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

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Abstract

I exploit the staggered rollout of subsidized collective childcare facilities across French municipalities, driven by successive national plans, to examine the impact of increased public childcare availability on parents' labor market outcomes and childcare choices between 2007 and 2015. I find no substantial effects on parental employment or paid parental leave uptake. Instead, the expansion of collective childcare primarily displaced more expensive formal alternatives, such as childminders and in-home care. These crowding-out effects underscore a key implication of family policy strategies that promote the coexistence of multiple childcare options.

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 consequence 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 sometimes been reported in the literature (e.g. Fitzpatrick, 2010; Havnes and Mogstad, 2011a), which has been usually 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 tax 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; 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 akin to 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) suggests that increased childcare provision has no effect on the child penalty in Austria (Kleven et al., 2024).

Null effects are thus not uncommon 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) or lastly the decision not to rely on an external childcare provider. 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.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).

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

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

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. The national guidelines states 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.[2] 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.

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.[3] 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. 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).

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.

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

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

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.[4]

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 who were entitled to either the CLCA or the PreParE in December of each year. 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. In order to obtain 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.

3.2 Labor market data

My labor market data are drawn from the *Déclarations Annuelles de Données Sociales* (DADS). By law,[5] 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). With few exceptions, maternity leave allowances paid by social security are not included in my measure of earnings. When a mother is on maternity leave, she is considered as having a positive number of days worked, but the number of hours paid during the maternity leave is equal to 0.

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. The main limitation of this dataset is the lack of information on self-employment, which could bias my estimates if mothers adjust their employment status in response to childcare availability. However, this bias is likely small, as self-employment concerned fewer than 5% of employed mothers with young children and only 4% of those interrupting their careers in 2007 (Galtier, 2011).

[4]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.

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

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, based on the 4-digit occupation variable.[6]

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.

A limitation of the data is the lack of information on children born before 2004 to individuals born in January, April, and July.[7] This leads to underestimating past fertility, but does not limit the identification of parents with young children over the time period of interest. I address this by interacting the number of children with a dummy for (potential) parents born in October, for whom data are complete, when conditioning on past fertility.

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

[6] *"563a – Childminders, baby-sitters and foster families"* category.

[7] Birth data are also missing for those born on October 2–3 (see Wilner, 2016). For those individuals, I am however able to use census data instead, as in Pora and Wilner (2025), which are comparable to birth records for October 1 or 4.

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.[8]

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, I impute zero labor earnings, and consider them to be outside the labor force.[9] 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%.

[8]Empirically, this is the case of 94% of parents throughout my time-period of interest. Assuming that childbirth can only reduce or maintain women's labor force participation, that being offered a childcare place can only strengthen it, and that it does not influence pre-birth labor supply decisions, this sample selection does not impede the identification of relevant average treatment effects. Specifically, these data allow the identification of the impact of childcare places among mothers who hold salaried jobs before they have children, a population of which the definition is not affected by the availability of collective childcare at the municipality level.

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

4 Empirical analysis

4.1 Granular childcare expansions

4.1.1 Treatment groups

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 make 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 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.

Table 3 describes these groups in terms of pre-treatment observables, i.e. using data from the 2006 Census at the municipality level. 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.

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 earlier, 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.

4.1.2 Timing of the treatment

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 $g = g(c)$.[10] In this within-group event-study setting, I estimate:

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

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. This parameter is identified thanks to variation in the timing of childcare expansions among municipalities that are affected by expansions of similar magnitude. In other

[10] 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.

words, it is not identified neither by (i) within-municipality differences in parental labor outcomes over time, as would be the case in an interrupted time-series approach, nor by (ii) differences in the evolution of parental outcomes between municipalities that are affected by an expansion and municipalities that do are not affected by an expansion, as would be the case in an usual difference-in-difference approach.

As noted by Borusyak, Jaravel, and Spiess (2024, Proposition 1), 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.[11] 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 (see e.g. Deaton and Paxson, 1994). 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, 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, a solution is 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, estimating a semi-dynamic version of Model 2, in which all coefficients $(\beta_{g\tau})_{\tau<0}$ are constrained to 0, allows to recover reasonable estimates of the $(\beta_{g\tau})_{\tau\geq 0}$.

Lastly, 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, 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, is a better-suited approach.

I follow this intuition 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

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

timing of the treatment (Sun and Abraham, 2021). However, moving to a correction based on a fully interacted model, suggested by Sun and Abraham (2021), does not affect my results (see Figure A.1).

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,[12] 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.

In their investigation of this fuzzy difference-in-difference approach, de Chaisemartin and D'Haultfœuille (2018) highlight that the standard Wald-DID estimator only identifies a Local Average Treatment Effect (LATE) under restrictive conditions, notably homogeneous treatment effects or constant treatment rates in the control group. While their proposed corrections are not applicable in my context, due to the lack of individual-level data on childcare take-up and the discrete nature of labor supply outcomes, this limitation is mitigated in practice. Indeed, treatment rates remain stable outside the

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

childcare shocks (see Figure 5). I also replicate my analysis in a subsample where the control group has a constant treatment rate by construction (municipalities without any EAJE-PSU facility in 2007). The results are consistent with my main findings and show no significant effect of facility openings on maternal labor earnings, supporting the robustness of my identification strategy (see Table A.6).

4.4 Identifying assumptions

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. Specifically, 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. Specifically, within each treatment group, I regress the timing of the childcare shock that took place between 2007 and 2015, on a set of pre-determined variables, observed in 2006. Table 4 displays the corresponding estimates for the main treatment group (P90-P100).[13]

The main lesson is that, within the P90-P100 treatment group, municipalities that were treated in the beginning of the 2007-2015 time-period were, in 2006, virtually indistinguishable from municipalities that were treated later on. Specifically, these municipalities differed very little in terms of labor market composition, couple and marriage formation and dissolution, and arrival of new residents. The only significant differences

[13] The corresponding results for the remaining groups are available upon request from the author.

are that (i) municipalities with lower coverage rate in 2007 were treated earlier, which is consistent with Subsection 2.3; (ii) larger municipalities tended to be treated earlier; and (iii) municipalities with higher birth rates were treated later. Even so, these differences explain very little (2% at best) of the variance of the timing of the local childcare shock. This weak predictive power suggests that the timing of childcare expansions was largely exogenous to factors likely to affect labor supply trends, thus lending support to the parallel trends assumption underlying my event-study design.

My framework rests on another assumption, namely that the spatial level relevant to the childcare choices is the municipality level. Qualitative surveys indeed show that the municipality of residence is the only universal criterion for the allocation of childcare places (Onape, 2012). To the best of my knowledge, available quantitative data do no allow to scrutinize this claim. However, this assumption follows for instance the strategy used by Berger, Panico, and Solaz (2021) who instrument individual childcare decisions by the exact same municipality-level collective childcare supply upon which I focus.

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.[14]

Finally, I map these dynamic estimates into a single effect for each treatment group

[14] 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.

by summing the coefficients with weights proportional to the sample size. Table 5 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 6 displays my estimates. Consistent with my previous findings, I cannot 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. The same goes on for fathers, who are left virtually unaffected by the provision of affordable collective childcare. This result is however less surprising given that men's labor supply changes very little in respond to the arrival of children (Kleven, Landais, and Søgaard, 2019).

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.

One might worry that my null results reflect the absence of treated parents in the sample, given the modest scale of the expansions and the aggregate nature of my treatment measure. However, under mild assumptions, I estimate that, with a 4.4% sampling rate, about 4,600 newly created childcare places should have been allocated to parents in my sample. Applying Chebyshev's inequality, the probability that the effective number deviates by more than 10% from its expected value is below 2%, suggesting that the absence of treated individuals is very unlikely to explain my results.

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. Between 2000 and 2016, the national *plans crèches* led to the creation of 150,000 new affordable collective childcare places. This expansion was almost entirely driven by identifiable local shocks, rather than by gradual increases in provision across municipalities. Relying on these shocks, and taking the upper bound of my estimates, I find that the additional places enabled about 8,000 more mothers of young children to hold a salaried job in 2016, which corresponds to a 0.4 percentage-point increase in their employment rate. This suggests that, despite their scale, the national plans had a limited aggregate impact on maternal labor supply over the period.

Appendix A.1 provides a range of additional robustness checks addressing potential identification concerns. Table A.1 replicates the main estimates (Table 6) using an alternative imputation strategy for the municipality of residence among non-employed individuals. Tables A.2 and A.3 incorporate county-specific and local-labor-market-specific time trends, respectively, to account for potential confounding from concurrent local policy changes. Table A.4 includes additional individual-level controls (birth cohort, education, and prior fertility decisions). Table A.5 presents placebo tests on parents who should not be affected by the childcare shock—namely, those observed before having children or whose children are aged 3 to 10—showing no significant effects and thus supporting the identification strategy. Finally, Figure A.1 replicates the event-study analysis using the estimator proposed by Sun and Abraham (2021), while Table A.6 focuses on municipalities where the shock corresponds to the opening of a first facility, thereby addressing concerns raised by de Chaisemartin and D'Haultfœuille (2018).

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.[15][16] Figure 7 displays my estimates. Consistent with this rationale, I find that the expansion

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

[16] Specifically, I implement the Sun and Abraham (2021) specification of the event-study design that allows for heterogeneous treatment effects across cohorts.

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.[17] 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:

$$S^{\text{indiv}}_{c,t} = \frac{4H^{\text{indiv}}_{c,t}}{1820\left(N^{\text{birth}}_{c,t} + N^{\text{birth}}_{c,t-1} + N^{\text{birth}}_{c,t-2}\right)} \tag{5}$$

where $H^{\text{indiv}}_{c,t}$ is the number of hours paid to childminders and nannies in municipality c during year t, and $N^{\text{birth}}_{c,t}$ 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 childminder'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 $S^{\text{indiv}}_{c,t}$ as the outcome, on a restricted subset of municipalities that experienced a childcare shock between 2010 and 2014.[18][19] Figure 8 displays my estimates. 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

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

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

[19] Here again, I implement the Sun and Abraham (2021) specification of the event-study design that allows for heterogeneous treatment effects across cohorts.

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.

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. My estimates suggest that, depending on which solution is substituted, the annual net cost of creating 150,000 new places ranges from a €369 million saving – if collective childcare substituted for at-home individual childcare – to a €738 million increase – if it substituted for childminders, which seem more likely, given that relying on childminders is much more common. These results are highly sensitive to assumptions on substitution patterns and labor supply responses—highlighting the need for better data on individualized childcare use to evaluate the plans' true fiscal and social impact.

Finally, Appendix A.2 presents a series of robustness checks designed to assess the credibility of my findings. Figures A.2 and A.3 replicate the event-study estimates while controlling for the number of eligible children in each municipality. Since this variable appears in the denominator of both the childminder demand and collective childcare availability measures, any measurement error could lead to spurious correlations through division bias. Figures A.4 to A.9 repeat the analysis while incorporating area-specific calendar time trends defined at different geographical levels: county (*département*), local labor market (*zone d'emploi*), and local living zones reflecting access to everyday services (*bassins de vie*). These trends are meant to absorb potential local policy changes that might be correlated with childcare shocks. Overall, the results appear robust to these alternative specifications.

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 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. 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 because 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; Berger, Panico, and Solaz, 2021). These potential lifecycle benefits must be taken into account to achieve a meaningful normative analysis of these policies.

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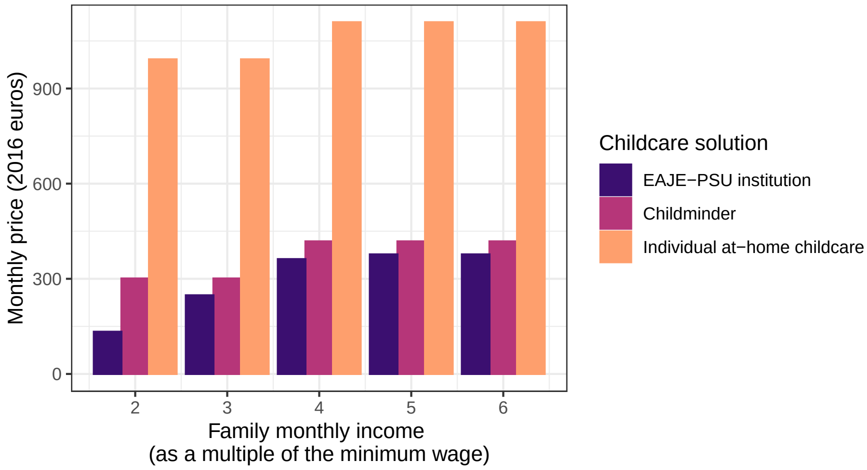
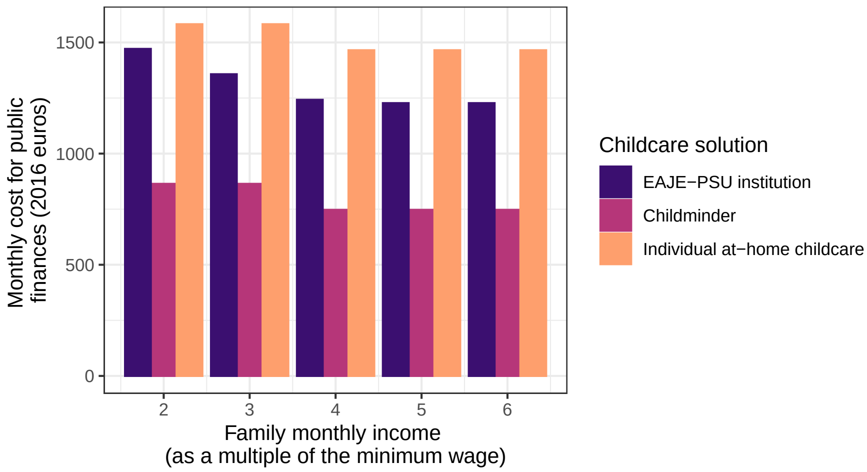
Figures

Figure 1 – Childcare prices along the income distribution

(a) Prices paid by families



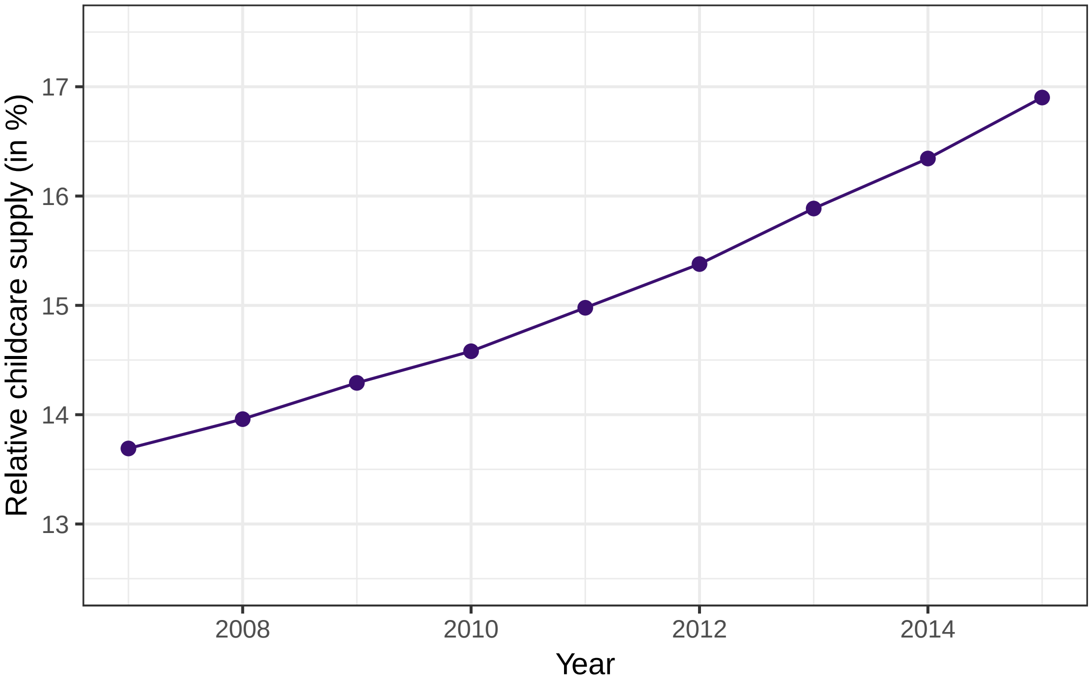
(b) Costs for public finances



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

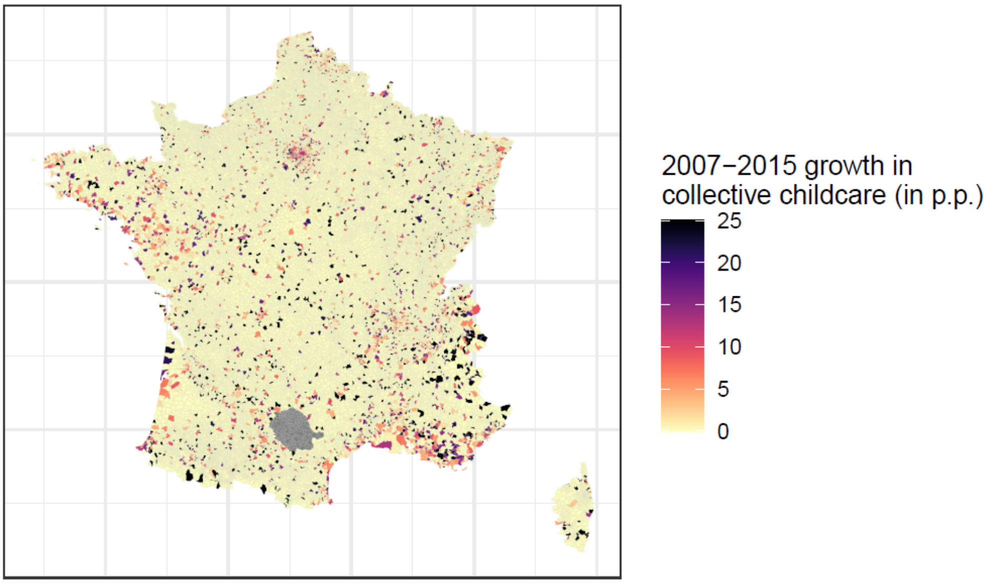


Estimates of the ratio EAJE-PSU childcare places offered to children aged 2 or less in metropolitan France (mainland France and Corsica).

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

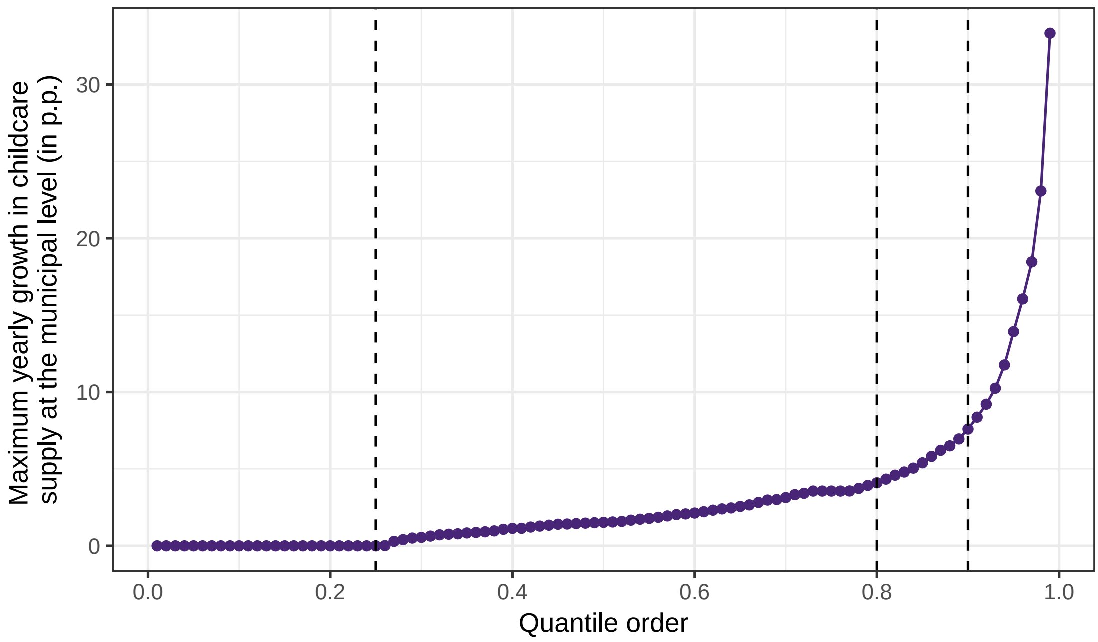


Estimates of the 2007-2015 growth in the ratio of EAJE-PSU placed offered to children aged 2 or less at municipality level.

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

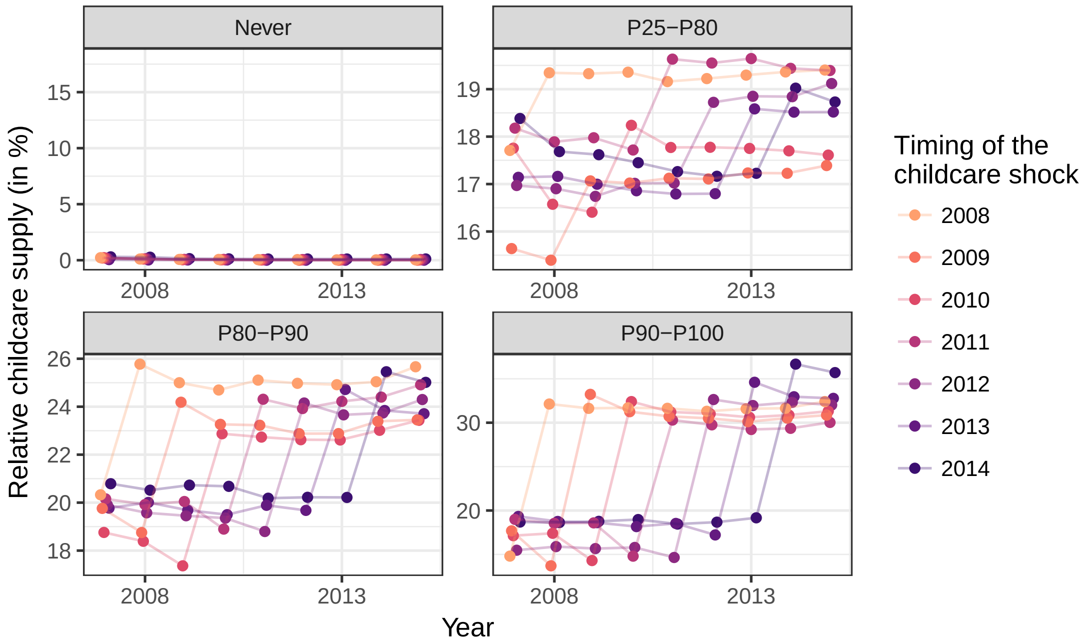


Estimates of the highest annual within-municipality growth in the ratio EAJE-PSU childcare places offered to children aged 2 or less.

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



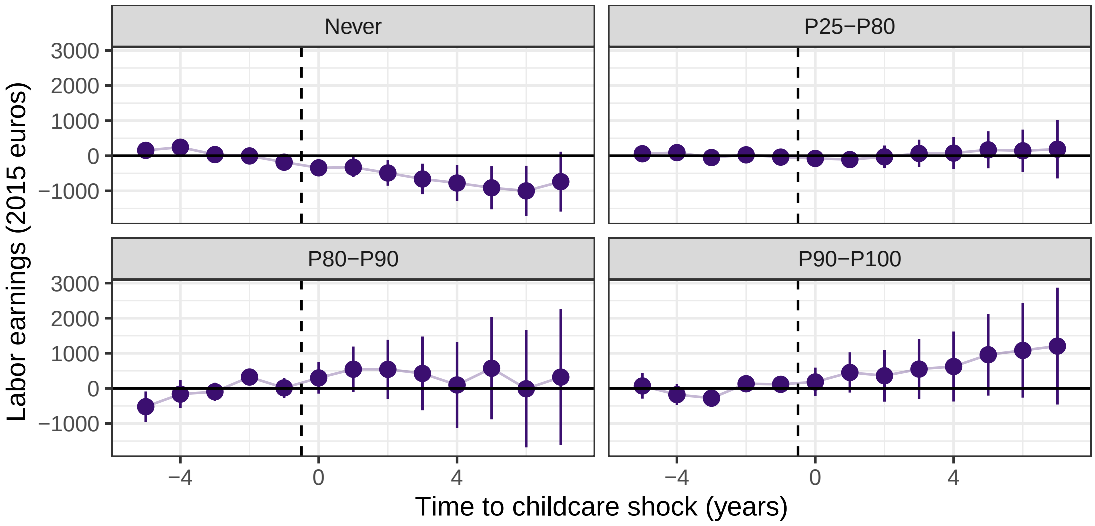
Estimates of the ratio of EAJE-PSU childcare places offered to children aged 2 or less at the municipality level.

Note. Data regarding the Tarn département are omitted.

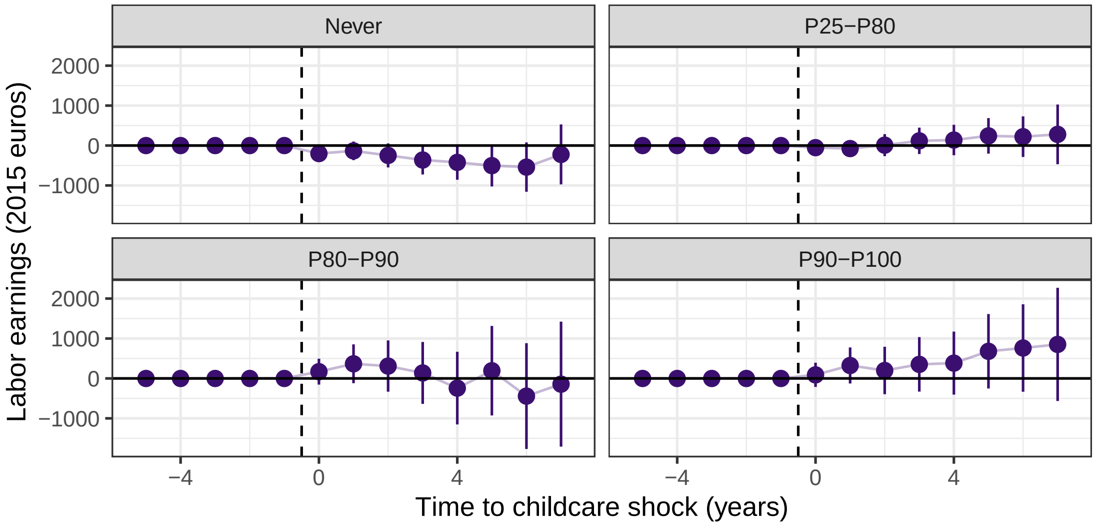
Source. EAJE-PSU records, CNAF. Birth records, Insee.

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

(a) Normalized pretrend

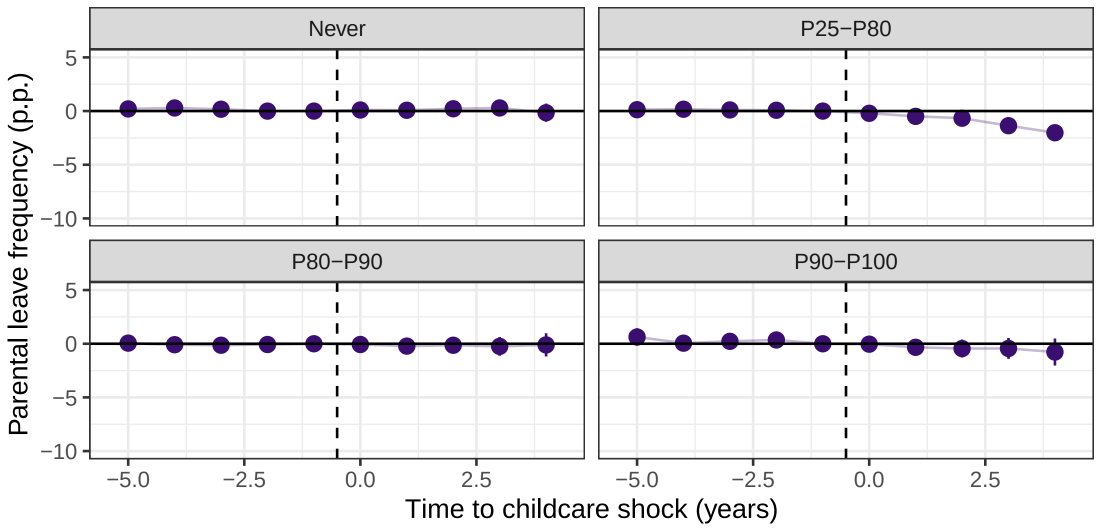


(b) Pretrend set to 0



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

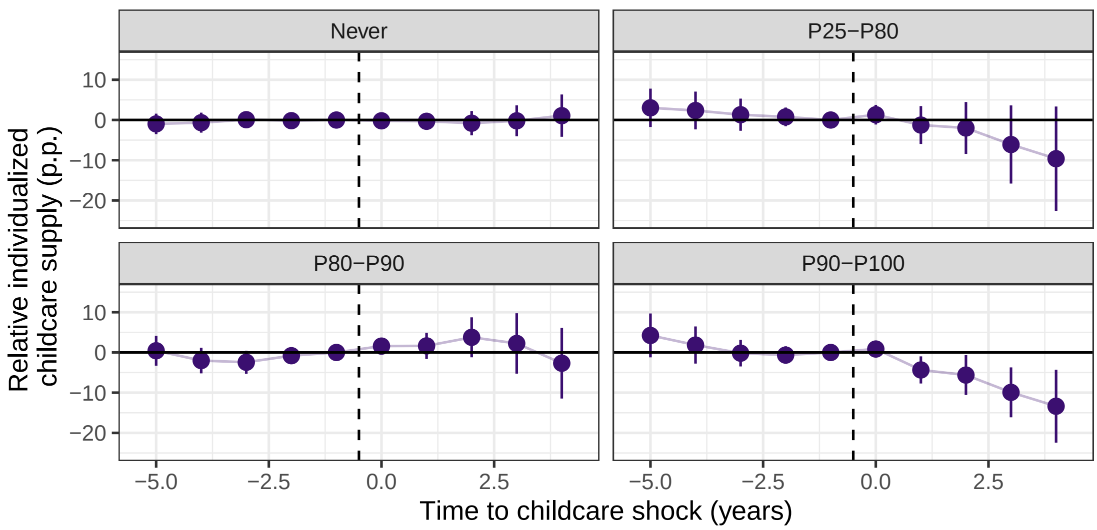


Event-study estimates of the effect of childcare shocks on the share of families receiving parental leave allowances.

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 – 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.

Table 4 – 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.

Table 5 – 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 6 – 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					
0–2	407.48	-1.73	-2.66	-0.066	0.681
	(894.34)	(3.58)	(11.23)	(0.126)	(0.473)
b. P90-P100 treatment group					
0–2	174.44	-3.07	-9.41	-0.027	0.722
	(944.92)	(3.78)	(11.82)	(0.134)	(0.486)
Fathers					
a. Full sample					
0–2	603.78	3.02	-2.64	0.005	0.053
	(1395.13)	(2.8)	(9.4)	(0.088)	(0.671)
b. P90-P100 treatment group					
0–2	-91.51	2.72	-1.85	-0.018	-0.09
	(1461.17)	(2.9)	(9.84)	(0.093)	(0.715)

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 Identification

A.1 Parental earnings

Table A.1 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender – alternate imputation strategy for individuals out of the labor force

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
0–2	255.7 (860.41)	-2.28 (3.51)	-2.57 (11.23)	-0.066 (0.126)	0.687 (0.472)
Fathers					
0–2	186.29 (1383.8)	1.2 (2.85)	-2.71 (9.4)	0.006 (0.088)	0.052 (0.671)

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. When out of the labor force, the imputation uses the next future municipality of residence (Table 6 uses the last observed municipality of residence). *Source.* EAJE-PSU records, CNAF. Birth records and DADS-EDP panel, Insee.

Table A.2 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender – *département*-specific time trends

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
0–2	191.56 (879.89)	-2.42 (3.53)	-8.39 (11.16)	-0.068 (0.128)	0.731 (0.466)
Fathers					
0–2	418.05 (1333.66)	0.54 (2.7)	8.14 (9.17)	0.007 (0.086)	0.249 (0.642)

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 A.3 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender – *zone d'emploi*-specific time trends

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
0–2	-256.91 (893.63)	-4.23 (3.44)	-3.12 (11.39)	-0.062 (0.128)	0.889 (0.512)
Fathers					
0–2	879.48 (1498.4)	3.1 (2.82)	6.69 (9.21)	0.052 (0.086)	0.208 (0.733)

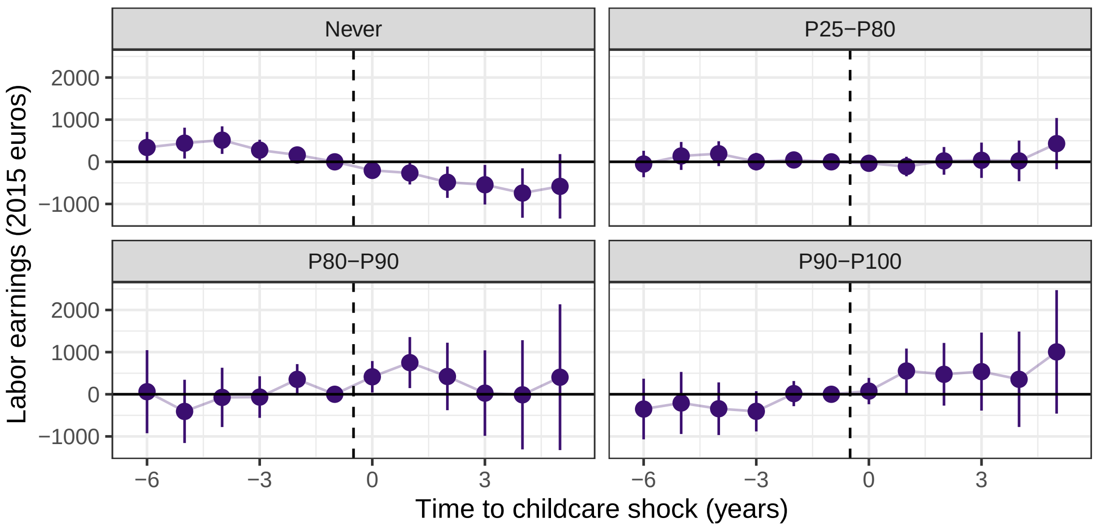
Dependent variable. Parents' labor outcomes. *Explanatory variables.* Childcare supply plus calendar-time dummies interacted with treatment group and Zone d'emploi dummies, plus municipality fixed effects. Childcare supply is instrumented by time-to-event dummies interacted with treatment group. Standard 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 A.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					
0–2	778.53	-0.49	2.4	0.006	0.706
	(806.14)	(3.4)	(10.93)	(0.121)	(0.425)
Fathers					
0–2	567.93	2.98	-2.92	0.002	-0.05
	(1333.54)	(2.8)	(9.25)	(0.087)	(0.628)

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.

Figure A.1 – Event-study estimates based on Sun and Abraham (2021) 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.

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

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)
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)
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)
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)
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.

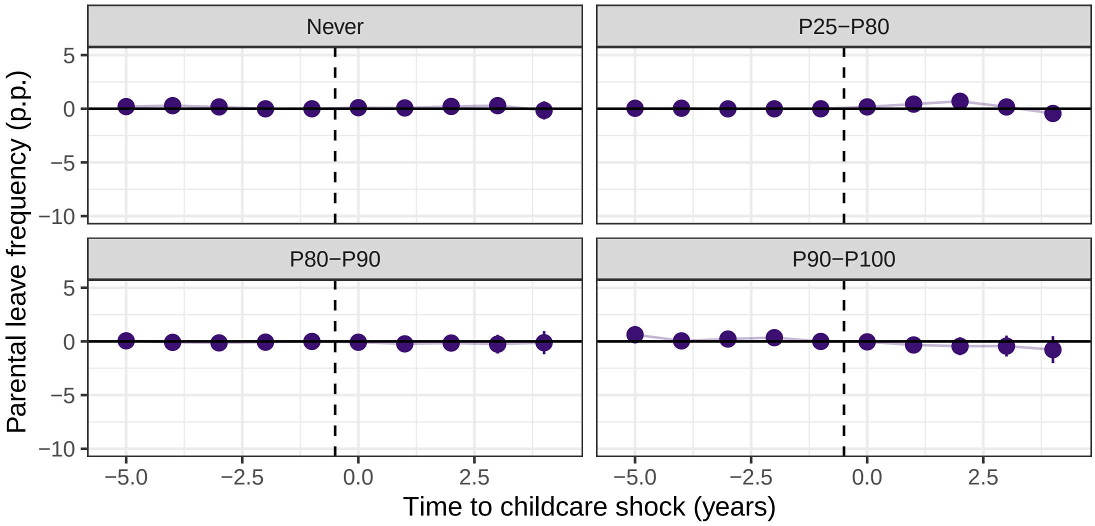
Table A.6 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes based on the opening of the first EAJE-PSU facility, by gender

Age of youngest child	Labor earnings (2015 euros)	Employment (p.p.)	Days	Hours per day	Hourly wages (2015 euros)
Mothers					
0–2	-496.21	0.49	-20.79	-0.036	0.03
	(1037.64)	(4.7)	(14.4)	(0.151)	(0.537)
Fathers					
0–2	-1390.23	1.09	-21.23	0.158	-0.76
	(1461.44)	(3.55)	(12.96)	(0.112)	(0.806)

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.

A.2 Substitution effects

Figure A.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

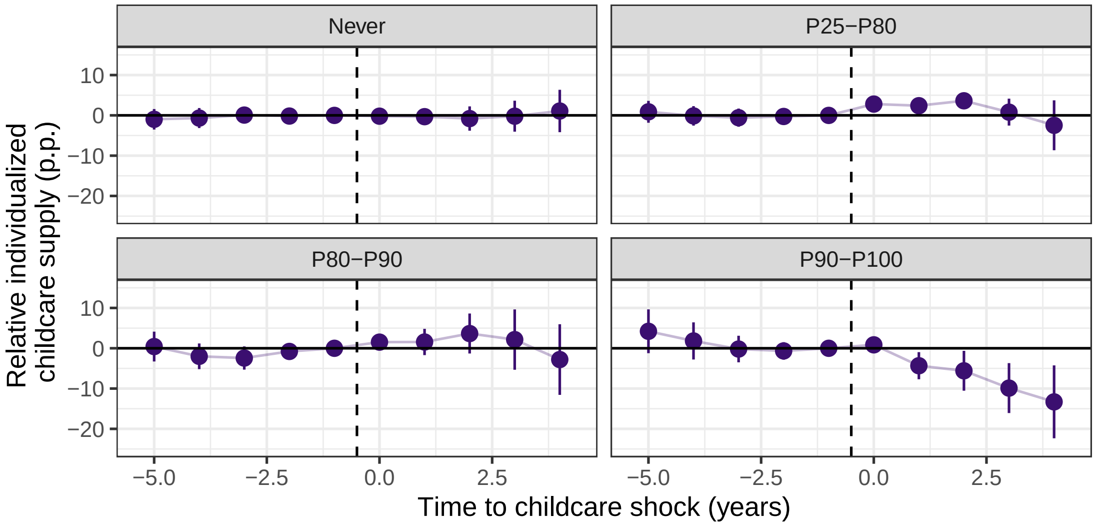


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less that receive parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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

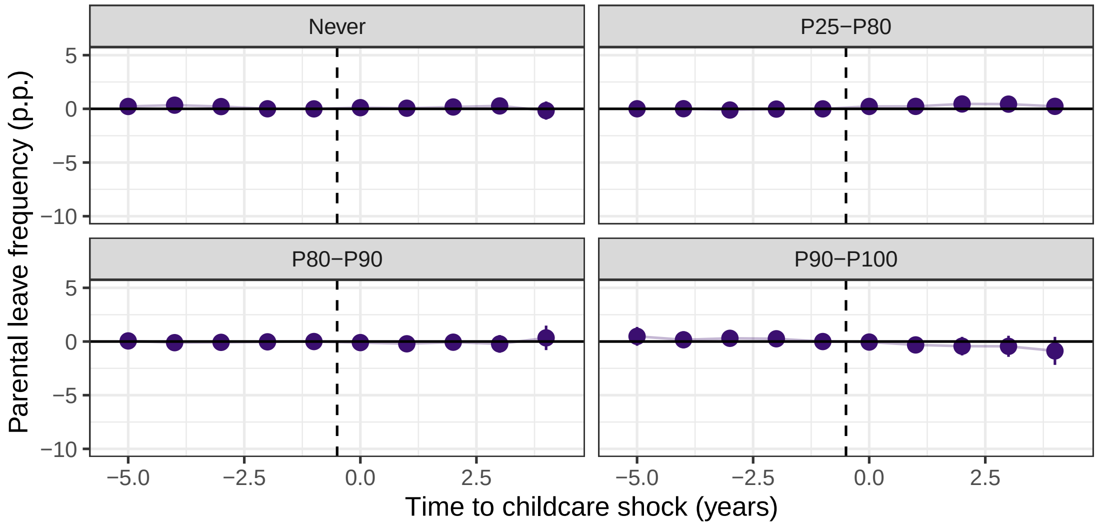


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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

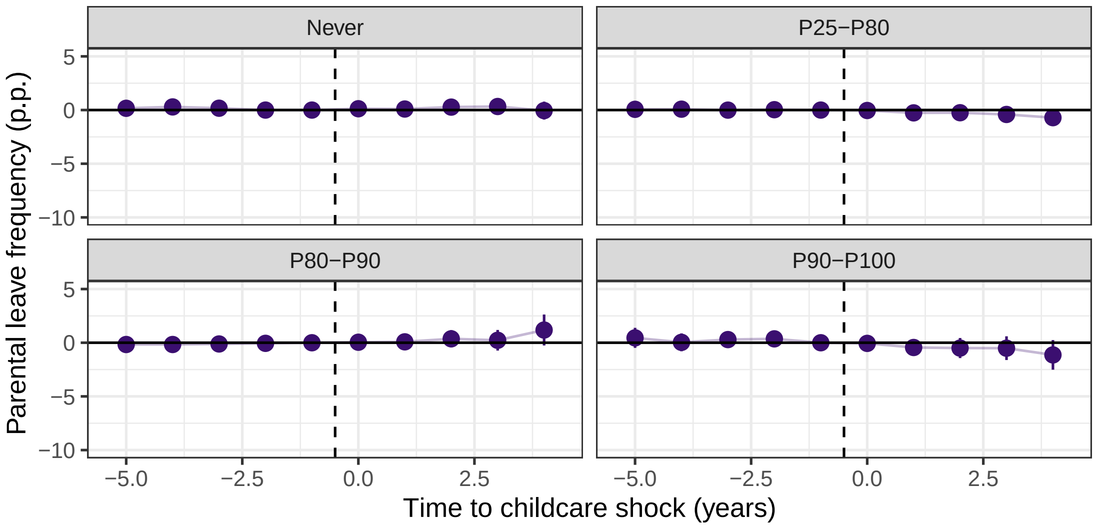


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less who received parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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

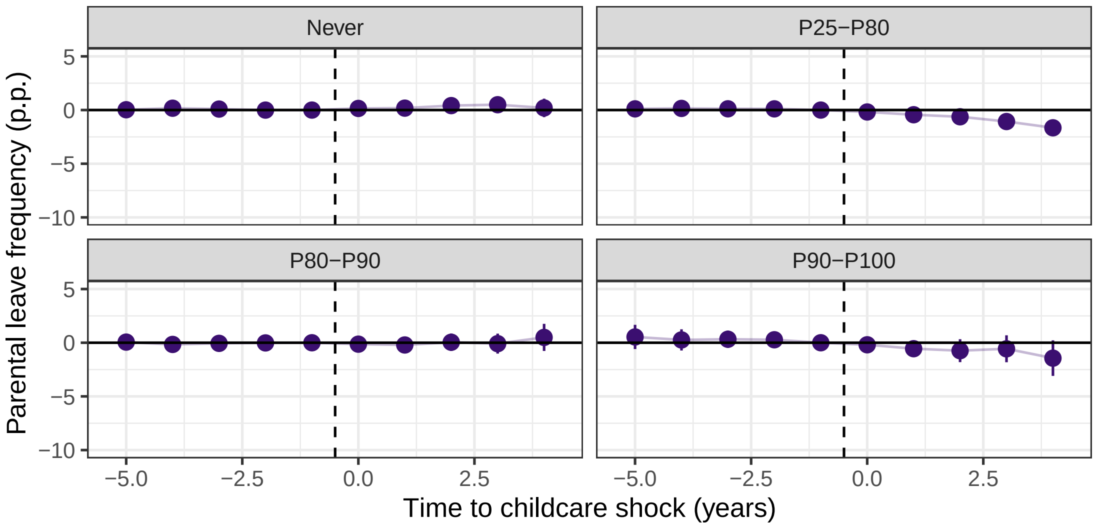


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less who received parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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

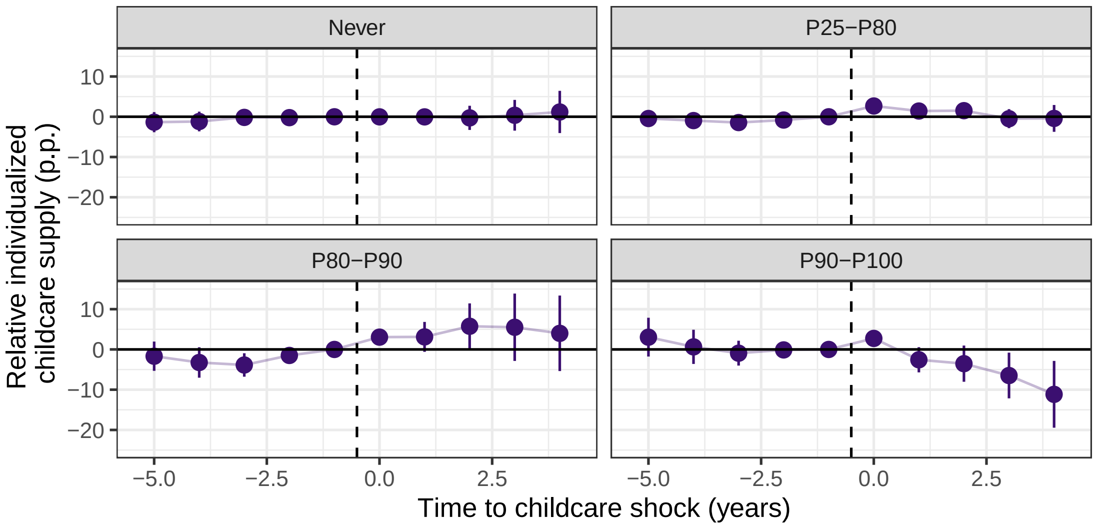


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less who received parental leave allowances in December.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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

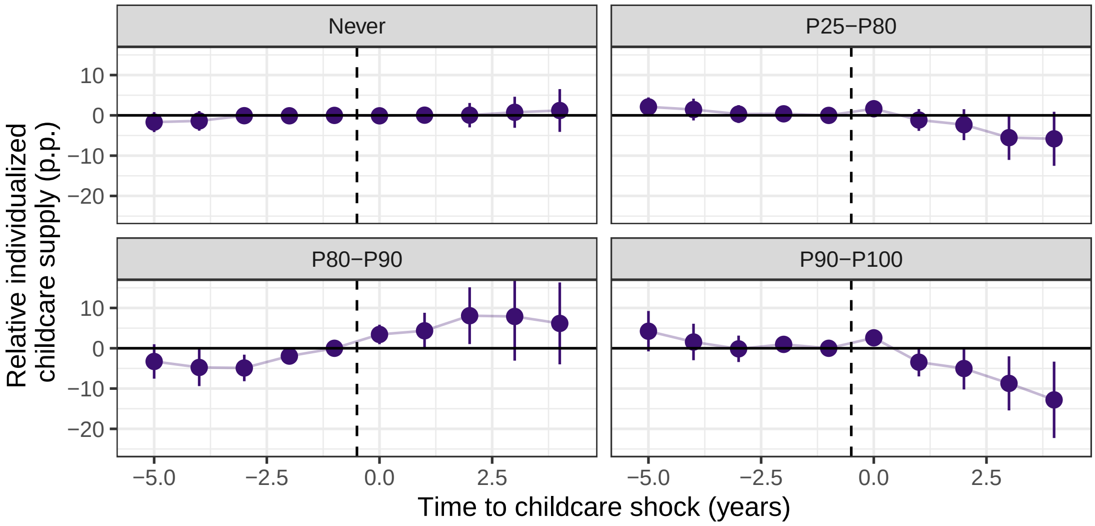


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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

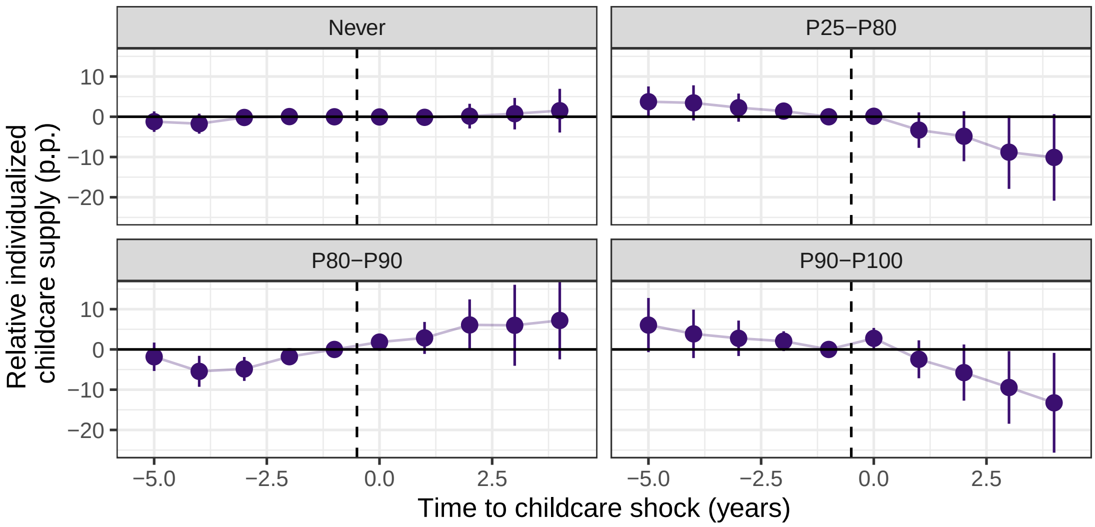


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and nannies.

Note. Data regarding the Tarn département are omitted.

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

Figure A.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.