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Collective Childcare and Parental Earnings in France

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Travailler autant en dépensant moins ? Les effets de l’accueil collectif du jeune enfant sur les revenus salariaux des parents

Résumé
Une série de plans nationaux destinés à augmenter la capacité des Établissements d’Accueil du Jeune Enfant (EAJE) a généré, dans certaines communes, des hausses soudaines, et survenant à des dates variables, de la capacité de ces établissements. Cela permet d’en identifier l’effet sur les revenus et l’offre de travail des parents, et leurs choix de mode de garde, entre 2007 et 2015. Ces plans n’ont affecté de façon significative ni les revenus salariaux des parents, ni le recours au congé parental. Ces solutions collectives se sont en revanche substituées aux assistantes maternelles et à la garde à domicile, plus coûteuses pour les familles. Cette substitution signale un écueil potentiel de politiques familiales qui soutiennent de multiples solutions d’accueil du jeune enfant.

Mots-clés : offre de travail, accueil du jeune enfant, event-study, congé parental.

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Abstract
I leverage the staggered expansion of subsidized childcare institutions across municipalities, induced by 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 neither in the labor outcomes of parents nor in the take-up of paid parental leave. 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.

Classification JEL : J13, J16, J18, J22.

1 Introduction

In international comparisons, France is widely seen as a success in terms of family policies that promote family-work conciliation and gender equality: among both OECD and EU countries, it ranks high in terms of both fertility rate, female employment rate and formal childcare coverage (see e.g. OECD, 2011). In contrast with other countries, childcare arrangements in France are extremely diverse: a long-lasting institutional history results in 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 a freedom in their arrangements that fits heterogeneity in preferences and constraints.

In this paper, I highlight a downside of this institutional setting. Namely, this diversity can result in inefficiency, because it leaves room to potentially large substitution effects across childcare solutions. As a result, massive investment plans aimed at increasing the overall provision of formal childcare may simply crowd out other subsidized solutions, instead of further enhancing family-work conciliation and gender equality. Specifically, I investigate the consequences in terms of parental labor earnings and labor supply on the one end, and childcare choices on the other hand, of a series of national plans launched in the 2000s and designed to increase the provision of particularly affordable collective childcare targeted at very young children and provided by daycare centers. My main results show that: (i) these plans did not trigger any substantial change as to the labor supply of parents, and especially 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 affordable childcare and maternal labor supply. Indeed, null effects are not uncommon in the literature devoted to the impact of affordable childcare on maternal labor supply (e.g. Fitzpatrick, 2010; Havnes and Mogstad, 2011a), which has been widely attributed to substitution effects across childcare solutions. However, with few exceptions, this mechanism remains somehow speculative because it usually involves the crowding-out of informal childcare solutions (e.g. childcare provided by a relative or a neighbor). The problem is thus mainly that these informal arrangements are not observed in the data with sufficient precision and frequency to enable a correct identification of the causal effect of the

supply of affordable childcare on childcare choices when these policies fail to enhance mothers' labor outcomes. By contrast, informal childcare solutions are quite uncommon in France when it comes to primary childcare providers, which implies that gathering data on the multiple formal childcare solutions at stake is to a large extent sufficient to cover almost all relevant childcare choices. I am therefore able to provide clean evidence that null effects of the provision of affordable childcare on maternal labor supply do indeed arise from crowding-out effects.

My empirical approach relies on the staggered expansion of affordable collective childcare across narrow geographical areas, induced by a succession of national plans that aimed at increasing the overall provision of collective formal childcare. Specifically, I leverage differences in the timing of massive 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 several detailed administrative datasets: childcare institutions and parental leave registers maintained by the Family branch of the French Social security, as well as both cross-sectional and longitudinal birth registers and payroll data made available by Insee.[1] This enables me to (i) recover the supply of affordable collective childcare provided by heavily subsidized institutions at a narrow geographical level; (ii) trace back labor market trajectories and fertility decisions of a large sample of individuals over the relevant time-period; and (iii) estimate changes in the demand for other formal childcare solutions over time.

I find that these sharp increases in the provision of affordable collective childcare at the municipal level did not trigger any substantial change in the labor outcomes of parents. My results are precise in the sense that they are incompatible with being offered a collective childcare spot shifting the probability for mothers of children aged 2 or less to be salaried employees by more than 5 percentage points.

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 families with very young children are entitled. While most empirical studies consider the provision of childcare and parental leave as two separate policies, this parameter bears substantial policy relevance as it predicts whether a change in the provision of childcare is likely to affect the demand for parental leave or not. This policy relevance is particularly salient in France in which (i) the Family branch

[1]The longitudinal data that contains information regarding both fertility events and measures of labor market attachment is known as *Panel DADS-EDP*.

of the Social security funds both these policies, and (ii) the law grants parents with the possibility to extend the duration of their parental leave if they are not offered a formal childcare spot[2]. I find that the expansion of affordable collective childcare institutions does not trigger any change as to 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 baby-sitters. Relying on the same approach, I provide evidence of a very substantial crowding-out of these childcare solutions after collective childcare expansions. Specifically, in municipalities that experienced the sharpest increases in the provision of affordable collective childcare, the magnitude of the medium-run drop in the supply of individual childcare is equivalent to that of the increase in the provision of collective childcare. This implies that the increased provision of childcare by daycare centers likely benefits parents that would have otherwise turned to individualized and more expensive formal childcare solutions. Given that these childcare solutions are disproportionately favored by households from the top of the income distribution (Villaume and Legendre, 2014), this suggests that such collective childcare expansions may to some extent subsidize affluent families.

This empirical policy evaluation exercise gives rise to both efficiency and equity concerns. Firstly, that expanding heavily subsidized collective childcare does not lead to changes in the labor supply of parents suggests that such policies can bear a significant net cost. However, I show this cost to depend to a very large extent on subsidies that are directed towards other formal childcare solutions, about which I cannot be fully conclusive. Secondly, the substantial crowding-out effects on individualized and more expensive childcare solutions make it plausible that these childcare expansions mostly benefit families from the higher end of the income distribution, thus further expanding income inequality across households (Barigozzi, Cremer, and Roeder, 2019).

Literature The main empirical challenge regarding the identification of the labor supply effects of childcare provision is to overcome the fact that childcare and labor supply decisions are made jointly, so that the causal impact of childcare on labor supply is not 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

[2]Article L531-4 du Code de la sécurité sociale

Garfinkel, 1992; Domeij, 2013; Bick, 2016) or quasi-experimental evidence arising from plausibly exogeneous policy changes (e.g. Gelbach, 2002; Baker, Gruber, and Milligan, 2008; Fitzpatrick, 2010; Gathmann and Sass, 2018; Carta and Rizzica, 2018).

Especially relevant to this paper are studies that exploit heterogeneity between geographical areas in the timing of publicly subsidized childcare expansions induced by national-level policy reforms to infer the causal impact of the provision of affordable childcare on mothers labor outcomes (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 abstract from both the endogeneity of individual childcare choices and the endogeneity of local childcare availability with respect to labor supply by relying on a difference-in-difference framework *à la* Duflo (2001). Specifically, they leverage the fact that some areas experience large and sudden increases in the provision of affordable childcare, while other areas do not experience such increases, 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, the evolution of labor outcomes in the treated group would have been the same as the evolution of labor outcomes 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.

As for the results, this literature is somehow contrasted between papers that find substantial positive effects of the provision of affordable childcare on maternal labor supply, and others that emphasize null effects. In the US and Canada, Blau and Currie (2006) report estimates of the elasticity of maternal labor supply with respect to the price of childcare of 20 studies; 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 age of the targeted children, 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 the child penalties *à la* 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

the increased childcare provision had no effect on the child penalty in Austria.

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

The French setting has been covered by few papers. Among them, both Choné, Le Blanc, and Robert-Bobée (2004) and Allègre, Simonnet, and Sofer (2015) resort to a joint modeling of childcare choices and labor supply decisions, and reach different conclusions as to the effect of childcare prices on childcare choices and maternal labor supply, which may arise from differences in how detailed the childcare data they rely on is. Closer to a quasi-experimental approach, Maurin and Roy (2008) rely on the difference between families that landed a childcare spot and those who did not among all families that applied in a particular city, and find a positive effect on maternal labor supply. Goux and Maurin (2010) focus on the availability of early school for 2-years old, and a find positive impact for single mothers, but not for cohabiting mothers. Lastly, Givord and Marbot (2015) rely on a policy reform that took place in 2004 and induced a sharp decrease in childcare prices 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 my data and section 4 details my identification strategy. In section 5, I present my results regarding parental earnings and labor supply. Section 6 investigates the underlying mechanisms, i.e. substitution across childcare solutions. Lastly, section 7 concludes.

2 Institutional setting

2.1 Early childcare coverage

France is among OECD countries with the largest access to early childcare outside 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 only focus on childcare that targets children under age 3 given that in France, children aged 3 get into school, the enrollment rate being over 99%.

Interestingly, France is also among the OECD countries with the largest inequality in terms of childcare enrollment between children from low-income and high-income households. Indeed, whereas nearly 75% of children from the highest income tertile are enrolled in early childcare outside home, a level only that is only reached in the top tertile in Denmark, this is only the case of 31% of children from the lowest income tertile, which basically matches the average enrollment rate in the UK (OECD, 2016). Additionally, France achieved this large childcare coverage by fostering very diverse childcare arrangements, without restricting itself to daycare centers that only account for a fraction of the total: formal individualized childcare solutions, e.g. childminders and to a lesser extent individual at-home childcare are also quite common childcare solutions. Such individualized childcare solutions are especially favored by affluent families: 42% of families of the top income quintile resorted to childminders and baby-sitters as their main childcare solution in 2013, as opposed to 3% of families that belonged to the lowest quintile. Relying heavily on informal solutions is quite infrequent 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).

Early childcare is heavily subsidized in France: including compensated parental leave schemes, public expenditures devoted to early childcare public policies amount to 16G€ in 2015 (IGAS/IGF, 2017). In this paper, I focus on one type of formal childcare provided outside home, which basically consists in daycare centers, i.e. formal and collective solutions, in contrast with formal individualized solutions (e.g. childminders or at-home childcare provided by baby-sitters) 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 the total theoretical capacity in formal early childcare in 2014 (IGAS/IGF, 2017).

2.2 EAJE-PSU institutions

EAJE institutions are, broadly speaking, daycare centers that provide childcare to children up to age 6; however, because as of age 3 almost 100% of children attend school, they are more generally targeted towards children aged 0 to 2.[3] They are subjected to a strict regulation, based on the Public Health Code, and can only open when granted authorization by local authorities (either at the département-level for private institutions, or at the municipality-level for public institutions), after an agreement has been offered by Maternal and Child Health Services. This authorization defines, for each institution, a capacity measured in number of childcare spots. These institutions are often operated by local authorities, sometimes through an association.

Specifically, I investigate the provision of childcare provided by EAJE institutions funder under the *Prestation de Service Unique* (PSU) scheme.[4] Local offices (*Caisse d'Allocations Familiales*, CAF) of the Family branch of the French social security (*Caisse Nationale d'Allocations Familiales*, CNAF) fund a large share of EAJE institutions through this scheme. It covers 66% of the hourly cost of childcare, after families' contribution has been deducted. To get this funding, EAJE institutions have to meet several requirements: (i) having been granted authorization to open by the relevant authorities; (ii) make their spots available to all families; (iii) base the prices families pay on a national fee schedule that makes this type of childcare particularly affordable to families; and (iv) sign a target and management agreement with the relevant 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 of a full-time childcare spot for parents 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 appear much more expensive, especially for families

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

[4] This leaves aside collective institutions that are not funded under the PSU scheme. These institutions, coined as *micro-crèches PAJE* are subsidized by the *Complément Mode de Garde*, a childcare allowance; their prices are usually much higher than those offered by EAJE-PSU institutions: the price paid by families is usually between that paid to childminders and that paid for at-home childcare.

who belong to the lowest end of the income distribution.[5]

To achieve these low prices, EAJE-PSU institutions are heavily subsidized. In 2015, the average total hourly cost of an EAJE spot was €8.86 (Clément and Aho, 2018), the average operating cost of a full-time EAJE spot 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).[6] Overall, the operating costs of EAJE-PSU institutions in 2015 amounted to 6 billions euros.

Access to the childcare spots provided by EAJE-PSU institutions is decided at the local level. Criteria may vary from one place to the other, but they generally take into account place of residence of the parents, their employment status and the socio-economic background of the family.

Lastly, EAJE institutions were opened 222 days a year on average in 2015, which basically represents 5 days a week except for July and August (Onape, 2017). When opened, their opening hours covered on average 11 hours a day. With respect to an ideal situation in which all childcare spots were occupied full-time thoughout the year, the occupancy rate was about 70%. These quantities varied very little throughout my time-period of interest.

2.3 National expansion plans

Until the beginning of the 2000s, the development of EAJE institutions mostly resulted from decisions made by local authorities. In June 2000, the first national *plan crèche* was launched. Its aim was mainly to subsidize the increase of the availability of formal collective childcare, either by expanding pre-existing institutions, 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 the national level by the CNAF, and implemented by local authorities with the help of local CAF offices. Between 2000 and 2016, the creation of 150 000 new childcare spots have so been subsidized, 2/3 of which were so through the opening of new institutions. Regardless of whether they were directly subsidized by these plans or not, the number of collective childcare spots increased by 70 000 between

[5] A reason for this is that individualized childcare solutions are subsidized through tax credits and tax rebates that make them particularly appealing to families who pay large income taxes. Figure A.1 displays the prices paid by families along the income distribution, based on a case-study approach, which makes the fact that the price difference between EAJE-PSU institutions and other individualized arrangement is much larger at the bottom of the distribution.

[6] Subsidized childcare is not restricted to EAJE-PSU institutions, as a massive share of their cost is also taken care of by public institutions. Figure A.2 displays the related burden for public finances, making it salient that, for instant, at-home childcare bears substantial public costs.

2007 and 2015, my time-period of interest. This is therefore a relatively modest increase: as a comparison, the number of children aged 2 or less over the same period was between 2.3 and 2.4 millions.

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, all *plans crèches* had induced a 2 billions euros expenditure, notwithstanding the yearly operating cost of the childcare spots they allowed to create (IGAS/IGF, 2017). The average cost for the creation of a childcare spot was estimated to be about €27 000 in 2009, 29% of which were financed through the national expansion plans (Onape, 2010).

2.4 Parental leave policies

Various parental allowances were merged in 2004 into the *Prestation d'Accueil du Jeune Enfant* (PAJE). It comprises a one-shot means-tested bonus at childbirth (*prime de naissance*), monthly means-tested benefits (*allocations familiales*), a childcare subsidy (*Complément libre choix du Mode de Garde* (CMG)), and some child benefits granted when parents interrupt their careers or work part-time (previously *Complément Libre Choix d'Activité* (CLCA) and now *Prestation Partagée d'Éducation de l'enfant* (PreParE)).

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

Lastly, in Appendix A.2, I resort to survey data that enables to elicit the preferences of parents regarding childcare, and the constraints they meet in their childcare choices. The main lessons are that (i) there is substantial demand from parents for childcare provided in collective institutions; (ii) this demand is likely

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

3 Data

My analysis combines several administrative registers 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

3.1.1 Childcare institutions registers

First, I rely on data provided by the CNAF, the Family branch of the French social security, to get information as to the supply of affordable collective childcare at the municipal level. Specifically, this data covers all EAJE institutions that are funded under the PSU scheme. For each municipality between 2007 and 2015, the data provides with the number of EAJE institutions funded under the PSU scheme within each municipality, and the number of childcare spots they offer, as defined by their opening agreement, granted by local authorities, that specifies a maximum capacity for each institution.[7]

3.1.2 Parental leave data

The Family branch of the French social security also provides with data regarding the take-up of paid parental leave. Specifically, for each municipality from 2009 to 2018, this dataset provides with the number of families who who were entitled to benefit from either the CLCA or the PreParE in December. To be considered entitled to these allowances, families have to apply, fill in a form, and meet several criteria. As a result, this is a meaningful measure of the number of families that benefit from these parental leave allowances, as it only covers families who applied and are eligible.

[7] I discard data regarding one département (Tarn), for which the data would suggest that no EAJE-PSU institution existed in 2007, whereas many municipalities had such institutions in 2008. In 2007, the Tarn département accounted for 0.6% of the total French population.

There are however a number of caveats to this data. First, it does not allow to separate full-time parental leave takers from part-time parental leave takers. Second, it does not provide information as to which parent took the leave. Third, in 2015 the data regarding the take-up of PreParE – the then newly created parental leave allowance, offered to parents of children born in 2015 or later – is missing. For this reason, when investigating the take-up of parental leave benefits, I only rely on data that covers the 2009-2014 time-period, and only consider the take-up of the CLCA allowance. Lastly, due to statistical secrecy concerns, the data does not provide information on municipalities in which fewer than 5 families took the leave. To the extent that childcare institutions concentrate in large enough municipalities, this does not appear to be a major threat to my analysis.

3.2 Labor market data

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

3.2.1 Parental earnings and labor supply

I take advantage of a longitudinal version of the DADS at the individual level. This dataset contains detailed information at the individual-year level; individuals are identified by an anonymized individual identifier based on the NIR, a social security number, that allows me to track them over time. Starting in 1967, the sample covers individuals born in October in odd-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 the parity of their year of birth. While information regarding earnings is available since the creation of the dataset, information regarding hours paid is

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

only available since 1995, with the exception of central State civil servants, for whom the information is not displayed before 2009.[9]

The most relevant limitation of this dataset is that it does not contain information as to self-employment. This may prove problematic if, as suggested by Connelly (1992), mothers tend to turn to self-employment in order to deal more effectively with children-related time constraints. Specifically, if increases in the provision of affordable childcare induce transitions from non-employment and salaried employment to self-employment, then my estimates will be biased downwards. Reversely, 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 remain limited: in 2007, less than 5% of mothers with children aged less than 3 who held a job were self-employed, and 4% of those who interrupted their careers were previously self-employed (Galtier, 2011).

3.2.2 Individualized childcare

As of 2009, the DADS files also cover salaried employees who are directly paid by households, which enables me to get information regarding childminders and baby-sitters that provide at-home childcare. Specifically, I rely on this feature to compute aggregate earnings and hours paid to childminders and baby-sitters, based on 4-digit occupation, at the municipality level from 2009 to 2015, from a comprehensive, cross-sectional version of the dataset.

3.3 Fertility data

My analysis also relies on birth registers. Birth registers are filled by an individual who was present at the time of birth, usually the father, but it can also be a doctor or a midwife. They are filled at the town hall of the birthplace. I again rely on two different versions of these registers.

[9] For these observations, I resort to a measure of working time expressed in full-time equivalent to impute working hours before 2009. Specifically, the data contains information as to 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 paid to full-time full-year working individuals (1820 hours a year throughout my time-period of interest).

3.3.1 Cross-sectional registers

Firstly, I take advantage of cross-sectional comprehensive birth registers. This dataset displays the municipality in which mothers lived at the time of the birth. I rely on this feature to compute the number of children born to women living in a given municipality for any year between 2005 and 2015. This allows me to recover an approximate measure of the evolution of potential demand for childcare at a narrow geographical level.

3.3.2 Longitudinal registers

Secondly, I rely on a longitudinal version of these registers at the individual level, which is an extract of the *Échantillon Démographique Permanent* (EDP). This dataset contains information regarding the timing of childbirths for parents. Thanks to the NIR, it is possible to merge this dataset with the longitudinal version of the DADS. It covers individuals born on October 1-4 regardless of the parity of their year of birth; since 2004, information regarding individuals born on January 2-5, April 1-4 and July 1-4 is available,

A potential issue with the data is that there is no information regarding children born before 2004 to individuals who were born in January, April and July.[10] As a result, the number of children for these individuals is biased downwards. Because my time-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 parents are born in October (in which case the data is correct) or not (in which case I underestimate the number of children parents have).

3.4 Data preparation

I first estimate the supply and the potential demand for childcare at a narrow geographical level. To this end, I combine the CNAF dataset and the comprehensive cross-sectional birth registers at the municipality level. This data provides me, for

[10] In addition, some childbirth-related data was incomplete during the 1990s in administrative birth registers for individuals born October 2-3 (for details, see Wilner, 2016): as a result, for these individuals I rely on the census rather than birth records, as do Pora and Wilner (2019). The quality of this data is comparable with that of individuals born October 1 or 4 for whom administrative birth records are available since 1967.

each municipality and each year, with (i) the number of childcare spots available in each municipality, and; (ii) the number of children born to mothers who live in such geographical area. This allows me to compute a relative supply measure, i.e. the share of children who are potentially covered by childcare provided by daycare centers. Specifically, I define the relative childcare supply $S_{c,t}$, where c denotes municipality and t stands for a particular year, as the ratio:

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

where $N_{c,t}^{\text{spots}}$ is the number of spots supplied by EAJE-PSU childcare institutions in municipality c during year t, and $N_{c,t}^{\text{birth}}$ the number of children born to women that lived in c at time t. In other words, this measure assumes that there is no mobility of children during the first three years of their life.

Figure 1 displays the evolution of the relative supply at the national level between 2007 and 2015. It increased by roughly 3.5 percentage points, and almost linearly over years. An interesting feature of this continuous expansion of affordable childcare is that it was very heterogeneous across geographical units. In Figure 2, I display a map that traces back the change in relative childcare supply level for each municipality, from 2007 to 2015. This map makes it very clear that this moderate increase concentrated in relatively few areas, that experienced strong increases in the provision of affordable childcare, in contrast with most municipalities in which the supply barely changed.

In a second step, I recover the data at the individual level. I restrict the sample to individuals who experienced childbirth between 2005 and 2015, so that they actually have children of the targeted age group at some point between 2007 and 2015, and to individuals between ages 20 and 60. Because the municipality of residence is only observed in the labor market data, I further impose that these individuals have been salaried employees at least once between 2002 and 2015.[11] I drop 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 regarding very low working times, I winsorize earnings at the quantile of order 0.9999, and drop individual-year observations that either (i) have an annual number of days paid under 18 or; (ii) have hours paid per day inferior to 1/20 of

[11] Empirically, this is the case of 94% of parents throughout my time-period of interest.

the legal duration of work for full-time workers or; (iii) have hourly wages under 90% of the minimum wage. For these observations, and for individuals who are not found in my labor market data for a particular year, I impute labor earnings equal to 0, and consider them not to participate in the labor market.[12] 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 hand.

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 individual-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 when considering observations that are anterior to the first job they held (see Figure 3). 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.

I finally merge this individual-level data with the geographic-level data related to affordable childcare. This leaves me with 1.5 millions observations of parents with children aged 0 to 2, that account for 430 000 individuals. Table 2 displays summary statistics on the sample. The gender gap in labor outcomes is extremely salient: on average, while they tend to be more educated, mothers of young children earn slightly more than half the average earnings of their male counterparts. This gap is largely driven by labor supply decisions: among those who hold a salaried position, the gender gap in hourly wages is much smaller yet sizable, about 15%.

4 Empirical analysis

4.1 Endogeneity of childcare provision

My choice of empirical strategy stems from a basic simultaneity problem: while on the one hand parents' labor supply decisions, and by extension parents' labor market outcomes may depend on the local provision of affordable childcare services through their reservation wage, on the other hand, this supply is likely to adjust to changes in the demand, and thus to changes in the share of mothers who want to spend more time in the labor market, and less time taking care of their children,

[12] By contrast, all other observations correspond to individuals who do participate in the labor market.

and would likely rely on formal childcare services to do so. Because of this classical endogeneity problem, the causal effect of the provision of childcare on parents' labor outcomes is not identified from the mere correlation between parents labor outcomes and the local provision of childcare services. As a result, researchers have to resort to more sophisticated designs to identify this causal parameter of interest.

In this paper, I adapt the difference-in-difference design *à la* Duflo (2001) adopted by Havnes and Mogstad (2011a) to an event-study approach that takes advantage of the heterogeneity in the timing of childcare expansions across geographical areas. The remainder of this section aims at clarifying how I proceed and the underlying assumptions.

4.2 Granular childcare expansions

My empirical approach leverages the granularity of the national-level childcare expansion, i.e. the fact that (i) the smooth increase in the provision of childcare at the national level (Figure 1) is actually concentrated on a few municipalities in which the provision increased sharply, in contrast with most municipalities in which it remained flat (Figure 2); and (ii) among these municipalities in which the childcare provision increased massively, this rise is generally attributable to a single event, i.e. a sharp increase in the provision of affordable childcare between two consecutive years, for instance due to the opening of a new institution, as opposed to a continuous increase over the years.

To make this granularity more salient, and to take advantage of it, I first compute, for each municipality, the maximum growth in relative childcare supply $S_{c,t}$ between two consecutive years. 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 never experienced any growth in childcare supply of any kind between 2007 and 2015: as it turns out, these are to a large extent municipalities in which the supply is actually nonexistent all over the relevant time-period, plus very few municipalities in which the supply decreased due to the closure of an institution. Within municipalities that did experience some kind of growth, there exists a considerable heterogeneity in the magnitude of the maximum yearly growth: the 80th percentile of the distribution is equal to 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 in which the supply never increases (bottom 25%), those that rank between the 25th and the 80th percentile, then those that rank between the 80th and the 90th percentile, and finally the top 10%. This choice to separate groups according to the position in the distribution of a continuous variable is nothing but obvious; however it is somehow similar to that of Havnes and Mogstad (2011a) who choose to partition municipalities depending on whether they are below or above the median. Furthermore, and in contrast to theirs, my approach does not rely on heterogeneity across these groups.

I then recover 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 in which the supply never increases, the counterfactual treatment time is drawn randomly in the distribution of actual treatment timings in the other groups.

In Figure 5, I display the average relative supply of affordable childcare over time, within each treatment group, depending on the timing of the municipal childcare shock. Within the never treated group, this supply is roughly equal to 0 from 2007 to 2015. When it comes to the three other groups, the figure makes it salient 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 timing of the shock itself. Basically, the P25-P80 group gathers municipalities in which the supply was 16-18% and increased by 1 percentage points; the P80-P90 group gathers municipalities in which collective childcare institutions could cover about 20% of children, and increased their supply by 5 percentage points. Lastly, in the P90-P100 group, municipalities had a pre-shock coverage of 15-20%, and experienced a sharp 15 percentage points increase.

Another relevant point is that the evolution of the relative childcare supply is extremely flat both before and after the childcare shock. The contribution of municipalities to the national-level increase in the provision of affordable collective childcare thus mostly relates to these shocks, as opposed to a continuous increase in the local supply. In Figure 6, I decompose the yearly growth at the national-level between (i) the contribution of shocks for each treatment group; (ii) the residual contribution of within-municipality growth when municipalities do not

face a shock; and (iii) a composition shift.[13] It makes it extremely clear that the national-level increase is first and foremost the result of these shocks, especially of those of the top of the distribution. As a result, the analysis of the consequences of these shocks is to a large extent sufficient to recover the overall consequences of collective childcare expansions at the national level, and thus to the evaluation of the national *plans crèches* over the relevant time-period.

4.3 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 $g = g(c)$.[14] 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 city 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 makes the time effects 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.[15] 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 colinearity, the $\beta_{g\tau}$ coefficients are only identified up to a constant plus a linear trend.

As a solution against this underidentification problem, Borusyak and Jaravel

[13] In Appendix C, I detail this straightforward accounting decomposition.

[14] Because the relevant municipality is that 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 spots.

[15] One can replace municipality fixed effects with cohort (time-of-the-treatment) fixed effects without any change as to the identification properties of the model.

(2017) note that, in settings in which the assumptions that (i) the treatment is exogenous conditional on unit (here: municipality) fixed-effects, and that (ii) there are no anticipation effects are both credible, coefficients belonging to the subset $(\beta_{g\tau})_{\tau<0}$ should all be equal to 0. As a result, they advise to first estimate Model 2 while setting two coefficients of the subset to 0, which is akin to the approach to APC models proposed by Mason et al. (1973); this makes it possible to test the hypothesis that other coefficients are also equal to 0. Once made sure that this no-pretrend assumption holds, they recommend to estimate a semi-dynamic version of Model 2 in which all coefficients $(\beta_{g\tau})_{\tau<0}$ are constrained to 0.

Lastly, an important point of their paper is 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 negatively long-run effects: as a result, the estimator does not have the no-sign reversal property, so that even the sign of the effect can be wrong. Instead, they advise to first fit the semi-dynamic model, and then manually sum the coefficients of the subset $(\beta_{g\tau})_{\tau\leq 0}$, for instance with weights proportional to the sample size.

I follow closely their recommendation. My only departure is that as a first step, I do not normalize the pre-trend by setting two coefficients to 0. Instead, I rely on a solution to the underidentification of APC models proposed by Deaton and Paxson (1994). Specifically, my approach is basically to impose 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 $(\beta_{g\tau})_{\tau\leq 0}$ 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 (Abraham and Sun, 2018). In Appendix D.1.5, I show that moving to a correction based on a fully interacted model does not affect my results.

4.4 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 esti-

mates. 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:

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

When divided by 100, the λ parameter represents the causal effect of a 1 percentage point increase in the provision of childcare, expressed as the fraction of children aged 2 or less that are covered by local EAJE-PSU institutions, on their parents labor outcomes. This parameter also has an interpretation at the individual level, in an intention-to-treat sense: it corresponds to the effect of being offered a childcare spot[16] on parents' labor outcomes, for the restricted subset of parents that would not have been offered such a spot before the local childcare expansion, but actually are due to this shock. This interpretation rests on a Stable Unit Treatment Value Assumption that states that, within municipalities and conditional on whether they are assigned a childcare spot or not, labor supply decisions of parents be independent of the assignment of childcare spots to other families. In other words, there should be no peer effects in terms of labor supply, which is to some extent 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.

This fuzzy difference-in-difference framework has been recently investigated by econometricians who raise several issues as to its ability to identify causal parameters of interest (de Chaisemartin and D'Haultfœuille, 2018). In Appendix D.1.6, I discuss these concerns and provide with solutions to address them in the specific setting of this paper.

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

5 Parental earnings and labor supply effects

5.1 Graphical analysis

Figure 7 displays my estimates of the event-study approach to the labor earnings of mothers with children aged 0 to 2 respectively. First, I display my estimates of the full dynamic model, in which the pretrend is normalized following the approach proposed by Deaton and Paxson (1994). Such estimates allow me to verify that all coefficients that correspond to time-periods that predate the childcare expansions are not significantly different from 0, which is the case. In other words, within each treatment group, and before they are treated, the evolution of mothers' labor earnings is parallel across municipalities with different timing 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 that 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 the provision of affordable collective childcare, by considering the first two treatment group. Consistent with the rationale, I find that such shocks have no effect on the labor outcomes of mothers, which is reassuring as to the credibility of my identifying assumptions.[17]

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. Additionally, these estimates are much more precise than my semi-dynamic estimates, so that I am to a large extent able to rule out economically significant effects: in the P90-P100 group, the aggregate effect of collective childcare expansions on mothers' salaried employment rate cannot exceed

[17]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.

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, except that I rescale them using the magnitude of the childcare shock as a first stage.

Table 4 displays my estimates. Consistent with my previous findings, I cannot detect any significant effect of the provision of affordable collective childcare on parents', and especially mothers' labor outcomes. While my standard errors may be quite large when it comes to overall labor earnings, they are sufficiently small when it comes to 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. When restricting to the P90-P100 treatment group, the upper bound of the 95% confidence interval is 4.3 percentage points.

To make sure that these results are driven by municipalities that experienced substantial increase in the provision of collective childcare, as opposed to other cities in which 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, which means that they do indeed arise from the top of the distribution of childcare shocks.

Lastly, in Appendix D.1, I discuss a variety of concerns with respect to 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 landing a childcare spot does not induce massive changes as to the labor outcomes of mothers.

5.3 Empirical evaluation of the *plans crèches*

5.3.1 Aggregate effect

Between 2000 and 2016, the national plans that aimed at expanding the provision of affordable childcare enhanced the supply by 150 000 new childcare spots (IGAS/IGF, 2017). As I argue in Section 4, my approach is sufficient to capture the effect of all these plans, at least over the 2007-2015 period, given that my municipal childcare shocks represent almost all the increase in affordable collec-

tive childcare capacity at the national level. Hence, taking my most optimistic estimate, i.e. the upper bound of my 95% confidence interval, those newly created spots induced 8 000 more mothers of young children to hold a salaried job in 2016.

There were about 1.9 millions mothers with children under age 3 in 2016, with a salaried employment rate of 67%. As a result, a back-of-the-envelope calculation, based on the upper bound of the 95% confidence interval, suggests that the national *plans crèches* contributed to at most a 0.4 percentage point increase in the labor force participation of mothers between 2000 and 2016.[18] As a comparison, the counterfactual increase, had the efficiency of the plans reached that of the Norwegian policies investigated by Andresen and Havnes (2019), amounts to 2.5 percentage points. Empirically, this rate varied very little between 2007 and 2015: it fluctuated between 66% and 67%.

5.3.2 Cost-benefit analysis

Given that the average yearly operating cost of a full-time childcare spot in an EAJE-PSU institution is €15,000, of which parents contribute less than 20%, this represents at least a €230,000 public expenditure per year without career interruption, without taking into account the fixed costs that are paid to create additional childcare spots. As an alternate measure, my most optimistic estimate is that each additional euro of public expenditures directed towards the operating cost of childcare spots translates at most in a €0.18 increase in the earnings of mothers who benefit from these spots, my baseline estimate being €0.03. As I argue in Section 6, these estimates do not take into account substitution across childcare solutions that may dramatically change the picture.

6 Substitution across childcare solutions

I now investigate the crowding-out of other childcare solutions by daycare centers expansion, which can explain my null effects regarding 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 baby-sitters.

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

6.1 Paid parental leave

I take advantage of the CNAF dataset that provides information regarding the number of families that receive parental leave allowances at the municipality level as of 2009. Specifically, I divide this number by that of children aged 2 or less to recover a share of parents who benefit from parental leave allowances, either for a full-time or for a part-time parental leave.

I then apply my event-study analysis to this data, on a restricted subset of municipalities that experienced a childcare shock between 2010 and 2014.[19] Specifically, I implement the Abraham and Sun (2018) specification of the event-study design that allows for heterogeneous treatment effects across cohorts. Figure 8 displays my estimates. Consistent with the rationale, I find that the expansion of affordable collective childcare institutions does not trigger any substantial change as to the share of families with young children who benefit from parental leave allowances.

6.2 Individualized childcare

I rely on a cross-sectional and comprehensive version of the DADS dataset that provides me with information regarding earnings and hours paid to childminders and baby-sitters, who are paid directly by households, as of 2009. Specifically, I aggregate hours at the municipality level all along the 2009-2015 time-period.

Childminders are subject to a strict regulation in terms of child-to-adult ratios, as are collective childcare institutions. Specifically, in 2009 the law changed and moved the maximum number of children a childminder can take care of from 3 to 4.[20] As a result, I propose a measure of the relative supply of formal individual childcare at the municipality level as the total number of hours paid to childminders and baby-sitters, multiplied by 4 and, divided by (i) the number of annual hours of a full-time employment spell (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 baby-sitters 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. This measure approximates the concept of how many

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

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

hours childminders and baby-sitters work, relative to how much they would be working if all children were to be taken care of by them. It is nevertheless not a perfect measure of this relative supply concept, because: (i) in the law, the 4 children threshold includes children born to childminders, who I cannot observe; and (ii) childminders' capacity is fixed by an agreement quite similar to that of EAJE institution, that dictates a maximum number of children they can look after, and depends on their education, experience, equipment, so that 4 is an upper bound for this capacity. However, in 2014, the average number of children per childminder was 3.3 (Vroylandt, 2016) so that even though 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.[21] Here again, I implement the Abraham and Sun (2018) specification of the event-study design that allows for heterogeneous treatment effects across cohorts. Figure 9 displays my estimates. I find that in the medium run, in municipalities that experienced the largest shocks on the supply of collective childcare, substitution effects dominate: the demand for childminders and baby-sitters drops substantially. The magnitude of my estimates, about 13 percentage points, is almost equal to the magnitude of the collective childcare expansions at play (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.

In Appendix D.2, I assess the robustness of these results as to various concerns regarding the validity of my identification strategy, e.g. the correlation with other policy changes or division bias. I find my results regarding the take-up of parental leave and the demand for individualized childcare not to be affected by these issues.

6.3 Impact on public finances

The cost-benefit analysis of Section 5 relies on the operating cost of a childcare spot offered by an EAJE-PSU institution. However, in the French context in which collective childcare is only one among multiple subsidized childcare solutions, the analysis should take into account the counterfactual burden for public finances of the childcare solution families would have chosen had they not been offered a spot in an EAJE-PSU institution. Because of this, the national plans that aimed at

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

expanding the provision of collective childcare may prove beneficial, even with null effects regarding parental labor earnings and labor supply.

A severe limitation of my data is that it does not allow to disentangle childminders from at-home childcare provided by baby-sitters. Indeed, these two childcare solutions bear very different consequences as to public finances: broadly speaking, the cost for public finances of a spot offered by a childminder is 60% of that of an EAJE-PSU spot, whereas tax credits that subsidize at-home childcare induce a burden that represents 120% of the cost of an EAJE-PSU spot (Figure A.2).

I extend the cost-benefit analysis by taking into account these crowding-out effects, and the differential burden induced by substitution across childcare solution. Based on the medium-run effect of my event-study analysis, I assume a full crowding-out of individualized childcare solution, i.e. that every family who landed a collective childcare spot 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. Because 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 spot to be €15,000, of which 82% bear on public finances, (ii) the collective cost of a childminder spot to be 60% of this burden, and (iii) null effects on parental labor supply and full crowding-out, suggests that the long-run burden of creating 150 000 collective childcare spots amounts to an additional 738 millions euros of public spending per year. In the second case, i.e. assuming that the substitution effects only affect at-home childcare, and the burden induced by tax credits directed towards this childcare solution to represent 120% of that of a collective childcare spot, the long-run effect of this plan corresponds to a 369 millions euros reduction in annual public spending.

In Table 5, I sum up these results, also considering an alternate counterfactual scenario in which families who received a childcare spot thanks to the national plans are drawn randomly from the population of families that resort either to a childminder or to at-home childcare.[22] The interpretation of the implied benefit-cost ratios is not straightforward because it depends on the directions of two dif-

[22]Resorting to a childminder is more than 20 times more frequent than resorting to at-home childcare.

ferent effects. Firstly, it depends on whether the plans have increased or decreased salaried employment and salaried earnings of mothers: because 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 discarded, especially since families would resort to more costly and more flexible childcare solutions had they not been granted a collective childcare spot. These counterfactual solutions may therefore offer easier family-work conciliation than my treatment of interest – e.g. because the choice in hour coverage is wider than in EAJE-PSU institutions – and the decrease in childcare prices also leaves room to possible income effects.

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

Because substitution effects lower the impact of the plans on public finances, my estimates appear less precise that they were when omitting this crowding-out effects. The sensitivity of these estimates, and of the implied benefit-cost ratios, to assumptions made as to 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, without accounting for the fixed-costs paid to create additional collective childcare institutions. Additional data regarding individualized childcare would thus prove a valuable asset to implement a full policy analysis of these national plans.

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 registers, I show that such expansions did not trigger any substantial change in the labor earnings and labor supply of parents of the targeted age groups. Interpreted as a LATE, my instrumental variable estimates suggest that, among mothers who landed a collective childcare spot thanks to

these expansions, this treatment did not lead to a stronger attachment to the labor market. This is because the expansion of affordable collective childcare did not make mothers any less likely to benefit from paid parental leave allowances. I provide evidence that instead, these expansions resulted first and foremost in a substantial crowding-out of individualized and more costly childcare solutions.

Because these estimates are only informative about the choices of parents who were offered a childcare spot thanks to the national plans that I investigate, these results do not contradict the intuition that the lack of affordable childcare solutions may constrain some mothers to remain out of the workforce when they have young children (see Appendix A.2). Instead, they draw attention to the selection of recipients of these newly created childcare spots, who my results suggest would have otherwise relied on other formal childcare solutions.

Two channels may explain such results. The first one deals with selection into application: it might be that families who would most benefit from a spot are less likely to apply, which may stem from 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 induce some families not to apply for a collective childcare spot, even though landing a spot would actually change their childcare-work arrangements. The second entails selection into treatment: in this setting, among actual applicants, childcare spots would be more likely to be offered to families who eventually benefit less from them. Survey data suggests for instance that, because they aim at enhancing family-work conciliation, about two thirds of EAJE-PSU institutions 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 results in inefficiencies (Yamaguchi, Asai, and Kambayashi, 2018). Disentangling the two channels therefore bears relevant policy implications, but requires additional data regarding preferences towards childcare, application and selection into collective institutions.

Lastly, this empirical policy evaluation exercise does not take into account how childcare choices affect children themselves, which can result in substantial changes in the very long-run. Indeed, early childcare solutions may induce changes in children's skills set and health, and therefore affect their future education, labor market prospects and criminal behavior (see e.g. Havnes and Mogstad, 2011b; García et al., 2020). Taking these potential lifecycle benefits into account is required to achieve a correct normative analysis of these policies.

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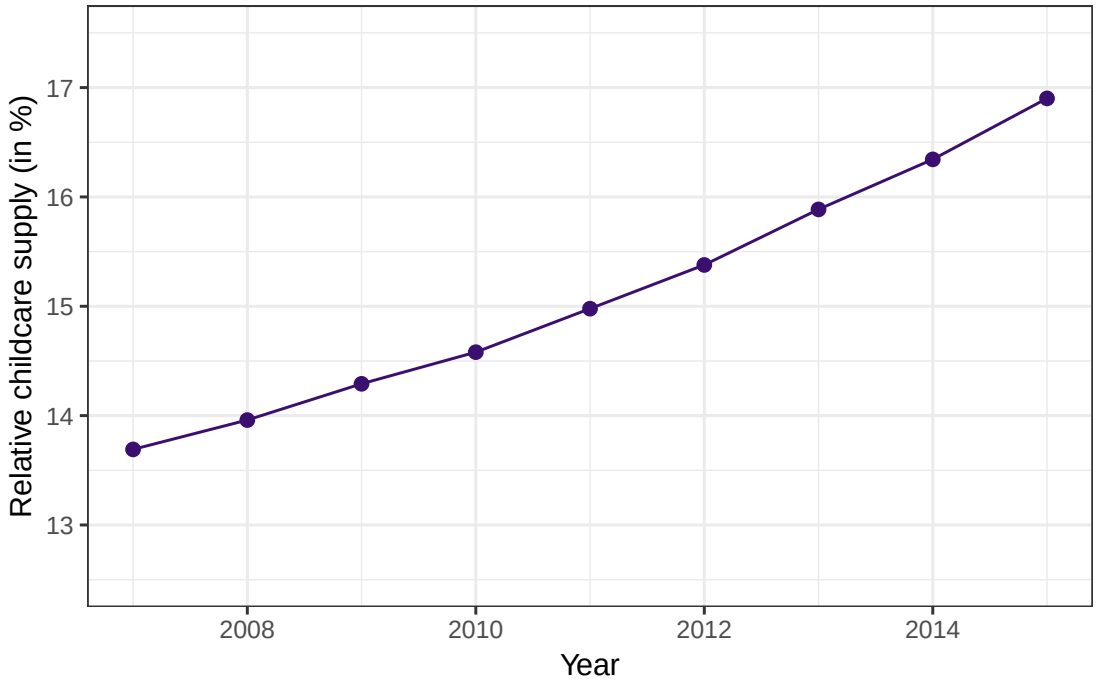
Figures

Figure 1 – Evolution of the relative supply of affordable collective childcare by EAJE-PSU institutions at the national level from 2007 to 2015

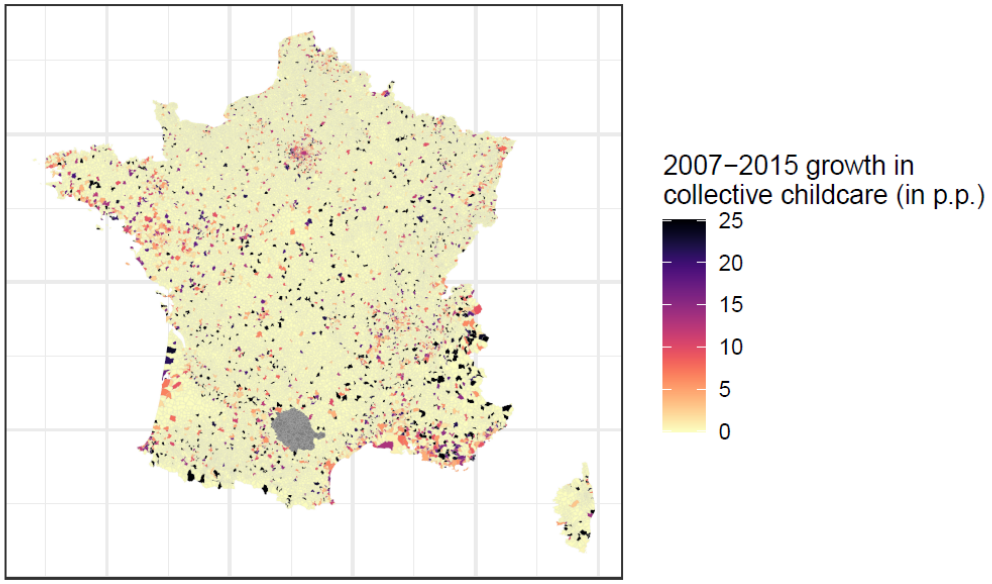


Estimates of the ratio of childcare spots offered by EAJE-PSU institutions to children aged 2 or less in metropolitan France.

Note. Data regarding the Tarn département is omitted.

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

Figure 2 – Spatial distribution of the 2007-2015 growth in relative supply of affordable collective childcare by EAJE-PSU institutions

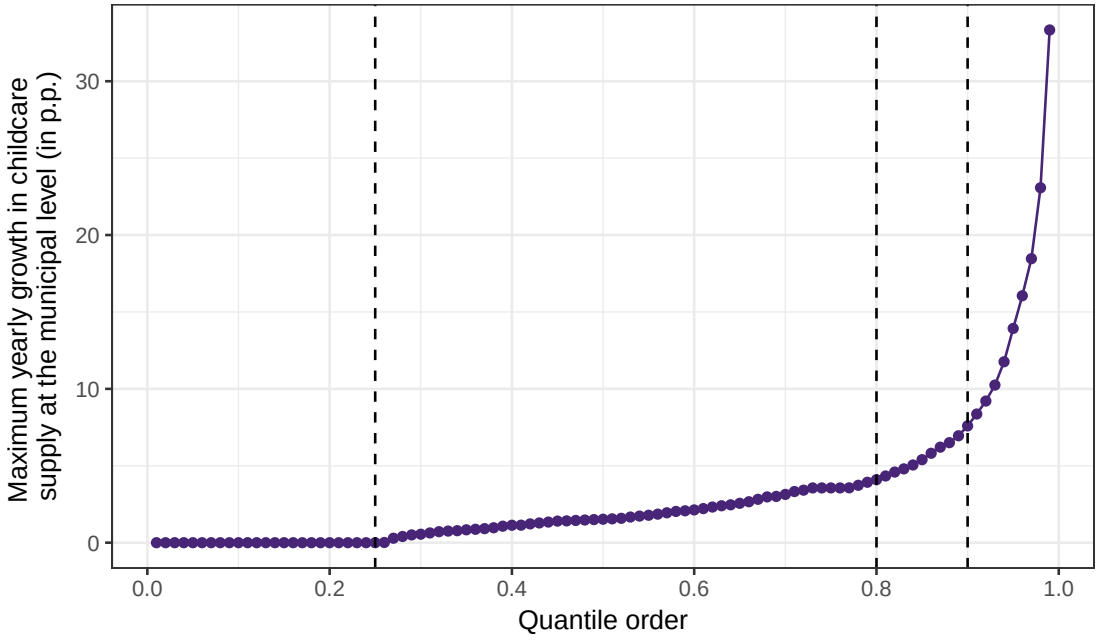


Estimates of the 2007-2015 growth in the ratio of childcare spots offered by EAJE-PSU institutions to children aged 2 or less at the municipality level.
Note. Data regarding the Tarn département is omitted.
Source. EAJE-PSU registers, CNAF. Birth registers, Insee.

Figure 3 – 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

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

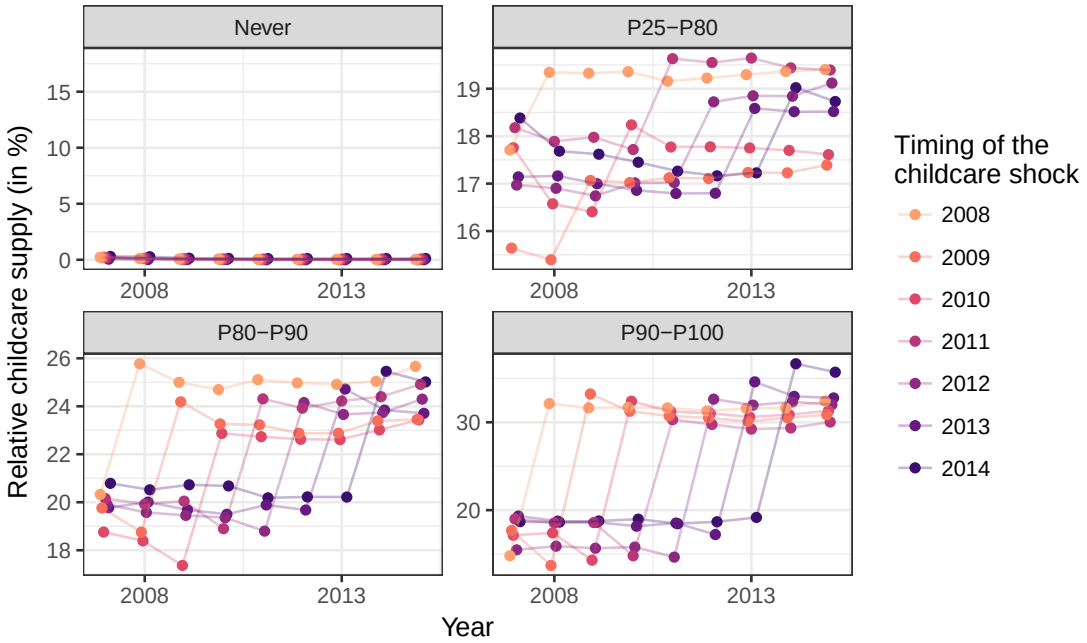


Estimates of the highest within-municipality growth in the ratio of childcare spots offered by EAJE-PSU institutions to children aged 2 or less.

Note. Data regarding the Tarn département is omitted.

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

Figure 5 – Evolution of the relative supply of affordable collective childcare by EAJE-PSU institutions, by treatment group and timing of the childcare shock

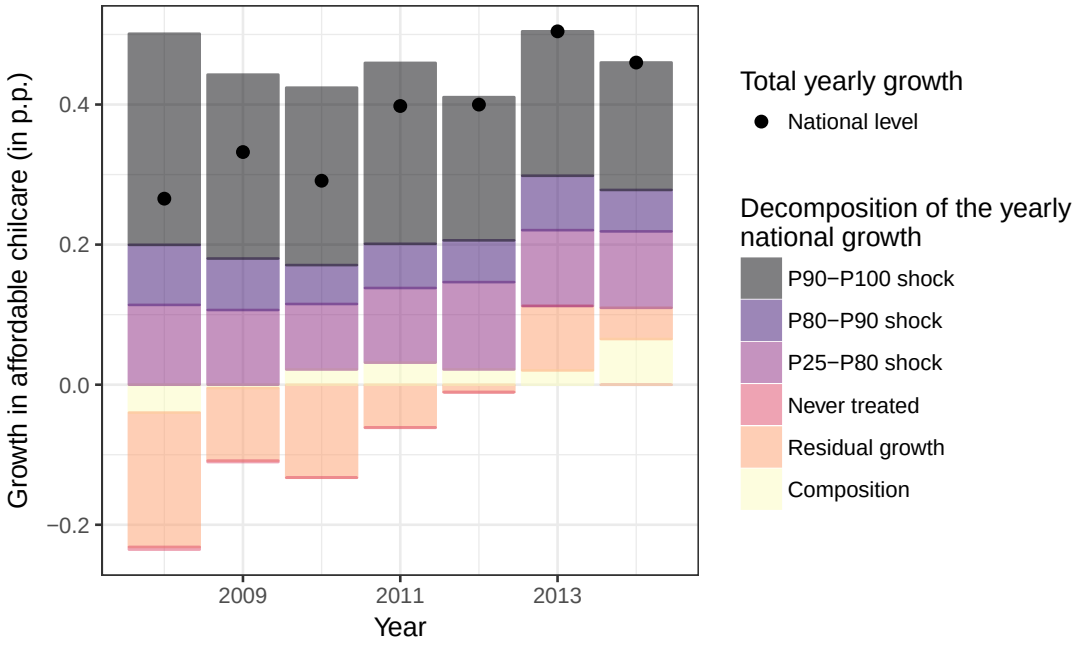


Estimates of the ratio of childcare spots offered by EAJE-PSU institutions to children aged 2 or less at the municipality level.

Note. Data regarding the Tarn département is omitted.

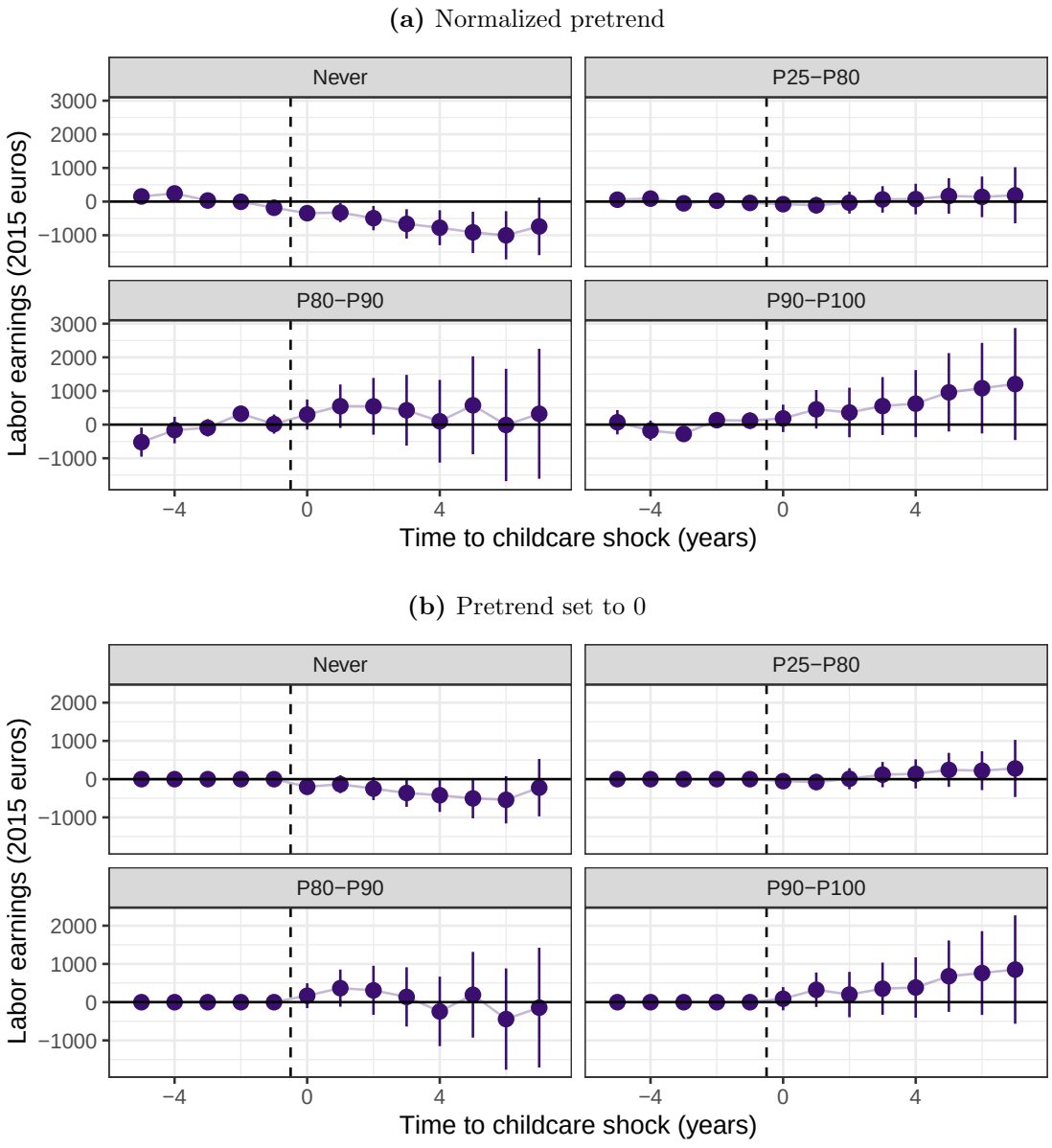
Source. EAJE-PSU registers, CNAF. Birth registers, Insee.

Figure 6 – Decomposition of the national-level yearly growth in the relative supply of affordable collective childcare by EAJE-PSU institutions from 2008 to 2014



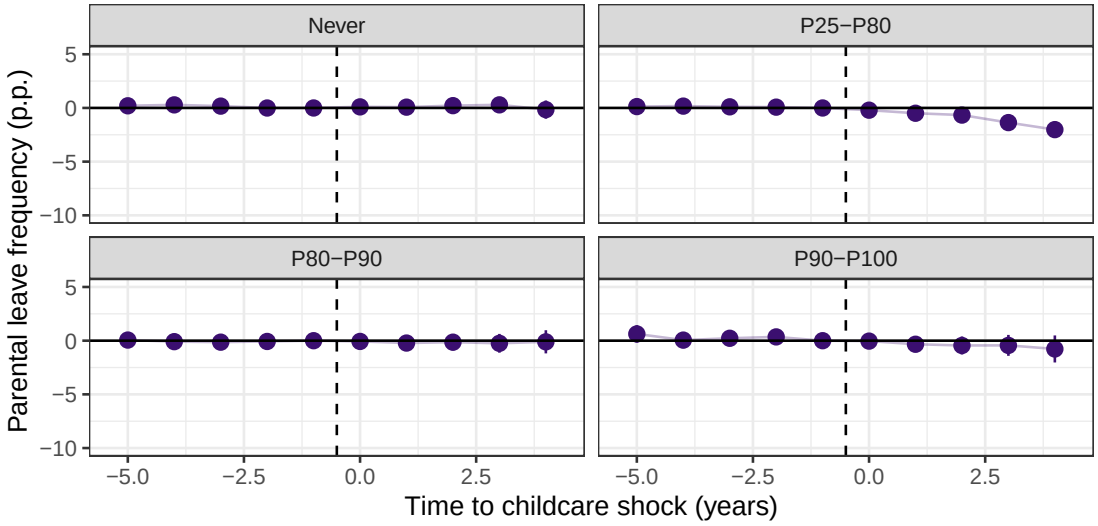
Estimates of the contribution of childcare shocks and composition shifts to the yearly growth in the supply of childcare by EAJE-PSU institutions at the national level (see Appendix C).
Note. Data regarding the Tarn département is omitted.
Source. EAJE-PSU registers, CNAF. Birth registers, Insee.

Figure 7 – Event-study estimates 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).
Note. Data regarding the Tarn département is omitted.
Source. EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

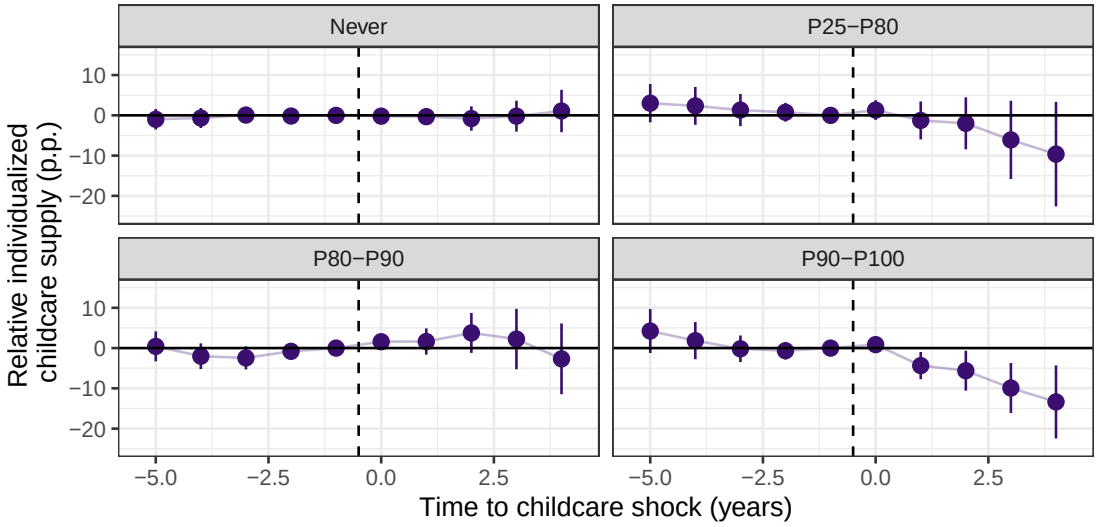
Figure 8 – Event-study estimates of the impact of the childcare shock on the take-up of paid parental leave, by treatment group



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

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

Figure 9 – 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 baby-sitters.

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

Tables

Table 1 – Data description

Dataset	Source	Main variables	Individual identifier	Municipality identifier
EAJE registers	CNAF	# childcare spots		✓
PAJE registers	CNAF	# familes who receive parental leave benefits		✓
DADS panel	Insee	Earnings, days and hours worked	✓	✓
DADS comprehensive registers	Insee	Earnings, days and hours worked, detailed occupation		✓
Birth registers	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
College graduates**	0.22	0.17	0.18	0.15
High-school drop-outs**	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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers 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. Childcare supply by EAJE-PSU institutions 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers 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 the 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

Table 5 – Empirical policy evaluation: counterfactual scenarios

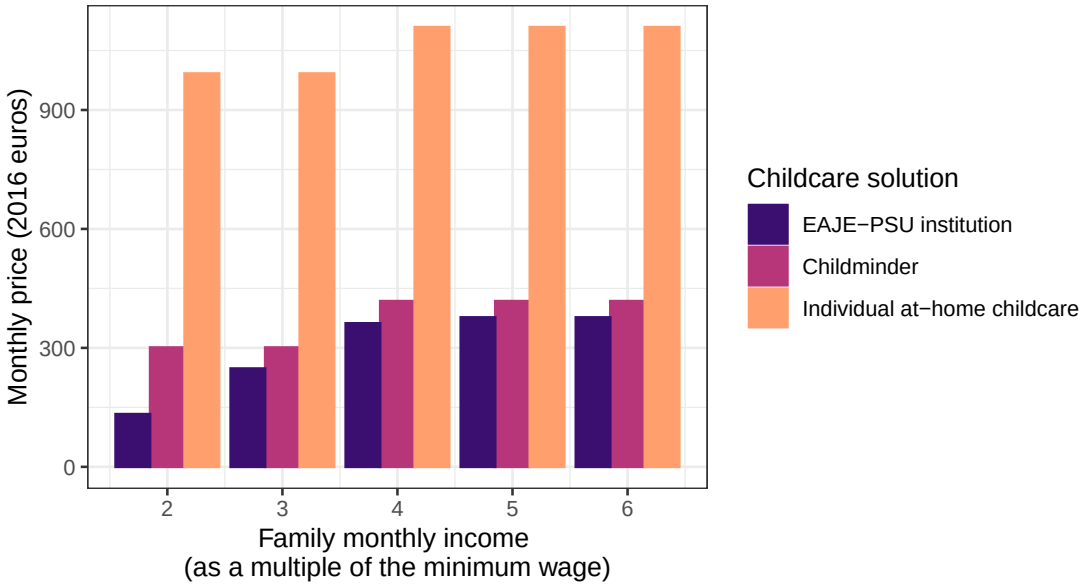
Counterfactual solution	No substitution	Childminders	At-home childcare	Mixed-case
*Long-run operating cost**				
For one spot (in €)	+12 300	+4 900	-2 500	+4 600
National plans (in M€)	+1 845	+738	-369	+688
*Benefit-cost ratio: additional years without career interruption per M€***				
Best case	+4.3	+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
*Benefit-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

*Without taking into account the fixed cost paid to create additional childcare spots. **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 is omitted. *Source.* EAJE-PSU registers and PAJE registers, CNAF. Birth registers, comprehensive DADS registers and DADS-EDP panel, Insee.

A Institutional background

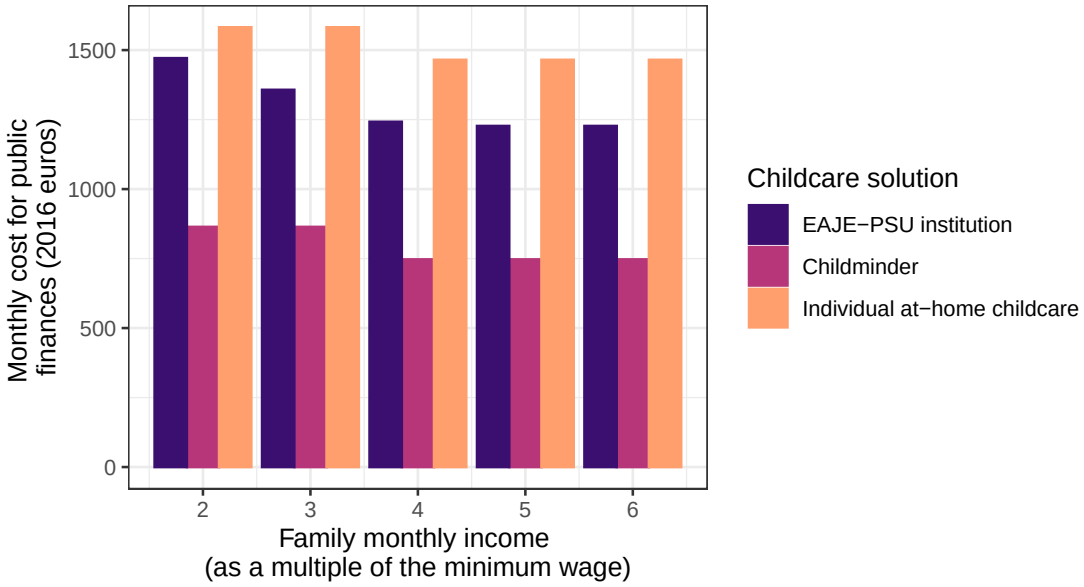
A.1 Childcare prices and costs

Figure A.1 – Childcare prices along the income distribution



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

Figure A.2 – Childcare costs for public finances along the income distribution



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

A.2 Childcare preferences and choices

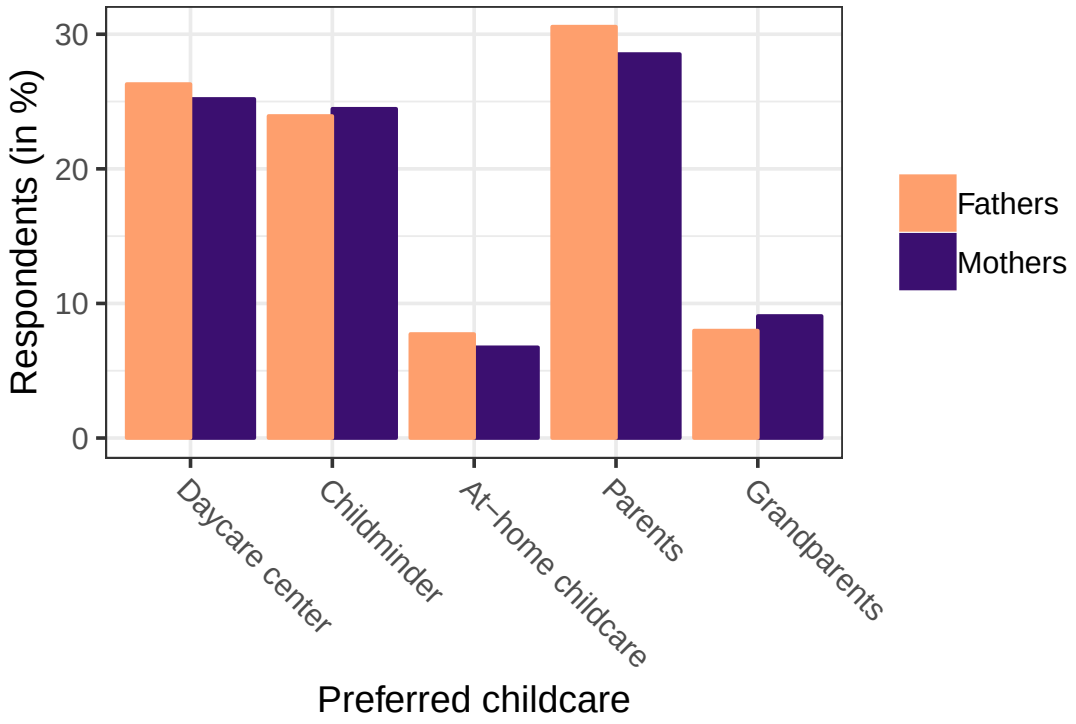
On top of these institutional pushes towards increasing provision of affordable, formal collective childcare, there is strong demand for these services. In 2010, Insee implemented a complementary module to the French Labor Force Survey (*Enquête Emploi*) devoted the family-work conciliation. In this survey, 1999 individuals with children younger than 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 this data to shed further light on potential demand for the kind of childcare solutions upon which this paper focuses.

Firstly, childcare provided by daycare-centers is among parents' preferred childcare arrangements: over 25% of both mothers and fathers of children younger than 3 mention it as the ideal childcare solution (Figure A.3). While the proportion of parents that view childcare provided by parents as ideal is slightly larger (about 30%), there is no other childcare solution that is more frequently mentioned by parents as their preferred option.

Secondly, while 67% of parents that indeed rely on this childcare arrangement view it as ideal, only 32% of parents that mention it as their preferred solution actually use it (Figure A.4). Additionally, parents who declare childcare provided by daycare-centers to be the ideal childcare solution are the most likely to report difficulties in access to childcare: 31% of them report such difficulties (Figure A.5), the most salient reason for these difficulties being the lack of availability of their desired childcare solution, which is mentioned by 70% of the latter.

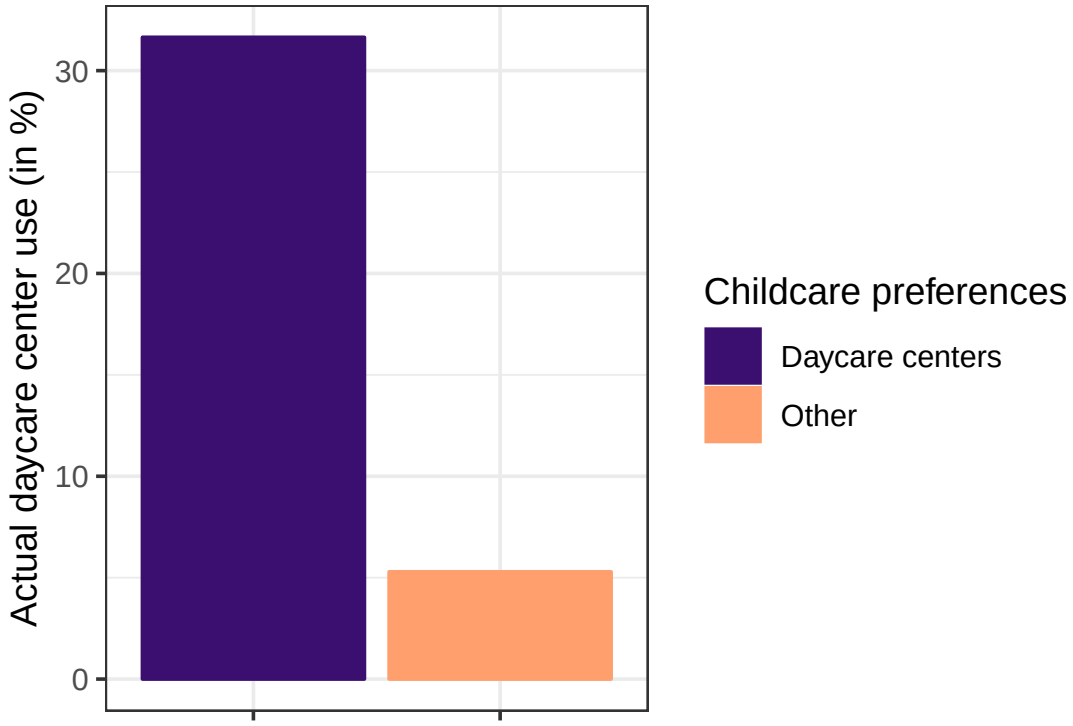
Lastly, among mothers of young children who are not working full-time, and who do not rely on childcare provided by daycare centers, those who view it as the ideal childcare solution are the most likely to report that either the insufficient availability, or the cost of childcare impacted their labor supply decision (Figure A.6).

Figure A.3 – Ideal childcare solution according to parents of children under age 3



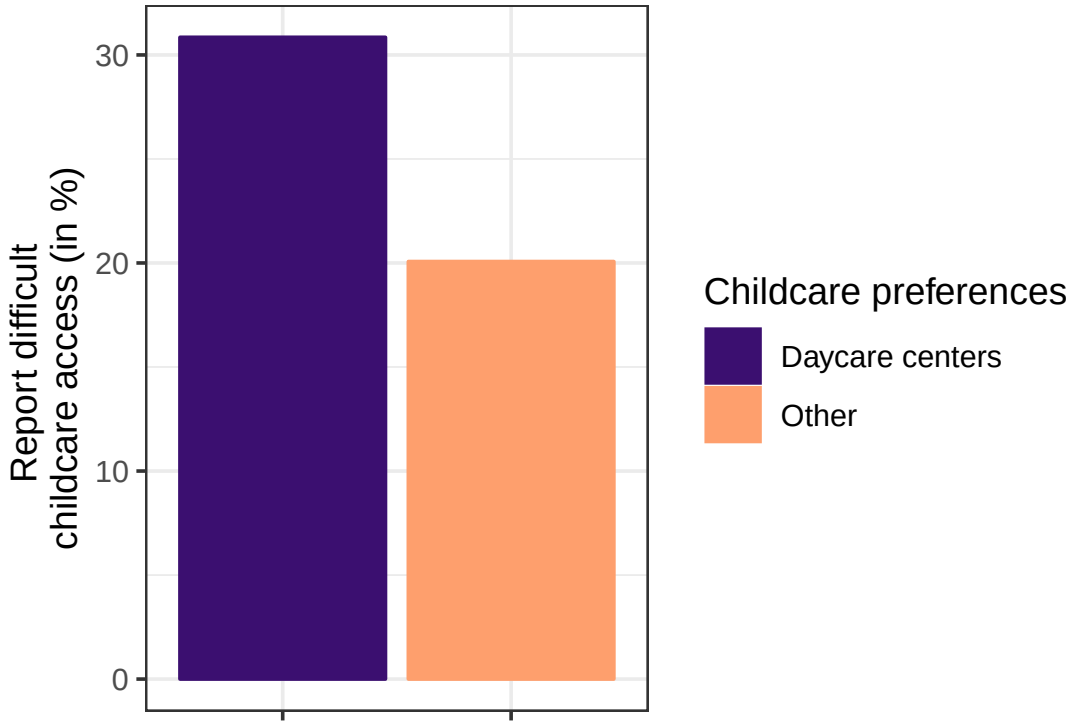
Ideal childcare solution for children aged the same as their youngest child, according to parents of children under age 3.
Source. LFS complimentary module 2010, Insee.

Figure A.4 – Actual childcare choices of parents of children under age 3



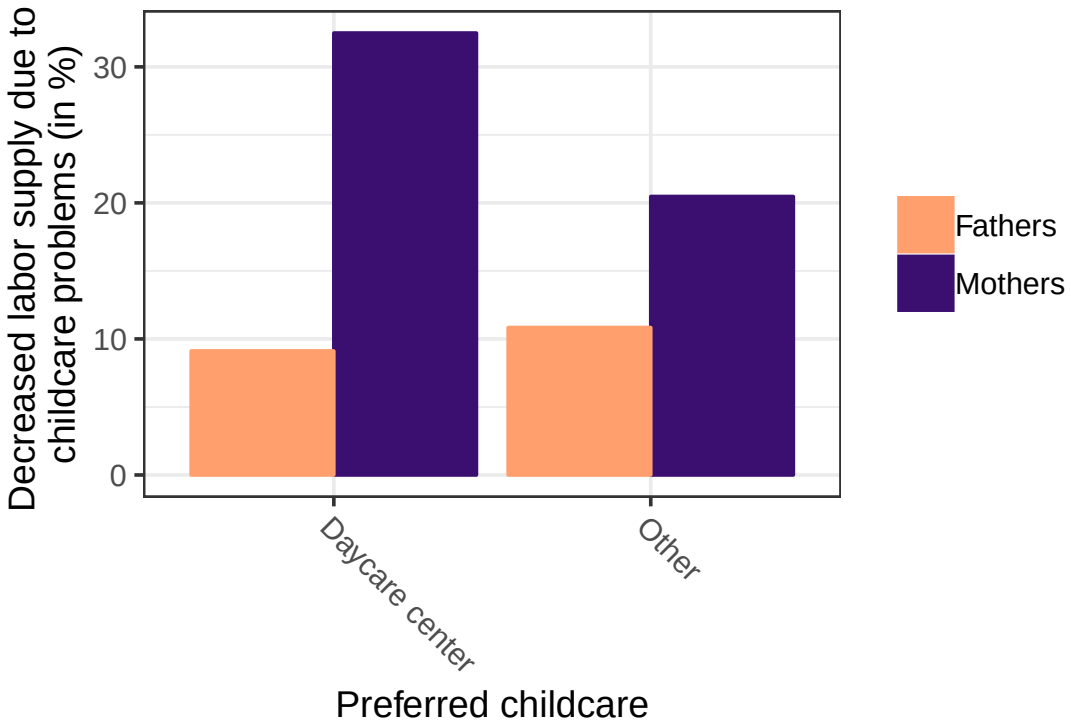
Actual childcare solution for parents of children under age 3, by preferred childcare solution.
Source. LFS complimentary module 2010, Insee.

Figure A.5 – 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.6 – Impact of the availability of childcare solutions 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 declare they decreased their labor supply due to the lack of appropriate childcare solutions, by gender and preferred childcare solution.

Source. LFS complimentary module 2010, Insee.

B Earnings and working time measures

B.1 Earnings

My measure of labor earnings relies on net annual earnings. This measure aggregates all wages paid to an individual, including performance pay and bonuses, paid vacations, 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 contributions, public pension schemes, unemployment benefits and other contributions including two flat-rate taxes on earned income (CSG and CRDS) are subtracted to this amount to compute our measure of net annual earnings. In that sense, I measure earnings before income taxes 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 this setting, 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 employer subsequently subtracts the maternity leave allowances that the employer advanced from the measure of earnings. Because the reimbursement occurs after the maternity leave itself, the decline in earnings may occur a few weeks after the maternity leave. Because I consider annual earnings, this problem is restricted to childbirths that occur at the end of the calendar year.

Lastly, in some firms the employer may be bound by collective agreement to complement earnings during maternity or sick leaves in addition to Social Security-provided allowances. This complement is part of labor earnings as measured by the DADS.

B.2 Days

In the DADS dataset, days paid refer to the duration during which an employee is part of the workforce of a firm within a given year. As a result, maternity and sick leaves, or paid vacations 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.

B.3 Hours

In the DADS dataset, hours paid refer to hours for which the worker is paid according to the 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 basis.

If one of these conditions does not hold, 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.

As to workers whose compensation does not depend on the time worked, but who do not belong to one of the above-mentioned occupations, i.e., typically managers ("forfait-jour"), employers provide the number of days only. A number of hours is ascribed to these observations based on the legal duration of work for full-time workers, the number of work days, and the implied hourly wages.

Because during a maternity leave, an employee is not paid by her employer for any hours but is instead paid by the Social Security Administration (and possibly receives a complementary payment from her employer), hours paid during a maternity leave are equal to 0. Workers who are not paid by the hour are an exception to this rule because their hours are imputed based on their days paid which do not vary during maternity leaves. 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 that belong to the upper part of the hourly wage distribution.

C Decomposition of the national yearly growth in childcare coverage

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

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

where N_t^{spots} denotes the number of childcare spots available at the national level, and N_t^{birth} the number of childbirths 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 yearly growth of childcare coverage at the national level can be decomposed:

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

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 by which municipalities with higher past childcare supply may expand faster or slower 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.

D Identification

D.1 Parental earnings

D.1.1 Measurement error

Regarding 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 such 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, up until they find a new job.

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

Table D.1 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes, by gender

Age of the 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

D.1.2 Other policy shocks and compositional shifts

My approach identifies the causal impact of collective childcare institutions on parents' labor outcomes to the extent that childcare shocks upon which it relies do not correlate with other changes that would affect the outcome. While it is not possible to test this assumption directly, it remains possible to verifiy 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 only stems 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 developed by Insee to delimit local labor market.[23] This is particularly useful given that my time-period of interest contains the Great Recession, of which the impact might be heterogeneous across local labor markets. Tables D.2 and D.3 display my results, which are consistent with my main estimates.

As an attempt 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 cohort (year of birth), education, and past fertility decisions, i.e. the total number of children.[24] Table D.4 displays my estimates, that are once again consistent with my previous findings.

[23] 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 the main part of the labor force necessary to occupy the offered jobs.

[24] 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, whereas it suffers from a left-censoring issue if they are not) and calendar time fixed effects to circumvent the left censoring issue mentioned in section 3

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

Age of the 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

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

Age of the 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

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

Age of the 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 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

D.1.3 Placebo groups

These checks are however insufficient 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 shifts in the composition of the 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 institutions target children aged 0 to 2, there should not be any direct effect of childcare expansions on their labor outcomes. Results for these groups appear alongside my main estimates in Table 4. Consistent with the rationale, I cannot detect any significant effect for these groups, which strengthens the credibility of my findings. If anything, it might be that the earnings of fathers increase in municipalities that experience the largest shocks. This would be the case if these massive expansions are more likely to occur early in municipalities in which the labor market grows steadily. However, this would suggest that my estimates regarding the labor market outcomes of mothers are biased upwards, which gives even more strength to my claim that positive maternal labor supply effects of childcare expansions are negligible at best.

D.1.4 Sampling issues

It could be argued that because (i) I focus on very modest expansion plans (see Section 2); (ii) I only rely on information as to 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 there are actually no parents who received a recently created childcare spot in my sample. The probability of this event is not 0 so that 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 spots were created between 2007 and 2015 (see Section 2). Assuming that (i) this increase was linear overtime (see Figure 1) and that (ii) childcare spots are reallocated every three years,[25] the increase resulted in 105 000 additional childcare allocation decisions. Second, my sample of parents is based on their birthday, i.e. whether they were born or not in the 16 relevant days of the EDP sample (see Section 3), so that the sample rate is 4.4%.

Consider $\hat{n}^{\text{allocations}}$ the number of additional childcare allocation decisions induced by the national increase in coverage that benefit parents of 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:

$$\hat{n}^{\text{allocations}} = \sum_{i=1}^{M} B_i \tag{9}$$

Given that the allocation of childcare spots does not depend on whether *parents* are born on the 16 EDP days or not,[26] 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 $\hat{n}^{\text{allocations}}$ writes $Mp(1-p)$. It is then easy to apply Chebychev's inequality:

$$\mathbb{P}(|\hat{n}^{\text{allocations}} - Mp| \geq \alpha Mp) \leq \frac{1-p}{\alpha^2 Mp} \tag{10}$$

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

[25] This is the most conservative estimate. If childcare spots are reallocated more frequently, then the probability of large deviations gets smaller.

[26] It may depend on their *children*'s date of birth given that childcare spots are frequently offered from September to July, as they are made available by children aged 3 leaving the EAJE institutions and beginning to attend preschool.

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

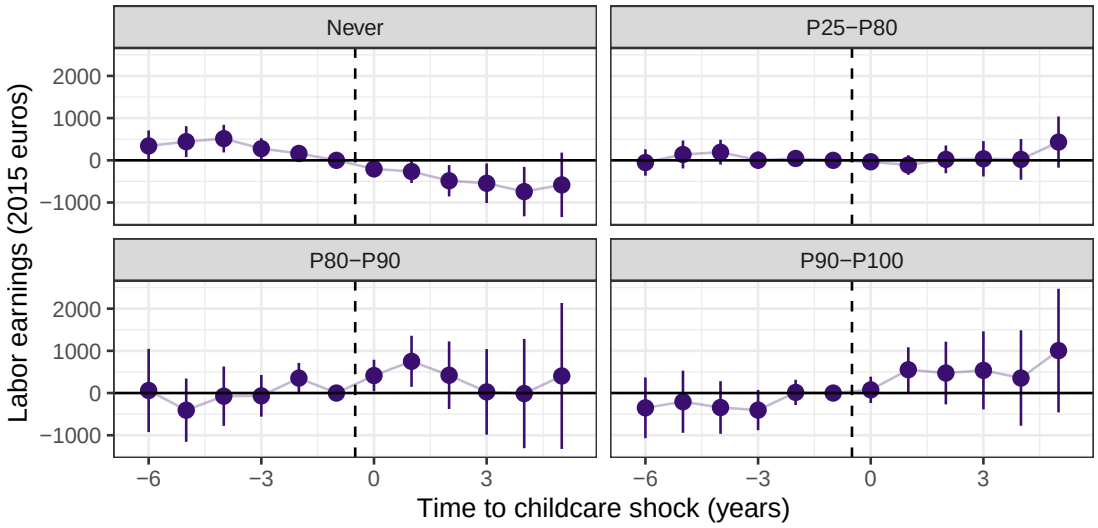
D.1.5 Event-study framework

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. This approach is however based on the premise that while treatment effects may be dynamic, i.e. vary depending on whether units are observed one year, two years, etc. away from 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, Abraham and Sun (2018) show that when this homogeneity assumption fails, the canonical event-study estimates are a weighted sum of cohort-specific treatment effects with possibly many 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, they propose to estimate a fully-interacted model, and then to manually average coefficients over cohorts using weights proportional to sample size.

Figure D.1 replicates my event-study analysis with their approach. My results are robust to this concern.

Figure D.1 – Event-study estimates *à la* Abraham and Sun (2018) 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 registers, CNAF. Birth registers and DADS-EDP panel, Insee.

D.1.6 Fuzzy difference-in-difference

The fuzzy difference-in-difference setting *à la* Duflo (2001) upon which my approach is based has been recently subjected to investigation by de Chaisemartin and D'Haultfœuille (2018), who arise concerns as to its ability to identify causal parameters of interest. They show that under usual assumptions, the canonical Wald-DID estimator only identifies a Local Average Treatment Effect[27] 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 enable 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 spot) be observed at the individual level; or (ii) that the outcome be continuously distributed. In my particular setting, I cannot observe whether individuals indeed resort to collective childcare spots, so that I cannot use individuals who land a childcare spot 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 in 0. It appears therefore impossible to rely on the alternative estimators developed by de Chaisemartin and D'Haultfœuille (2018).

However, it is likely that this problem is not a major threat to my identification strategy. First, the treatment rate varies very little but for the shock, as made evident by Figure 5, so that deviations from the assumption that this treatment rate is actually constant are small if anything. Second, it is possible to replicate my approach in a setting in which, in the control group of municipalities, the childcare coverage rate is constant by assumption. To this end, I restrict to municipalities that did not have any EAJE-PSU institution in 2007, and in which an institution opened at some point between 2008 and 2014, and define my childcare shock as the opening of this first institution. In this setting, the treatment rate in the control group, i.e. in municipalities in which a collective childcare institution will open at some point, but has not done so yet, is by construction equal to 0.

Table D.5 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

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

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

Table D.5 – Instrumental variable estimates of the impact of affordable collective childcare on parents' labor outcomes based on the opening of the first EAJE-PSU institution, by gender

Age of the 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 is omitted. *Source.* EAJE-PSU registers, CNAF. Birth registers and DADS-EDP panel, Insee.

D.2 Substitution effects

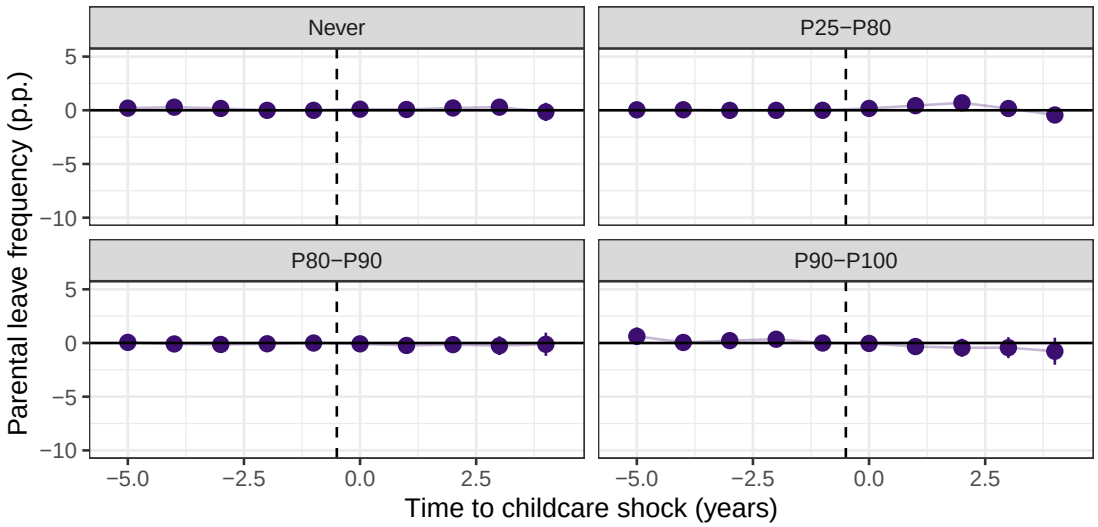
D.2.1 Placebo groups

Similarly to what was the case when investigating labor outcomes effects, my partition of municipalities in 4 treatment group allows me to consider effects in municipalities in which 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 the 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 the take-up of paid parental leave.

D.2.2 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 supplies of different types of childcare soàlutions would 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 D.2 and D.3 display my estimates. I find that this does not change my results.

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

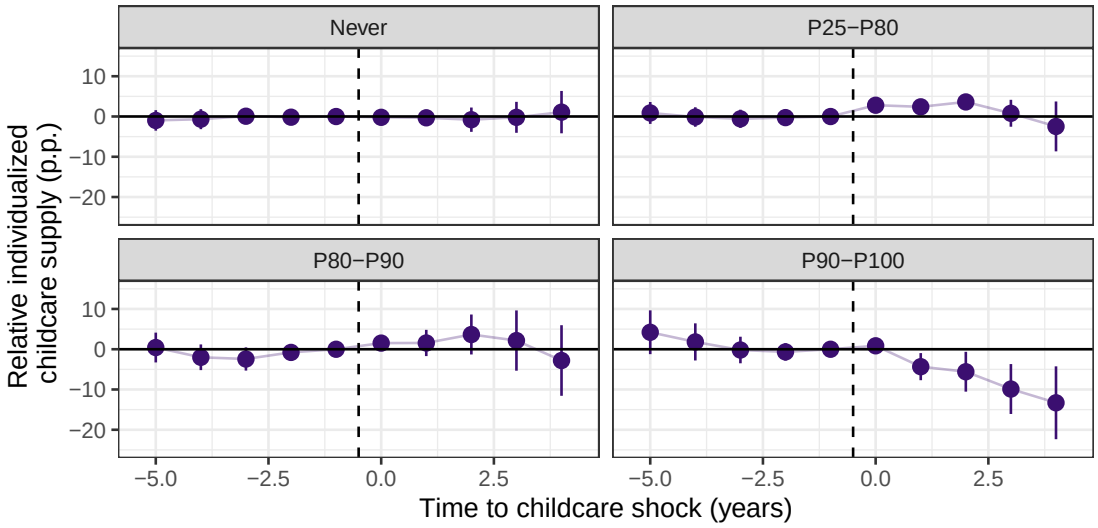


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less that receive parental leave allowances in December.

Note. Data regarding the Tarn département is omitted.

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

Figure D.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 baby-sitters.

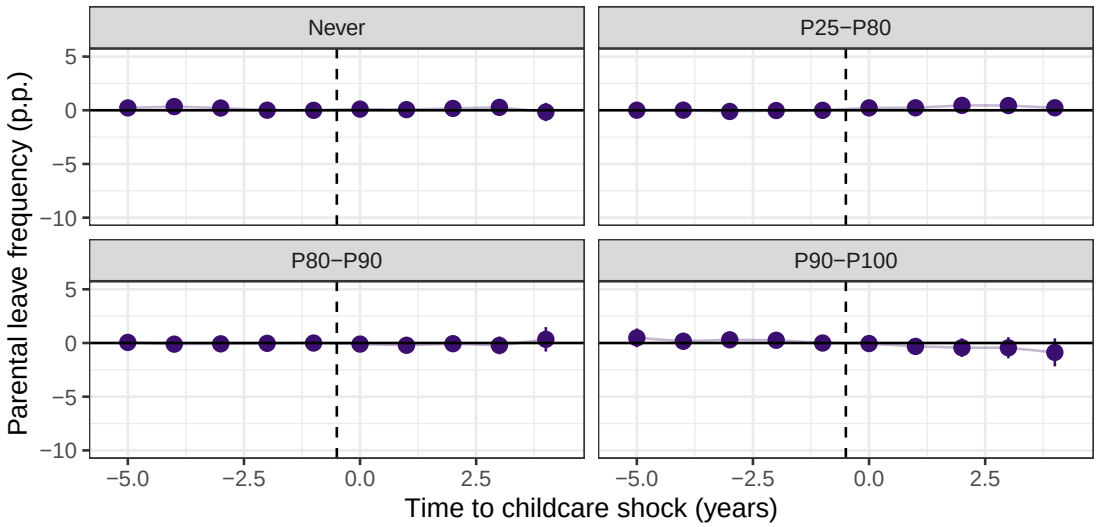
Note. Data regarding the Tarn département is omitted.

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

D.2.3 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 département, the level at which local offices of the CNAF operate, Zones d'emploi, to consider local labor market effects, and lastly *Bassins de vie*, a geographical unit defined by Insee that captures the provision of local everyday services at a narrow geographical level. For all these levels, I replicate my analysis while adding treatment group × calendar time × geographical unit fixed effects. Here again, this requires moving to the baseline specification of the event-study design, which creates some bias in my estimates of the short-run effects. Figures D.4 to D.9 display the corresponding estimates. I find qualitatively similar effects, which supports the validity of my identification strategy.

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

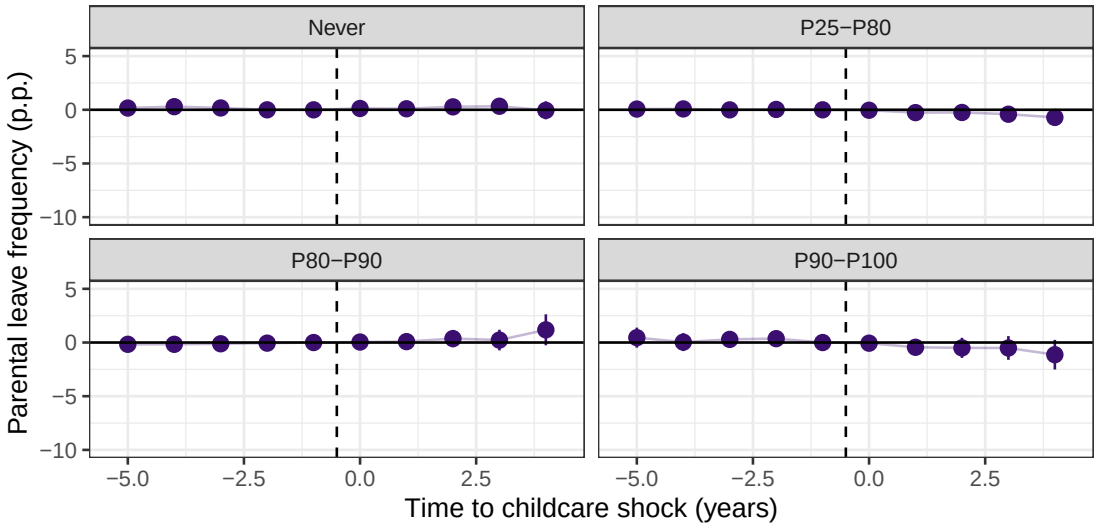


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less that receive parental leave allowances in December.

Note. Data regarding the Tarn département is omitted.

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

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

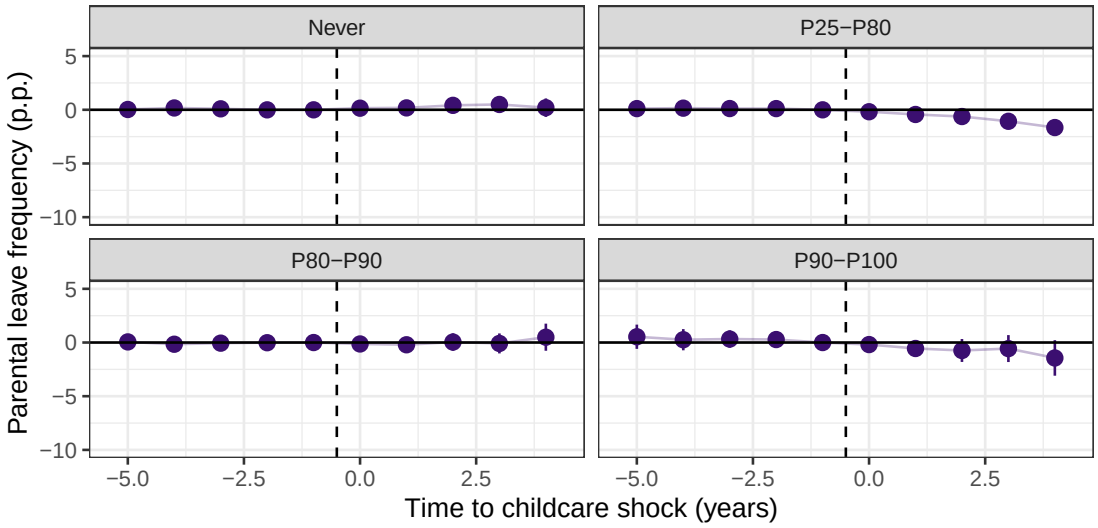


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less that receive parental leave allowances in December.

Note. Data regarding the Tarn département is omitted.

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

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

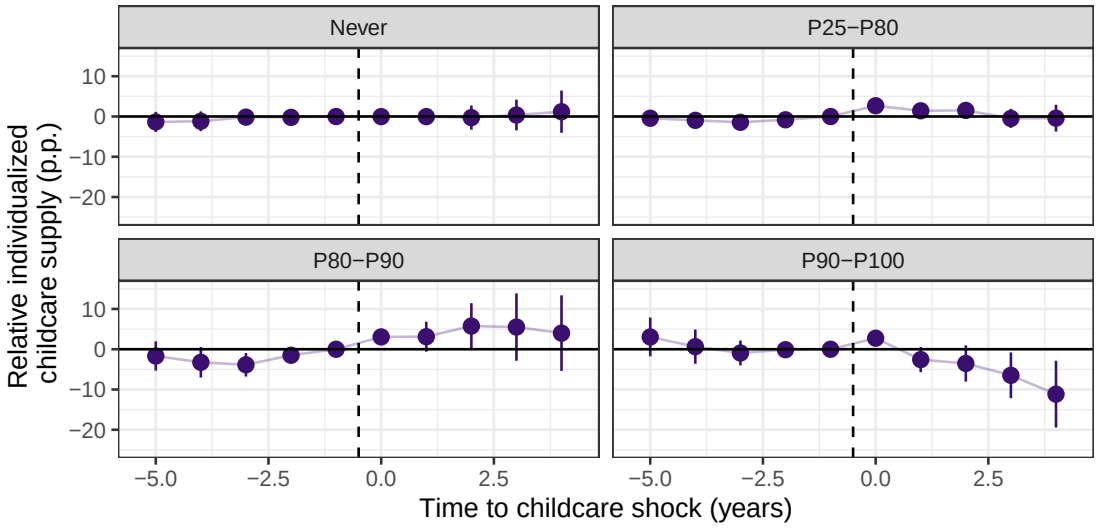


Event-study estimates of the effect of childcare shocks on the share of families with children aged 2 or less that receive parental leave allowances in December.

Note. Data regarding the Tarn département is omitted.

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

Figure D.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

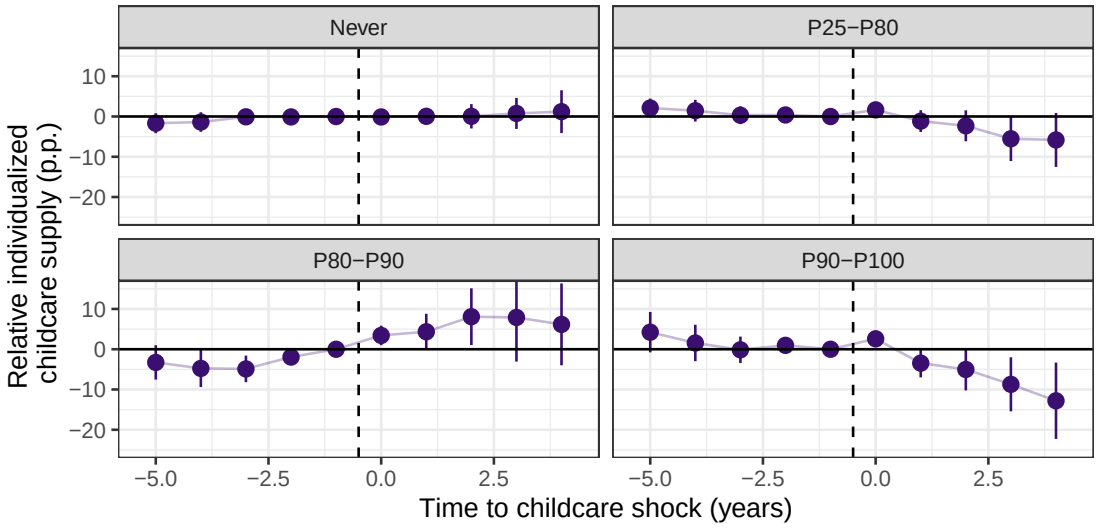


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and baby-sitters.

Note. Data regarding the Tarn département is omitted.

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

Figure D.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

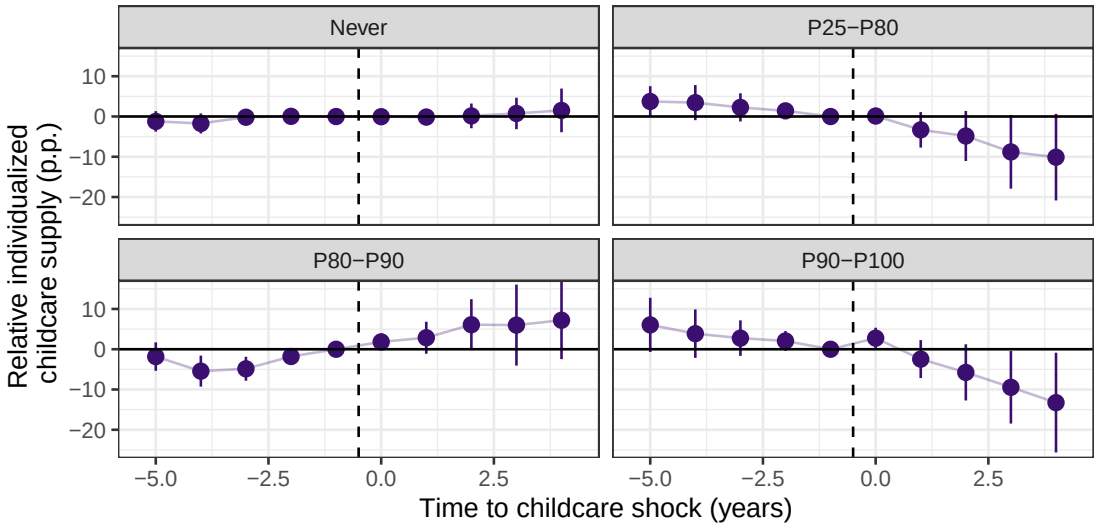


Event-study estimates of the effect of childcare shocks on the relative supply of individualized childcare by childminders and baby-sitters.

Note. Data regarding the Tarn département is omitted.

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

Figure D.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 baby-sitters.

Note. Data regarding the Tarn département is omitted.

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

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