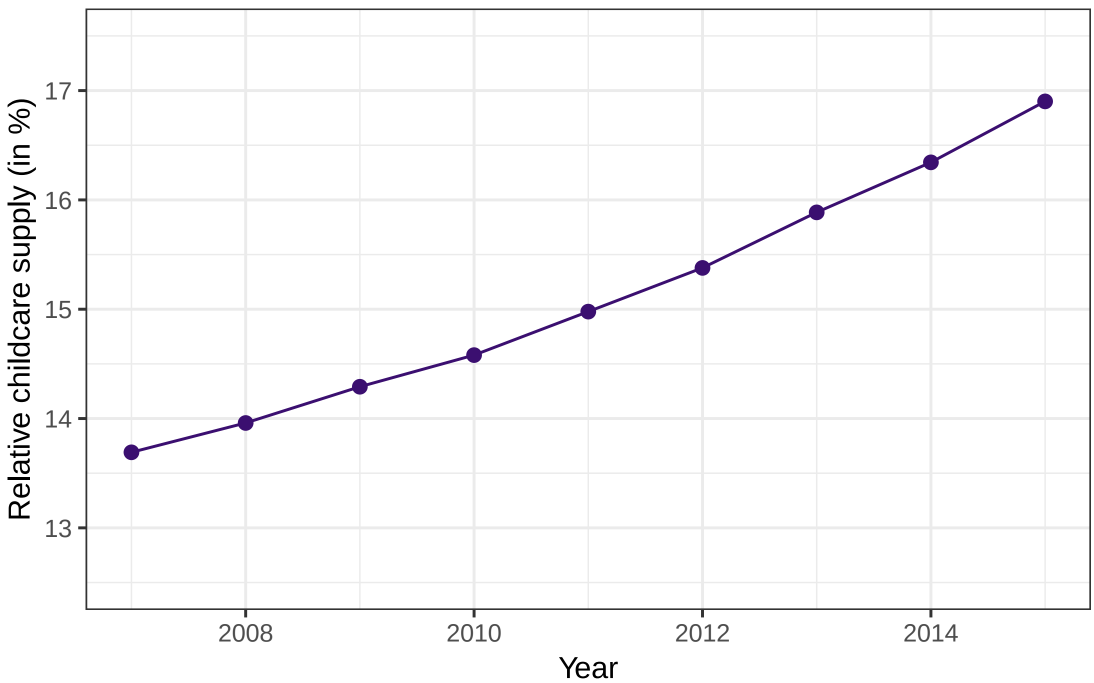


(b) Costs for public finances



Monthly price paid by families and monthly cost for public finances along the income distribution, by choice of childcare solution.
Source. CNAF, case-study estimates (Onape, 2017).

Figure 2 – Relative supply EAJE-PSU affordable collective childcare at the national level from 2007 to 2015

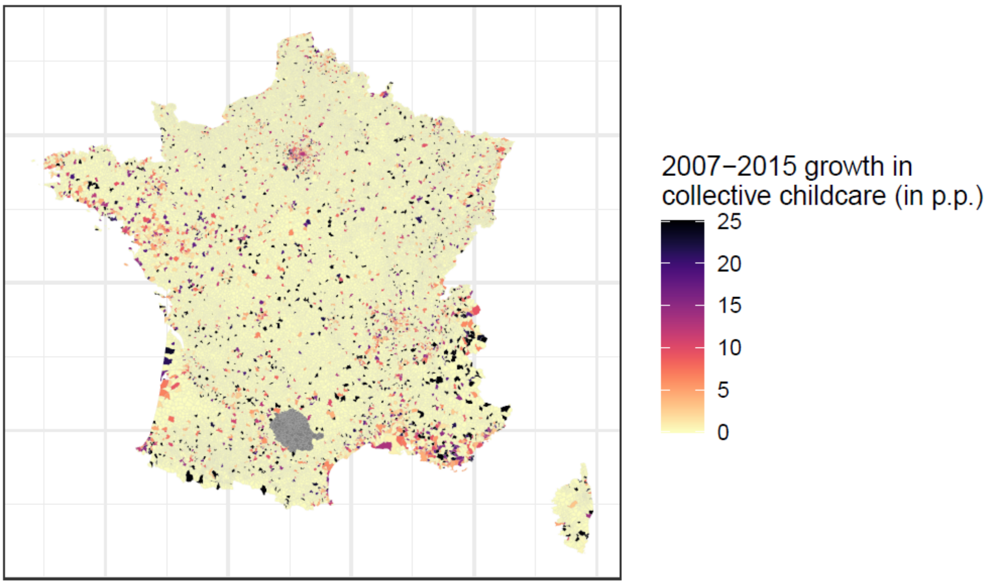


Estimates of the ratio EAJE-PSU childcare places offered to children aged 2 or less in metropolitan France (mainland France and Corsica).

Note. Data on the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records, Insee.

Figure 3 – Spatial distribution of the 2007-2015 growth in relative supply of EAJE-PSU affordable collective childcare

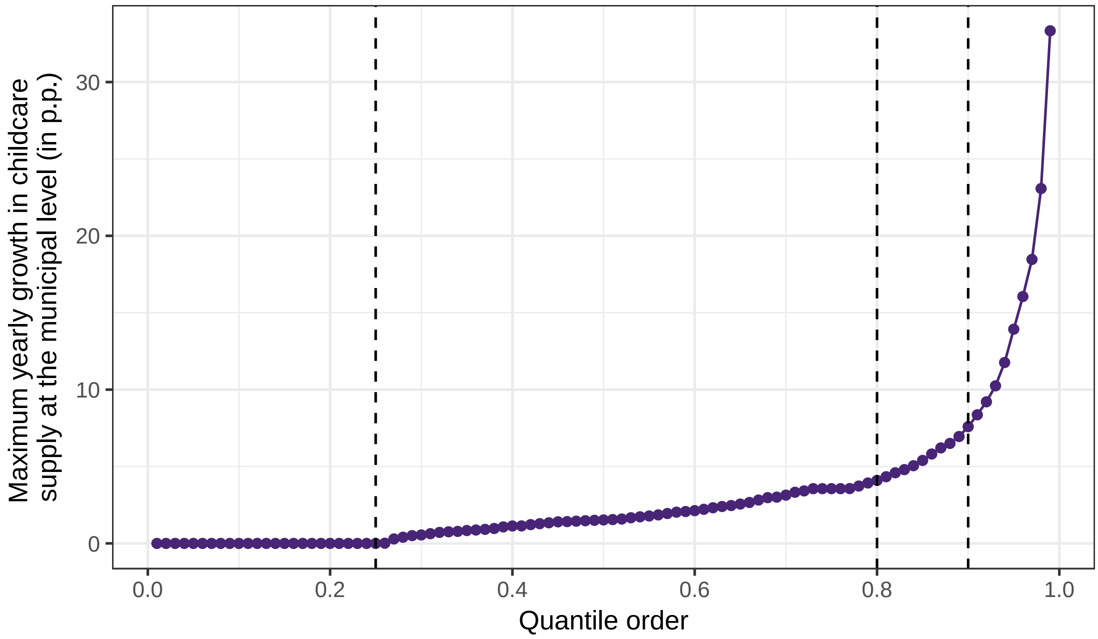


Estimates of the 2007-2015 growth in the ratio of EAJE-PSU placed offered to children aged 2 or less at municipality level.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records, Insee.

Figure 4 – Distribution of maximum annual within-municipality growth in affordable collective childcare coverage

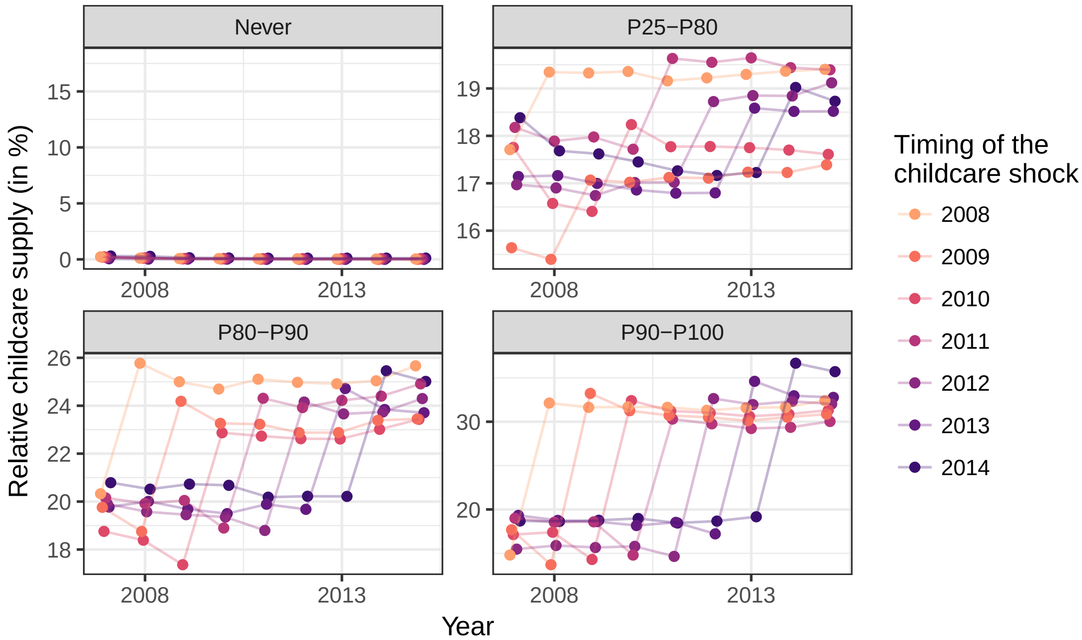


Estimates of the highest annual within-municipality growth in the ratio EAJE-PSU childcare places offered to children aged 2 or less.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records, Insee.

Figure 5 – Relative supply of EAJE-PSU affordable childcare, by treatment group and timing of the childcare shock



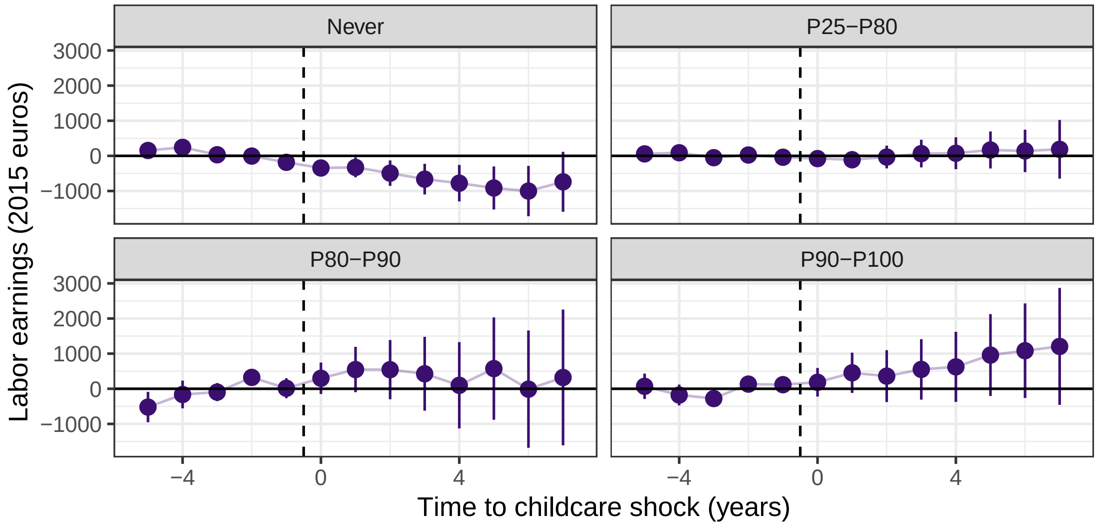
Estimates of the ratio of EAJE-PSU childcare places offered to children aged 2 or less at the municipality level.

Note. Data regarding the Tarn département are omitted.

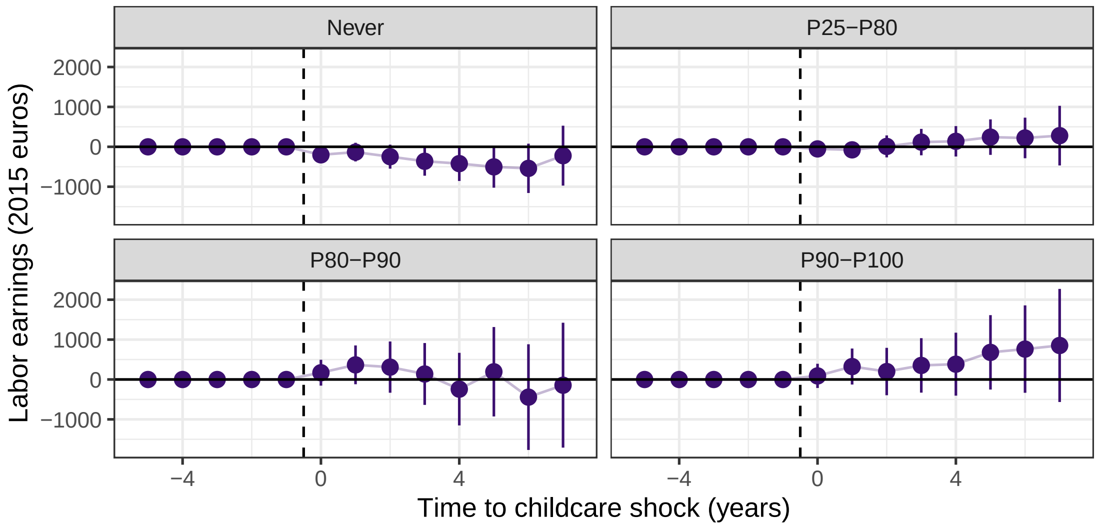
Source. EAJE-PSU records, CNAF. Birth records, Insee.

Figure 6 – Event-study estimates of the impact of the childcare shock on mothers' labor earnings, by treatment group

(a) Normalized pretrend

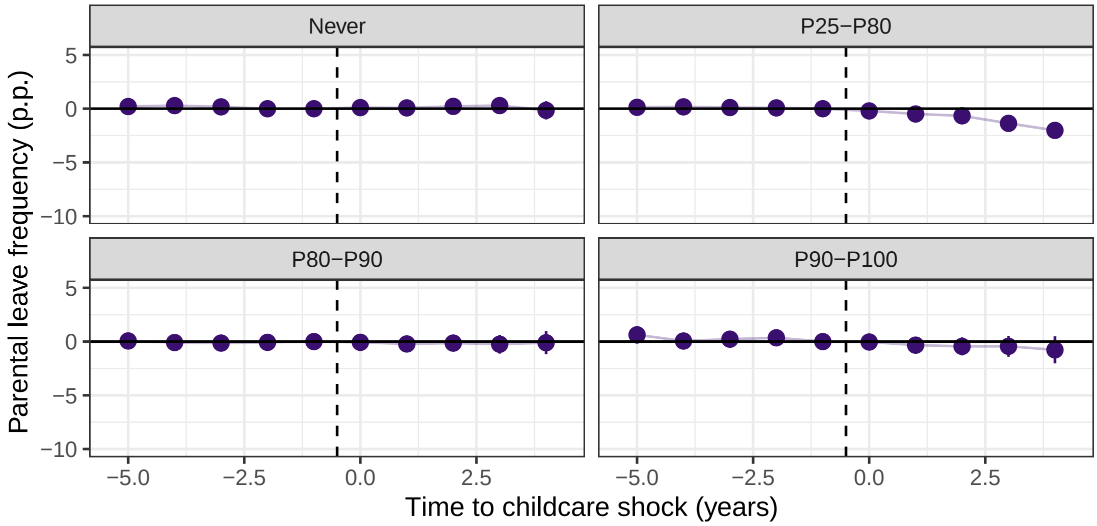


(b) Pretrend set to 0



Event-study estimates of the effect of childcare shocks on mothers' labor earnings (Model 2).
Note. Data regarding the Tarn département are omitted.
Source. EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

Figure 7 – Event-study estimates of the impact of the childcare shock on paid parental leave take-up, by treatment group

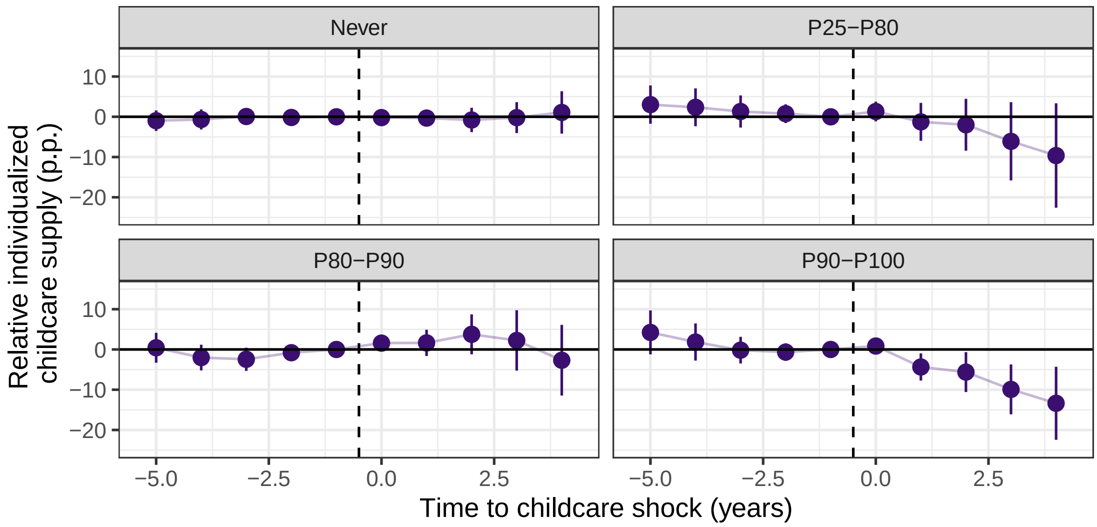


Event-study estimates of the effect of childcare shocks on the share of families receiving parental leave allowances.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU and PAJE records, CNAF. Birth records, Insee

Figure 8 – Event-study estimates of the impact of the childcare shock on the supply of individualized childcare, by treatment group



Event-study estimates of the effect of childcare shocks on individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records and comprehensive DADS records, Insee

Tables

Table 1 – Data description

Dataset	Source	Main variables	Individual identifier	Municipality identifier
EAJE records	CNAF	# childcare places		✓
PAJE records	CNAF	# families receiving parental leave benefits		✓
DADS panel	Insee	Earnings, days and hours worked	✓	✓
DADS comprehensive records	Insee	Earnings, days and hours worked, detailed occupation		✓
Birth records	Insee	Date of birth		✓
EDP panel	Insee	Date of birth of parents' children, education	✓	

Table 2 – Summary statistics

	Mothers		Fathers	
# Observations	740,412		775,658	
# Individuals	212,108		221,335	
	Mean	Standard Deviation	Mean	Standard Deviation
a. Individual characteristics				
Age	31.4	5.1	34.1	6.2
Number of children*	1.8	0.9	1.8	1.0
Higher education**	0.22	0.17	0.18	0.15
Lower secondary education**	0.09	0.08	0.14	0.12
b. Treatment rate				
Childcare supply	15.0	14.8	15.0	14.5
c. Labor outcomes				
Earnings (2015€)	10,760	12,490	19,460	19,600
Employment	0.67	0.47	0.82	0.38
Days worked	317	125	345	122
Hours per day	4.0	1.3	4.8	1.1
Hourly wages (2015€)	12.1	5.8	14.1	9.1

*Among individuals born in October. **Among those with available information. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

Table 3 – Event-study estimates of the impact of childcare expansions on parents' labor outcomes

Treatment group	Childcare supply (p.p.)	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers with children aged 0 to 2						
Never	0.04	-287.58	-0.49	-4.69	-0.014	-0.024
	(0.05)	(154.23)	(0.7)	(2.12)	(0.023)	(0.073)
P25-P80	1.98	51.51	0.6	2.3	0.016	-0.031
	(0.27)	(139.63)	(0.6)	(1.86)	(0.019)	(0.076)
P80-P90	5.03	128.88	0.97	0.43	-0.023	-0.224
	(0.38)	(337.47)	(1.12)	(3.57)	(0.041)	(0.171)
P90-P100	17.55	348.57	0.46	1.02	0.002	0.266
	(0.78)	(301.11)	(1.1)	(3.52)	(0.037)	(0.156)
Fathers with children aged 0 to 2						
Never	0	268.26	0.01	2.41	-0.006	0.065
	(0.05)	(215.56)	(0.51)	(1.58)	(0.016)	(0.102)
P25-P80	1.97	257.38	0.03	0.96	0.028	0.08
	(0.27)	(196.45)	(0.51)	(1.5)	(0.015)	(0.09)
P80-P90	5.13	-840.95	1.22	-1.79	0.004	-0.826
	(0.38)	(495.65)	(0.94)	(3.34)	(0.029)	(0.243)
P90-P100	16.94	-226.41	1.23	-4.15	-0.017	-0.07
	(0.7)	(443.8)	(0.89)	(2.92)	(0.026)	(0.242)

Dependent variable. EAJE-PSU childcare supply and parents' labor outcomes. *Explanatory variables.* Time-to-event and calendar-time dummies, interacted with treatment group, plus municipality fixed effects. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

Table 4 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
a. Full sample					
−5–−1	-710.47	-3.23	-1.02	-0.008	-0.412
	(1065.09)	(3.99)	(15.72)	(0.146)	(0.44)
0–2	407.48	-1.73	-2.66	-0.066	0.681
	(894.34)	(3.58)	(11.23)	(0.126)	(0.473)
3–10	-27.31	0.04	8.62	0.079	-0.626
	(793.09)	(2.56)	(9.1)	(0.091)	(0.393)
b. P90-P100 treatment group					
−5–−1	-1008.4	-3.95	4.07	0.029	-0.503
	(1116.39)	(4.26)	(16.97)	(0.156)	(0.459)
0–2	174.44	-3.07	-9.41	-0.027	0.722
	(944.92)	(3.78)	(11.82)	(0.134)	(0.486)
3–10	552.18	1.65	12.09	0.131	-0.522
	(819.24)	(2.65)	(9.53)	(0.096)	(0.404)
Fathers					
a. Full sample					
−5–−1	1389.02	5.76	-1.01	-0.122	-0.349
	(1477.61)	(3.91)	(14.13)	(0.123)	(0.758)
0–2	603.78	3.02	-2.64	0.005	0.053
	(1395.13)	(2.8)	(9.4)	(0.088)	(0.671)
3–10	2170.21	2.9	-3.66	0.064	0.757
	(1288.64)	(2.28)	(7.9)	(0.073)	(0.657)
b. P90-P100 treatment group					
−5–−1	1512.79	4.02	-0.4	-0.04	-0.104
	(1543.95)	(4.01)	(15)	(0.131)	(0.798)
0–2	-91.51	2.72	-1.85	-0.018	-0.09
	(1461.17)	(2.9)	(9.84)	(0.093)	(0.715)
3–10	2897.72	3.95	0.18	0.052	0.872
	(1353.67)	(2.35)	(8.2)	(0.076)	(0.692)

Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply and calendar-time dummies interacted with treatment group, plus municipality fixed effects. Childcare supply is instrumented by time-to-event dummies interacted with treatment group. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

A Institutional background

A.1 EAJE-PSU institutions

EAJE facilities are strictly regulated, in accordance with the Public Health Code, and cannot operate without authorization from local authorities (either at the département level for private facilities, or at municipal level for public ones), after an accreditation by the Maternal and Child Health Services. For each facility, this authorization defines a maximum capacity in terms of the number of childcare places.

The PSU funding covers 66% of the hourly cost of childcare, after families' contributions have been deducted. To obtain it, an EAJE facility must meet several requirements: (i) it has been authorized to open by the relevant authorities; (ii) its daycare places are open to all families; (iii) its pricing is based on a national fee schedule that makes this type of childcare particularly affordable to families; and (iv) it has signed an agreement on targets and management practices with the local CAF office.

In the national fee schedule, the upper bound of the *hourly* price paid by families is about 0.06% of their total *monthly* income, with a lower and an upper threshold on the total fees. A general rule of thumb is that the direct cost for parents of a full-time childcare place is between 5% and 10% of household income (IGAS/IGF, 2017). In 2015, the average hourly price that families paid was €1.80 (Clément and Aho, 2018). By contrast, other formal childcare solutions, i.e. childminders or at-home childcare were much more expensive, especially for families at the lowest end of the income distribution.[21]

To achieve these low prices, EAJE-PSU childcare facilities are heavily subsidized. In 2015, the average total hourly cost of an EAJE place was €8.86 (Clément and Aho, 2018), the average operating cost of a full-time EAJE place was €15 000, of which the Family branch of the Social security contributed up to 44.4%, local authorities 19.1%, other public stake-holders 18% and families 18.1% (Onape, 2017).[22] Overall, the operating costs of EAJE-PSU facilities in 2015 amounted to €6 billion.

Lastly, EAJE facilities were opened for 222 days a year on average in 2015,

[21] A reason for this is that individualized childcare solutions are subsidized through tax credits and tax rebates that make them particularly appealing to families that pay large income taxes.

[22] Childcare subsidies are not restricted to EAJE-PSU facilities, as other forms of childcare are also heavily subsidized.

which amounts to around 5 days a week excluding July and August (Onape, 2017), and for 11 hours a day on average. With respect to an ideal situation of full-time occupancy of all childcare places throughout the year, the occupancy rate was about 70%. These quantities varied very little throughout my period of interest.

A.2 National plans

To benefit from subsidies related to the national plans, the instigators of a project, usually a municipality, have to apply to the relevant local CAF office. National guidelines and criteria edicted by the CNAF are then used to assess the eligibility of each project. Unfortunately, the detail of these guidelines and criteria and their changes over time is not available. In 2013, the national guidelines stated that local CAF offices only rank projects according to the coverage rate, i.e. the number of formal childcare places relative to the number of children aged 3 or less.[23] As a result, municipalities with low coverage rate are given a higher priority. Additional criteria can be used to offer additional subsidies to applicants, e.g. if municipalities have a particularly low coverage rate or are relatively poor so that local taxes are less likely to cover the costs of the project.

These investment plans represent a substantial burden for public finances: in 2016, it was estimated that since the launch of the first plan in 2000, spending on the *plans crèches* had totaled €2 billion, not counting the annual operating costs of the childcare places created (IGAS/IGF, 2017). The average cost of creating a childcare place was estimated about €27 000 in 2009, 29% of which was financed through the national expansion plans (Onape, 2010).

[23] Lettre circulaire n° 2013-149 de la Direction des politiques familiales et sociale

A.3 Parental leave policies

Various parental benefits were merged in 2004 into the *Prestation d'Accueil du Jeune Enfant* (PAJE). The PAJE comprises means-tested lump-sum payment after a birth (*prime de naissance*), monthly means-tested benefits (*allocations familiales*), a childcare subsidy (*Complément libre choix du Mode de Garde* (CMG)), and some benefits that may be granted when a parents interrupt his or her career or opts to work part-time (previously *Complément Libre Choix d'Activité* (CLCA) and now *Prestation Partagée d'Éducation de l'enfant* (PreParE)). Additionally, parents are entitled by law to extend the duration of their parental leave if they are not offered a formal childcare place.[24]

These child benefits date back to 1985 and were introduced with the creation of *Allocation Parentale d'Éducation* (APE) initially restricted to mothers of 3 or more children. The APE was extended to mothers of 2 children in 1994, and was replaced by the CLCA in 2004, becoming effective with the first birth and providing a fixed non-means-tested monthly amount for the maximum duration of 6 months. The CLCA was replaced in 2015 by PreParE that introduced incentives to split parental leave between both parents. Contrary to Sweden, for instance, the benefits do not depend on parents' past income: they amount to approximately €400 per month in the case of career interruption and to nearly €200 in the case of 80% part-time work. Several papers have shown that these benefits encourage some mothers to reduce their labor supply (Choné, Le Blanc, and Robert-Bobée, 2004; Piketty, 2005; Lequien, 2012; Joseph et al., 2013).

[24] Article L531-4 of the French Social Security Code.

A.4 Childcare preferences and choices

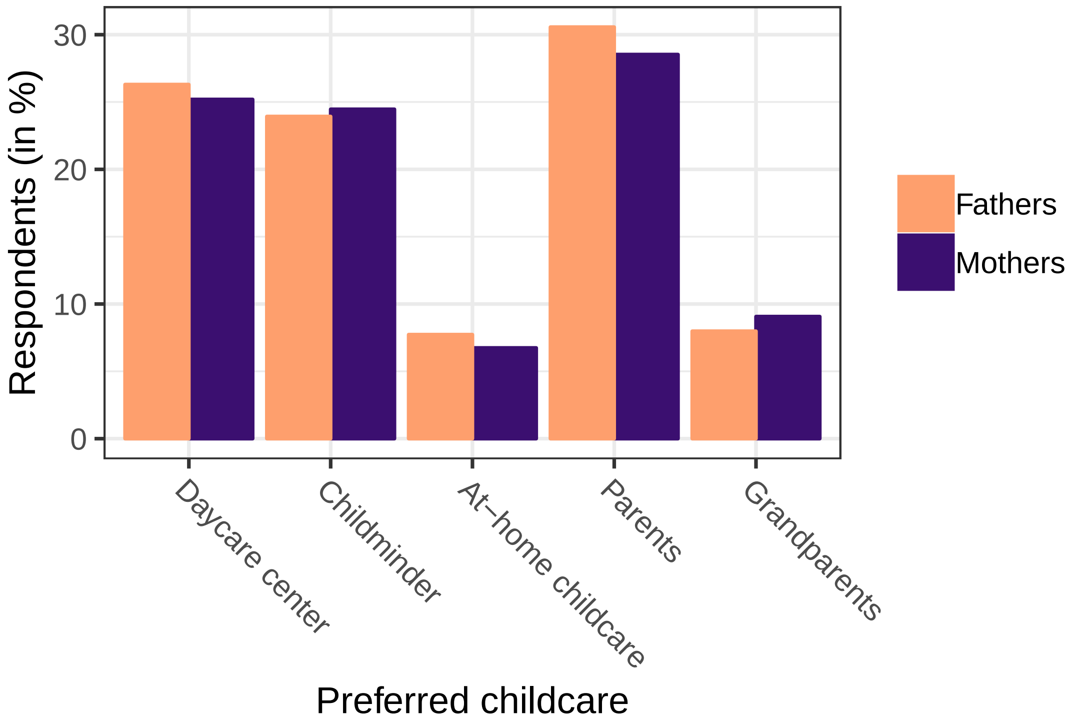
On top of these institutional pushes towards increasing provision of affordable, formal collective childcare, there is strong demand from parents for these services. In 2010, Insee implemented a complementary module to the French Labor Force Survey (*Enquête Emploi*) devoted the work-family balance. In this survey, 1999 individuals with children under age 3 were asked what type of childcare solution they thought was ideal for children the same age as their youngest child; what was their actual choice of childcare arrangement; what kinds of constraints they met when making this choice; and lastly, whether this choice impacted their labor supply decisions. I take advantage of these data to shed further light on potential demand for the kind of childcare solutions examined in this paper.

Firstly, daycare centers are among parents' preferred childcare arrangements: over 25% of both mothers and fathers of children under age 3 mention it as the ideal childcare solution (Figure A.1). While a slightly higher proportion of parents view childcare by parents as ideal (about 30%), no other childcare solution is more frequently mentioned by parents as their preferred option.

Secondly, while 67% of parents who indeed rely on this childcare arrangement view it as ideal, only 32% of parents who mention it as their preferred solution actually use it (Figure A.2). Additionally, these are most likely to report difficulties in accessing childcare: 31% of them report difficulties of this kind (Figure A.3), among whom 70% mention the lack of availability of their desired childcare solution as the main problem encountered.

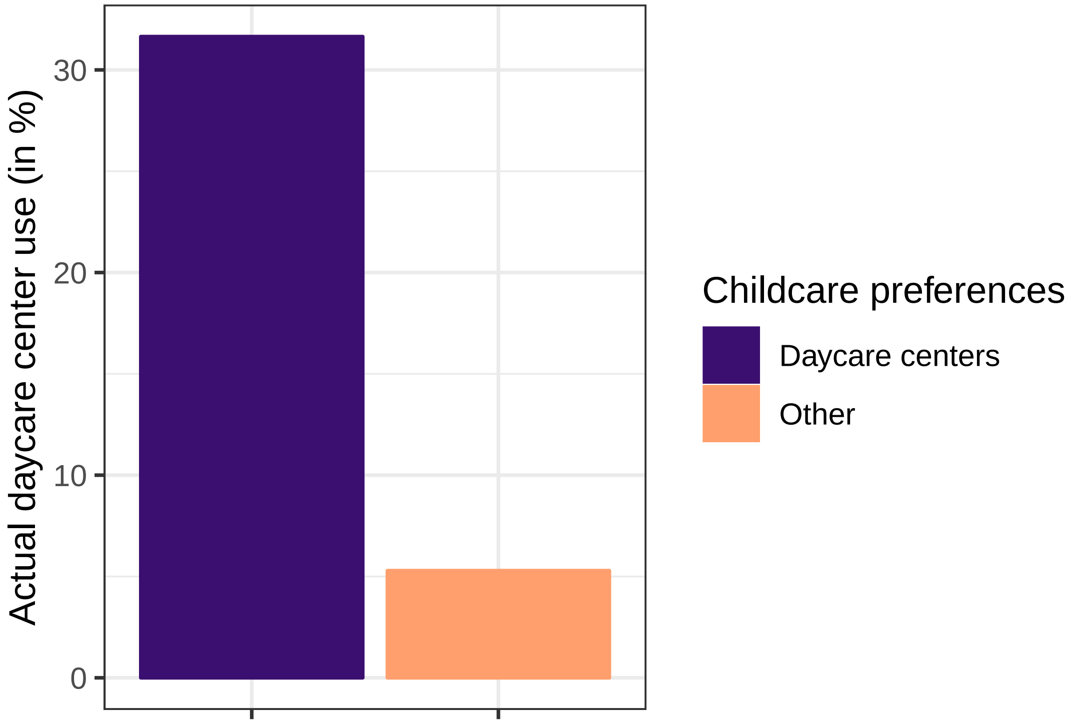
Lastly, among mothers of young children who are not working full-time and who do not use daycare centers, those who consider them to be the ideal childcare solution are the most likely to report that either insufficient availability, or the cost of childcare impacted their labor supply decision (Figure A.4).

Figure A.1 – Ideal childcare solution reporting by parents of children under age 3



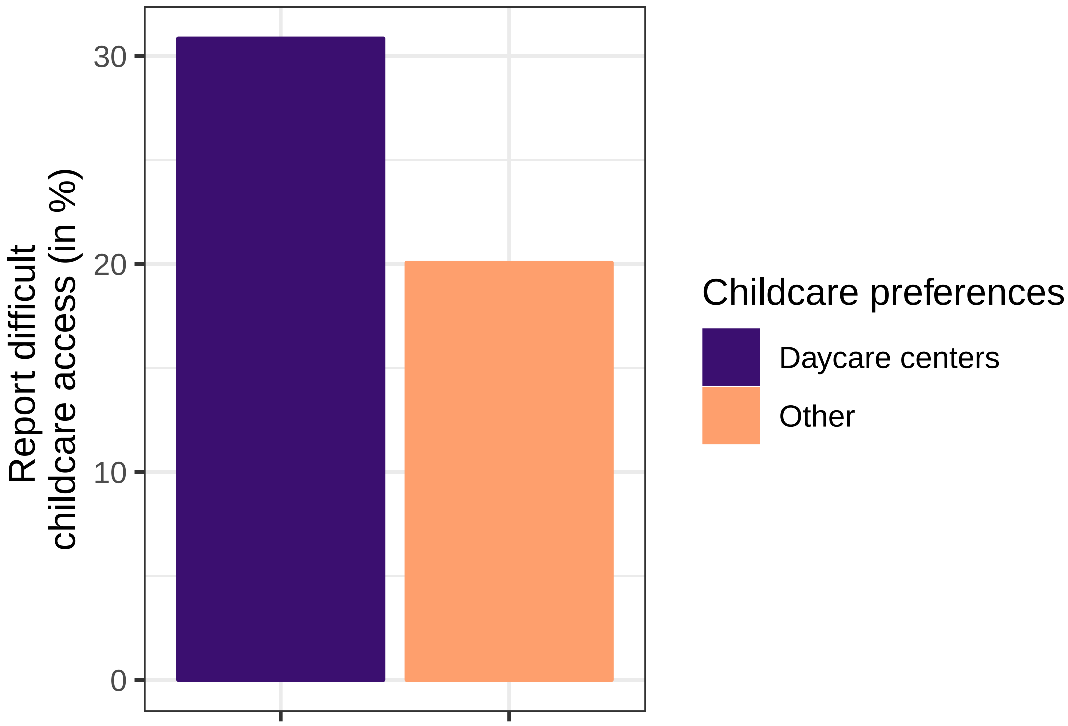
Ideal childcare solution for children of the same as their youngest child, reported by parents of children under age 3.
Source. LFS complimentary module 2010, Insee.

Figure A.2 – Actual childcare choices of parents of children under age 3



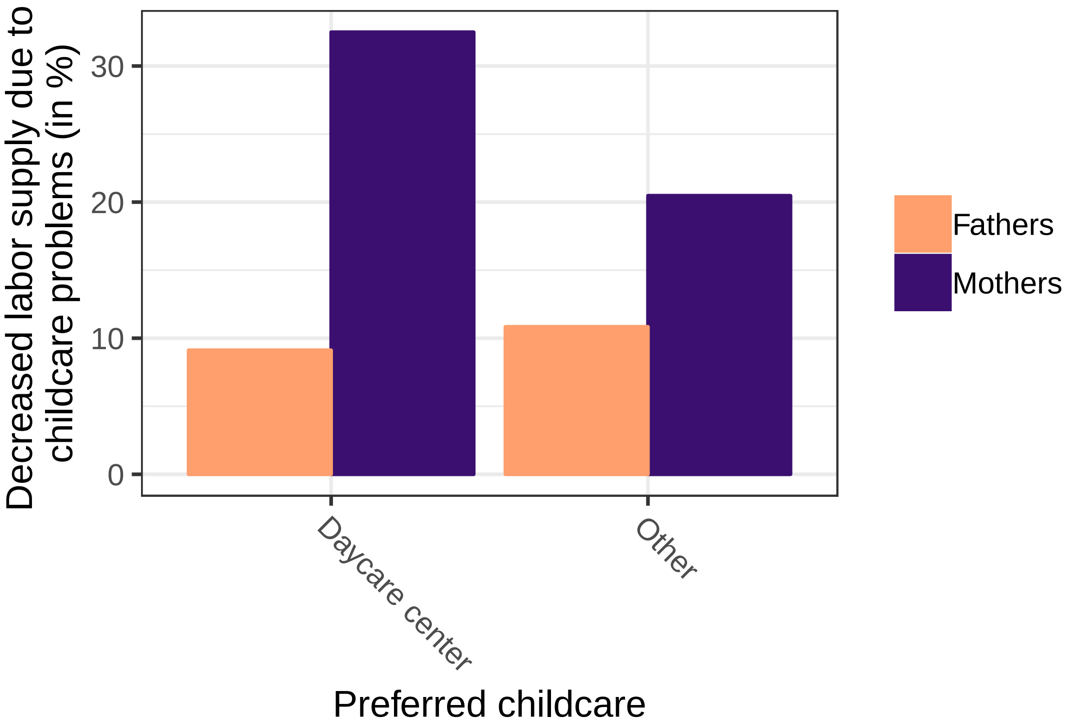
Actual childcare solution used by parents of children under age 3, by preferred childcare solution.
Source. LFS complimentary module 2010, Insee.

Figure A.3 – Difficulties in childcare access as reported by parents of children under age 3



Share of parents of children under age 3 who report difficulties in access to childcare, by preferred childcare solution.
Source. LFS complimentary module 2010, Insee.

Figure A.4 – Impact of childcare availability on labor supply decisions, as reported by parents of children under age 3



Share of parents of children under age 3 who do not work full-time who report reducing their labor supply due to a lack of appropriate childcare, by gender and preferred childcare solution. *Source.* LFS complimentary module 2010, Insee.

B Data

B.1 Family insurance data

The parental leave dataset provided by CNAF suffers from a few issues. First, it does not allow to distinguish between full-time and part-time parental leave-takers. Second, it does not provide information on which parent took the leave. Third, in 2015 there were no data on the take-up of PreParE – the newly created parental leave allowance, available to parents of children born in 2015 or later. For this reason, when investigating the take-up of parental leave benefits, I rely solely on data that cover the 2009-2014 period, and only consider the take-up of the CLCA allowance. Lastly, for reasons of personal data protection, the dataset does not provide information on municipalities where fewer than 5 families took the leave. To the extent that childcare facilities are generally located in municipalities where number are well above this threshold, this does not appear to be a major obstacle to my analysis.

B.2 Labor market data

B.2.1 Earnings and working time measures

Earnings My measure of labor earnings is based on net annual earnings. This measure aggregates all wages paid to an individual, including performance pay and bonuses, annual leave, in-kind benefits, the share of severance payments that exceeds the legal minimum, and early retirement benefits (to the extent that these benefits exceed an amount approximately equal to the minimum wage) but excludes stock-options. Social security and public pension contributions, unemployment contributions and other contributions, including two flat-rate taxes on earned income (CSG and CRDS), are subtracted from this amount to compute our measure of net annual earnings. I thus measure earnings before income tax but after some transfers.

Maternity leave allowances are paid by the Social Security administration, and as such are not part of my measure of earnings. They may, however, be paid through the employer (*subrogation*), in which case the employer pays the employee the equivalent of maternity leave allowances during her maternity leave, and is later reimbursed by the Social Security administration. The maternity leave allowances that the employer advanced are subtracted from my measure of earnings. As the amount is reimbursed after the maternity leave has begun, the woman's decline in earnings may occur a few weeks after the start of her maternity leave. Because I consider annual earnings, this problem is restricted to births that occur at the end of the calendar year.

Lastly, in some firms the employer may be required under a collective agreement to complement earnings during maternity or sick leaves on top of the allowances paid by the Social Security. This complement is counted by the DADS as labor earnings.

Days In the DADS dataset, days paid refers to the duration of an employee's presence in a firm's workforce within a given year. As a result, maternity leave, sick leave, or paid annual leave are part of this measure of days, whereas a period of unemployment between two distinct employment spells is not. Additionally, this measure of days is capped at 360.

Hours In the DADS dataset, hours paid refers to hours for which the worker is paid under their labor contract. The data on hours is reported by employers when they fill out payroll tax forms. Before making the data available, Insee performs three checks:

- the total number of hours for a given individual × employer × year observation should not exceed an industry-specific threshold of 2,500 hours per year in a small subset of industries (mostly manufacturing industries, transportation, hotels and restaurants), and 2,200 hours per year in the rest of the private sector;
- the implied hourly wages should exceed 80% of the minimum wage;
- the total number of hours should be positive, with the exception of a narrow subset of occupations (mostly journalists and salespersons) working on a fixed-price or commission basis.

If one of these conditions is not met, Insee ascribes hours to the observation to make the hourly wage consistent within narrow cells defined by 4-digit occupation, full-time or part-time status, age and gender.

For workers whose pay does not depend on the time worked, but who do not belong to any of the above-mentioned occupations, i.e., typically highly-qualified personnel working in a "day rate" (*"forfait-jour"*), employers provide the number of days only. A number of hours is ascribed to these observations based on the legal working hours of full-time workers, the number of work days, and the implied hourly wages.

During a maternity leave, as an employee is not paid by for any hours by her employer but is instead paid by the Social Security (and may receive a top-up payment from her employer), hours paid are equal to 0. Workers not paid by the hour are an exception to this rule because their hours are imputed based on days paid, which do not vary during maternity leave. As a result, the DADS dataset overestimates hours paid – and underestimates hourly wages – for such workers during years when they give birth to children. In general, these are qualified workers in the upper part of the hourly wage distribution.

B.2.2 Parental earnings and labor supply

I take advantage of a longitudinal version of the DADS dataset that contains detailed information at the person-year level; individuals are identified by an anonymized personal identifier based on their social security number (NIR), that allows me to track them over time. Starting in 1967, the sample covers individuals born in October in even-numbered years; as of 2002, it also covers individuals born on January 2-5, April 1-4, July 1-4 and October 1-4 regardless of their year of birth. While information on earnings has been available since the creation of the dataset, information on hours paid is only available from 1995, with the exception of central government civil servants, for whom this information is not given before 2009.[25]

The limitation of this dataset most relevant for my study is the absence of information on self-employment. This may prove problematic if, as suggested by Connelly (1992), mothers tend to turn to self-employment as a more effective way to deal with child-related time constraints. Specifically, if increased affordable childcare provision gives rise to transitions from non-employment and salaried employment towards self-employment, then my estimates will be biased downwards. Conversely, if these increases cause mothers to shift from self-employment to non-employment or salaried employment, then my estimates will be biased upwards. However, this potential bias is likely to be limited: in 2007, less than 5% of mothers with children aged below 3 who held a job were self-employed, and just 4% of those who interrupted their careers were previously self-employed (Galtier, 2011).

A caveat of the data is that residence is not observed when individuals are not in the labor force. As a result, I have to impute the municipality of residence for person-year observations that correspond to individuals without a job. Specifically, I first impute the municipality of residence to individuals without a job using the municipality of residence when they last held a job, and the municipality of residence when they first held a job for observations that precede the first job held (see Figure B.1). As a robustness check, I consider the reverse method of imputation (i.e. using the municipality of residence at the time they hold their next job), and find that it does not change my results.

B.2.3 Individualized childcare

As of 2009, the DADS files also cover salaried employees who are directly paid by households, so this gives me information on childminders and nannies who provide at-home childcare. Specifically, I rely on this feature to compute aggregate earnings

[25]For these observations, I use a measure of working time expressed in full-time equivalent to impute working hours before 2009. Specifically, the dataset contains information on working time measured in full-time equivalent, that takes values between 0 and 1. I rescale this measure by multiplying it by the median hours of paid work by full-time workers over a full year (1820 hours a year throughout my period of interest).

and hours paid to childminders and nannies, based on 4-digit occupation,[26] at the municipality level from 2009 to 2015, from a comprehensive, cross-sectional version of the dataset.

[26] The DADS dataset contains an occupation variable based on the *Professions et Catégories Socioprofessionnelles* (PCS) classification. Specifically, I use the most detailed level of this classification, and focus on observations that belong to the *"563a – Childminders, baby-sitters and foster families"* category.

Figure B.1 – Imputation of the municipality of residence for jobless observations

	Municipality of residence								
Raw data	.	.	A	A	.	.	B	B	.
Baseline imputation	A	A	A	A	A	A	B	B	B
Robustness check	A	A	A	A	B	B	B	B	B

B.3 Fertility data

B.3.1 Cross-sectional records

I take advantage of cross-sectional comprehensive birth records to retrieve the mother's municipality of residence at the time of the birth. I use this feature to compute the number of children born to women living in a given municipality in any year between 2005 and 2015. This allows me to recover an approximate measure of the trends in potential demand for childcare at a narrow geographical level.

B.3.2 Longitudinal records

I use on a longitudinal version of these records at the individual level extracted from the *Échantillon Démographique Permanent* (permanent demographic sample, EDP) to obtain information on the timing of births. Thanks to the NIR, this dataset can be merged with the longitudinal version of the DADS. It covers individuals born on October 1-4 whatever their year of birth; information regarding individuals born on January 2-5, April 1-4 and July 1-4 is available from 2004.

A potential issue with the data is the absence of information on children born before 2004 to individuals who were born in January, April and July.[27] As a result, the number of children for these individuals is biased downwards. Because my period of interest is 2007-2015, this does not affect the identification of parents of young children in the data, but only the information regarding their past fertility decisions, i.e. whether they have older children or not. For this reason, when controlling for past fertility decisions, I always interact the number of children with a dummy variable that indicates whether the parents are born in October (in which case the data are correct) or not (in which case I underestimate the number of older children that parents have).

[27] In addition, some birth-related data for the 1990s were incomplete in administrative birth records for individuals born on October 2-3 (for details, see Wilner, 2016). For these individuals I use the census rather than birth records, as do Pora and Wilner (2019). The quality of this data is comparable to that concerning individuals born on October 1 or 4 for whom administrative birth records are available from 1967.

B.4 Sample definition

I exclude all individuals who have ever worked in the childcare industry, so that my results are not driven by the increasing labor demand in this sector. To insure against measurement error in the upper tail of the earnings distribution, and for very low working times, I winsorize earnings at the quantile of order 0.9999, and drop person-year observations that either (i) have fewer than 18 annual paid working days or (ii) have paid daily working hours below 1/20 of legal full-time hours or (iii) have hourly wages under 90% of the minimum wage. For these observations, I consider individuals to be out of the labor force, so that their labor earnings equal 0.

C Treatment groups composition

My empirical framework splits municipalities into four groups according to the magnitude of the largest increase in the provision of collective childcare between two consecutive years: a never treated group in which the supply never increases, and then 3 groups based on cutoffs at the 80th and the 90th percentile of the distribution. While my strategy is not based on between groups comparison, in Table C.1, I describe the composition of these groups in terms of municipality characteristics before my time-period of interest begin, i.e. in terms of observables in the 2006 Census.[28] This description is nevertheless useful to get a better sense of the composition of the P90-P100 group which is key to my identification strategy.

Specifically, I consider several potentially relevant dimensions:

- municipality size, as defined by the total 2006 population in the census;
- potential and actual birth rates, approximated by:
  - the share of women aged 15 to 49 in the total population;
  - the ratio of children aged less than 1 over the number of women aged 15 to 49;
- migration, as measured by the share of inhabitants who lived, 5 years after the data was collected, in another municipality, either in metropolitan France or abroad;
- couple formation, as measured by the share of single women (men) in the population of women (men) aged 20 to 49;
- marriage formation and dissolution, as measured by:
  - the share of married women (men) in the population of women (men) aged 20 to 49;
  - the share of divorced women (men) in the population of women (men) aged 20 to 49;

[28] This information was not necessarily collected in 2006: since 2004, the French Census is collected annually. Specifically, all French municipalities are surveyed over a five-year period. As a result, five census surveys were conducted from 2004 to 2008, and were finally combined to produce the Census results, dated 2006, i.e. the medium year. As a result, a slight part of the results relate to a time-period possibly affected by the treatment (year 2008).

- female labor force participation, as measured by the share of women who declared themselves to be housewives in the population of women aged 20 to 49;

- labor market composition, as measured by the share of women (men) who are managers or professionals in the population of women (men) aged 20 to 49.

Because I estimate labor supply effects at the individual-level of parents with potentially affected children, I weight this municipality-level data by the number of children aged 2 or less in 2007. As a result, larger municipalities are given much more weight than smaller municipalities in Table C.

Overall, the P90-P100 group is composed of relatively small municipalities, around 7,600 inhabitants. However, these municipalities do not depart much from other municipalities in terms of potential and actual birth rates, the share of inhabitants who did not live in these municipalities five years before the data was collected or female labor force participation. Marriage rates may be relatively high, and the share of both men and women with managerial or professional positions relatively low.

Table C.1 – Summary statistics at the municipality level: by treatment group

	Never		P25-P80		P80-P90		P90-P100	
# Municipalities	31,205		1,858		763		2,372	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Collective childcare supply in 2007 (in %)								
Childcare	0.2	5.4	18.2	8.4	20.2	11.4	17.6	23.4
Municipal population (2006)								
Pop.	1,400	1,400	254,200	568,500	21,900	18,500	7,600	7,900
Potential and actual birth rate (in %)								
Pot. mothers	21.9	2.5	25.3	2.6	23.9	2.3	22.9	2.7
Birth rate	5.8	2.5	5.2	1.0	5.3	1.0	5.2	1.3
Immigration rate (in %)								
Mig. (from Fr.)	24.3	7.7	21.1	5.5	23.3	4.8	24.5	6.2
Mig. (abroad)	0.8	1.6	2.4	1.5	1.8	1.1	1.4	1.6
Share of single individuals (in %)								
Single (f)	20.6	6.8	40.5	8.9	34.5	7.5	29.2	8.1
Single (m)	29.8	7.3	43.2	7.7	38.3	6.6	34.9	7.6
Share of married and divorced individuals (in %)								
Married (f)	56.8	7.9	41.4	8.9	46.8	7.3	50.8	7.9
Married (m)	48.6	8.0	37.7	7.8	42.3	6.6	44.9	7.5
Divorced (f)	5.9	2.8	7.8	1.7	7.9	1.8	7.5	2.1
Divorced (m)	4.9	2.4	5.1	1.1	5.2	1.2	5.3	1.5
Female labor force participation (in %)								
Housewives	9.7	5.8	10.7	5.2	9.9	4.6	9.6	4.5
Labor market composition (in %)								
Man. and Prof. (f)	5.5	5.4	10.3	7.6	11.1	8.2	8.4	6.4
Man. and Prof. (m)	9.6	7.9	16.1	10.0	18.3	12.3	14.7	10.3

Note. Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and 2006 Census, Insee.

D Policy evaluation

D.1 Aggregate labor supply effect

Figure 5 shows that the evolution of relative childcare supply is extremely flat both before and after the childcare shock. The contribution of municipalities to the national-level increase in affordable collective childcare provision is thus mostly attributable to these shocks, rather than a continuous increase in local supply. Figure D.1, decomposes the yearly growth at national level between (i) the contribution of shocks for each treatment group; (ii) the residual contribution of within-municipality growth when municipalities do not experience a shock; and (iii) a composition shift.[29] It clearly shows that the national-level increase is attributable first and foremost to these shocks, especially of those at top of the distribution. As a result, it is largely sufficient to analyze the consequences of these shocks to capture the overall consequences of collective childcare expansions at the national level, and thus to evaluate the impact of the national *plans crèches* over the relevant time period.

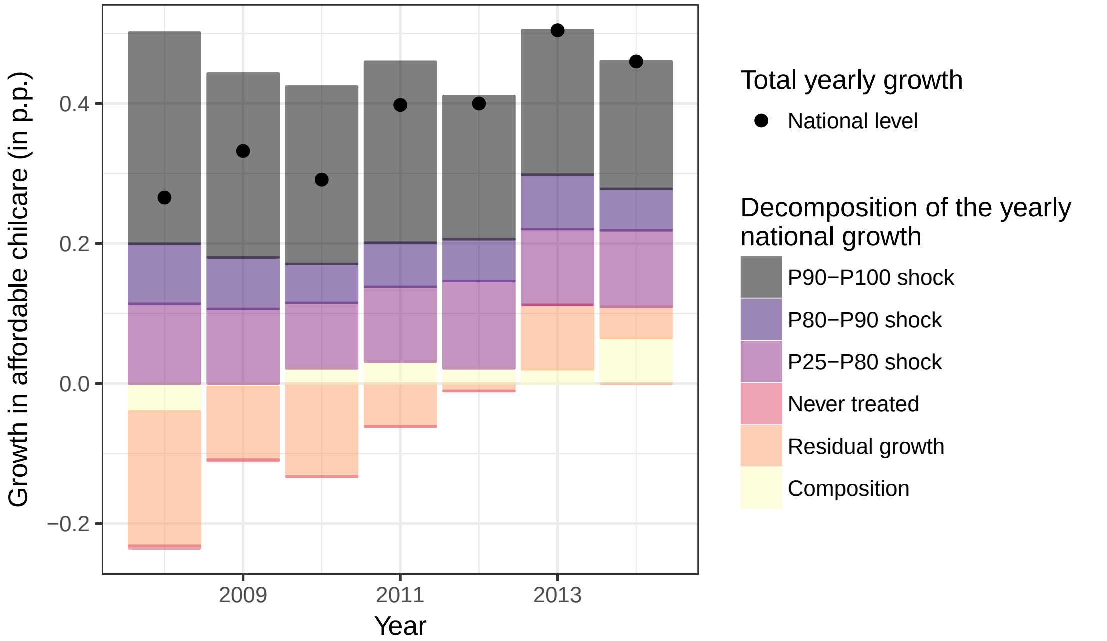
Between 2000 and 2016, 150 000 new childcare places were created under the national plans to expand affordable childcare provision (IGAS/IGF, 2017). Hence, taking my most optimistic estimate from Table 4, i.e. the upper bound of my 95% confidence interval, these newly created places enables 8 000 more mothers of young children to hold a salaried job in 2016.

There were about 1.9 million mothers with children under age 3 in 2016, with a salaried employment rate of 67%. As a result, a rough calculation, based on the upper bound of the 95% confidence interval, suggests that the national *plans crèches* contributed, at most, to a 0.4 percentage-point increase in the labor force participation of mothers between 2000 and 2016.[30] As a comparison, had the efficiency of the plans reached that of the Norwegian policies investigated by Andresen and Havnes (2019), the counterfactual increase would have amounted to 2.5 percentage points. Empirically, mothers' salaried employment varied very little between 2007 and 2015, fluctuating between 66% and 67%.

[29] Appendix E details this straightforward accounting decomposition.

[30] By contrast, my point-estimate estimate implies a 0.1 percentage point drop in the salaried employment rate.

Figure D.1 – Decomposition of national-level annual growth in relative supply of EAJE-PSU affordable collective childcare from 2008 to 2014



Estimates of the contribution of childcare shocks and composition shifts to the annual growth in EAJE-PSU childcare supply at national level (see Appendix E).

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU recordss, CNAF. Birth records, Insee.

D.2 Impact on public finances

I map my empirical results into a simple cost-effectiveness evaluation exercise. In the French context where collective childcare is only one among many subsidized childcare solutions, the analysis has to take into account the counterfactual burden for public finances of the childcare solution families would have chosen had they not been offered a place in an EAJE-PSU facility. Because of this, the national plans to expand collective childcare provision may prove beneficial, even with null effects on parental labor earnings and labor supply.

A severe limitation of my data is that they do not allow me to disentangle childminders from at-home childcare provided by nannies, two childcare solutions that have very different consequences for public finances: broadly speaking, the cost for public finances of a place offered by a childminder is 60% that of an EAJE-PSU place, whereas the public cost of tax credits that subsidize at-home childcare represents 120% of an EAJE-PSU place (Figure 1).

I design my cost-effectiveness analysis to take into account these crowding-out effects, and the differential burden associated with substitution across childcare solutions. Based on the medium-run effect of my event-study analysis (see Figure 8), I assume a total crowding-out of individualized childcare solutions, i.e. that every family who obtained a collective childcare place would have otherwise resorted to either a childminder or at-home childcare. I also assume a null effect as to parental earnings and parental labor supply. As I cannot disentangle one from the other, I consider two cases that can be regarded as bounds on the plausible actual scenario: firstly, if the crowding-out only affects childminders, and secondly, if it only affects at-home childcare.

In the first case, where the substitution effects only affect childminders, the burden for public finances increases. Assuming (i) the annual operating cost of a collective childcare place to be €15,000, of which 82% is covered by public finances, (ii) the collective cost of a childminder place to be 60% of this burden, and (iii) null effects on parental labor supply and total crowding-out, then the long-run burden of creating 150 000 collective childcare places amounts to an additional €738 million of public spending per year. In the second case, i.e. assuming that the substitution effects only affect at-home childcare, and that the public cost of tax credits for this solution to represent 120% those of a collective childcare place, the long-run effect of this plan corresponds to a €369 million reduction in annual public spending.

Table D.1 sums up these results, also considering an alternate counterfactual scenario in which families who receive a childcare place under the national plans are drawn randomly from the population of families that use either a childminder or at-home childcare.[31] Interpretation of the implied effectiveness-cost ratios is not straightforward as it depends on the directions of two different effects. Firstly, it depends on whether the plans have increased or decreased mothers' salaried employment and salaried earnings: as my confidence intervals always include 0, the worst-case scenario implies a reduction in mothers' labor outcomes. While at first glance, negative maternal labor supply effects seem unlikely, they should not be overlooked, especially since families would use more costly and more flexible childcare solutions had they not been granted a collective childcare place. These counterfactual solutions may therefore offer a better work-life balance than my treatment of interest – e.g. because childminders' working hours are more flexible than those of EAJE-PSU facilities – and the decrease in childcare prices also leaves room for possible income effects.

Secondly, these estimates also depend on whether the national plans produce an increase or a decrease in public spending: substitution from at-home to collective childcare would reduce the burden on public finances. In other words, negative (positive) estimates in the best-case scenario imply both a reduction (increase) in public spending and better labor outcomes for mothers, whereas negative (positive) estimates in the worst-case scenario correspond to an increase (reduction) in public spending paired with poorer labor outcomes for mothers.

As substitution effects lower the impact of the plans on public finances, my estimates appear less precise than they were when omitting these crowding-out effects. The sensitivity of these estimates, and of the implied effectiveness-cost ratios, to assumptions about the composition of the crowding-out effects is extremely salient: these assumptions imply very different conclusions as to the impact of these plans on public finances, not counting the fixed-costs of increasing collective childcare provision. Additional data on individualized childcare would thus be very useful for implementing a full policy analysis of these national plans.

[31] Using a childminder is more than 20 times more frequent than using at-home childcare.

Table D.1 – Empirical policy evaluation: counterfactual scenarios

Counterfactual substitution	No solution	Childminders	At-home childcare	Mixed-case
*Long-run operating cost**				
For one place (in €)	+12 300	+4 900	-2 500	+4 600
National plans (in M€)	+1 845	+738	-369	+688
*Effectiveness-cost ratio: additional years without career interruption per M€***				
Best case	+4.3	+10.8	-21.7	+11.5
Baseline	-1.4	-3.5	+6.9	-3.8
Worst case	-7.1	-17.8	+35.7	-19.2
*Effectiveness-cost ratio: salaried earnings gains per €****				
Best case	+0.18	+0.44	-0.87	+0.47
Baseline	+0.03	+0.08	-0.16	+0.09
Worst case	-0.11	-0.27	+0.53	-0.29

*Excluding the fixed cost paid to create additional childcare places. **The baseline is based on the point estimate, and the best (worst) case scenario is based on the upper (lower) bound of the 95% confidence interval of the estimated employment effect (see Table 4). ***The baseline is based on the point estimate, and the best (worst) case scenario is based on the upper (lower) bound of the 95% confidence interval of the estimated salaried earnings effect (see Table 4). *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records and PAJE records, CNAF. Birth records, comprehensive DADS records and DADS-EDP panel, Insee.

E Decomposition of the national annual growth in childcare coverage

Let S_t denote the relative supply of EAJE-PSU affordable collective childcare at the national level on year t:

$$S_t = \frac{N_t^{\text{places}}}{N_t^{\text{birth}} + N_{t-1}^{\text{birth}} + N_{t-2}^{\text{birth}}} \tag{6}$$

where N_t^{places} denotes the number of childcare places available at the national level, and N_t^{birth} the number of births that occurred at time t. The national-level supply S_t is a weighted sum of municipality-level supplies, with weights equal to the share of children aged 2 or less who live in each municipality:

$$S_t = \sum_c w_{c,t} S_{c,t} \tag{7}$$

As a result, the annual growth in childcare coverage at the national level can be decomposed:

$$\begin{aligned}
S_t - S_{t-1} &= \sum_c \left(w_{c,t} S_{c,t} - w_{c,t-1} S_{c,t-1}\right) \\
&= \sum_c \left(\left(w_{c,t} - w_{c,t-1}\right) S_{c,t-1} + w_{c,t}\left(S_{c,t} - S_{c,t-1}\right)\right) \\
&= \underbrace{\sum_c \left(w_{c,t} - w_{c,t-1}\right) S_{c,t-1}}_{\text{Composition}} \\
&\quad + \sum_g \underbrace{\sum_c \mathbb{1}\{c \in g\}\mathbb{1}\{E_c = t\} w_{c,t}\left(S_{c,t} - S_{c,t-1}\right)}_{\text{Shocks of the treatment group } g} \\
&\quad + \underbrace{\sum_c \mathbb{1}\{E_c \neq t\} w_{c,t}\left(S_{c,t} - S_{c,t-1}\right)}_{\text{Residual growth}}
\end{aligned} \tag{8}$$

where g denotes treatment group and E_c denotes the timing of the childcare shock in municipality c. The composition term corresponds to a compositional shift whereby municipalities with higher past childcare supply may expand more quickly or slowly than their counterparts with lower past coverage. The two other terms correspond to (i) the contribution of shocks for each treatment group; (ii) the contribution of within-municipality growth before or after shocks.

F Identification

F.1 Sample selection

I develop a very simple framework to provide proof that my sample selection does not impede the identification of the causal effect of childcare on maternal labor supply. Because the municipality of residence is only observed for individuals who are salaried employees, the data only cover individuals who have been, at some point in there lives, salaried employees. Salaried employment being a key measure of labor supply, this raises concerns regarding the validity of my results.

To simplify the problem, I consider a two-period version of the problem: individuals are observed twice, once before childbirth ($t = 0$), and once when they have very young children ($t = 1$). My dependent variable, denoted as Y_{it}, is a dummy variable that equals 1 if individual i is observed in salaried employment at time t, and 0 otherwise. Individuals are assigned a treatment, i.e. a childcare place: this treatment is represented by D_i, a dummy variable. I rely on a potential outcome framework that allows heterogeneous effects of the treatment. In other words, each individual i is associated with a quadruplet $(Y_{it}(d))$ with (t, d) in $\{0, 1\}^2$. The actual outcome writes $Y_{it} = (1 - D_i)Y_{it}(0) + D_i Y_{it}(1)$. In the end, individuals are only observed if $Y_{i0} = 1$ or $Y_{i1} = 1$.

I then consider a few simplifying assumptions:

Assumption 1 (Time-monotonicity). $\forall i \in \mathcal{I}\ \forall d \in \{0, 1\}\ Y_{i0}(d) \geq Y_{i1}(d)$

Assumption 2 (Exogeneity). $D_i \perp\!\!\!\perp (Y_{it}(d))_{(t,d)\in\{0,1\}^2}$

Assumption 3 (Treatment-monotonicity). $\forall i \in \mathcal{I}\ Y_{i1}(0) \leq Y_{i1}(1)$

Assumption 4 (No anticipation). $\forall i \in \mathcal{I}\ Y_{i0}(0) = Y_{i1}(0)$

Assumption 1 states that having children results in women leaving the labor force, or staying in employment, but never in women entering the labor force if the were not employed before; this assumption is based on the large literature devoted to the effect of fertility on labor supply and earnings. Assumption 2 simplifies the research design and considers it to replicate a randomized treatment. Assumption 3 states that being offered a childcare place cannot induce women to leave the labor force, but can only maintain or increase their labor force attachment with respect to the counterfactual situation. Lastly, Assumption 4 states that women do not base their pre-children labor force participation decision on the future attribution of a childcare place.

These restrictions allow to characterize observed and unobserved individuals in terms of their potential outcomes instead of their realized outcomes:

Observed individuals If $Y_{i0} = 1$ or $Y_{i1} = 1$ then (Y_{i0}, Y_{i1}) belongs to the subset $\{(0,1),(1,0),(1,1)\}$. However, $(0,1)$ is not allowed by A1. As a result, $Y_{i0} = 1$, so that by A4 $Y_{i0}(0) = Y_{i0}(1) = 1$. Conversely, if $Y_{i0}(0) = 1$ then $Y_{i0} = 1$.

Unobserved individuals If $D_i = 1$, then $Y_{i0} = Y_{i1} = 0$ implies $Y_{i0}(1) = 0$ and $Y_{i1}(1) = 0$, so that by A4 $Y_{i0}(1) = 0$ and by A3 $Y_{i1}(0) = 0$. If $D_i = 0$, then $Y_{i0} = Y_{i1} = 0$ implies $Y_{i0}(0) = 0$ and $Y_{i1}(0) = 0$, so that by A4 $Y_{i0}(1) = 0$, which implies by A1 $Y_{i1}(1) = 0$. Conversely if for all t and all d $Y_{it}(d) = 0$ then $Y_{i0} = Y_{i1} = 0$.

This observation justifies the following result:

Proposition 1 (Average treatment effects)**.** *Under Assumptions 1-4, (i) the difference in average realized outcome between the treatment and the control group among the observed population identifies a local average treatment effect; (ii) the average treatment effect in the overall population equals this estimand multiplied by the share of the observed population.*

Proof. Let us first consider (i):

$$\begin{aligned}
& \mathbb{E}[Y_{i1}|D=1,\ Y_{i0}+Y_{i1}>0] - \mathbb{E}[Y_{i1}|D=0,\ Y_{i0}+Y_{i1}>0] \\
= \ & \mathbb{E}[Y_{i1}(1)|D=1,\ Y_{i0}(0)=1] - \mathbb{E}[Y_{i1}(0)|D=0,\ Y_{i0}(0)=1] \\
\overset{A2}{=} \ & \mathbb{E}[Y_{i1}(1)|\,Y_{i0}(0)=1] - \mathbb{E}[Y_{i1}(0)|\,Y_{i0}(0)=1] \\
= \ & \mathbb{E}[Y_{i1}(1)-Y_{i1}(0)|\,Y_{i0}(0)=1] \qquad (9)
\end{aligned}$$

The average treatment effect in the overall population writes:

$$\begin{aligned}
& \mathbb{E}[Y_i1(1)-Y_{i1}(0)] \\
= \ & \mathbb{P}(Y_{i0}+Y_{i1}>0)\mathbb{E}[Y_i1(1)-Y_{i1}(0)|Y_{i0}+Y_{i1}>0] \\
& +\mathbb{P}(Y_{i0}+Y_{i1}=0)\mathbb{E}[Y_i1(1)-Y_{i1}(0)|Y_{i0}+Y_{i1}=0] \\
= \ & \mathbb{P}(Y_{i0}(0)=1)\mathbb{E}[Y_i1(1)-Y_{i1}(0)|Y_{i0}(0)=1] \\
& +P(\forall t\ \forall d\ Y_{it}(d)=0)\mathbb{E}[Y_i1(1)-Y_{i1}(0)|\forall t\ \forall d\ Y_{it}(d)=0] \\
= \ & \mathbb{P}(Y_{i0}=1)(\mathbb{E}[Y_{i1}|D=1,\ Y_{i0}+Y_{i1}>0] - \mathbb{E}[Y_{i1}|D=0,\ Y_{i0}+Y_{i1}>0]) \qquad (10)
\end{aligned}$$

□

F.2 Childcare shock exogeneity

My event-study framework rests on the assumption that the counterfactual trend in parents' labor outcomes *absent* the local childcare shock is mean-independent of the year when this shock took place. In other words, the decision to open a large number of childcare places should not depend on observables (for decision-makers and not necessarily for the econometrician) that predict different trend in parental labor outcomes before the decision is made.

As I detail in Subsection 2.3 and Appendix A.2, the attribution of subsidies directed towards the opening of new childcare places by local CAF offices is mostly based on local measures of childcare coverage, which seems compatible with this assumption. However, municipalities decision to first apply remains unknown, which could seriously question the validity of this assumption.

To assess the plausibility of my identifying assumption in this context, I resort to the 2006 Census at the municipality level. This allows me to test whether, within treatment groups, the timing of childcare expansions correlates with municipality-level characteristics, observed before the decision is made, that could plausibly affect the evolution of parental labor outcomes. Specifically, I consider several potentially relevant dimensions:

- municipality size, as defined by the total 2006 population in the census;
- potential and actual birth rates, approximated by:
  - the share of women aged 15 to 49 in the total population;
  - the ratio of children aged less than 1 over the number of women aged 15 to 49;
- migration, as measured by the share of inhabitants who lived, 5 years after the data was collected, in another municipality, either in metropolitan France or abroad;
- couple formation, as measured by the share of single women (men) in the population of women (men) aged 20 to 49;
- marriage formation and dissolution, as measured by:
  - the share of married women (men) in the population of women (men) aged 20 to 49;

  - the share of divorced women (men) in the population of women (men) aged 20 to 49;

- female labor force participation, as measured by the share of women who declared themselves to be housewives in the population of women aged 20 to 49;

- labor market composition, as measured by the share of women (men) who are managers or professionals in the population of women (men) aged 20 to 49.

This information is extracted from the 2006 Census. However, it was not necessarily collected in 2006: since 2004, the French Census is collected annually. Specifically, all French municipalities are surveyed over a five-year period. As a result, five census surveys were conducted from 2004 to 2008, and were finally combined to produce the Census results, dated 2006, i.e. the medium year. As a result, a slight part of the results relate to a time-period possibly affected by the treatment (year 2008). My results are nevertheless robust to omitting municipalities in which childcare expansions take place in 2008.

I use this information, as well as the level of relative collective childcare supply as measured in 2007, to predict the timing of the municipality-level childcare shock. Specifically, I estimate, separately for each treatment group g, a simple linear model:

$$E_c = \eta_g + \theta_g' X_c + \upsilon_c \tag{11}$$

where E_c is the date at which municipality c experiences the childcare expansion, η_g a group-specific intercept, X_c a vector of observable characteristics as measured in the 2006 Census, and υ_c an idiosyncratic shock of mean 0. To be consistent with the fact that my empirical framework estimates labor supply effects at the individual level of parents with children in the relevant age groups, I weight observations by the number of children aged 2 or less in 2007 (as observed in birth records).

Table F.1 displays my estimates of the vector of coefficients θ_g for the P90-P100 group, which is the most relevant group in my framework.[32] It makes it very clear that within the P90-P100 treatment group, municipalities that were treated in the beginning of the 2007-2015 time-period are virtually indistinguishable from their counterparts that were treated later on.

[32] Results for the other treatment groups are available upon request.

First, most variables are not significantly correlated with the timing of the childcare expansion. The only variables for which the correlation is significantly different from 0 at usual thresholds are the initial level of relative childcare supply, the city size and the birth rate.

Second, even for these variables, the effect sizes remain tiny: the coefficients would imply for instance that municipalities with initial collective childcare supply 20 percentage points above the mean are treated 0.1 years later in average. Similarly, municipalities with 50,000 inhabitants more than the mean are treated one year earlier: there were only 122 municipalities with population larger than 50,000 in France in 2006. The coefficient on the birth rate may seem large, but the average birth rate in the P90-P100 is 0.05, and its standard deviation is 0.01, which implies very small differences across municipalities.

Third and lastly, even in the full specification, observable characteristics explain very little of the dispersion in the timing of childcare expansions. Indeed, this linear model explains less than 2% of the variance of the timing variable. The poor predictive performance of this models suggests that the exact timing of childcare expansion is too a large extent independent of these characteristics that could plausibly imply different trends in parental labor supply decisions. As a result, it supports the credibility of my key identifying assumption (parallel trends in the counterfactual parental labor supply decisions *absent* the shock).

Table F.1 – OLS estimates of the association between observable characteristics in the 2006 Census and the timing of the local childcare expansion

	(1)	(2)	(3)	(4)	(5)
Childcare	0.56	0.64	0.60	0.60	0.65
	(0.19)	(0.19)	(0.19)	(0.19)	(0.20)
Pop. (10,000s)		−0.24	−0.25	−0.26	−0.23
		(0.06)	(0.07)	(0.08)	(0.08)
Pot. mothers		−2.68	−3.75	−2.50	−2.33
		(1.86)	(2.00)	(2.17)	(2.18)
Birth rate		9.44	9.23	10.24	8.56
		(3.53)	(3.56)	(3.80)	(3.96)
Mig. (from Fr.)			0.53	0.68	1.08
			(0.79)	(0.81)	(0.86)
Mig. (abroad)			4.59	3.94	3.99
			(3.02)	(3.16)	(3.28)
Single (f)				1.01	0.43
				(2.40)	(2.47)
Single (m)				−0.13	0.03
				(2.36)	(2.37)
Married (f)				2.72	1.84
				(3.24)	(3.30)
Married (m)				−1.90	−1.24
				(3.31)	(3.35)
Divorced (f)				1.40	0.68
				(3.07)	(3.11)
Divorced (m)				5.49	4.94
				(3.79)	(3.81)
Housewives					1.38
					(1.19)
Managers (f)					−0.19
					(1.59)
Managers (m)					−0.30
					(1.05)
Observations	2,372	2,372	2,372	2,372	2,372
R^2	0.004	0.02	0.02	0.02	0.02
Adjusted R^2	0.003	0.02	0.02	0.02	0.02

Dependent variable. Timing of the municipality-level childcare shock. *Explanatory variables.* Relative childcare supply as measured in 2007 and observable characteristics at the municipality-level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and 2006 Census, Insee.

F.3 Parental earnings

F.3.1 Measurement error

For the individual-level data, the most prominent source of measurement error in my approach stems from the fact that while the treatment assignment is based on the municipality of residence, I do not observe this location when individuals are not in salaried employment. As a result, I choose to impute the location based on past locations, when individuals were salaried employees. This basically makes the strong assumption that individuals who are outside the labor force do not move until they find a new job.

As a robustness check, I make the reverse choice in terms of imputation strategy, i.e. using future municipality of residence to impute location when individuals are outside the labor force. I find my results to be very robust to these changes.

Table F.2 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
−5–−1	-200.22	0.27	-1.25	-0.008	-0.408
	(1086.99)	(3.97)	(15.73)	(0.146)	(0.44)
0–2	255.7	-2.28	-2.57	-0.066	0.687
	(860.41)	(3.51)	(11.23)	(0.126)	(0.472)
3–10	-80.89	-0.07	8.53	0.079	-0.642
	(812.43)	(2.69)	(9.1)	(0.091)	(0.393)
Fathers					
−5–−1	2409.05	10.83	-0.82	-0.124	-0.354
	(1462.53)	(3.97)	(14.13)	(0.123)	(0.758)
0–2	186.29	1.2	-2.71	0.006	0.052
	(1383.8)	(2.85)	(9.4)	(0.088)	(0.671)
3–10	2316.38	2.59	-3.59	0.064	0.778
	(1247.21)	(2.26)	(7.9)	(0.073)	(0.657)

Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply and calendar-time dummies interacted with treatment group, plus municipality fixed effects. Childcare supply is instrumented by time-to-event dummies interacted with treatment group. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

F.3.2 Other policy shocks and compositional shifts

My approach identifies the causal impact of collective childcare on parents' labor outcomes to the extent that the childcare shocks upon which it is based do not correlate with other changes that would affect the outcome. While this assumption cannot be tested directly, it is possible to verify that more restricted versions of this assumption do hold.

To this end, I check that my results are not driven by other local policy changes or shocks to the local labor markets by further interacting the calendar time × treatment group fixed effects of Model 2 with geographical area dummies. As a result, the identification of my parameter of interest stems solely from differences in the timing of the childcare shock across municipalities of the same treatment group and that belong to the same geographical area. I implement this strategy at two distinct levels. First, I consider the département level, given that the local offices of the Family branch of the French social security operate at this level. Second, I consider *Zones d'emploi*, a statistical zoning system developed by Insee to delimit local labor markets.[33] This is particularly useful given that my period of interest covers the Great Recession, whose impact might be heterogeneous across local labor markets. Tables F.3 and F.4 display my results, which are consistent with my main estimates.

To verify that my results are not driven by changes in the composition of potentially treated parents, I also modify Model 2 to include individual-level covariates. Specifically, I consider birth cohort (year of birth), education, and past fertility decisions, i.e. total number of children.[34] Table F.5 displays my estimates, that are once again consistent with my previous findings.

[33] A Zone d'emploi is defined by Insee as a geographical area within which most of the labor force lives and works, and in which firms can find most of the labor force necessary to fill the available jobs.

[34] In this case, I interact the number of children with a sample dummy (i.e. a dummy variable that indicates whether parents are born on October, in which case their past fertility is perfectly observed, or not, in which case it is left-censored) and calendar time fixed effects to circumvent the left censoring issue mentioned in section 3.

Table F.3 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
−5–−1	-192.6	-0.32	-2.34	-0.021	-0.524
	(1061.43)	(3.99)	(15.38)	(0.146)	(0.43)
0–2	191.56	-2.42	-8.39	-0.068	0.731
	(879.89)	(3.53)	(11.16)	(0.128)	(0.466)
3–10	202.28	-0.31	7.24	0.082	-0.416
	(763.37)	(2.56)	(8.89)	(0.09)	(0.389)
Fathers					
−5–−1	1397.31	5.93	-5.43	-0.108	-0.126
	(1578.18)	(3.9)	(13.78)	(0.126)	(0.791)
0–2	418.05	0.54	8.14	0.007	0.249
	(1333.66)	(2.7)	(9.17)	(0.086)	(0.642)
3–10	2492.3	2.8	-1.13	0.076	0.745
	(1279.51)	(2.18)	(7.51)	(0.071)	(0.644)

Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply plus calendar-time dummies interacted with treatment group and departement dummies, plus municipality fixed effects. Childcare supply is instrumented by time-to-event dummies interacted with treatment group. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

Table F.4 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
−5–−1	-57.03	-0.92	-3.39	0.124	-0.442
	(1070.35)	(4.02)	(15.29)	(0.145)	(0.476)
0–2	-256.91	-4.23	-3.12	-0.062	0.889
	(893.63)	(3.44)	(11.39)	(0.128)	(0.512)
3–10	644.67	2.61	4.05	0.132	-0.496
	(834.44)	(2.54)	(9)	(0.09)	(0.421)
Fathers					
−5–−1	1020.11	3.76	-1.81	-0.167	-0.162
	(1653.5)	(3.94)	(14.11)	(0.127)	(0.788)
0–2	879.48	3.1	6.69	0.052	0.208
	(1498.4)	(2.82)	(9.21)	(0.086)	(0.733)
3–10	2120.35	2.7	-1.63	0.042	0.68
	(1310.76)	(2.19)	(7.58)	(0.073)	(0.672)

Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply plus calendar-time dummies interacted with treatment group and Zone d'emploi dummies, plus municipality fixed effects. Childcare supply is instrumented by time-to-event dummies interacted with treatment group. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

Table F.5 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
−5–−1	-902.74	-3.17	-5.57	-0.043	-0.506
	(966.87)	(3.94)	(15.37)	(0.14)	(0.398)
0–2	778.53	-0.49	2.4	0.006	0.706
	(806.14)	(3.4)	(10.93)	(0.121)	(0.425)
3–10	34.65	0	9.56	0.096	-0.517
	(736.65)	(2.54)	(8.93)	(0.089)	(0.353)
Fathers					
−5–−1	658.17	5.58	-2.69	-0.119	-0.635
	(1397.97)	(3.87)	(13.96)	(0.122)	(0.708)
0–2	567.93	2.98	-2.92	0.002	-0.05
	(1333.54)	(2.8)	(9.25)	(0.087)	(0.628)
3–10	1315.84	2.86	-5.36	0.057	0.243
	(1203.58)	(2.27)	(7.85)	(0.073)	(0.585)

Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply plus calendar-time dummies interacted with treatment group, plus municipality fixed effects and parents' education interacted with birth cohort (year of birth) and total number of children (interacted with a sample dummy and calendar time dummies). Childcare supply is instrumented by time-to-event dummies interacted with treatment group. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

F.3.3 Placebo groups

However, these checks are not sufficient to assess the credibility of my design if childcare expansions coincide with shocks that take place at the municipality level, as opposed to the département or Zones d'emploi level, and if these shocks do not correlate with compositional shifts in pool of potentially treated parents. As an additional attempt to test the credibility of my identification strategy, I replicate it on placebo groups, i.e. subsets of individuals who should not be directly affected by childcare expansions.

To this end, I consider two groups: parents taken one to five years before the birth of their first child, and parents whose youngest child is aged 3 to 10. Because EAJE-PSU facilities target children aged 0 to 2, there should not be any direct effect of childcare expansions on the labor outcomes of these groups. Results appear alongside my main estimates in Table 4. Consistent with this rationale, I cannot detect any significant effect for these groups, which strengthens the credibility of my findings. If anything, the earnings of fathers may even increase in municipalities that experience the largest shocks. This would be the case if these massive expansions were more likely to occur early in municipalities where the labor market is growing steadily steadily. However, this would suggest that my estimates for the labor market outcomes of mothers are biased upwards, which gives even more strength to my claim that the positive maternal labor supply effects of childcare expansions are negligible at best.

F.3.4 Sampling issues

It could be argued that because (i) I focus on very modest expansion plans (see Section 2); (ii) I rely solely on information on the local aggregate provision of childcare, as opposed to individual offers made to parents; and (iii) I do not rely on comprehensive data but only on a sample of parents, my null effects are driven by the fact that my sample does not actually include any parents who received a recently created childcare place. The probability of this event is not 0, so this is always a possibility. However, with mild assumptions I am able to quantify the probability of these huge deviations.

First, the data indicates that 70 000 childcare places were created between 2007 and 2015 (see Section 2). Assuming that (i) this increase was linear overtime (see Figure 2) and that (ii) childcare places are reallocated every three years,[35] the increase resulted in 105 000 additional childcare allocation decisions. Second, my sample of parents is based on their birthday, i.e. whether or not they were born in the 16 relevant days of the EDP sample (see Section 3), so the sampling rate is 4.4%.

Consider $\hat{n}^{\text{allocations}}$ the number of additional childcare allocation decisions stemming from the national increase in coverage that benefit the parents in my sample. For each decision i, let B_i denote a dummy variable that is equal to 1 if this spot is allocated to a parent of my sample. Then:

$$\hat{n}^{\text{allocations}} = \sum_{i=1}^{M} B_i \tag{12}$$

Given that the allocation of childcare places does not depend on whether *parents* are born on the 16 EDP days or not,[36] B_i variables can be assumed to be independent Bernoulli variables of parameter p equal to the sampling rate. As a result, the variance of $\hat{n}^{\text{allocations}}$ is written $Mp(1-p)$. It is then easy to apply Chebychev's inequality:

$$\mathbb{P}(|\hat{n}^{\text{allocations}} - Mp| \geq \rho Mp) \leq \frac{1-p}{\rho^2 Mp} \tag{13}$$

With $M = 105,000$, $p = 0.044$ and $\rho = 0.1$, an upper bound for the probability that the number of additional childcare allocation decisions that benefit parents in my

[35] This is the most conservative estimate. If childcare places are reallocated more frequently, then the probability of large deviations decreases.

[36] It may depend on their *children*'s date of birth given that childcare places are frequently offered from September to July, when made available by children aged 3 leaving the EAJE facilities to attend preschool.

sample deviates by more than 10% from its expected value is 2%.

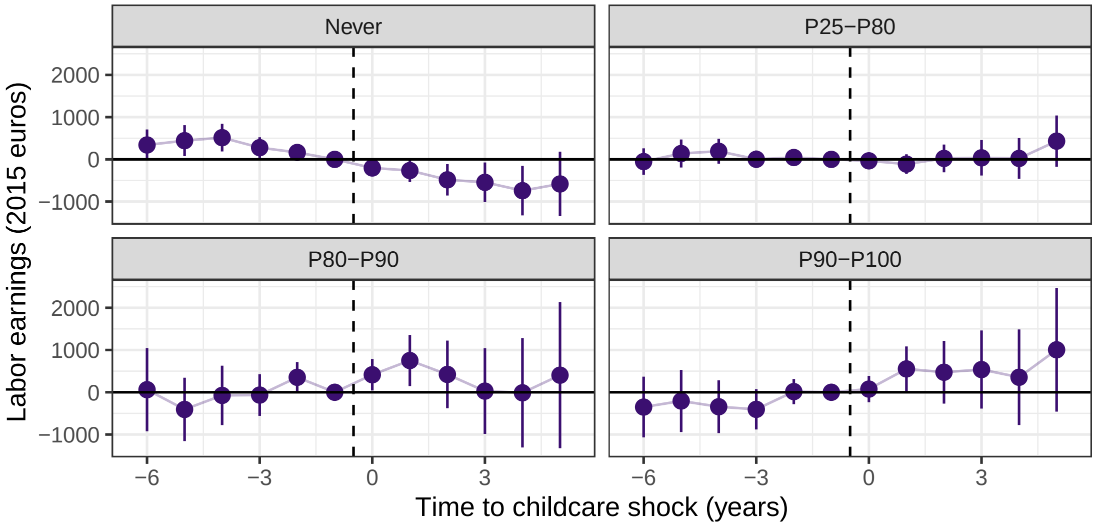
F.3.5 Event-study framework

My graphical analysis is based on an event-study framework that relies on a no-pretrend assumption to disentangle calendar-time from time-to-treatment effects. However, this approach is based on the premise that while treatment effects may be dynamic, i.e. they may vary depending on whether units are observed one year, two years, etc. after childcare expansions, treatment effects are actually homogeneous across units that belong to different cohorts, as defined by the timing of the childcare shocks.

In a recent investigation of this setting, Sun and Abraham (2020) show that when this homogeneity assumption fails, the canonical event-study estimates are a weighted sum of cohort-specific treatment effects with many potentially negative weights. As a result, this framework does not enable to properly test the hypothesis that treatment effects are equal to 0 before treatment. Instead, the authors propose to estimate a fully-interacted model, and then to manually average coefficients over cohorts using weights proportional to sample size.

Figure F.1 replicates my event-study analysis based on their approach. My results are robust to this concern.

Figure F.1 – Event-study estimates based on Sun and Abraham (2020) of the impact of the childcare shock on mothers' labor earnings, by treatment group



Event-study estimates of the effect of childcare shocks on mothers' labor earnings (Model 2).
Source. EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

F.3.6 Fuzzy difference-in-difference

The fuzzy difference-in-difference setting proposed by Duflo (2001) which serves as the basis of my approach has been recently subjected to investigation by de Chaisemartin and D'Haultfœuille (2018), who raise concerns about its ability to identify causal parameters of interest in realistic settings. They show that under usual assumptions, the canonical Wald-DID estimator only identifies a Local Average Treatment Effect (LATE)[37] if either (i) treatment effects are homogeneous; or (ii) the treatment rate (here, the childcare coverage) is constant in the control group.

They propose several corrections that make it possible to identify this LATE when neither of these assumptions are plausible. However, these corrections require either (i) that actual treatment (i.e. the use of a childcare place) be observed at the individual level; or (ii) that the outcome be continuously distributed. In my particular setting, I cannot observe whether individuals do indeed take up collective childcare places, so I cannot use individuals who land a childcare place before childcare expansions to implement their correction. Additionally, my outcomes of interest are not continuously distributed: given that labor supply decisions at the extensive margin are at play, the distribution of labor earnings displays a large mass at 0. It would appear, therefore, thatr the alternative estimators developed by de Chaisemartin and D'Haultfœuille (2018) are not applicable here.

However, this problem is probably not a major threat to my identification strategy. First, the treatment rate varies very little except for the shock, as made evident by Figure 5, so that deviations from the assumption that this treatment rate is actually constant are small or non-existent. Second, my approach can be replicated in a setting where, in the control group of municipalities, the childcare coverage rate is constant by construction. To this end, I restrict the analysis to municipalities with no EAJE-PSU facility in 2007, and where a facility opened at some point between 2008 and 2014, and define my childcare shock as the opening of this first facility. In this setting, the treatment rate in the control group, i.e. in municipalities where a collective childcare facility will open at some point, but has not done so yet, is by construction equal to 0.

Table F.6 displays the corresponding Wald-DID estimates. While my standard errors are larger, because I only rely on a very restricted subset of municipalities, the results are in line with those obtained using all childcare shocks: I cannot

[37] Specifically: the average treatment effect for those individuals who are offered a childcare place due to the childcare expansion, but would not have been so had they been observed before the expansion.

detect any significant change in the labor earnings of mothers with young children after the creation of a childcare facility.

Table F.6 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes based on the opening of the first EAJE-PSU facility, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
−5–−1	-1787.34	0.34	-10.6	-0.431	-0.513
	(1316.63)	(5.55)	(21.33)	(0.207)	(0.542)
0–2	-496.21	0.49	-20.79	-0.036	0.03
	(1037.64)	(4.7)	(14.4)	(0.151)	(0.537)
3–10	41.53	1.49	-3.77	0.004	-0.26
	(898.11)	(3.64)	(12.15)	(0.119)	(0.421)
Fathers					
−5–−1	390.44	-0.11	-14.29	-0.066	0.854
	(1915.66)	(5.4)	(19.58)	(0.176)	(0.954)
0–2	-1390.23	1.09	-21.23	0.158	-0.76
	(1461.44)	(3.55)	(12.96)	(0.112)	(0.806)
3–10	1493.43	4.4	0.78	-0.058	0.1
	(1306.43)	(2.9)	(9.66)	(0.089)	(0.625)

Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply and calendar-time dummies, plus municipality fixed effects. Childcare supply is instrumented by time-to-event dummies. Standard errors are clustered at the municipality level. *Note.* Data regarding the Tarn département are omitted. *Source.* EAJE-PSU records, CNAF. Birth recordss and DADS-EDP panel, Insee.

F.4 Substitution effects

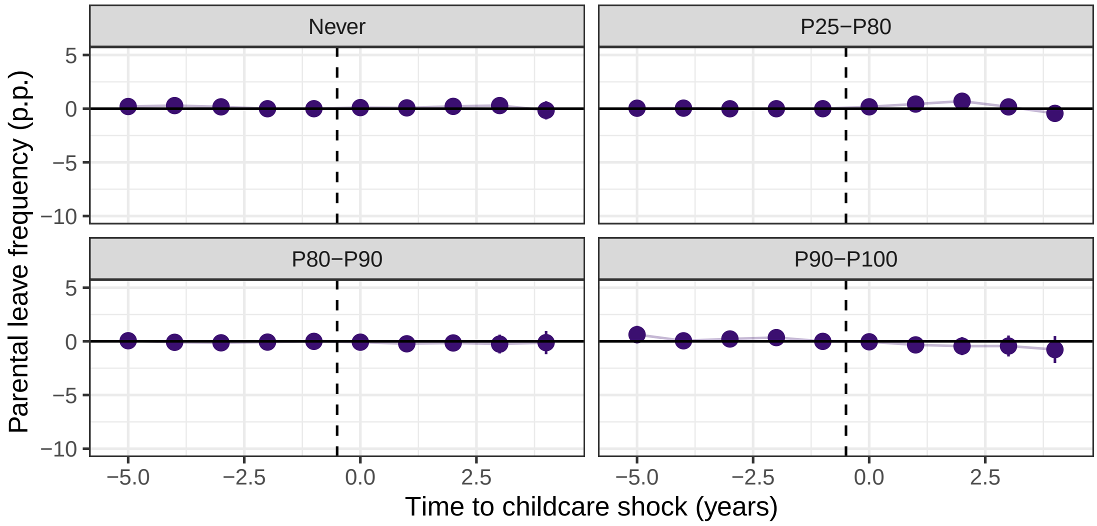
F.4.1 Placebo groups

As what was the case when investigating labor outcomes effects, my division of municipalities in four treatment group allows me to consider effects in municipalities where changes in the supply of collective childcare were actually either non-existent or negligible, which can be regarded as placebo groups. Even though standard errors may be large, I cannot detect any change in demand for individualized childcare in these municipalities, which is reassuring as to the validity of the assumptions upon which my identification strategy is based. This also holds for my estimates regarding the impact of collective childcare on paid parental leave take-up.

F.4.2 Division bias

I then investigate whether these results are affected by some kind of division bias. This could be the case because I use a measure of the number of children aged 2 or less as the denominator in both my measure of collective and individualized childcare and my measure of the frequency of paid parental leave. As a result, measurement error in this number may generate spurious correlations; specifically, the correlation between the supply of different types of childcare solutions might be biased towards 1. To investigate this possibility, I replicate my analysis while adding my measure of the number of children as a covariate in the regression. Figures F.2 and F.3 display my estimates. I find that this does not change my results.

Figure F.2 – Event-study estimates of the impact of the childcare shock on paid parental leave take-up, by treatment group, controlled for changes in the number of children

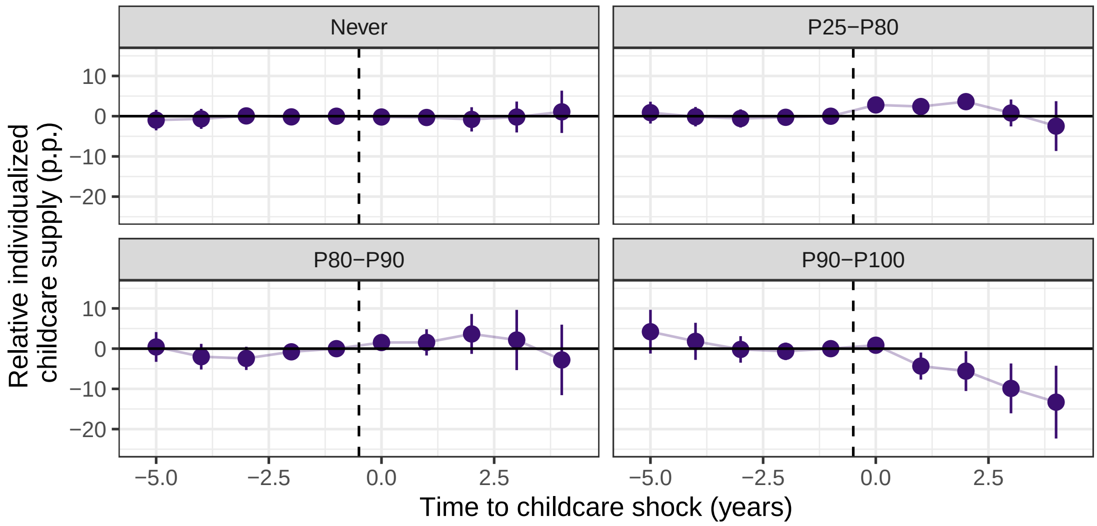


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less that receive parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records and PAJE recordss, CNAF. Birth records, Insee.

Figure F.3 – Event-study estimates of the impact of the childcare shock on the supply of individualized childcare, by treatment group, controlled for changes in the number of children



Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

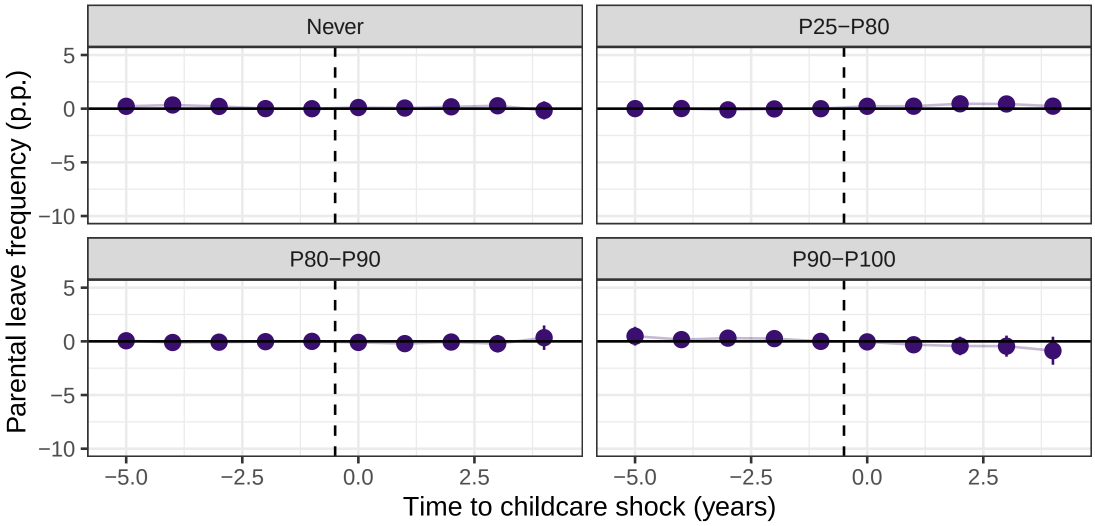
Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records and comprehensive DADS records, Insee.

F.4.3 Other policy changes

As I did for labor supply effects, I verify that my results are not driven by other non-relevant policy changes by further conditioning my analysis on various geographical units. Specifically, I consider the département, the level at which local offices of the CNAF operate, the Zone d'emploi, to consider local labor market effects, and lastly the *Bassin de vie*, a geographical unit defined by Insee that captures the provision of local services at a narrow geographical level. For all these levels, I replicate my analysis while adding treatment group × calendar time × geographical unit fixed effects. Figures F.4 to F.9 display the corresponding estimates. I find qualitatively similar effects, which supports the validity of my identification strategy.

Figure F.4 – Event-study estimates of the impact of the childcare shock on paid parental leave take-up, by treatment group, with département-level calendar time fixed effects

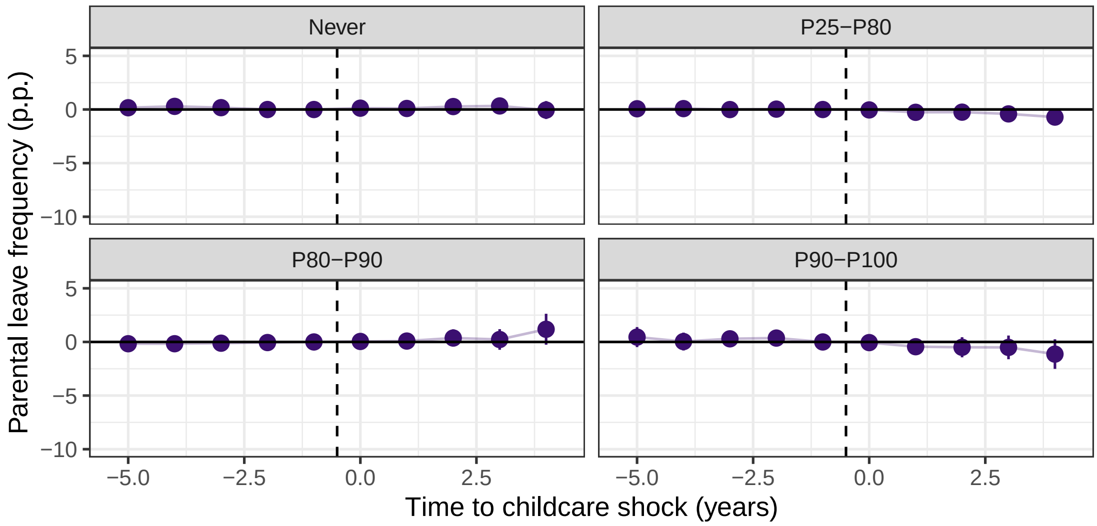


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less who received parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records and PAJE records, CNAF. Birth records, Insee.

Figure F.5 – Event-study estimates of the impact of the childcare shock on paid parental leave take-up, by treatment group, with Zone d'emploi-level calendar time fixed effects

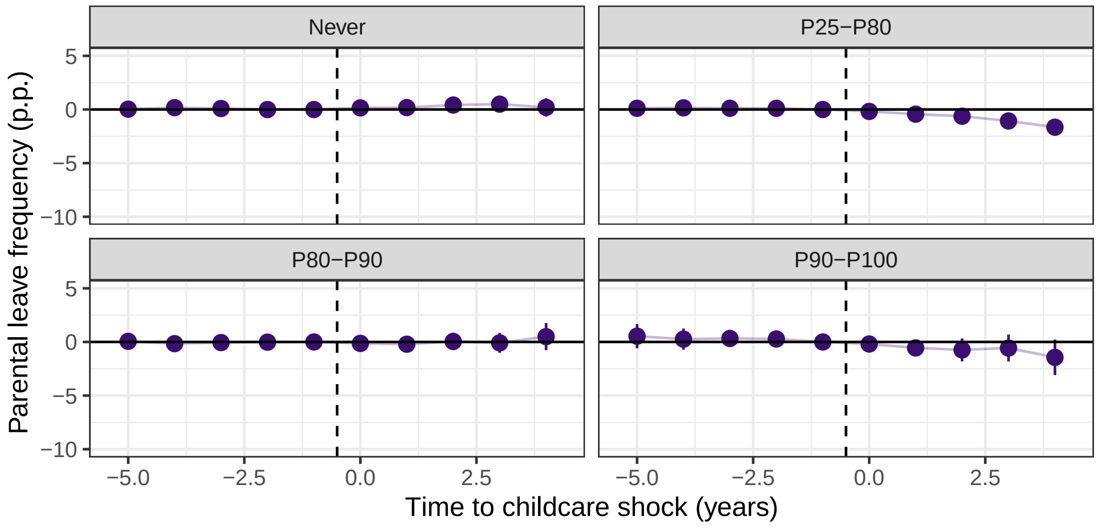


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less who received parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records and PAJE records, CNAF. Birth records, Insee.

Figure F.6 – Event-study estimates of the impact of the childcare shock on paid parental leave take-up, by treatment group, with Bassin de vie-level calendar time fixed effects

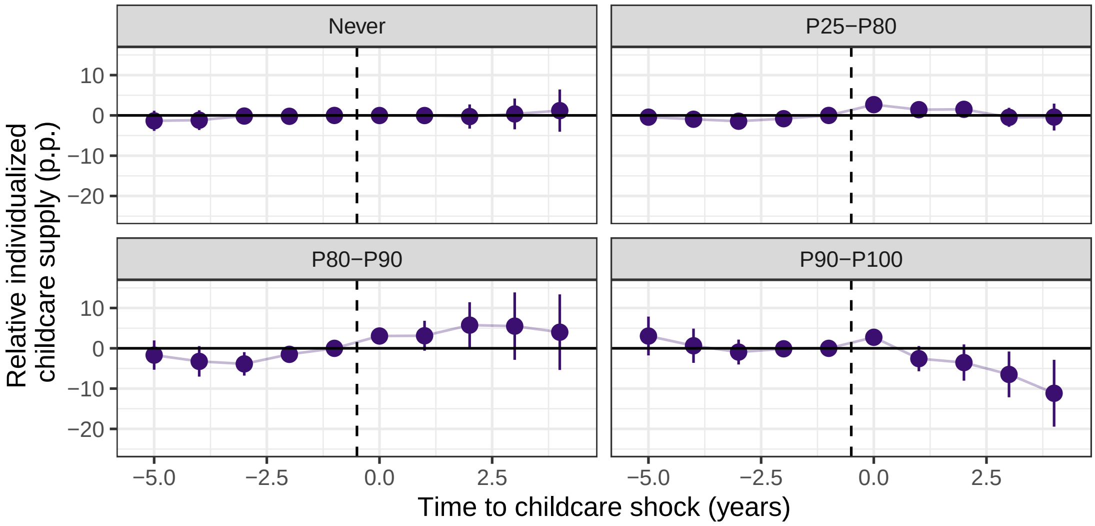


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less who received parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records and PAJE records, CNAF. Birth records, Insee.

Figure F.7 – Event-study estimates of the impact of the childcare shock on the supply of individualized childcare, by treatment group, with département-level calendar time fixed effects

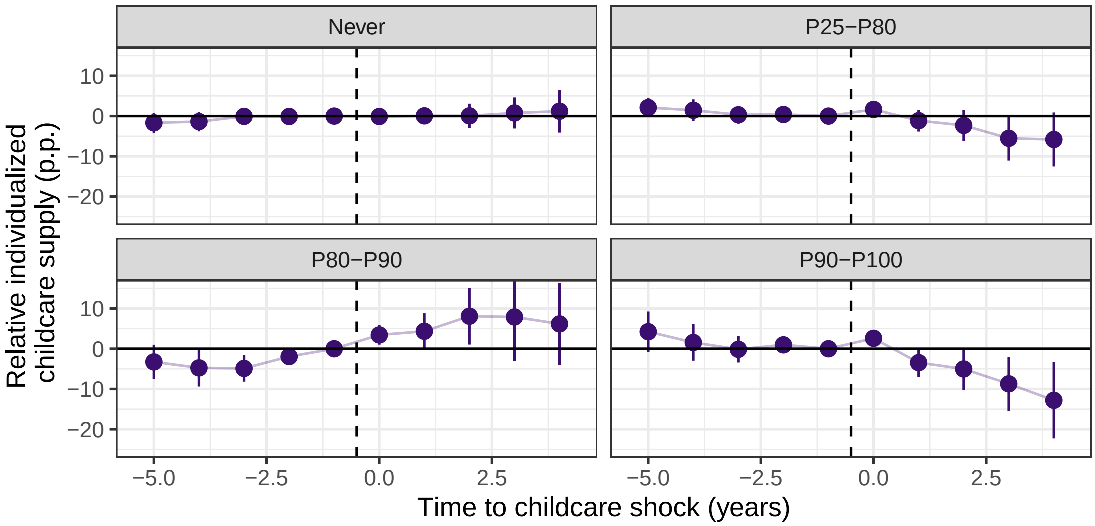


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records and comprehensive DADS records, Insee.

Figure F.8 – Event-study estimates of the impact of the childcare shock on the supply of individualized childcare, by treatment group, with Zone d'emploi-level calendar time fixed effects

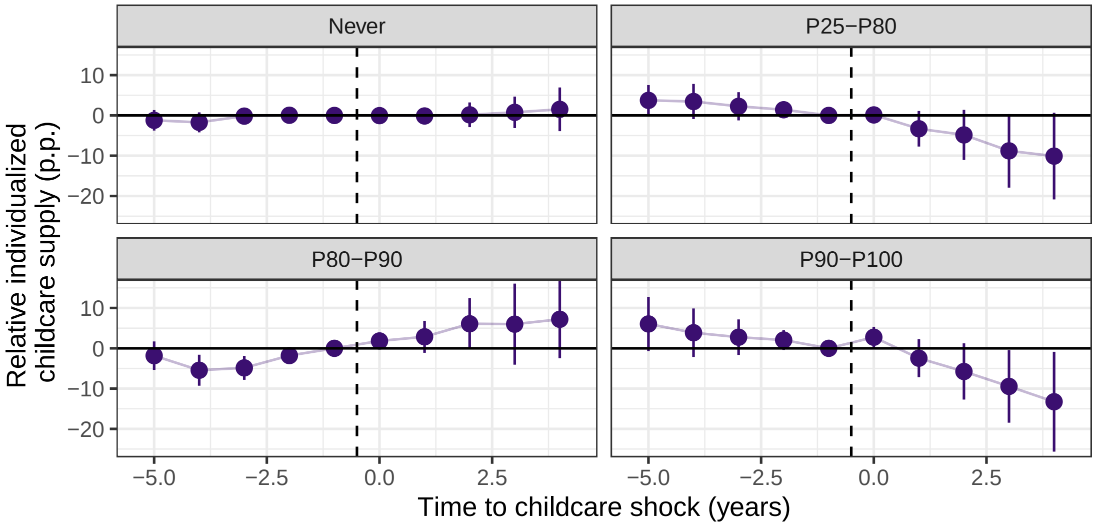


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records and comprehensive DADS records, Insee.

Figure F.9 – Event-study estimates of the impact of the childcare shock on the supply of individualized childcare, by treatment group, with Bassin de vie-level calendar time fixed effects



Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

Source. EAJE-PSU records, CNAF. Birth records and comprehensive DADS records, Insee.